

Calculation and Tabulation in the Nineteenth Century:  
Airy versus Babbage

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## Abstract

Charles Babbage (1791-1871) is widely recognised as the great ancestral figure in the history of computing. He designed the first automatic calculating engines and failed to realise a complete design in physical form.

This thesis argues that historical accounts of Babbage's work are based on a set of contemporary sources provided by Babbage himself and that the subsequent interpretation of the major movement to automate calculation and tabulation in the nineteenth century is dominated by Babbage's own account of events. George Biddell Airy (1801-1891), Astronomer Royal for forty five years, consistently rejected arguments advocating the utility of the engines. Airy had a defining influence on the fate of the engines yet his views barely feature in the canon. Using new archival sources, Airy's views are explored through a series of case studies: Babbage's Difference Engine No. 1 and the Swedish calculating engine used at the General Register Office for the 1864 Life Table. In addition, his views on the use of calculating aids are explored using instances in which he was petitioned by inventors, specifically by Thomas Fowler and William Bell.

The thesis situates Airy's views in the context of manual methods used in the production of printed mathematical tables – techniques that automatic calculating engines were intended to replace – and in the context of contemporary expectations of the largely unbuilt engines. The treatment includes new work on the mathematical implications of the engines, specifically Babbage's speculations on computation as a systematic method of solution, and presents a revisionist view of tabular errors as the primary motive for the development of the engines.

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'I wish to god these calculations had been executed by steam'

– Charles Babbage, 1821

'What shall we do to get rid of Mr. Babbage and his Calculating Machine?'

– Sir Robert Peel, 1842

'I think it likely that he lives in a sort of dream as to its utility'

– George Biddell Airy, 1842

'Professor Airey says the thing is a humbug; other scientific men say directly the contrary'

– William Charles Macready, 1837

'There exists no memory except upon paper'

– George Biddell Airy, 1854

'It is the Age of Machinery, in every outward and inward sense of that word'

– Thomas Carlyle, 1838

## Abbreviations

The following abbreviations are used in footnotes to refer to frequently cited sources.

- Biog.* Airy, Wilfrid, ed. *Autobiography of Sir George Biddell Airy*. Cambridge: Cambridge University Press, 1896.
- CDB* *The Concise Dictionary of National Biography*. Oxford: Oxford University Press, 1994. 3 vols.
- CWB* Swade, Doron. *The Cogwheel Brain: Charles Babbage and the Quest to Build the First Computer*. London: Little, Brown, 2000.
- Expo* Babbage, Charles. *The Exposition of 1851 or Views on the Industry, the Science, and the Government of England*. 2 ed. London: John Murray, 1851.
- G&F* Lindgren, Michael. *Glory and Failure: The Difference Engines of Johann Müller, Charles Babbage, and Georg and Edvard Scheutz*. Trans. McKay, Craig G. 2 ed. Cambridge, Massachusetts: MIT Press, [1987], 1990.
- GoS* Morrell, Jack, and Arnold Thackray. *Gentlemen of Science: Early Years of the British Association for the Advancement of Science*. Oxford: Clarendon Press, 1981.
- LEC* Collier, Bruce. *The Little Engines that Could've: The Calculating Engines of Charles Babbage*. 2 ed. New York: Garland, [1970], 1990.
- Notice* Fowler, Hugh. "Biographical Notice of the late Mr. Thomas Fowler, of Torrington with some account of his inventions." *Transactions of the Devonshire Association for the Advancement of Science, Literature and Arts* 7 (1875): pp. 171-178.

- Passages* Babbage, Charles. *Passages from the Life of a Philosopher*. London: Longman, Green, Longman, Roberts & Green, 1864.
- PC* Hyman, Anthony. *Charles Babbage: Pioneer of the Computer*. Oxford: Oxford University Press, 1982.
- Works* Campbell-Kelly, Martin, ed. *The Works of Charles Babbage*. 11 vols. London: William Pickering, 1989.

## Introduction

*Another age must be the judge*

– Charles Babbage, 1837.

The starting point of this work lies in curatorial life in a museum, in this case the Science Museum, London, where I worked as curator of computing for fourteen years. The central article of faith in museums is a belief in the primacy of original artefacts. Physical artefacts are the *explanandum* of curatorial life, and history is one of the main discourses that curators invoke to ‘explain’ artefacts and articulate their meanings.

The cultural environment of object-rich museums and the constituencies from which curators are recruited encourage a technocentric approach to history, especially in science museums. This has certainly been the case in the past, though noticeably less so latterly, as history graduates have taken their place alongside science and engineering graduates, reflecting perhaps the cultural and intellectual movements of relativism and contextualisation that began to occupy the methodological high ground in the 1960s and 1970s. However, the internalist legacy, though weakened, is not dead, and to some the thesis that ‘machines make history’, the rallying call of the technological determinist, still operates as a subjective presupposition even to those with a self-conscious knowledge of the narrowness of this view.<sup>1</sup> Physical artefacts, specifically the mechanical relics, incomplete assemblies, and a large technical archive are the material legacy of

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<sup>1</sup> The quoted phrase is borrowed from the title of Robert Heilbroner’s classic paper “Do Machines Make History?”. See Heilbroner ([1967], 1994). For a discussion of the ‘tenacious grip [of technological determinism] on the popular and academic imagination’ see Bimber (1994), p. 80.



Charles Babbage's efforts to build his calculating engines. This material provided the initial stimulus for this work.

Charles Babbage (1791-1871) is widely recognised as the great ancestral figure in the history of computing.<sup>2</sup> He famously invented computers – vast automatic mechanical calculating engines – and equally famously failed to build them. Genius and failure are inextricably linked with his name. He failed despite substantial government funding, decades of design and development, the benefit of the finest engineering, and the social advantages of an affluent and well-connected Victorian gentleman of science.

Summary histories of computing overwhelmingly ascribe the reasons for Babbage's failures to the limitations of Victorian mechanical engineering. Martin Davis, for example, writes, 'Babbage never succeeded in constructing his engine, in large part because of the limitations of nineteenth century technology'.<sup>3</sup> A history of the Science Museum published in 1957 states that '[Babbage's] ideas were eminently sound and it was only the backwardness of light-engineering and instrument-making techniques . . . which robbed him of the success he deserved'.<sup>4</sup> Nathan Rosenberg, in *Perspectives on Technology*, writes, 'Babbage's failure to complete his ingenious scheme was due to the inability of contemporary British metal-working to deliver the components which were indispensable to the machine's success'.<sup>5</sup>

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<sup>2</sup> There has been confusion about the date and location of Babbage's birth. He is variously reported as being born in London, Teignmouth or Totnes in 1791 or 1792. Hyman resolved this by reference to parish baptismal records and established that Babbage was born in Walworth, Surrey on 26 December 1791. See *PC*, p. 11. Errors continue in secondary sources as well as many biographical reference works.

<sup>3</sup> Davis (1988), p. 152.

<sup>4</sup> *The Science Museum: The First Hundred Years*. London: HMSO, 1957, p. 48. Quoted in *G&F*, p. 258.

<sup>5</sup> Quoted in *G&F*, p. 258. See also Aiken (1946), p. 7.

Most assertions of this kind do not identify the specific deficiencies of contemporary technology, and the 'limitations of technology' thesis is sometimes taken to imply that parts could not be made with sufficient precision for a technically viable machine. George Julius, for example, inventor of the Julius Totalisator for dog-track betting, explicitly identifies 'limitations' with manufacturing precision: 'One of the greatest obstacles that Babbage had to contend with in his work was the difficulty of obtaining accurately-cut gears and accurate machine work generally'.<sup>6</sup>

The 'limitationist' view offers the convenience of brevity and the appeal of causal simplicity. However, its narrowest interpretation, the unachievability of requisite precision, has become increasingly untenable even in its own self-referential terms. Those knowledgeable about nineteenth century workshop practice do not accept that precision was a limiting consideration. The successful construction of Babbage's Difference Engine No. 2, completed in 2002 at the Science Museum, supports this view.<sup>7</sup> This machine was built to Babbage's original designs dating from 1847-9. Care was taken to make parts to no greater precision than is known from measurement to have been achievable by Babbage himself, and in several cases parts are deliberately less precise. That the machine works supports the view that the designs were sound and moreover that the requisite precision and repeatability were achievable using the practices and machinery available in the mid-nineteenth century.

While the narrow interpretation of the limitationist thesis is weakened by this piece of experimental history the limitationist thesis overall is arguably defensible, to some extent at least, if the notion of 'technology' is broadened beyond issues of

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<sup>6</sup> Julius (1920), p. 14.

<sup>7</sup> For an account of the construction see *CWB*, Chapters 12-18. Also, Swade (1993).

achievable precision to include less readily quantifiable factors in contemporary manufacturing: issues of standardisation and the interchangeability of parts; the implications of standardisation for the social organisation of labour; the perceived need to operate at the limits of available technology; and the lack of techniques with inherent repeatability (stamping and die-casting, for example) to economically factor hundreds of near-identical parts in a manufacturing culture in transition between craft and mass production. But still, by and large, these considerations, narrow and broad, belong to the technological determinist's position which accounts for the fate of the engines in technological terms.

The starting point of this thesis is the collapse, actual or notional, of the technological determinist's account of Babbage's failure to complete any of his engines in physical form, and this work is an attempt at what might fill at least some of the space vacated by the technological determinist's account.

Inverting the thesis that 'machines make history' is the notion that 'history makes machines' – that cultural, social, political and economic considerations are the essential factors in historical causation insofar as the quest for historical cause is a legitimate pursuit at all.<sup>8</sup> Once the technological determinist account ceases to occupy the centre-ground, multiple contributory causes rush to fill the space. There is Babbage's allegedly difficult personality, the lack of credible progress after decades of design and development, massive public expense, the political intrigues of scientific life, allegations of personal vendettas, problematic funding, runaway costs, bad management, an unresolved dispute between Babbage and his engineer Joseph Clement, an unfavourable entrepreneurial climate, political instability in an age of political and scientific reform, the cultural divide between

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<sup>8</sup> John Vincent argues that 'cause is simply the wrong word to use, it raises the hopes of unattainable accuracy'. Vincent (1995), p. 46.

pure and applied science, and disagreement amongst experts on whether automatic calculating machines offered any benefits at all. Fighting for air under the rubble of contributory cause is the figure of George Biddell Airy (1801-1892), Astronomer Royal from 1835-1881. His views were a major determinant in the fate of the calculating engines. Yet he has been largely unheard.

The historical canon treating Babbage's efforts to build calculating engines is founded on five main contemporary texts: a dauntingly long paper by Dionysius Lardner, published in the *Edinburgh Review* in 1834 that provides the most extensive contemporary account of Babbage's Difference Engine and its state of progress to that time; a statement drawn up in 1843 by Sir Harris Nicolas chronicling the fraught circumstances of the engine project from 1822 until the final withdrawal of Government support in 1842; a statement by Babbage published anonymously in the *Philosophical Magazine* in 1843; Richard Weld's *History of the Royal Society* published in 1849, chapter eleven of which is devoted to an account of Babbage's engine project; finally, a biographical work by Harry Wilmot Buxton written between 1872 and 1880, not published till 1988.<sup>9</sup>

These sources largely recount the same set of circumstances and agree in their essential features: the inspired conception in 1821 of automatic calculation by machinery at a meeting between Babbage and John Herschel;<sup>10</sup> Babbage's declared understanding based on an unminuted interview in 1822 with John Robinson, Chancellor, that the government had committed to fund the engine to completion; three favourable reports by Royal Society committees in 1823, 1829 and 1830, attesting the feasibility, progress and prospective utility of the engine; the halt in 1833 of the construction when Joseph Clement, Babbage's engineer,

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<sup>9</sup> For the background to the publication of the Buxton Memoir see Hyman (1988), p. xiii.

downed tools; and the final withdrawal of Government support in 1842 following a meeting between Babbage and the Prime Minister, Robert Peel.

Though four of the accounts are authored by hands other than Babbage's they are, without exception, directly based on sources provided to the authors by Babbage himself: Lardner and Babbage collaborated closely on the content of the article;<sup>11</sup> Babbage states openly that Nicolas's account 'was drawn up . . . from papers and documents in my [Babbage's] possession';<sup>12</sup> Weld invited Babbage to give his account and acknowledges his indebtedness to Babbage for an 'unpublished statement drawn up by Mr Babbage' and for the 'original documents which are in Mr Babbage's possession' that Weld examined;<sup>13</sup> finally, Buxton's account was based directly on manuscripts given to him by Babbage.<sup>14</sup>

The voice in each of these accounts is unmistakably Babbage's. The repetition of quoted sources and the similarity of the rhetorical positions brands them as coming from the same stable. The blurring of authorship and attribution is added to by Babbage reprinting two of the accounts in full in his own published works: Nicolas's account is reprinted in *Passages*; Weld's account is included as an Appendix in *Exposition*.<sup>15</sup> The historical canon is effectively dominated by Babbage's own account, and his voice, loud and strong to begin with, is amplified by the particular soft spot history appears to reserve for thwarted geniuses and for

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<sup>10</sup> See Chapter 3, p. 118, ft. 26 for note on precedence. Also Chapter 3, p. 112, ft. 10 for reference to Babbage's accounts of the conception of the invention.

<sup>11</sup> See Chapter 3, p. 167 *et seq.* Also Buxton *Memoir*, Hyman (1988), p. 288.

<sup>12</sup> *Passages*, p. 69.

<sup>13</sup> For Weld's invitation to Babbage see Weld to Babbage, 13 May 1847, BL Add Ms 37193, ff. 544-5. For Weld's acknowledgement see Weld (1849), *Works*, Vol. 10, p. 150, ft. 1.

<sup>14</sup> Hyman (1988), p. xiii.

<sup>15</sup> See *Passages*, pp. 68-96 and *Works*, Vol. 10, pp. 149-163.

'men ahead of their time' – a gruesome phrase which violates all but the crudest historical sensitivities.

The subtext of Babbage's self-portrayal is that of a wronged genius. He writes that he regards the Government's final and absolute withdrawal in 1842 from further support for the stalled engine project as a betrayal of the Chancellor's original undertaking to fund the Engine to completion. He repeatedly wrote of his sacrifices and of his grievances at the failure of the scientific establishment as well as Government to confer on him honours or position in recognition of his labours. And he publicly declared his conviction that he was the victim of a conspiracy rooted in professional jealousy and malice. Babbage's villain is Airy, who emerges from the account as an influential behind-the-scenes advisor who allowed a personal grudge to bias his counsel to government against the engines.

Historical accounts since Babbage's time have failed to provide a corrective balance to Babbage's self-portrayal. Anthony Hyman, in his influential biography of Babbage, colludes with Babbage and presents him as a genius surrounded by fools.<sup>16</sup> Robert Peel is portrayed as a scientific illiterate, a classicist on whom the bounty of science and invention was lost.<sup>17</sup> He calls Henry Goulburn, Peel's Chancellor a 'mediocrity'.<sup>18</sup> Lardner is portrayed as a clown – 'a scientific Falstaff . . . even now . . . occasionally mistaken for a serious figure'. And Airy, Babbage's durable antagonist, is described as the 'prototype of the scientific bureaucrat', pedantic and unimaginative.<sup>19</sup>

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<sup>16</sup> Hyman (1982). *PC*.

<sup>17</sup> *Ibid.*, p. 52.

<sup>18</sup> *Ibid.*, p. 79.

<sup>19</sup> *Ibid.*, p. 191.

George Biddell Airy was consulted by government for his views on both Babbage's calculating engine and the Swedish engine designed by Georg Scheutz and his son Edvard. In 1842, when invited to give an opinion on Babbage's Engine, he pronounced it 'worthless'. Airy's single damning utterance is frequently quoted to reinforce his image as influential but unimaginative, a Salieri to Babbage's Mozart. Yet Airy's one-word dismissal of the engines was not an irritable aberration. With one notable exception Airy consistently rejected the practical and economic utility of automatic calculating engines. As Astronomer Royal, as *de facto* scientific advisor to government and the most influential civil scientist of his generation, he was the official arbiter of utility and his views had a determining influence on the fate of Babbage's engines, the Scheutzes' engine and other mechanical calculating aids. Yet the circumstances in which he was consulted, his views and their argued justification are almost entirely missing from the canon.

There are three major scholarly monographs in a large and growing literature on Babbage's life and works. Bruce Collier's doctoral thesis *The Little Engines that Could've* is a detailed and authoritative study of the historical development of Babbage's ideas on calculating engines as well as the complex circumstances surrounding the attempts to fund and build his machines.<sup>20</sup> As historical chronicle, and as a sensitive and detailed account of the evolution of Babbage's ideas, Collier's work remains unsurpassed. Hyman's biography, *Pioneer of the Computer*, is the standard reference on Babbage's life and excels in its portrayal of the social context of the times and of Babbage's political and scientific life. It is in the genre

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<sup>20</sup> Collier ([1970], 1990). *LEC*.

of admiring biographies and is largely uncritical of its subject.<sup>21</sup> Michael Lindgren's *Glory and Failure*, also a doctoral thesis, is a study of the difference engines of Johann Müller, Georg and Edvard Scheutz, and Babbage and provides relief from the largely Anglocentric view that has tended to dominate Babbage studies.<sup>22</sup> This work is unusual in its mix of genres: it features a detailed technical study of the calculators and also frames the movement to mechanise calculation in the context of the market, practical need and cultural context.<sup>23</sup> The work excels in its treatment of the Scheutz machines citing Swedish sources not previously used, and goes further than other studies in situating the movement to mechanise calculation in a theoretical framework of technological change. In addition to these works, a series of published and unpublished articles by Allan Bromley provide the most detailed analysis and interpretation of Babbage's technical designs for his engines and for the Analytical Engine in particular. Bromley's masterly studies are essentially internalist: they are little concerned with the historical context of mechanised calculation.

In Collier's work, Airy is barely mentioned except as someone who declared Babbage's engines to be worthless.<sup>24</sup> As mentioned earlier Hyman gives Airy short shrift dismissing him as an uninspired administrator.<sup>25</sup> Lindgren is alone in giving Airy more than nominal treatment in discussing Airy's advice in 1857 to the Treasury. However, all these studies, Lindgren's included, omit reference to two

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<sup>21</sup> Maboth Moseley's biography, *Irascible Genius* published in 1964 is largely discounted by Babbage scholars as a work of journalism. Sources are not specifically cited and aspects of interpretation have been challenged. My own semi-popular *Cogwheel Brain* is also excluded from this survey: new content, especially on Airy, is based on material in this thesis which predates publication of *CWB* in 2000.

<sup>22</sup> Lindgren, ([1987], 1990), *G&F*.

<sup>23</sup> See Swade (1991, *Isis*), p. 535-6.

<sup>24</sup> *LEC*, p. 101.

<sup>25</sup> *PC*, p. 191.



major archival sources: the extensive collection of Airy's papers preserved in the Cambridge University Library as part of the records of the Greenwich Royal Observatory, and the letter books of the General Register Office (GRO) at the Public Record Office, Kew, which contain bound clerks' copies of all inbound and outbound correspondence. This hitherto unused material provides a more complete account of Airy's role in the fate of calculating devices and engines and also reveals attitudes to new technology at the GRO, specifically in relation to the introduction of arithmometers – manual desk-top calculators first announced in the early 1820s at roughly the same time as the engine advocates sought support for their ambitious new machines.

With the benefit of these additional sources this thesis describes for the first time the occasions and circumstances in which Airy was consulted for his views on the calculating engines, and examines the basis for his consistent opposition to the machines. This material redresses the imbalance created by Babbage's effective monopoly of the canon by finally giving Airy a turn on the rostrum. Airy's role is explored through a series of case studies in which the circumstances, occasion and outcome of each consultation is treated in turn. The value of this work is that of chronicle: it fills a major gap in the existing accounts of the central events in the prehistory of automatic computation.

Historical accounts routinely link the movement to automate calculation in the nineteenth century to contemporary arguments based on the large number of errors found in printed mathematical tables. The work that has had a defining influence in identifying the elimination of errors as the primary purpose of the engines is Lardner's lengthy article published in 1834. Lardner collaborated closely with Babbage preparing it. Possibly because of this, as well as because of the

inflated generosity with which the article promotes Babbage's invention, Babbage has 'inherited' from Lardner the notion that the elimination of errors was the primary motive and purpose of the machines.

This thesis takes a revisionist view of Lardner's work. An examination of Babbage's earliest expectations for his engines suggests that errors in tables did not feature as prominently in Babbage's motives as Lardner's article implies. New findings on the mathematical implications of the engines are presented, specifically on Babbage's earliest speculations on computation as a new branch of analysis. The method here is the close reading of texts, in this case the five papers Babbage wrote during the second half of 1822, the period in which he recorded his earliest conception of the potential benefits of his invention. A study of the circumstances in which Lardner's article was written suggests that showmanship was a factor in the prominence given to errors in Lardner's justification for the engines and that his published grounds for advocacy should not be taken at face value. Finally, the article is considered in the political context of the engine project and the promotional value of Lardner's advocacy to the collapsing fortunes of Babbage's engine project.

The thesis situates the debate about the merits or otherwise of calculating engines in the context of manual methods used in the production of mathematical tables, that is to say in the context of the processes and techniques that the engines were intended to replace. Methods of table production are discussed in the first two chapters with special reference to the susceptibility of various processes to errors. Errors in tables became a practical, scientific and economic issue at least in part as a result of public advocacy of the benefits of the new machines, and the discussion of table-making, which appears in the first chapters,

is informed by contemporary events and perceptions covered in later chapters. The supposed benefits of calculating engines were for the most part unrealised and the manual practices described remained current for most of the century, largely unaffected by mechanisation.

Giving Airy his say allows him to be seen as a more equal opponent for Babbage who has so far received the lion's share of attention in historical accounts. While the value of psycho-history remains embraced and reviled in equal measure, it may be worthwhile, within the editorial licence of an Introduction, to make some observations about the temperament, proclivities and career of the two main protagonists.<sup>26</sup>

Airy and Babbage were as unlike as chalk and cheese in class, taste, habit and fortune. Babbage went to Cambridge as the son of a wealthy banker. Airy, ten years his junior went as a 'sizar' – a kind of student servant who received tuition in exchange for college duties. Both excelled at mathematics. Airy was a brilliant star. He was top of his mathematics year in all three of his undergraduate years and graduated as Senior Wrangler and first Smith's Prizeman in 1823. He recounts that when the second-year results were posted, his name was separated from the rest by two lines indicating that he had scored double the marks of his nearest rival.<sup>27</sup> He recalls the ceremony at which he received his degree in 1823: 'rarely has the Senate House rung with such applause as then filled it. For many minutes, after I was brought up in front of the Vice-Chancellor, it was impossible to proceed with the ceremony on account of the uproar'.<sup>28</sup> Babbage, on the other hand, did not sit the examinations and graduated without honours (with a 'poll' degree): he either

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<sup>26</sup> For a discussion of the role of psychoanalysis in history see Gay (1985).

<sup>27</sup> *Biog.*, p. 36.

<sup>28</sup> *Biog.*, p. 40.

disdained to compete in the Senate House examinations, or was disqualified from doing so.<sup>29</sup> He was independent-minded, radical and even rebellious. Disappointed with his mathematics tutors he pursued his own course of study which featured Continental theories outside the staid conventions of the Cambridge mathematics curriculum. He instigated the Analytical Society, which sought reform to Cambridge mathematics, and he admired Napoleonic France with which Britain was still at war.

Where Babbage failed to secure paid professional appointment and protested his exclusion, alleging prejudice because of his liberal radicalism, Airy was the most successful career scientist of his generation, rising from a brilliant, self-motivated student to one of the most eminent consultant engineers of his age. He advanced in steady steps from a Fellow at Trinity College, Cambridge, to the Plumian Professorship in 1828, with care of the Cambridge Observatory, and from there to the coveted post of Astronomer Royal, in 1835, with responsibility for the Greenwich Royal Observatory. He reformed the organisation of the Observatory, restored it to prominence through resolute leadership, and managed it with stern authority. As Astronomer Royal he commanded the highest office in civil science for over 45 years and represented continuity in an age of turbulent reform. Through distinguished and conscientious service to government he crafted for himself the role of *de facto* chief scientific advisor on subjects in addition to and outside his official remit as Astronomer Royal. He served on, or gave advice to, some forty Commissions on non-astronomical subjects including docks, tidal harbours, lighthouses and coinage.<sup>30</sup>

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<sup>29</sup> The term 'poll' is derived from *οἱ πολλοί*. See Ball (1889), p. 170. For argument that Babbage was disqualified see Becher (1995), p. 408-9. For rebuttal see Wilkes (2000), p. 69.

<sup>30</sup> For a career portrait of Airy and his seminal contribution to the professionalisation of science see Chapman (1988, "Science and the Public Good"). Also Chapman (1988, "Private Research and Public Duty").

Airy, though without independent means, refused ordination, and was dependent on securing paid employment. In successive appointments he insisted on, and secured, a living as a professional scientist by resolutely negotiating respectable salaries as a condition of acceptance – this before science was a recognised profession.<sup>31</sup> He followed the rules, deferentially served his masters, and was rewarded. Babbage too refused ordination but his situation was more secure. He was supported by an allowance from his father, Benjamin, and was modestly well off until his mid-thirties. On Benjamin's death in 1827 Babbage inherited an estate worth about £100,000.<sup>32</sup> He was independently wealthy and well able to support his family and his scientific pursuits. In political and scientific life Babbage was given more to protest than persuasion, often lapsing into public outbursts of indignation and outrage on matters of great principle. To the dismay of his friends and colleagues he behaved as though being right somehow entitled him to be rude. He was a gentleman and the son of an affluent banker. He bucked the system. He could afford to, though he behaved as though unaware of the offence he often gave.

The contrast in their professional roles is illustrated by their respective involvements in the 'gauge war'. During the rapid expansion of the railway system in the 1830s and 1840s the feud between the protagonists of Brunel's seven-foot broad gauge and those supporting Stephenson's narrow gauge was fiercely contested. As Second Commissioner, Airy played a prominent part in the Railway Gauge Commission in 1845-6.<sup>33</sup> Babbage's role in the gauge dispute was half way

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<sup>31</sup> For reference to reluctance to enter the Church see Chapman (1988, "Science and the Public Good"), p. 38. Also *Biog*, p. 71 where Airy wrote that he 'had a great aversion to entering the Church'.

<sup>32</sup> *PC*, p. 64.

<sup>33</sup> Chapman (1988. "Science and the Public Good), p. 44.

between a volunteer and an *ad hoc* advisor, who investigated, for Brunel and the proprietors of the Great Western Railway (GWR), the technical merits of the two competing systems.<sup>34</sup> To assist in his experiments a locomotive and a second-class carriage were placed at Babbage's disposal. He gutted the carriage and equipped it with instruments and recording devices of his own design, to log vibration, tractional force, and the trajectory of the centre of gravity of the carriage as it was pulled around curved sections of track. For some five months in 1838 and early 1839 Babbage, at his own expense and using his own workmen, conducted his trials. His graph plotters and pen recorders used up about two miles of paper, and he duly concluded that the broad gauge was safer.<sup>35</sup> He used his findings to dramatic effect at a meeting of the proprietors of the GWR at the London Tavern in January 1839.<sup>36</sup> But it appears that he spoke from the floor and had no official standing. Brunel and the proprietors had reservations about openly associating themselves with Babbage having appointed their own advisors who counselled scrapping the broad gauge.<sup>37</sup>

The episode offers us two images. One is of Airy with the official standing of a Commissioner sitting in London at a plush table surrounded by eminent men empowered to influence important strategic decisions on the burgeoning railway industries. The other is of Babbage, an inspired experimentalist, careering around in his train with his graph plotters, a brilliant and awkward outsider, who, with no official standing, made his point from the floor at a meeting at the London Tavern.

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<sup>34</sup> *PC*, p. 158.

<sup>35</sup> For Babbage's own account see *Passages*, pp. 320-5.

<sup>36</sup> Babbage used his results to demolish Lardner's findings at this meeting. See below Chapter 3, p. 159 *et seq.*

<sup>37</sup> For an account of the meeting see Vaughan (1991), p. 117. For reference to Babbage's 'delicate position' see *PC*, p. 158.

As it happened, Babbage had again backed the wrong side: advocates for the narrow gauge prevailed, and the broad gauge, supported by Babbage and Brunel, was eventually phased out.<sup>38</sup> The two images capture something poignant about their respective professional roles. Babbage was a gentleman scientist, Airy a public servant.

Whereas Babbage ached for recognition, titles and civil honours and growled at their lack, Airy refused knighthoods three times, finally accepting in 1872 on the fourth time of asking.<sup>39</sup> Babbage was a *bon vivant* with a love of dining out and socialising. He sparkled as a host and raconteur. His Saturday soirées were glittering events attended by the social and intellectual elite of London. ‘All were eager to go to his glorious soirées’, wrote Harriet Martineau.<sup>40</sup> Dickens, Brunel, the actor William Macready, Darwin, Fox Talbot, ‘and the best of almost every class’ flocked to Babbage’s house at 1 Dorset Street.<sup>41</sup> With his brightly coloured waistcoats he even acquired a reputation as a bit of a dandy.<sup>42</sup> Airy, in the testimony of his son, Wilfrid, ‘avoided dinner-parties as much as possible – they interfered too much with his work – and with the exception of scientific and official dinners he seldom dined away from home’.<sup>43</sup>

Their emotional and domestic lives were starkly different, at least after 1827. Airy met and fell in love at first sight with Richarda Smith while on a walking tour in Derbyshire in 1824. The Duke of Devonshire had declared her to be ‘the most

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<sup>38</sup> Babbage had earlier supported James South in a case brought by Troughton and Simms over an allegedly defective telescope. South lost. See Chapter 4, p. 226 *et seq.*

<sup>39</sup> Airy refused knighthoods in 1835, 1847 and 1863. See *Biog.*, pp. 112, 187, 254, 196.

<sup>40</sup> Quoted in *PC*, p. 129.

<sup>41</sup> For references to Babbage’s soirées see *CWB*, pp.72-3; Desmond and Moore (1991), p. 212; H. P. Babbage (1910), p. 9; Neve and Messenger, eds. ([1887], 2002), p. 63.

<sup>42</sup> See *PC*., p. 174.

<sup>43</sup> *Biog.*, p. 9.

beautiful girl he ever saw'.<sup>44</sup> Airy and Richarda married about six years later when Airy had position and the means to do so. Airy's son records that 'his constancy had its reward, for he gained a most charming, and affectionate wife'.<sup>45</sup> They were married for forty five years in a union of apparently unbroken happiness and harmony. In comparison, Babbage's tale is tragic. He married Georgiana Whitmore in 1814 in defiance of his father on whom he was still financially dependent. There is every indication that they were devoted to each other and that the marriage was an emotionally rewarding one. In 1827, Georgiana died, presumably in childbirth. In the same year his second son died, as did his father, and a newborn son. Babbage was inconsolable and close to breakdown. Darwin recalled that Babbage 'was always worth listening to, but he was a disappointed and discontented man; and his expression was often or generally morose'. He added though that 'I do not believe that he was half as sullen as he pretended to be', and that he believed 'that his bark was much worse than his bite'.<sup>46</sup> Babbage never remarried and it has been suggested that the bitterness of his later public protests had its roots in personal disappointment.

Airy's and Babbage's response to music were worlds apart. In the words of his son, Airy 'was very fond of music and knew a great number of songs; and he was well acquainted with the theory of music'.<sup>47</sup> Again in the words of his son, Airy's 'powers of abstraction were remarkable: nothing seemed to disturb him; neither music, singing, nor miscellaneous conversation'.<sup>48</sup> Babbage abhorred

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<sup>44</sup> *Ibid.*, pp. 56-7.

<sup>45</sup> *Ibid.*

<sup>46</sup> Neve and Messenger, eds. ([1887], 2002), p. 63.

<sup>47</sup> *Biog.*, p. 12. See also, *ibid.*, p. 37.

<sup>48</sup> *Ibid.*, p. 9.



music. Organ grinders and street musicians drove him to distraction. He took to walking by day to escape the ‘*artistes*’ racketing near his house, and worked late into the night when he could find unbroken quiet. A sheet of his notebook is savaged by the stab of a pen dragged down in an ugly tear when he could stand it no longer. He was ridiculed and taunted in the streets for his campaigns against street musicians.<sup>49</sup>

Babbage detested public speaking and his course of twelve lectures in Astronomy at the Royal Institution in 1815 are thought to be his only public lectures on a scientific topic.<sup>50</sup> He published little of substance on his engines of either a scholarly or popular kind. Airy, on the other hand, though not a gifted orator, lectured widely as a public duty ‘to popularise science as far as lay in his power’ and especially the work of the Observatory which ‘he effected by articles communicated to newspapers, lectures, numerous Papers written for scientific societies, reports, debates, and critiques’.<sup>51</sup> Airy and Babbage competed for the Lucasian chair. Airy records that during the contest Babbage threatened legal proceedings.<sup>52</sup> Airy was appointed in 1826 and Babbage succeeded him in 1828 occupying the chair till 1839.<sup>53</sup> Babbage’s failure to reside in Cambridge or to deliver any lectures during his tenure angered many of his contemporaries, Airy included. Airy, on the other hand, immediately on appointment, drew up a list of

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<sup>49</sup> See *CWB*, pp. 212-14.

<sup>50</sup> Roberts (1988), Preface and Introduction, p. 1.

<sup>51</sup> *Biog.*, p. 12. For reference to Airy’s dedication to popularisation see *ibid.*, p. vi. For reference to Airy’s lectures see *ibid.* pp, 12, 195, 220, 234, 260.

<sup>52</sup> *Biog.*, p. 70. Airy’s reference to threatened legal action is cryptic. The episode invites further study.

<sup>53</sup> Ball (1889), p. 125. Babbage later recorded his gratitude for the appointment which he described as ‘the only honour I ever received in my own country’. See *Passages*, p. 34.

apparatus to be constructed for his Lucasian lectures and conscientiously fulfilled the duties of office.<sup>54</sup>

In the chapters that follow there is little technical detail of the working and principles of the engines except where such detail has relevance to the specifics of table making or to arguments articulated by one or another of the protagonists. Babbage designed two classes of engines, Difference Engines and Analytical Engines. Both were automatic in the sense that they embodied mathematical rule in mechanism and executed computational tasks on numerical information initially provided without the need for mathematically informed intervention by an operator. The essential difference between the two classes of engine lies in the generality of their use. Difference engines are so called because of the mathematical principle on which they are based, namely, the method of finite differences. These engines perform only one set of operations, repeated addition. In this sense they are special purpose machines: they crunch numbers the only way they know how – in accordance with the internal rules of their wheelwork. Any numbers entered into the machine are treated in the same way. The Analytical Engines, in contrast, were designed as general purpose machines. They have an internal repertoire of functions and were to be programmable using punched cards through which the operator could instruct the machine to execute operations in any sequence or repeated sequence. In the chapters that follow there is little reference to the specifics of Analytical Engines or to their intended capabilities. The conception of the Analytical Engine in 1834 played a part in the negotiations with successive government administrations and Babbage's references to it significantly influenced the fate of the Difference Engine. While Analytical Engines were capable of tabulation this was not their primary purpose and they fall outside the intended

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<sup>54</sup> *Ibid.*, p. 71.

scope of this thesis. There is therefore little coverage of the proposed capabilities of the Analytical Engine outside its role in influencing the fate of the Difference Engine.

The debate about calculating engines centred essentially on the issue of their 'utility'. The term is used repeatedly by both the engine advocates and the sceptics, though neither sought to define clear criteria for it. Babbage and his contemporaries used the term to refer in general to benefits of some kind, and for the most part the term is used in a non-specific way. The debate over the calculating engines offers a promising case study of what constituted arguments for utility during the decades, 1820 –1870, in which the uses of the machines were contested. Utilitarianism as a movement was an inseparable, dominant and implicit feature of the intellectual climate of the times, and the 'benefits' or 'disbenefits' of the engines articulated in the debate were informed, consciously and unconsciously, by utilitarian values. Inevitably utilitarian values influenced and even defined contemporary expectations of the engines, the stated grounds for their advocacy and rejection, the political circumstances of attempts to construct the machines, received perceptions about the importance of errors, and the actions of agents who had a defining role in their fate. There are many respects in which historical accounts of the circumstances of the engine debates, that would inform further discussion on issues of utility, remain incomplete. The first task here is therefore that of chronicle and exposition. In the course of this more complete account the terms in which utility was articulated become evident, and these form an inviting starting point for a further analysis. However, the discussion of the utility of the engines in the specific context of the utilitarian movement has been left for a later study.

None of Babbage's designs for calculating machines was fully realised in physical form during his lifetime. All that he accomplished were a few partial assemblies and demonstration pieces, and referring to the Difference Engines and Analytical Engines as though they are physical machines is a convenience of language. These names strictly refer to the machines' designs which are extensive and highly detailed, rather than physical artefacts. There is also a convention in the literature by which Babbage's 'Difference Engine' has the first letters capitalised whereas the difference engines of Scheutz, Wiberg, Grant and others do not. This is a conceit of precedence and is adhered to here for the sake of conformity.

We pay a final visit to our two adversaries before they formally engage. Allan Chapman provides this thumbnail sketch of Airy:

One is brought to the conclusion that Airy's greatest reward lay simply in fulfilling his public duty. To a man who derived deep satisfaction from the mere adding up of figures and whose hobby was the meticulous keeping of domestic accounts, one comes close to understanding what motivated the Astronomer Royal. It was neither abstract intellectual curiosity, ambition, love of power or fame. It was order, and the organisation of useful facts, in consequence of which he found in the careful discharge of his duties a satisfaction and end in itself.<sup>55</sup>

Babbage was a visionary, volatile and proud. Airy was a pragmatist, forthright and astute. They meet here to contest the utility of calculating engines. Given the fate of the engines, it is Airy who prevailed. Yet it is Babbage that posterity celebrates.

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<sup>55</sup> Chapman (1988, "Science and the Public Good"), p. 56.

## Chapter 1: Table Making: Calculation

*It is unworthy of excellent men to lose hours like slaves in the labour of calculation  
which could done by any peasant with the aid of a machine.*

– Leibniz, 1685.

### Introduction

The movement to automate calculation in the nineteenth century is a central feature in historical accounts of the prehistory of modern computing. The earliest attempts to automate calculation are inseparably linked to the difficulties in producing error-free mathematical tables. The advocates of calculating engines blamed the supposed deficiencies in printed tables on the manual methods used in their production, and engines were promoted as a replacement for existing practice.

The adequacy of manual methods was defended by those who opposed the engines, notable amongst whom were Airy, Thomas Young (Superintendent of the Nautical Almanac, 1818-1829), and Nils Selander (a Swedish astronomer).<sup>1</sup> At the same time the engine advocates, including Dionysius Lardner (lecturer and populariser of science), Francis Baily (astronomer), and Babbage, used alleged deficiencies in tables to argue the benefits of machines. In the course of their advocacy, protagonists and sceptics refer to the manual processes involved in

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<sup>1</sup> Young was appointed as Superintendent of the NA in 1818. On his death in 1829 he was succeeded by John Pond. See Wood (1954), pp. 304, 315. (Young was the first to identify and describe astigmatism, *Ibid.*, p. 99. Airy suffered from a severe form of this eye defect).

tables production, specifically calculation, transcription, verification, proofreading, typesetting and printing.

The purpose of this chapter, and the next, is to describe the processes and techniques involved in pre-automated tables production as a way of situating the debate on the utility of the engines in the context of contemporary practice.

The processes and procedures described were current in the early 1820s when the prospect of automatic calculating engines first emerged as a scientific, technical and political issue. However, the techniques discussed should not be seen as superseded by automation: as mentioned earlier, for the most part the engines remained unbuilt, and the processes described remained largely unchanged for most of the century.

### Historiography of Tables

In general the literature on the history of mathematical tables is lean and fragmentary. Lindgren writes:

Very little has been written about tables in more recent times and there is no book, whether old or new, which is entirely devoted to the history of the role of tables in society. The articles which have been published in this century on the subject, have been narrow in scope and most often devoted to one specific table . . . <sup>2</sup>

Nineteenth-century material is slightly less sparse, but not significantly so. Notable sources include works by Charles Hutton, James Lee Glaisher, and Augustus de Morgan. Hutton included a long historical Introduction to a set of tables first

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<sup>2</sup> *G&F*, p. 17.

published in 1785.<sup>3</sup> This is for the most part a technical history of tables and the development of the mathematical methods associated with their use. Glaisher's report on tables published in 1874 for the British Association, and the semi-popular encyclopaedia articles by Augustus de Morgan, which appeared in 1842 and 1861, provide synoptic bibliographies of printed tables. These list the scope of published tables, their lineage, reputation for accuracy, and often include an evaluation of reliability.<sup>4</sup> The main function of these works is that of survey, and they served, in part, as consumer guides. However, Hutton's Introduction and Glaisher's report reveal little of the processes, procedures and practices of table making or of the wider context of their production, and this applies to historical accounts of tables in general.

De Morgan's two encyclopaedia articles are partial and revealing exceptions. De Morgan (1806-1871), mathematician and authoritative connoisseur of tables and table making, interspersed his bibliographic entries with short descriptions of some of the techniques used in the preparation, verification, proof reading and printing of tables.<sup>5</sup> These brief annotations give insights, often inadvertent, into contemporary practices and attitudes and constitute a major source in an otherwise meagre haul.

The situation has only slightly improved in the sixteen years since Lindgren made his bleak observation. Croarken's study, *Early Scientific Computing in Britain*, for example, while not specifically devoted to table making, provides

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<sup>3</sup> Hutton (1801). Hutton's Introduction runs to one hundred and twenty-five pages and was reprinted in all editions up to and including the sixth, published in 1822, but was dropped in all subsequent editions. Lindgren comments that this indicates a decline in interest in the history of tables in the early decades of the century. See *G&F*, p. 292, Note 1.

<sup>4</sup> De Morgan (1842, 1861).

<sup>5</sup> For a brief biographical account of De Morgan see Rice (1996, *Mathematical Intelligencer*). For an account of de Morgan's appointment to the chair of mathematics at London University see Rice (1996, "Inspiration or Desperation?").

insights into both the techniques and the broader context of tabulation at the Nautical Almanac Office.<sup>6</sup> However, the scope of her study is primarily confined to the twentieth century and the pre-automated era is treated in summary largely as a gesture to prehistory. A forthcoming compilation of essays, *Sumer to Spreadsheet: The Curious History of Mathematical Tables*, is a single volume wholly devoted to the subject and can be seen as a response to the silence, but there is still no sustained treatment of the practices and processes involved in the production of printed tables, nor of the respects in which table making changed with increased mechanisation.<sup>7</sup>

The physical production of a book, whether literary or numerical, started with an 'authored source' and the chain of generic processes, specifically typesetting, proofing, printing, binding and distribution, began with the transfer of the authored source to the printer. In the case of printed tables the authored source is a numerical text, a manuscript in some cases, or an already printed table in others. For mathematical tables, the distinctive process of authorship is calculation. The generative rules are deterministic and exact, and dictate the desired sequence of numerical characters. The generative process of literary sources is manifestly different. The distinction is perhaps an obvious one. But it has an implication that is central to the ambitions of the engine advocates: the mechanisation of authorship, that is, the replacement of human agency by machine in the production of the authored source.

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<sup>6</sup> Croarken (1990).

<sup>7</sup> Campbell-Kelly *et al.*, eds. (2003, in press). The book includes an essay "The 'unerring certainty of mechanical agency': Machines and Table-making in the Nineteenth Century" which focuses on the changing role of technology in tables production, and draws on material in this chapter. See Swade (2003). Ivor Grattan-Guinness's article on the French tables project undertaken under Gaspard Marie de Prony in the late eighteenth century is a short but close study of a major tabulation venture and is among the few exceptions to the general neglect of the subject. See Grattan-Guinness (1990). Alex Craik's work (in press) on the tables of Edward Sang (1805-1890) is another. For discussion of De Prony's tables see below pp. 51, 56 *et seq.* For discussion of Sang's tables see below p. 65 *et seq.*



This chapter describes the main techniques of calculation and starts with the role of manual calculating aids in the preparation of the authored source.

### Mechanical Calculators

Relieving the drudgery of calculation features as the stimulus of some the earliest attempts to devise mechanical calculating aids. In 1685 Leibniz wrote, 'It is unworthy of excellent men to lose hours like slaves in the labour of calculation which could done by any peasant with the aid of a machine'.<sup>8</sup> The reference to 'excellent men' and 'worthiness' implies a hierarchy of values in which abstract, analytical and philosophical activity is ranked higher than repetitive task-specific activity.<sup>9</sup>

In the nineteenth century the engine advocates emphasised the numbing grind of routine calculation. There are references to 'the intolerable labour and fatiguing monotony of a continued repetition of similar arithmetical calculations'; 'that wearisomeness and disgust, which always attend to monotonous repetition of arithmetical operations'; 'the dull and tedious repetition of many thousand consecutive additions and subtractions'; and the 'mental drudgery' of constructing tables.<sup>10</sup> Luigi Menabrea, at the time an engineer, who attended Babbage's seminar in Turin on the Analytical Engine in 1840, echoes Leibniz when he wrote

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<sup>8</sup> Translation from the Latin 'Indignum enim est excellentium virorum horas servili calculandi labore perire, quae machina adhibita vilissimo cuique secure transcribi posset'. Quoted in Martin ([1925], 1992), p. 38. The date is taken from an object label in 'Computing and Mathematics' Gallery, Science Museum, which features a looser translation.

<sup>9</sup> The association of elevated social class with abstraction and analysis, and the role of machines in deskilling calculation recurs in Babbage's reference to the social organisation of labour in de Prony's great cadastral tables project in the late eighteenth century. See below, pp. 56 *et seq.*

<sup>10</sup> Sources in order: Babbage (1822), *Works*, Vol. 2 p. 6; *Ibid.* p. 15; Baily (1823), *Works* Vol. 2, p. 45; Juris, Judex (1861), *Works*, Vol. 1, p. 3.

of the stultifying effect of calculation on higher thought. 'And what discouragement', he asks, 'does the perspective of a long and arid computation cast in the mind of a man of genius, who demands time exclusively for meditation, and who beholds it snatched from him by the material routine of operations!'.<sup>11</sup>

The earliest mechanical calculators capable of basic arithmetic were conceived and constructed in the seventeenth century, notably by Wilhelm Schickard, Blaise Pascal and Gottfried Leibniz.<sup>12</sup> Decimal numbers were entered on dials using a stylus or slider and results were displayed on engraved or annotated discs. These devices were in the nature of ornate curiosities, *objets de salon* – exquisite, delicate and largely unreliable – rather than the workhorses needed for routine use. The *Pascaline* or *Pascale*, as Pascal's calculator became known, stimulated philosophical debate about the mechanisation of intellectual process, and was paraded before royalty, businessmen, government officials and professors.<sup>13</sup> But it was expensive, insufficiently robust for daily use, and only about a half-dozen are thought to have been sold.<sup>14</sup> Leibniz's 'reckoner', built between 1672 and 1674, was arithmetically more ambitious than the Pascaline and technically more complex. The Reckoner incorporated a new device, the stepped drum known as the Leibniz wheel, which dominated calculator design for the next two centuries.<sup>15</sup> However, through a combination of design and manufacturing

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<sup>11</sup> Menabrea (1842), *Works*, Vol. 3, p. 113.

<sup>12</sup> For a technical history of mechanical calculators see Williams (1985), pp. 122-158. For an uneven collectors' compendium of mechanical calculators from 1642 to 1925 see Martin (1992). A rough sketch of Schickard's 'Calculating Clock' in 1623 was found by Franz Hammer in 1935 though details were not published until 1957. Histories of calculation predating publication in 1957 invariably credit Pascal with the invention of the first mechanical calculator. For an accessible account of the reconstruction of Schickard's calculator see Augarten (1984), pp. 15-22.

<sup>13</sup> Augarten (1984), pp. 25, 27.

<sup>14</sup> *Ibid.* p. 30. For illustration of Pascal's calculator see Appendix III, Illustration 7.

<sup>15</sup> The development that broke Leibniz's clear run was the introduction of the variable-toothed gear, the pinwheel, patented by Frank Baldwin in the US 1875. The pinwheel was the basis for a new

deficiencies the Reckoner failed to work as intended and only one, largely unsuccessful, prototype appears to have been made.<sup>16</sup>

In the late eighteenth century, several calculators based on the Leibniz wheel were made, notably by Mattheus Hahn, Charles Stanhope, and Johann Müller.<sup>17</sup> Hahn's and Müller's calculators are extravagantly ornamental and sumptuous testaments to the instrument maker's art.<sup>18</sup> These devices, capable of basic arithmetic, worked, but again they were expensive, and few were made. The Stanhope calculators were more workmanlike and easier to manufacture, but these too were not made in any great number and remained for the most part curiosities, of only incidental practical use.<sup>19</sup>

It is fair to conclude from this brief survey that the legacy of manual mechanical calculators from the eighteenth century had little to offer the table-makers of the early nineteenth century. Specifically, in the early 1820s, when advocacy for the engines began to gain momentum, manual mechanical calculators were not yet technically viable, and had negligible practical influence on table making.

The mechanical calculating device that was potentially the strongest rival to the calculating engines was the arithmometer, patented and made public by Thomas de Colmar in 1820.<sup>20</sup> This desk-top device, with sliding dials and a rotary

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generation of compact key- and lever-operated desk-top machines. See Williams (1985), pp. 152-155.

<sup>16</sup> See *Ibid.*, p. 136.

<sup>17</sup> For details of Hahn's machine see Martin (1992), p. 45-51; for the Stanhope machines see Baxandall (1926), pp. 18, 19. For a detailed history of Müller's machine see *G&F*, pp. 64-70.

<sup>18</sup> For illustration of Müller's calculator see Appendix III, Illustration 8.

<sup>19</sup> For details of the Stanhope calculators see Baxandall (1926), pp. 9-10. The two machines shown belonged to Babbage. Also Augarten (1984), p. 37.

<sup>20</sup> Johnston (1997). See pp. 12, 13 and 14 for separate statements on the introduction of the arithmometer.

handle, was the first device robust enough for routine use. The arithmometer is often described as the first machine to be made commercially for general sale, and it is often misleadingly assumed that it was a viable product from the date of its introduction in the early 1820s.<sup>21</sup> However, Johnston, in his detailed study, has shown convincingly that it was over a half a century before the device approached proven reliability.<sup>22</sup>

The use of arithmometers at the General Register Office (GRO) illustrates the time lag between the announcement of the device and its adoption for routine use.<sup>23</sup> The origin of the GRO lies in The Registration Act of 1836. This Act marked a significant transition from an ecclesiastic system of record keeping by parish churches, to civil registration. The GRO, formed following the Registration Act, was responsible for processing decennial census data and conducted statistical analysis and tabular calculations on a daily basis.<sup>24</sup> Interest rates, life assurance and annuity premiums based on mortality statistics, were published in the English Life Table produced by the GRO. In the 1820s the life assurance industry boomed during a period of feverish financial speculation and in the middle decades of the century there was increasing pressure for new tables of interest as lending rates, which had been stable, began to reflect economic volatility.<sup>25</sup> During the following

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<sup>21</sup> See, for example, Lewin (1989) p. 219. For corrective see Johnston (1997). For a compendium of statements implying the arithmometer's immediate viability see Johnston (1997), p. 19, Note 1.

<sup>22</sup> Johnston (1997).

<sup>23</sup> The GRO Letter Book sources came to light while I was researching this thesis in 1993. Johnston's analysis of the use of arithmometers at the GRO is based on transcriptions of material I provided to him. For acknowledgement see Johnston (1997), p. 21, Note 76.

<sup>24</sup> For an account of changes in the social organisation of labour in the GRO following the introduction of various generations of information technology for the management of census data see Campbell-Kelly (1996). For a discussion of information handling at the GRO see Higgs (2003, in press). For the GRO's role following Civil Registration see Eyler (1979), Chapter 3.

<sup>25</sup> For a study of Babbage and life assurance see Campbell-Kelly (1992). George Peacock stated that of 624 new schemes in 1824-5 not more than one in five survived the first fanfare and several of those that did weather the gold rush, ruined their founders. See Wood (1954), p. 297.

decades, with the expansion of the industrial classes, tables of life assurance premiums and annuities needed constant revision and extension and the GRO was under increased pressure to expand the scope of its published Life Tables. In 1857, on the urging of William Farr, the GRO funded the construction of a calculating engine designed by Georg and Edvard Scheutz, father and son. The GRO's Scheutz engine was only the second complete difference engine to be made, and was purchased to assist in the preparation of the English Life Table of 1864. The circumstances of the purchase and its use are discussed in Chapter 5. The episode is cited here as it indicates the stimulus of demand in the use of new technology, and also the actions by senior GRO management to take a lead, with the attendant risks, in trialing yet unproven new machines.

The first recorded request for the purchase of an arithmometer, at a cost of £20, was not until January 1870 in a letter to the Treasury from George Graham, Registrar General, following trials of the device at the GRO – this some fifty years after de Colmar's first announcement.<sup>26</sup> Further requests were made in 1873 and 1877.<sup>27</sup> Johnston comments that at this time (the late 1870s) annual sales of arithmometers were no more than one hundred.<sup>28</sup> By July 1873 there were three arithmometers in use at the GRO and Graham wrote that he was

convinced of their utility; as ordinary arithmetical operations are performed by them, when used by careful men, with more accuracy and in less time than they can be executed by calculations. I am assured that for many purposes it is preferable to logarithms'.<sup>29</sup>

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<sup>26</sup> Graham to Treasury, 29 January 1870. RG29-2, Vol. 2, f. 111. Graham was Registrar General from 1842 to 1879. See Eyler (1979), p. 46.

<sup>27</sup> *Ibid.*, Graham to Treasury, 31 March 1873, RG29-2, Vol. 2, f. 149; 23 February 1877, f. 249.

<sup>28</sup> Johnston (1997), p. 16.

<sup>29</sup> *Ibid.*, Graham to Treasury, 28 July 1873, RG29-2, Vol. 2, f. 162.

While Graham is consistent in his support for the benefits of arithmometers it is clear that they were still, even in the 1870s, troublesome – noisy, subject to derangement, imprecisely made, and in frequent need of repair. In a tilt at the inferiority of French manufacture he suggests that ‘the liability of “arithmometers” to get out of order and their noise would be greatly diminished if they were made by better workmen – perhaps Englishmen’.<sup>30</sup> But problems persisted and the market was limited: in March 1877 Graham wrote that his repeated requests and suggestions for improvements were rebuffed by the manufacturers on the grounds that there was insufficient demand.<sup>31</sup> Private use appears to have been equally patchy: in 1872 an arithmometer costing £12 was acquired as a novelty by Henry Brunel, Isambard Kingdom’s son, who, clearly taken with the device, paid it an ambiguous tribute when he described it as ‘really a very useful article worth its weight in brass’.<sup>32</sup>

By the 1890s the arithmometer still remained unproven to the Treasury’s satisfaction. In the period following Graham’s resignation in 1879, his successor, Sir Brydges Henniker, argued for English-made arithmometers by S. Tate in preference to those from France.<sup>33</sup> The Tate machines incorporated modifications patented in 1884, to the de Colmar device.<sup>34</sup> In response to the request for a second Tate arithmometer in January 1893, the Treasury requested a report on the relative merits of the Tate and Thomas patterns. The evaluation was to include life-

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<sup>30</sup> *Ibid.*

<sup>31</sup> *Ibid.*, Graham to Treasury, 16 March 1877, f. 250.

<sup>32</sup> Quoted in Johnston (1997), p. 17.

<sup>33</sup> For Henniker’s succession see Eyler (1979), p. 190.

<sup>34</sup> For Tate’s device and patent references see Baxandall (1926), p. 21.

expectancy comparisons as well as comparative costs of repair.<sup>35</sup> The report, compiled by Henniker, is revealing about the take-up of arithmometers in commercial insurance companies as well as at the GRO, and demonstrates that even by the 1890s arithmometers had yet to become standard tools for routine use.<sup>36</sup> Subsequent exchanges between the GRO and the Treasury show that the reliability of arithmometers was still not established by the early years of the twentieth century.<sup>37</sup>

Though arithmometers went on to sell in their tens of thousands, it seems clear from their use at the GRO, as well as elsewhere, that reliability was problematic throughout the nineteenth century and that they had not matured as a product till the early part of the twentieth century.

It follows that arithmometers, though ultimately successful technically and commercially, had little practical impact on nineteenth-century table making practices. More specifically, they did not represent a credible practical challenge to automatic calculating engines during the early middle decades of the century when the utility of calculating engines was most fiercely debated.

Towards the end of the nineteenth century arithmometers had competition from a range of key- and lever-operated desk-top machines of which Brunsviga is among the best known. These were first favoured for commercial rather than scientific use and ultimately superseded arithmometers in the early decades of the twentieth century.<sup>38</sup> Though these machines were to play a significant role in

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<sup>35</sup> Frank Mowatt, Treasury, to Brydges Henniker, Registrar General, 14 January 1893. RG29-7, f. 80.

<sup>36</sup> Henniker to Treasury, 2 February 1893, RG29-3, Vol. 3, f. 129.

<sup>37</sup> See Chief Clerk (GRO) to Treasury, 16 June 1903, RG29-3, Vol. 3, f. 300.

<sup>38</sup> The development and take up of key- and lever-operated machines is well documented. See for example, Williams (1985) Chapter 3; Augarten (1984), pp. 69-83.

tabulation during the first half of the twentieth century, they too had negligible influence on nineteenth-century table-making practice.<sup>39</sup>

### Slide Rules

The mechanical calculators so far mentioned are essentially digital devices in that each digit has a discrete representation in the movement of a mechanical part. In such digital devices only discrete positions of moving parts are valid representations of numerical value, and transitional positions are logically indeterminate. Analog devices, of which the slide rule is an example, represent a distinct category of calculating aid in which numerical value is represented on a continuous scale.

Slide rules were in widespread use for both general and specialised calculation throughout the nineteenth century.<sup>40</sup> They offered the convenience of portability and the assurance of robustness. Such were its charms that Richard Delamain, who first published a description of a circular slide rule in 1630, wrote that his device was ‘as fit for use as well on horseback as on foot’.<sup>41</sup> In addition to ‘universal’ slide rules for generalised calculation, versions were produced with scales and divisions customised for a variety of special applications, many of them unlikely and exotic in their specificity. These include rules for estimating excise duties (conversions scales for cubic inches to bushels, and finding the mean

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<sup>39</sup> For the use of desk-top machines in tabulation see Croarken (1990), and Wilkins (2003, in press).

<sup>40</sup> For a brief authoritative account of the origins and development of the slide rule see Baxandall (1926). For a summary history of the slide rule see Croarken (1990), pp. 7, 8. For a chronology of slide rule development starting in 1620, classification of slide rules, their mathematical principles and physical examples see Stokes (1914), pp. 155-180

<sup>41</sup> Quoted in, Williams (1985), p. 114.



diameter of a cask), calculating the volume of timber, the weight of cattle, estimating varieties of interest rates, and specialised rules for applications in engineering.<sup>42</sup>

Ordinary slide rules featured scales for multiplication, division and extraction of square roots, as well as in some cases, calculation of trigonometrical functions and logarithms. Standard slide rules served well for quick and convenient calculation, but accuracy was limited, and this was the essential issue for table makers. The scales and divisions were read by eye and there is an element of subjective judgement in reading the last decimal places. Precision was variable and depended in part on the separation of the divisions, which tend to be compressed at the extremities of the range. Accuracy was typically limited to between two and four figures with increasing uncertainty in the last one or two significant digits. This was adequate for many applications, but not all: interest payments, insurance premiums and financial accounting often required exactness to the level of pennies in thousands, tens or hundreds of thousands of pounds; land surveys, especially for cadastral tables used for property taxation, required calculations with the precision of metres in distances of kilometres and tens of kilometres, and this level of precision was unattainable using analog scaled devices.

Attempts were made to increase precision by extending the effective length of the scales. Fuller's 'spiral rule' designed in 1878 consists of a cylinder with the graduated scales arranged helically (as in a screw thread) on the surface. With scales wrapped around the cylinder in this way, a Fuller's rule with a six-inch cylinder has an effective working length of over forty one feet and could be read

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<sup>42</sup> See Baxandall ([1926], 1975). The second edition, revised and updated, has a narrower and less exotic range of examples.

reliably to four places.<sup>43</sup> The Stanley Company of Glasgow exhibited a Fuller's Rule with logarithmic scales effectively eighty three feet long. The readable precision was cited as '4 and sometimes 5 figures' and this represents the limits of precision achievable by a scaled rule that remained conveniently portable.<sup>44</sup> Yet when it came to a comparison between slide rules and with tables it was no contest. Comrie observed that 'today schools are equipped with 4-figure tables, which are ten times as accurate as the common 10-inch slide rule with which the great majority of engineering calculations are done'.<sup>45</sup>

Where precision of less than three or four digits was needed, slide rules came into their own and were used both instead of printed tables and as technical aids to generating data for tables, particularly if based on observational data which were anyway limited to comparable levels of precision. The attraction of digital mechanical calculators, manual and automatic, was the prospect of extending the number of reliably calculable digits to six, twelve or twenty decimal places – well beyond the range of discrimination by eye using an analog device with graduated scales.<sup>46</sup> Slide rules, with their limited precision, were little use for mathematical, astronomical and financial calculation, and were not much used by table makers.

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<sup>43</sup> See Baxandall (1926), pp. 53, 54. Also, Williams (1985), pp. 117, 118. Fuller's rule is usually referred to as a 'spiral rule'. Since the geometry of the body on which the scales are inscribed is of fixed diameter the form described by the scales is strictly helical not spiral. A pedantic point.

<sup>44</sup> Stokes (1914), p. 174.

<sup>45</sup> Comrie (1948), v.

<sup>46</sup> The earliest Pascal machine was intended to work to six figures (see Baxandall (1926), p. 13). The earliest de Colmar instruments had provision for six-figure input and for twelve-figure results (*ibid.* 20). Eight-digit input and sixteen-digit results were typical of later arithmometers. Babbage's calculating engines were designed to calculate with up to thirty or fifty digits, and in one design there is provision for hundred-digit results. For illustration of a later de Colmar arithmometer see Appendix III, Illustration 9.

### Limitations of Manual Mechanical Calculators

The deficiencies of mechanical calculators inhibited their use while they evolved from erratic novelties into dependable workhorses. Though they had little practical relevance to table makers and to those who contested the engines, they were not without influence. Johnston reports that arithmometers were offered to learned societies for review and hoped-for endorsement, and that reports on their construction and use appeared in the years immediately following the first patent in 1820. Also that the patent specification and journal reports served to advertise and disseminate details of the invention.<sup>47</sup> Arithmometers were clearly a presence and their viability, prematurely claimed in early advertisement, was a constant prospect from the time of their first announcement.

In 1821, at the time of his first conception of his calculating engine, Babbage appears to have been neither knowledgeable nor interested in mechanical calculators or their history. His apparent ignorance seems to have been a source of concern to his supporters who feared that this failure would prejudice the escalating campaign to advance the engine project. In May 1823 Francis Baily wrote to Babbage urging remedial action:

I have just seen Major Colby who wishes to suggest to you whether it would not be proper to get all the information you can on *former* machines which have been constructed with a view to calculation. As I have not heard you touch on this subject . . . I would advise you to collect all the information you can on this point; and endeavour to show *how far* they went, and *why* they failed.<sup>48</sup>

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<sup>47</sup> Johnston (1997), p. 14.

<sup>48</sup> Baily to Babbage, 26 May 1823, BL Add Ms 37183, f. 30. Mentioned in *LEC*, p. 40. Emphasis original.

Babbage subsequently made amends and, as he and his invention became better known, solicited and unsolicited material on calculators was sent to him from a variety of sources.<sup>49</sup> Williams reports that Babbage's scientific library, regarded as second only to Augustus de Morgan's, and which included what was arguably the most comprehensive collection of printed tables, featured thirty five items on mechanical calculation, though published material on the subject was rare.<sup>50</sup> Babbage also had a small private collection of calculators, which included an original set of Napier's Bones, at least two pieces by Sir Samuel Morland and three by Stanhope.<sup>51</sup>

There is no evidence that the engine advocates saw the arithmometer as a threat to the ambitions of their calculating engines. Part of the reason was almost certainly that even reliable arithmometer-like devices had limited use in table making, and the respects in which this is so illustrates, by contrast, the Utopian ambitions of the engine advocates.<sup>52</sup> Multiplying two numbers using an arithmometer, for example, requires the operator to enter the digits on sliding dials, rotating a handle the correct number of times for each decade, correctly lifting and

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<sup>49</sup> The earliest relevant letter in Babbage's correspondence is from Stephen Lee enclosing a copy of a letter from Lord Stanhope about the calculators invented by his father Viscount Mahon, later the third Earl of Stanhope. See Lee to Babbage, 22 August 1822, BL Add Ms 37182, f. 435, referred to in *G&F* p. 61, Note 204. For an example of accumulated *ad hoc* material see paper given to Babbage by his son Dugald, written in Versailles, 10 July 1789, describing a multiplication machine invented by Basil Lord Daer, Earl of Selkirk. See BL Add Ms 37183, f. 72, September 1823.

<sup>50</sup> See Williams (1981), p. 236. The original auctioneer's listings are to be found in the Babbage papers at Waseda University, Tokyo. The publication dates of the volumes are cited in the auction inventory, but there is no indication of when Babbage acquired the items. Williams notes that Babbage 'seldom wrote his name in any except the very rare books and never indicated the date of purchase'. *Ibid.*

<sup>51</sup> Ownership is inferred from the Babbage's offer to lend these and other items for display in the International Exhibition of 1862 in London (see *Passages*, p. 154) and from the recorded provenance of the Stanhope calculators donated to the Science Museum by Babbage's son, Henry Prevost, who inherited the engine-related material when Babbage died in 1871. See Baxandall, pp. 9, 10.

<sup>52</sup> Babbage (1822) refers to his aspirations for the engines as 'Utopian' in his letter to Sir Humphry. See *Works*, Vol. 2, p. 13. Lindgren adopts this in referring the Babbage's Difference Engine No. 1 as 'The "Utopian" Invention'. See *G&F*, p. 34.

repositioning the moveable carriage, and repeating this procedure for each digit of the multiplier.<sup>53</sup> Use of the machine requires the continuous informed intervention of the operator and the correctness of the final result relies, not only on the repeated correct mechanical functioning of the device, but on the faultless execution by an operator of a sequence of manipulations which surround and intervene in the process. With 'intelligence' in part externalised in the machine, there is a new burden on the operator to transfer information to and from the seat of this new intelligence, and this represents a weakness in terms of risk of error. A further limitation is the absence of a permanent record of the outcome. Each new calculation replaces the last set of numbers in the mechanism, and the only way of retaining a record is for the operator to note the results by writing them down. Such transcription is again dependent on human agency, and each manual operation in the sequence is susceptible to error.

The grand promise of machinery was certainty, and of an order unachievable by fallible humans. It is clear from this example that, in the case of manual calculating devices, the supposed infallibility of mechanism provided security against error for only part of the overall process to which such security was exclusively confined. In more general terms it is clear that the manual calculating aids of the nineteenth century represent, at best, only partial replacement by machine, of human skill, manual and mental.<sup>54</sup>

It was the ambition of the engine advocates to replace manual calculation, transcription, and typesetting, by infallible machinery that would eliminate human agency. And these ambitions remained unthreatened by the reality, or the prospect, of arithmometer-like devices.

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<sup>53</sup> See Baxandall (1975), p. 11.

<sup>54</sup> For a discussion of machines and 'the embodiment of skill' see Schaffer (1994).

## Derived Tables

Because of the exacting labour of calculation and anxieties about accuracy, computing tables from scratch was avoided whenever possible. In the case of generic functions, such as logarithms and trigonometric functions, existing tables with established reputations for accuracy were used as the starting point. Table makers celebrated for the reliability of their tables, include Briggs, Vlacq, Vega, Taylor, Callet and Hutton, whose works were legendary, and volumes of their tables were highly prized.<sup>55</sup> By the nineteenth century recalculation of the major canons was extremely rare.<sup>56</sup> A particularly reputable table had an exceptionally long life partly because of the quality of the original work, but also because of the progressive elimination of discovered errors through long use. Vega's 1794 edition of Vlacq's tables went through ninety editions, the last being issued in 1924.<sup>57</sup> Campbell-Kelly observes, 'it is an extraordinary fact that for the best part of 300 years all published sets of tables were derived, directly or indirectly, from the two great logarithmic canons of Briggs and Vlacq'.<sup>58</sup> In a contemporary statement to the same effect Edward Sang states that:

the original work of Henry Briggs (1620), carried on in the laborious way indicated to him by John Nepair [sic] in his "Constructio," is the only foundation; and that the completion of the canon by Adrian Vlacq (1628) was

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<sup>55</sup> Edward Sang notes in 1874 that Vlacq's tables had been out of print for two hundred years and 'if found at all, its price is antiquaries' price'. Sang (1874), p. 424.

<sup>56</sup> See Jagger (2003, in press).

<sup>57</sup> Binder (1994), p. 1459. Vega's first work appeared in 1783 though de Morgan (1861) comments that he has 'never met with it'. It is almost certainly the famed 1794 logarithm tables that survived into the twentieth century, not the 1783 version as suggested by Binder.

<sup>58</sup> Campbell-Kelly (1988), p. 162.

the last of the original labour that has been bestowed on this matter so essential to the progress of exact knowledge.<sup>59</sup>

Reputable tables used as a datum were rechecked, reprinted, merged, extended, revised, reduced in the number of significant digits, or otherwise modified for new editions to meet new needs. Pocket editions for field use were mostly republished versions of more extended tables, reduced in the number of decimal places. The genealogy of specific editions of tables was an essential determinant in their suitability as a source. The annotated bibliographies of tables advertised the 'pedigree' or 'stable' of particular editions, and entries were often accompanied by a critique of accuracy. Poor tables were not spared damning reviews. Glaisher quoting Hutton's view of de Haan's logarithm tables published in 1771 wrote that it was 'so erroneously printed that no dependence can be placed in it, being the most inaccurate book of tables I ever knew; I have a list of several thousand errors which I have corrected in it'.<sup>60</sup> De Morgan openly declared a pirated version of Lalande's tables stereotyped in 1831 as 'useless'.<sup>61</sup>

A well documented example of a set of tables derived from existing tables is Babbage's *Tables of Logarithms of the Natural Numbers, from 1 to 108,000* published in 1827. The example is revealing not only because it illustrates the use of an existing set of tables of high repute as a starting point, but because Babbage, by then sensitised to the processes of table making, documented the separate stages of verification, and these illustrate the extraordinary lengths that were required to secure any improvements in reliability.

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<sup>59</sup> Sang (1873-74), p. 375.

<sup>60</sup> Glaisher (1874), p. 131.

<sup>61</sup> De Morgan (1861), p. 1004.

*Logarithms* was prompted by the need for an accurate set of tables for practical use in the field for the Ordnance Survey of Ireland undertaken by Thomas Colby in 1825. Existing tables were insufficiently reliable, or expensive, out of print, or too cumbersome for field use.<sup>62</sup> Babbage's starting point was the second edition of Callet's stereotyped tables of 1795, itself derived substantially from William Gardiner's work of 1742 and 'much esteemed for accuracy'. Callet's second edition was judged to be 'one of the most correct and convenient, as well as extensive works in existence'.<sup>63</sup> Babbage had Callet's tables reset from the printed original which eliminated manual transcription and the attendant risk of copying errors. Nine separate stages of checking followed during which the proofs were read for basic accuracy and also checked against existing tables calculated to a larger number of digits than the seven required – this to ensure that the last-place figures were correctly rounded up or down. The Preface to *Tables* records the checking stages:

The proofs of the present tables were read three times: first, with the marked copy of Callet's logarithms; secondly with a copy of Hutton's logarithms, fourth edition, 1804; thirdly, with a copy of Vega's logarithms, folio, 1794. They were now received from the printer, and again compared with the logarithms of Vega as far as 100,000; the last 8,000 being read with those of Callet. Fifthly, the first 20,000 were read with those in the *Trigonometria Artificialis* of Briggs, folio Goudae, 1633. They were next returned to the printer, and stereotyped, and the proofs from the plates were read; sixthly, with the logarithms of Vega as far as 47,500; seventhly, with the whole of the logarithms of Gardiner, quarto, London, 1742; eighthly, with the logarithms of Taylor, quarto 1792; and ninthly, by a different set of readers they were again read with the logarithms of Taylor.<sup>64</sup>

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<sup>62</sup> See Campbell-Kelly (1988), p. 162.

<sup>63</sup> De Morgan (1842), p. 498.

<sup>64</sup> Babbage (1827), *Works*, Vol. 2, p. 74-75. The tables cited are listed as in the inventory of Babbage's personal collection of tables now housed in the Crawford Library, Edinburgh. Full names occurring here for the first time are, Henry Briggs, Charles Hutton, Michael Taylor, and



There was a further check not cited here: the residue of doubtful entries was checked against de Prony's manuscripts in Paris that Babbage viewed during his visit in 1826.<sup>65</sup>

The extreme lengths to which Babbage went paid off. His *Logarithms* acquired a reputation for accuracy. In 1833, J. R. Young observed that he could find no error 'and that I have no doubt that they amply deserve the reputation for accuracy that they have obtained'.<sup>66</sup> In 1842 and again in 1861 de Morgan described them as 'now exceedingly correct' and there were at least twelve new editions or reprintings between 1827 and 1915.<sup>67</sup> Nine errors were found in the first edition. These were corrected in the 1831 edition, and Van Sinderen maintains that between 1831 and 1915 no new errors were found.<sup>68</sup> Errors were subsequently found in the logarithms of numbers between 100,000 and 108,000, largely because proofreading the end of the range was undertaken in a tent in Ireland in storm conditions. The errors were largely unit errors in the last digit of about 450 entries, rather than gross errors of magnitude. The reputation of *Tables* has been a durable one: in 1962 the table was described as 'one of the most accurate ever printed'.<sup>69</sup>

Babbage's chronicle of care demonstrates the elaborate lengths needed to effect any improvement of existing tables. It is also significant that the effort involved did not involve recomputation except in instances of discrepancies other

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George von Vega. See Editor's Note, *Works*, Vol. 2, p. 74, ft. for fuller bibliographic details of works cited in this passage.

<sup>65</sup> For further details see below p. 59, ft. 100.

<sup>66</sup> Quoted in Campbell-Kelly (1988), p. 166.

<sup>67</sup> De Morgan (1842), p. 499; (1861), p. 1033.

<sup>68</sup> Van Sinderen (1988), p. 176. Glaisher reports two errors, one remarkable for the fact of the correctness of the entry in Vega against which it was twice checked. See Glaisher (1874), p. 59.

<sup>69</sup> See Campbell-Kelly (1988), p. 165. Original source cited is Fletcher *et al.* (1962).

than last digit variations. Rather, the entire effort is directed towards establishing and maintaining the integrity of information in representation and distribution. Campbell-Kelly's comment that Babbage's *Logarithms* are 'an achievement of craftsmanship, rather than genius' emphasises that the central challenge for the nineteenth century table makers was not abstract theory or mathematical inspiration, but information processing.<sup>70</sup>

The suitability of existing tables, even reputable ones, as a source of derived tables, did not go unquestioned. In the latter half of the century Edward Sang undertook a colossal recalculation of all the major logarithmic, trigonometric and astronomical tables.<sup>71</sup> In 1874 *Nature* published a Note attacking Sang's project. The piece argued that it would be much less trouble to hand-correct a printed copy of Vlacq's tables to eliminate errors that were by then well-known: 'any one who chooses can, without much expenditure of trouble, render his copy of Vlacq all but free from error – much more accurate than any new table could be'.<sup>72</sup> Sang was clearly stung by this and countered by undermining the reliability of even reputable tables as a suitable datum for new editions. He argued that correcting any given copy did not ensure accuracy as 'there will still remain a great uncertainty, arising from the fact that two copies of Vlacq may not be in accordance with each other'. He refers to an erratum sheet in Taylor's Tables that acknowledges non-specific printing errors in an unknown number of individual copies in the same edition. Sang states that the cause of this was that 'the moveable types had been drawn out by the inking dabber, and erroneously replaced by the pressman'.<sup>73</sup> The

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<sup>70</sup> Campbell-Kelly (1988), p. 168.

<sup>71</sup> Sang's project is discussed in greater detail below. See p. 65 *et seq.*

<sup>72</sup> Quoted in Sang (1874-75), p. 423.

<sup>73</sup> *Ibid*, p. 424.

concern here was again not known errors, but those still undiscovered. Sang's argument against correction of existing tables can be seen as a justification for his own vast undertaking to recompute tables from scratch. But the spectre of inconsistencies in individual copies in the same printing would have been damaging and daunting to his opponents, and the claim was one not verifiable by any practical test.

Printed tables were not the only source of derived tables. The extensive cadastral tables prepared in Paris under de Prony's direction during the 1790s were intended to serve in part as a master source for derived tables reduced in scope or precision.<sup>74</sup> While it was always the intention to publish de Prony's tables this was never realised except in partial and fragmentary form.<sup>75</sup> However, the manuscripts were used as a primary source against which to check new tables, as well as a direct source for new editions.<sup>76</sup> De Prony's tables were calculated to between fourteen and twenty-five places depending on the range and function, and

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<sup>74</sup> De Prony's table project is discussed later in this chapter. General Derrécagaix, Director of the Geographical Service to the Army, citing Lagrange *et al.* states that as well being used for important calculations, the tables were 'to serve as a type and model for the construction and verification of tables of any size'. Derrécagaix (1891), p. II.

<sup>75</sup> Derrécagaix (1891) reports that a version of the tables reduced to twelve figures was in preparation for printing by the printer and publisher Fermin Didot. Typesetting was two thirds complete and a hundred stereotype plates already made when the project was abandoned following the collapse of the monetary system in France. Grattan-Guinness reports that some 500 pages of proofs were available in 1802 when printing stopped (Grattan-Guinness (1990), p. 180). Attempts to print de Prony's tables persisted. In 1809 Didot sought to fund a stereotyped edition through reader subscriptions. Benefits to subscribers included concessionary rates (seventy two francs rather than the rate to the public of ninety six francs, and 300 francs for three folio volumes), as well as the gratitude of posterity as named as benefactors in all editions. The offer included arrangements for the return of any moneys via a *notaire* in the event that the target of 300 subscribers was not achieved (*Catalogue des Livres du Fonds de Fermin Didot*, 1809, p. 4, 5. I am indebted to Ivor Grattan-Guinness who made this source available originally provided by Margaret Bradley). The venture evidently failed. The fame of the tables and the expectations that they would be published were considerable. Edward Sang recalled that as a schoolboy aged about ten in 1815 'the almost awe with which we listened to descriptions of the extent and value of the renowned Cadastre Tables'. After Didot yet another attempt failed. This was by Davies Gilbert who proposed in 1819 in the Commons that the French and British governments share the expense of publishing de Prony's logarithm and trigonometry tables. See Sang (1873-1874), p. 375. Also Sang's 1890 account in Horsburgh (1914), p. 40).

<sup>76</sup> For the number of figures in the various ranges of de Prony's tables see *Inventaire Général et Sommaire, Observatoire de Paris*, B6, 1-19. For comments on inconsistency of cited precision see p. 61, ft. 105.

this made them particularly suitable for checking last-place accuracy of tables worked to fewer places. It has already been noted that Babbage checked his seven figure tables of logarithms published 1827 against the manuscripts calculated to fourteen places.<sup>77</sup> An example of de Prony's tables serving as a source for a new edition is the substantial volume of eight-figure logarithms published in Paris 1891 by the Geographical Service of the army. The data were taken directly from de Prony's manuscript version at the Paris Observatory after being rigorously checked.<sup>78</sup> Here the source was a supposedly authoritative manuscript, rather than a volume of printed tables tested in use.

Derived tables ran the risk of new errors being introduced during transcription, typesetting and printing, and the integrity of the copy relied on the pains taken during proofreading to ensure exact coincidence between primary and derived sources. The main anxiety of deriving tables from already existing tables was the propagation of undetected hereditary errors that are immune to detection using coincidence checks. In at least one instance the inadvertent propagation of errors served a forensic purpose in exposing unlicensed copying: in 1827 Babbage reported that a Chinese edition of logarithms at the Royal Society that lacked acknowledgement to any other work, contained the same six errors traced to Vlacq's tables printed at Gouda in 1628.<sup>79</sup> Undiscovered errors, deliberate and inadvertent, 'watermark' an edition and allow the lineage of derived tables to be traced. However, the dangers of hereditary errors were outweighed by the deterrent of recomputation which was undertaken only exceptionally. Campbell-Kelly writes:

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<sup>77</sup> See Campbell-Kelly (1988), p. 163, 165. Also, Hyman, p. 61.

<sup>78</sup> Derrécagaix (1891), p. III. Derrécagaix describes the checking procedures including verification by differencing.

<sup>79</sup> Babbage (1827, *Notice*), *Works*, Vol. 2, p. 69.

Both Brigg's and Vlacq's tables contained many known and unknown errors, but so great was the labour of computation and so great the opportunity for error, that all the table makers who followed "simply" edited, corrected and reduced these tables to practical precision – typically seven decimal places.<sup>80</sup>

However, when the existing canon of generic tables needed extending, or when new functions required calculation, there was no alternative to computing from scratch, and it is to calculation that we now turn.

### Subtabulation

Subtabulation has particular significance in the engine debates: the repetitive low-level calculations associated with the method of differences was the main candidate for replacement by machines. The history of mathematical techniques for table making, subtabulation included, is not easy to trace. Textbooks on methods of numerical analysis tend to be ahistorical and the current state of knowledge presented as though timelessly true.<sup>81</sup> Historical treatments tend to focus on general methods to evaluate functions, with little or no attention given to numerical methods for the repeated evaluation of a function for uniformly incremented values of the argument.<sup>82</sup>

Many of the techniques used by nineteenth-century table makers were based on mathematical theory well-established at the time, and the task of the table

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<sup>80</sup> Campbell-Kelly (1988), p. 162.

<sup>81</sup> There are many technical monographs on numerical analysis. For a representative twentieth century example see Scarborough ([1930], [1950], 1955). Babbage appears to have been among the first to identify numerical methods for machine computation as a potentially distinct branch of analysis. For discussion see Chapter 3, p. 142 *et seq.*

<sup>82</sup> For an historical survey see Schreiber (1994), pp. 585-594.

makers was to translate the formulae into operational sequences to be executed by the human computers.<sup>83</sup> Some techniques were specialised and developed in response to specific needs.<sup>84</sup> By and large, tabulation techniques were adopted and adapted from established mathematical theory and these would be described in the prefaces of published volumes. But there appears to have been no great incentive to collate and systematise mathematical techniques for tabulation as a distinct body of knowledge and enshrine it in publication. While this may be a regrettable, it does not materially hinder the line of argument in that the manual techniques of immediate relevance are those that served as a model for replacement by the engine advocates, and sufficient is known about these for our purposes.

A table can be produced by substituting incrementally increasing values of the argument into the mathematical formula, and redoing the calculation to produce each new value of the function. However, for a combination of reasons discussed below, including skills levels and difficulties of verification, evaluation by repeated substitution *ab initio* was avoided whenever possible. Instead values of the function for more widely spaced intervals of the argument were calculated first and the intermediate values found by interpolation. The widely spaced values of the function, called 'pivotal values', are analogous to fence-posts and the intermediate

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<sup>83</sup> The use of 'computer' to refer to a person was commonplace in the nineteenth century. For occurrences staggered in time see, for example, Babbage to the Duke of Wellington, 1822, *Works* Vol. 3 p. 8; Thomas Colebrooke, *Works* Vol. 2 p. 58, 1825; Beauford to Airy, 14 April 1837, RGO6/2, leaf 4; Airy to Trevelyan, 30 September 1857, PRO T1/6098B/19264. This usage continued well into the 20th century. It was not until the emergence of electronic vacuum tube machines into the workplace in the mid-1950s that dictionary definitions were amended to refer to machines rather than people. For 20th-century occurrences see, for example, Comrie, L. J. (1933); Scarborough (1955), p.1; Sadler (1958), p. 3. Pioneers of electronic computers active in the 1950s recall the human usage. Computers are not the only devices to enjoy human antecedents. In the late nineteenth century 'typewriter' referred to the person who typed. For an example of known mathematical theories being adapted for tabulation see Grattan-Guinness (1990) p. 182. The example is one where Euler's forward difference methods were combined with an iterative formula by Mouton, a seventeenth century astronomer as the basis for operational instructions given to de Prony's computers.

<sup>84</sup> A twentieth-century example is that of bridging differences largely devised by L. J. Comrie, Superintendent of the Nautical Almanac from 1931-1936. See Sadler (1958), p. 3.

values can be likened to a series of intervening slats. Scarborough reflects this in his definition of interpolation as:

. . . the art of reading between the lines of a table, and in elementary mathematics the term usually denotes the process of computing intermediate values of the function from a set of given or tabular values of that function.<sup>85</sup>

The process of generating intermediate tabular values by interpolation came to be called subtabulation and Wilkes states that the use of interpolation by subtabulation dates from the eighteenth century.<sup>86</sup> Reflecting widespread standard practice in the nineteenth century, D. H. Sadler wrote that ‘interpolation, particularly subtabulation, was perhaps the most powerful tool for large-scale computation before the general use of calculating machines’.<sup>87</sup>

A common technique of subtabulation used the method of differences in which each next tabular value is generated by a series of repeated additions.<sup>88</sup> The value of the method was the elimination of the need for multiplication and division, and the reduction of the arithmetical processes to those of simple addition and subtraction. The date of the first use of this technique does not appear to be known. Hutton comments that Briggs originated the method and described it 1624, though the term ‘method of differences’ was adopted only in the nineteenth century. Lindgren suggests that Briggs was the first to write about the technique but that it was known well before the seventeenth century.<sup>89</sup>

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<sup>85</sup> Scarborough (1955), p. 51.

<sup>86</sup> Wilkes (1991), p. 142.

<sup>87</sup> Sadler (1956), p. 3. Sadler was Superintendent of the NA from 1936-1970. See *Superintendents of The Nautical Almanac & Heads of HM Nautical Almanac Office* [<http://www.nao.rl.ac.uk>].

<sup>88</sup> For a technical description see Scarborough (1955), p. 53 *et seq.*. For an accessible illustration see Williams (2003). For a review of the mathematics of these procedures see introductory section to, *Interpolation and Allied Tables*: HMSO, 1956, prepared by the Nautical Almanac Office.

<sup>89</sup> See G&F, p. 311, Note 1.

In operational terms the use of the method started with mathematicians who chose the formulae for the function to be tabulated, chose the particular form (typically a series expansion consisting of a number terms), fixed the range of the table (the start and end values of the independent variable), decided the number of decimals to be worked to, and calculated the pivotal values. If the method of differences was used, as was common, then the mathematicians also calculated the set of differences required to start the process, and these, together with the pivotal values, the starting line of initial values, and a set of procedural instructions, were given to the computers. The computers started with the first pivotal value, and calculated the next tabular value by a series of additions made to the starting line. The series of additions was then repeated using the values from the previous calculation. Each repetition of the process generates the next tabular value and the process continues until the new pivotal value is reached. Subtabulation runs of as many as one hundred to two hundred values between pivotal values were not uncommon.

The great French cadastral tables prepared during the 1790s have a unique place in the canon of table making, and the project illustrates several essential features of contemporary practice. The project was directed by Gaspard Clair Francois Marie Riche de Prony who set up the Bureau de Cadastre in Paris for the purpose, and the venture constitutes the most ambitious single tabulation project undertaken to that time.<sup>90</sup> De Prony's project is representative in that it used the methods of calculation were typical of current table-making practice. It is also

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<sup>90</sup> It is unclear when the project formally started. Glaisher reports that the French Government approved in 1784 a proposal from Carnot *et al.*, for new tables. See Glaisher (1874), p. 56. The project was clearly well under way by 1794: Grattan-Guinness states that by this date 700 results were being produced per day, and that the project was completed in 1801 (Grattan-Guinness (1990), p. 180). Derrécagaix reports that the major work was completed between 1794 and 1799, though this could refer to a subset of results rather than the whole project. See Derrécagaix (1891) p. II. For examples of inconsistencies in the project dates. See Grattan-Guinness (1990), p. 179, ft. 7.



anomalous in that its scale and scope were unprecedented, and the distinctive solutions adopted by de Prony to systematise the process by the division of labour, were then unique in table making practice.

The project can be framed in different ways. In utilitarian terms the purpose of the project was the production of cadastral tables in France for accurate land survey so that property could be used a basis for taxation.<sup>91</sup> This can be seen as part of the larger ambitions of metrication which was to establish a universal standard of measurement based on a unit (the metre) the measure of which was derived from the distance between the North Pole and the Equator.<sup>92</sup> In cultural terms the project can be seen as a clerical activity of the Enlightenment – rationalism and the reduction of the world to number.<sup>93</sup> At a political level the project can be seen as part of distancing France from its pre-revolutionary past. There were an estimated 250,000 different units of measure on France and this diversity was seen as a legacy of protected interests and the privileges of class; metrication, using a ‘natural’ measure was intended to create a rational and uniform standard dissociated from the *ancien régime*.<sup>94</sup> These different categories of interest intersected in de Prony’s project. The production of new cadastral tables was explicitly intended to monumentalise the French metric system. De Prony described the ambitions of the project in March 1801, the year the project was completed:

Since it was desired to give to everything relating to the French metric system a grandeur that would excite attention and a superiority over what been done

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<sup>91</sup> Grattan-Guinness (1990), p. 179.

<sup>92</sup> The metre was defined as one ten-millionth of the distance from the North Pole to the equator. See Alder (2002), p. 1.

<sup>93</sup> See Swade (1991, *ISIS*).

<sup>94</sup> See Alder (2002), p. 3.

to date in order to inspire confidence, I was engaged expressly not only to compose tables which would leave nothing to be desired, but to make of them a monument of calculation as large and imposing as had ever been created or even conceived of.<sup>95</sup>

The aspirational extravagance of the statement was doubtless in part celebratory of post-revolutionary France. However, Grattan-Guinness notes that the occasion on which the notice was read marked the start of attempts to raise funds to finance publication, and the grandiloquence of the aims could have been doing promotional work.<sup>96</sup>

Explicitly following the principles of the division of labour, articulated by Adam Smith in *Wealth of Nations*, de Prony distributed the work to three groups reflecting the hierarchy of mathematical skills involved.<sup>97</sup> The first group consisted of five or six high-ranking mathematicians with sophisticated analytical skills, notable amongst whom were Legendre and Carnot. This group chose the analytical formulae most suited to evaluation by numerical methods, and specified the number of decimals and the numerical range the tables were to cover. The second group of lesser mathematicians, seven or eight in number, combined analytical and computational skills, and this group calculated the pivotal values using the formulae provided and the sets of starting differences. They also prepared templates for the computers, and the first worked row of calculations, as well as the instructions for the computers to carry the sequence to completion.

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<sup>95</sup> Quoted by Derrécaigaix (1891) p. II. The date on which de Prony read the notice (March 1801) is given as the first germinal year IX of the French revolutionary calendar. The excerpt quoted by Derrécaigaix is taken from Prony, G F C M Riche de. *Notice sur les Grandes Tables*. Paris: Baudouin, 1801.

<sup>96</sup> Grattan-Guinness (1990), p. 180.

<sup>97</sup> Babbage recounts the anecdote in which de Prony happened upon Adam Smith's 1776 Treatise in a bookshop. It fell open at the chapter on the division of labour at the time he had reckoned that with only three or four skilled collaborators they would all be dead before the work was done. Smith used the manufacture of pins as an example of the division of labour and de Prony is reported to refer to 'manufacturing his logarithms like pins'. See *Works*, Vol. 8 pp. 136-137.

The third group was the largest and consisted of sixty to eighty computers. These had no more than a rudimentary knowledge of arithmetic and carried out the most laborious and repetitive part of the process. Babbage wrote that the third group required 'the least knowledge and by far the greatest exertions'.<sup>98</sup> Many of the low level computers engaged in de Prony's project were out-of-work hairdressers. With the guillotining of the aristocracy the hairdressing trade, that had tended the elaborate coiffures of the elite, was in recession. The hairstyles of the aristocracy became a loathed symbol of the defunct pre-revolutionary regime and many hairdressers turned their hand to rudimentary arithmetic.<sup>99</sup>

Babbage was well aware of the French cadastral project. He stood in awe of its scope and scale and described the project to Humphry Davy as 'the most stupendous monuments of arithmetical calculation which the world had yet produced'. Two sets of tables were produced, each running to eighteen volumes, and Babbage estimated that the logarithm sections alone contained some eight million figures.<sup>100</sup> The division of labour, and the generation of results by repeated procedures, are features of mechanised production, and de Prony's system featured as a powerful model in Babbage's early arguments for the utility of calculating machines for the production of tables. Babbage argued the labour-

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<sup>98</sup> Babbage (1832), *Works*, Vol. 8, p. 138. Babbage referred to the groups as 'classes'. The human computers, who performed the 'lowest processes of arithmetic', made up the 'third class', with ordinality providing a scale of superiority in the hierarchy of skills. It was the work of the lowest class that was the primary candidate for replacement by Babbage's machine. For Babbage's reference to 'classes' and 'lowest processes' See Babbage (1832)), *Works*, Vol. 8. pp. 138, 141.

<sup>99</sup> Grattan-Guinness (1990), p. 179.

<sup>100</sup> Babbage to Davy, 3 July 1822. *Works*, Vol. 2 pp. 10, 11. Babbage gives two main accounts of de Prony's project. The first is in his letter to Davy. The second is an slightly extended account in his chapter "On the Division of Mental Labour". In *On the Economy of Machinery and Manufactures*, Babbage (1832), *Works*, Vol. 8, pp. 135-137. Babbage viewed de Prony's tables during a visit to Paris and used the opportunity to verify unresolved entries in the proofs of the seven-figure logarithm tables he was himself preparing. Hyman dates the visit as occurring in Autumn 1826 (Hyman (1984, p. 61). Certainly Babbage had seen the tables before publication in 1827: in his Preface he pays express tribute to Laplace, President of the French Board of Longitude, and to the Board for their warmth and helpfulness in facilitating access to the manuscripts. See Babbage (1827), *Works*, Vol. 2, p. 76.

saving benefits of his engines using de Prony's hierarchy of skills, and his case represents a rare instance of an attempt to quantify utility.<sup>101</sup> Babbage later generalised the model, and the disaggregation of process into separate tasks and the economics of the purchase, regulation and surveillance of skills appropriate to each task, later became known as the 'Babbage Principle'.<sup>102</sup>

Independent groups of computers calculated the same results sometimes using different, but mathematically identical, expressions. On completion these were passed to the second group in the hierarchy which compared the two sets of results.<sup>103</sup> The first class of de Prony's mathematicians had the best of it in being largely exempt from numerical work. The second class of mathematician bore the brunt and responsibility of the numerical preparatory work, while the third class, the computers, had the largest share of the drudgery.

The burden of calculation on the mathematicians was further increased by the need to work to a larger number of decimal places than required for eventual printing. One reason for this was to ensure that rounding errors did not accumulate during the successive additions to the point at which they affect the smallest significant digit. If fifteen significant digits were needed in the printed answer, the calculation would sometimes be carried out to twenty or twenty five decimals.<sup>104</sup> Babbage reported that de Prony's table of natural sines was tabulated to twenty

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<sup>101</sup> For discussion of Babbage's argument see Chapter 3, p. 144 *et seq.*

<sup>102</sup> For Babbage's articulation of the principle see Babbage (1832), *Works* Vol. 8, p. 125. For the description of de Prony's project see "On the Division of Mental Labour", *ibid.*, pp. 135. For discussion of wider cultural significance see Schaffer (1994). Also, Pickering (1997).

<sup>103</sup> *Ibid.*

<sup>104</sup> An extreme example of this is given by Benjamin Herschel, Babbage's son, in which calculations for tables of logarithms using the method of differences was carried out to twenty decimal places to ensure the correctness to seven figures. This was not only to cater for rounding errors but because for functions with non-constant differences (logarithms, for example) the deviations that result from series approximation fluctuate. See Babbage, Benjamin Herschel (1872), *Works*, Vol. 2, p. 232.

five decimal places, and one set of logarithms to nineteen places.<sup>105</sup> In the second half of the nineteenth century Edward Sang calculated logarithms to twenty eight places for correctness to twenty five places, and trigonometrical tables to thirty three places for correctness to thirty.<sup>106</sup> The inclusion of additional guard digits to ensure correctness in the last significant printed digits was part of best practice and this compounded the volume of numerical calculation and increased the risk of error.

The generic precision of the tables was in any event high for reasons that remain obscure. Commenting on de Prony's tables Grattan-Guinness writes: 'what remains unexplained is the reason why de Prony chose to calculate these tables to such extraordinary numbers of decimal places in the first place'. He goes on to suggest, perhaps only half-seriously, that the excessive accuracy was de Prony's way of prolonging the project to avoid, or at least delay, a threatened transfer to the Pyrenees, a posting that would flout his wish to remain in Paris.<sup>107</sup> It is possible that the exorbitant precision of de Prony's tables was an expression of the

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<sup>105</sup> There are inconsistencies in reports of both the range and decimal precision of several of the tables. In his letter to Davy, Babbage lists seven tables contained in the cadastral works and cites the number of decimals to which each was worked (Babbage (1822), *Works*, Vol. 2, p. 10). For example, Babbage reports natural sines to be calculated to twenty-five places, compared to the twenty places reported in the catalogue of the *Observatoire*. The precision of de Prony's logarithm tables is often reported as fourteen decimals and they are catalogued as such. Inspection of the two sets of originals in Paris shows that the most significant digit, which first appears at the top of the first column of results, is not repeated in subsequent entries if it remains unchanged, leaving long runs of fourteen residual digits without prefix. The omission of unchanging leading digits may be can give the impression of lower generic accuracy than is actual, and this may be a contributory cause to at least some of the inconsistencies. Babbage warned against the practice of omitting repeated leading digits. See Babbage (1827), *Works*, Vol. 2, p. 78, Rule 6.

<sup>106</sup> Sang's tables were compiled over a period of forty years starting in about 1848. See Horsburgh (1914), pp. 38-40. Craik (2002), p. 34. For discussion of Sang's tables see below p. 65 *et seq.*

<sup>107</sup> Grattan-Guinness (1990), pp. 178, 183. Comrie states that four and six figure tables cover 95% of computational requirements (Comrie (1964), p. v). Lalande stated that five-figure logarithms were 'perfectly sufficient for ordinary operations of surveying and practical astronomy', and seven places 'for higher branches of astronomy and geodetics' (Horsburgh (1914), p. 40). In 1890 Sang stated that fourteen places represent an accuracy 'far, very far, beyond what can ever be required in any practical matter' (See Knott (1914), p. 41). Derrécaigaix records that seven figures were insufficient for calculation of compensation (Derrécaigaix (1891), p. 1).

'grandeur' of the ambitions of the metrication programme, and that its essential purpose was to outdo the scope of existing tables.

Despite the scale and prominence of de Prony's project, surprisingly little descriptive material survives. Grattan-Guinness comments that even the address of the Bureau de Cadastre remains elusive and that

no useful information exists on the organization of the work-room, the full personnel, or the budgets for the project; and apparently the waste sheets used for producing or checking the tables were discarded after use.<sup>108</sup>

However, the general procedures described, albeit from incomplete records, reflect the essential features typical of nineteenth century practice: the use of subtabulation for which only rudimentary arithmetic was required; the division of labour between mathematicians who did the theoretical and preparatory work and the computers who undertook the repetitive arithmetical calculations.

### Errors and Subtabulation

There is no evidence to indicate systematic attempts to measure or estimate overall error rates, or to disaggregate errors in published tables to establish the relative frequency of errors in the different stages of production. Lardner's survey in 1834 of published errata in forty volumes of tables was a landmark attempt to quantify the overall reliability of printed tables, but its purpose was essentially rhetorical. There is no evidence that error rates were monitored routinely as a form of ongoing 'quality control' during tables production. Lardner's survey is discussed in Chapter 3, and an indicative assessment of the reliability of de Prony's tables is

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<sup>108</sup> *Ibid.*, p. 179.

presented in Chapter 2.<sup>109</sup> The brief remarks that follow relate specifically to sources of errors in the techniques and processes of manual calculation.

It is clear that the results produced by computers depended on the accuracy of their own work and on the accuracy of the initial values by the mathematicians to the computers. There was also the risk of secondary errors propagated from existing tables if these were used in the course of the new calculation. Both primary and secondary errors in the material prepared by the mathematicians were common to both sets of independent computers, and errors in correctly calculated results based on erroneous data would evade detection by consistency checks. Babbage warns that ‘these errors are so much the more dangerous because independent computers using the same tables will agree in the same errors’.<sup>110</sup> To save having to repeat the calculation from scratch for each pivotal value, the mathematicians often used the same technique (numerical integration from differences) for these wider intervals, as was used by the computers for the finer interpolations.<sup>111</sup>

It seems that the mathematicians higher up the hierarchy, who excelled at theoretical and analytical work, seemed more prone to commit errors of basic arithmetic than the drudges below, and may have been exempted from routine calculation, not only for economic reasons (computers, who comprised the ‘third class’ in the process were much cheaper to hire than distinguished professional

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<sup>109</sup> For discussion of Lardner’s survey see Chapter 2, p. 107 *et seq.*; Chapter 3, p. 111 *et seq.*. For indicative assessment of reliability of de Prony’s tables see Chapter 2, pp. 81-83.

<sup>110</sup> Babbage (1822), *Works*, Vol. 2, p. 8. This point is also made by Francis Baily (Baily ([1823], 1989), *Works*, Vol. 2, p. 48, item 6). The comment refers to tables of reciprocal numbers of particular use in the in the calculation of starting differences. Computers preparing the first Nautical Almanac published in 1766 ‘were furnished with such books and tables as would be necessary to assist their calculations’. See Forbes (1965), p. 394.

<sup>111</sup> For de Prony’s mathematical methods see Grattan-Guinness (1990), p. 181-183.

mathematicians), but on the grounds of higher error rates. Of de Prony's scheme Babbage observed:

It is remarkable that nine-tenths of this class [the low-level computers] had no knowledge of arithmetic beyond the two first rules which they were this called upon to exercise, and that these persons were usually found more correct in their calculations, than those who possessed a more extensive knowledge of the subject.<sup>112</sup>

There are susceptibilities to error that do not feature explicitly in the accounts of tables production, but are inherent in the techniques used, and these placed an additional and responsibility on the mathematicians. One such is the determination of the spacing of pivotal values. The polynomial expansion for most functions is an approximation and is valid only within a fixed interval. The mathematicians chose the pivotal values sufficiently far apart to reduce the overall number of pivotal values (and so transfer the greater burden onto the computers), and close enough to each other to ensure that the interpolated results between pivotal values stayed true to the function to the requisite accuracy.<sup>113</sup> If the intervals between the pivotal values were too great then towards the end of the run the results calculated by the computers, even if free from error, would not be true to the function being tabulated. Such errors would be common to independent computations and therefore undetectable using verification techniques based on comparison. The calculations to determine the fixed intervals between the pivotal values were themselves complex and laborious.

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<sup>112</sup> Babbage (1832), *Works*, Vol. 8, p. 138. Lardner echoes this: 'the computers who committed fewest errors were those who understood nothing beyond the process of addition'. Lardner (1834), *Works*, Vol. 2 p. 131, ft. \*.

<sup>113</sup> See *Interpolation and Allied Tables*: HMSO, 1956, p. 8.



### The Tables of Edward Sang (1805-1890)

De Prony's project is strictly a late eighteenth-century enterprise which marginally spilled over into the nineteenth century.<sup>114</sup> However, there was a nineteenth-century counterpart, comparable in scale and ambition to the great French enterprise. This was the mammoth undertaking by Edward Sang to recompute from scratch the major canons.

Sang cites the rumoured deficiencies of de Prony's tables as the stimulus for his colossal undertaking. In his account written in 1890 he recalled the genesis of the project:

In 1819 the British Government, at the instigation of Gilbert Davies [sic], approached the French Government with a proposal to share the expense of publishing the Cadastre Tables, and a commission was appointed to consider the matter. The negotiations, however, fell through, for reasons which were never very publicly made known; but in the session 1820-21 the rumour was current amongst us students of mathematics in the University of Edinburgh, that the English Commissioners were dissatisfied of the soundness of the calculations – and so it was that the idea of an entire recalculation came in to my mind.<sup>115</sup>

Over a period of forty years, starting in about 1848, Sang manually compiled logarithmic, trigonometric and astronomical tables worked to a greater number of decimals, over a larger range, than de Prony.<sup>116</sup> The manuscripts filled forty-seven

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<sup>114</sup> Grattan-Guinness gives the finish date as 1801. Grattan-Guinness (1990), p. 180. For uncertainties in the duration of the project above see p. 56, ft. 90.

<sup>115</sup> Sang's 1890 account is quoted *in extenso* by Cargill G. Knott. See Knott (1914), p. 40. Sang was a student in Edinburgh between 1818-1824. See Craik (2002), p. 33. Davies Gilbert (1767-1839) is described as 'a wealthy dilettante of science'. See GoS, p. 37, Note 6. He was MP for Bodmin, 1806-1832, and an active parliamentary lobbyist for science and art. He was President of the Royal Society 1827-30. *CDBN*. Hyman suggests that it was Gilbert's stifling of reform of the Royal Society that prompted Babbage's savage attack on the RS in his *Decline of Science*, published in 1830. See *PC*, p. 97.

<sup>116</sup> Knott (1914), p. 40. For a listing of the tables and their scope see *ibid.*, pp. 44-47. Also, p. 38.

volumes. Ranges of logarithms and prime numbers were worked to twenty-eight decimal places and some trigonometric functions were tabulated to thirty-three places. Unlike de Prony, who enlisted a 'large army of computers', Sang was assisted only by two of his daughters, Jane and Flora, who undertook much of the subtabulation.<sup>117</sup> Julius Bauschinger reporting to the Royal Society of Edinburgh in 1905 wrote, 'I can only express my highest admiration regarding this gigantic work, which I could never have believed it possible for a single man to accomplish'.<sup>118</sup>

As with de Prony, the accuracy of Sang's tables has been disputed. In a recent study Alex Craik states that Sang's logarithms of primes 'appear to be virtually free from error'.<sup>119</sup> However, Craik observes that this view is not one that was always held. Prof. James W. Glover of the University of Michigan, wrote in 1923 to the Royal Society of Edinburgh that:

I regret to say that the errors in the logarithms of the composite numbers are very numerous . . . I have reformed several pages of his entries . . . and besides the errors, all terminating in 5, which you signalise, I have found one or two errors in every page. It is very difficult to conjecture how such a state of things arose . . . They are models of clear systematic writing showing great care in forming every figure . . . Yet it appears that these volumes contain very numerous errors . . . There is, I suppose, just the hope that these lapses, inexcusable as they are, are confined to a part of the work. But this would take a good deal of proving, if indeed proper confidence could ever be re-established. In the meantime, the tables must be considered as condemned.<sup>120</sup>

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<sup>117</sup> For a clear inventory of the balance of works compiled by Sang senior and the daughters see Craik (2002), p. 35. In summary, Jane compiled five volumes, Flora sixteen, and Edward twenty six. The reference to an 'army of computers' is Sang's. See Knott (1914), p. 39.

<sup>118</sup> Quoted in Craik, *ibid.* p. 37.

<sup>119</sup> *Ibid.*, p. 39.

<sup>120</sup> *Ibid.*, p. 40.

In defence of Sang, Craik suggests that the deficiencies identified by Glover resulted from the use of a pre-existing table of twenty-eight figure logarithms that had become worn from overuse. His argument is that the deficiencies are localised and therefore unrepresentative of the whole.<sup>121</sup> Craik is robust in his defence of Sang's view that the tables 'surpassed the Cadastre tables in accuracy and extent' and regards Glover's criticisms as anyway 'unjustifiably harsh'.<sup>122</sup> With the reliability of the tables contested it is difficult reach a definitive position on the overall reliability of Sang's work compared to de Prony's. The balance of evidence favours Sang: the rigour of his methods, the display of transparency in the procedures, and the recording of all intermediate results lend circumstantial support to Craik's defence.<sup>123</sup> It is also fair to say that the nature of the criticisms of de Prony's tables is more damaging than those directed at Sang. In the early 1870s the accuracy of de Prony's tables was the subject of a dispute between Sang and Pierre Alexandre Francisque Lefort. Sang argued that de Prony's methods were flawed, and concludes with damaging finality that 'the method followed in the calculation of the Cadastre table of de Prony was an egregious blunder. The result in accordance with the method'.<sup>124</sup> The allegation that the deficiencies were methodological is largely substantiated by the force of his argument and by the examples he cites. On the other hand, the worst that has been said against Sang's tables is that there is evidence of a lapse, mitigated by circumstances that suggest that the deficiencies are anomalous.

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<sup>121</sup> *Ibid.*

<sup>122</sup> *Ibid.* pp. 38, 40.

<sup>123</sup> Unlike de Prony, Sang kept 'a complete and clear record of all the steps by which . . . results were reached' and he advertises this as a demonstration of accountability. See Knott (1914), p. 43.

<sup>124</sup> Sang (1874-75), p. 431

That so colossal a task could be accomplished by one man with only the help of two of his daughters undermines factory production as a model for such work. The mathematical techniques Sang used have a strong component of craft and practitioner's skill compared to the 'mechanical' repetition of banal procedures in de Prony's scheme.<sup>125</sup> Though three Sangs took four times longer than de Prony's 'army' of about one hundred staff, Sang's project demonstrates that tabulation on a comparable scale was apparently achievable without the factory model. Reliability was not perceived to be a casualty of 'cottage' methods: prior to the 1920s the accuracy of Sang's tables was uncontested, and contemporary acceptance of high levels of reliability in Sang's tables would have indicated that infallible machines were not the only solution to the problems of human error, and that precision and reliability were indeed achievable by human agency unaided by machinery.<sup>126</sup>

Though Sang's table-making activities represent the largest single tabulation undertaking in the nineteenth century, and offer a model to rival that of de Prony, they do not feature in the arguments for or against the utility of calculating machines. Babbage, for one, makes no mention of Sang in his entire collected works. At the time that Peel's government finally axed Babbage's engine project in 1842, after two decades of design, construction, and funding, Sang had yet to begin his forty-year task, and it is possible that with the public debates largely over, Sang's work was ignored by those advocating or disparaging the machines. However, though open advocacy for engines diminished after 1842, Babbage remained actively engaged with the engines till his death, in 1871. Yet his writings make no mention of Sang's work either in attack or defence. It appears that

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<sup>125</sup> For a summary of Sang's methods see Craik (2002) p. 40.

<sup>126</sup> The number of decimals for practical use was disputed and there were allegations that Sang's tables owed more to de Prony than Sang was willing to acknowledge. (See Craik (2002), pp. 37, 38). But there are no contemporary allegations that impugn the reliability of the tables.

Babbage did not even own a copy of any of Sang's tables: there is no reference to Sang in the auctioneer's inventory of Babbage's library which included the most extensive contemporary collection of published tables and related works.<sup>127</sup>

Babbage's silence might seem to concede the damage Sang's work does to his cause. But those hostile to the engines are also silent.

The absence of reference to Sang's work in the engine debates raises the question of audience, and the machinery of publicity. It is difficult to know how widely known was Sang's larger enterprise, when the details of the project became known, by what route, and to whom. Sang's five-figure logarithms for numbers up to 10,000 were published in 1859, and are listed in de Morgan's extended bibliography of tables published in 1861, but without any descriptive comment on any unusual methods used in their compilation.<sup>128</sup> The range of these tables is modest, and appear not to have attracted any special attention. Sang's twenty eight figure logarithms of numbers up to 10,000 were displayed in manuscript at a meeting at the Royal Society of Edinburgh in December 1874 when his paper identifying flaws in de Prony's work was read.<sup>129</sup> This was three years after Babbage's death. If his undertaking was not visible until it was near completion in the late 1880s, its emergence would have post-dated Babbage's death by about a decade, long after the engines had dropped off the agenda of political and scientific life. The balance of evidence suggests that by the time Sang's project was sufficiently advanced to feature as a factor in the debates, the political heat had already gone out of the dispute, and the question of the utility of the machines was no longer relevant.

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<sup>127</sup> See auctioneer's catalogue, Sotheby, Wilkinson, and Hodge (1872), pp. 34-157. A copy is held in the Babbage papers in the Waseda University Library, Tokyo.

<sup>128</sup> De Morgan (1861), p. 1007.

<sup>129</sup> Sang (1874-75), p. 421.

## Summary

Primary and secondary literature on the history of tables tends to be fragmentary and, in general, gives little detailed attention to the processes involved in the production of tables. However, sufficient is known about nineteenth-century mathematical methods and operational procedures to interpret the arguments for and against the engines.

From the early 1820s manual mechanical calculators were developed in parallel with automatic calculating engines. However, mechanical calculators were of little practical use to table makers: the legacy of the eighteenth century, though rich in conception and inventiveness, left little in the way of devices robust and reliable enough for routine use; the arithmometer, introduced in 1820, the strongest potential rival to the calculating engines, took over fifty years to establish itself as a viable product, and had little practical significance until towards the end of the century. Because of the dependence of arithmometer-like devices on human agency for their operation, they were unthreatening to the Utopian ideal of the engine advocates, who sought to eliminate human agency from the whole train of processes.

By the nineteenth century the main canons of generic tables had already been computed. Because of the unavoidable vulnerability of manual methods to error, recomputation was rare. The extensive recomputation of logarithm tables by Edward Sang during the second half of the century was exceptional. Whenever possible new editions of tables were derived from existing tables with reputations for reliability and tables were computed from scratch only when there was little alternative. Babbage's logarithm tables, derived from Callet's and meticulously checked, is a rare example of a newly published logarithm tables of any significant

extent in the nineteenth century. The most widely used technique for manual calculation was interpolation by subtabulation, and the commonest version of this was using the method of finite differences. The advantage of the method, to both manual and machine tabulation, was the elimination of the need for multiplication and division, and the simplification of the arithmetical processes to repeated addition and subtraction. This reduced skills levels, and therefore the cost, of large scale manual tabulation projects of which the calculation of the French cadastral tables, under de Prony, is the most ambitious example. For the engine advocates, the requirement for addition and subtraction only, simplified the design of the mechanism to practicable levels.

The dependence on human agency made each stage of the tabulation process vulnerable to error, and errors in tables played a defining role in the advocacy for the engines. However, there is no evidence that error rates were quantified or systematically analysed during table making, and the relative vulnerabilities of different processes to error tend to be impressionistic and anecdotal. To ensure last-figure accuracy there was a need to work to larger numbers of digits than was required for eventual printing. This placed an additional burden on the mathematicians and table makers, and represented additional risk of errors. However, the need for such additional 'guard digits' does not wholly explain the excessively large numbers of digits to which tables were worked, and the reasons for this remain obscure.

The social organisation of the workforce in the production of the French cadastral tables extended Adam Smith's principle of the division of labour to mental processes. The division of labour, and the generation of results by repeated procedures, are features of mechanised production, and de Prony's system featured as a powerful model in Babbage's early arguments for the utility of

calculating machines. Edward Sang's massive 'single-handed' tables project, undertaken in the latter half of the century, undermines the argument that reliability and high precision were achievable only through a factory model of industrial production. However, Sang's work does not feature in the engine debates, and his project appears to have matured too late to play a significant role in the disputes.



## Chapter 2: Verification, and Generic Process

*It is hardly credible, to those who have not tried, how much the perceptions are dulled by the monotonous comparison of one column of figures with another, how many and how gross errors both eye and ear, when tired, will suffer to pass.*

– Augustus de Morgan, 1842.

### Introduction

The previous chapter discussed the difficulties of generating numerical information for tables, and highlighted the vulnerability to error of manual methods. The next step for the table makers was to disseminate the tables to users and this presented a new set of difficulties. The primary medium of transmission was print and paper, and handing over of the authored source to the printer was the start of a series of generic processes that drew on several well-established trades and practices specifically typesetting, proofreading and printing. Each of these was reliant on a greater or lesser extent on human agency and, inevitably, was vulnerable to error.

The engine advocates argued that machinery would effectively eliminate the risk of error not only in calculation, but also in transcription, typesetting, and printing. The Utopian ideal of the engine advocates was to extend the infallibility of machinery to the whole train of processes, and Babbage's earliest designs incorporated the capability for automatic typesetting, and the automatic production of 'stereotypes', or moulds, from which printing plates could be cast for use in conventional printing presses. The promise of machinery was that all sources of human error would be eliminated once and for all, and that the generation of the

authored source, as well as its dissemination through printing, would yield to the ‘untiring action and unerring certainty of mechanical agency’.<sup>1</sup>

Before being handed over to the printers, the tabulations were checked for accuracy using various verification techniques. This chapter first describes verification processes. It then discusses the generic processes of typesetting, proofreading, stereotyping, and printing, with special reference to the needs of the table makers, specifically the management of errors and attempts to increase readability through improved typography.

#### Verification by Double Computation

One of the main techniques of avoiding errors of calculation was for the same calculation to be performed by different computers, and then for the independently computed results to be compared for discrepancies. The principle of the method was that two computers, working without collaboration, are unlikely to make the same mistake. The practice appears to have been well-established by the nineteenth century. Nevil Maskelyne, Astronomer Royal, used the system in the preparation of the Nautical Almanac, first published in 1767.<sup>2</sup> The Almanac tabulated the distance of the moon from the sun and the bright stars at intervals of a few hours and allowed mariners to determine ship’s longitude at sea using the method of lunar distances.<sup>3</sup> In Maskelyne’s scheme one computer was called the ‘computer’, the second computer was referred to as the ‘anticomputer’ and the

<sup>1</sup> The phrase is Lardner’s. See Lardner (1834), *Works*, Vol. 2, p. 169.

<sup>2</sup> Maskelyne (b. 1732), was the fifth Astronomer Royal and served from 1765 to until his death 1811. For an admiring biography of Maskelyne see Howse (1989).

<sup>3</sup> Forbes (1965), pp. 391-401. For an account of the importance of astronomical navigation to colonisation and trade see Ronan (1967), Ch. III, pp. 47-9.

checker was called the ‘comparer’.<sup>4</sup> The computer and anticomputer worked separately, usually from their own homes, in a ‘cottage’ network, and their results were sent to the Astronomer Royal who sent them to the comparer for consistency checking. The effectiveness of the technique relies in part on the absolute independence of the computers. Two computers, Keech and Robbins, were instantly dismissed when, hired to assist in the preparation of the Almanacs for 1771 and 1772, they were found to be ‘acting collusively’.<sup>5</sup>

The technique of double computation was not foolproof. It was not unknown for computers to produce the same incorrect result despite insulation from each other. Such errors would avoid detection by a comparer and Lardner moralised with characteristic puff that ‘falsehood in this case assumes that character of consistency, which is regarded as the exclusive attribute of truth’.<sup>6</sup> Lardner cited three instances of discovered errors being first masked by their agreement.<sup>7</sup> He reported that de Prony on occasions found ‘three and even a greater number of computers, working separately and independently, to return him the same numerical result, and *that result wrong*’.<sup>8</sup> It seems that de Prony was not alone. Lardner reports that Lieutenant W. S. Stratford had similar experiences preparing the Nautical Almanac in the 1830s.<sup>9</sup> In the third instance cited by Lardner, Francis Baily, suspecting an error in a newly published set of astronomical tables which

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<sup>4</sup> *Ibid.*, p. 85. For use of the term ‘anticomputer’ see Croarken (2002, unpublished).

<sup>5</sup> *Ibid.* Also Forbes (1965), p. 394. Forbes does not cite the date of the dismissal. Croarken (2002, unpublished) gives the date as 1770. Croarken also reports that in addition to firing the two miscreants Maskelyne demanded that they pay for the additional work of the comparer.

<sup>6</sup> Lardner (1834), *Works*, Vol. 2, p. 134.

<sup>7</sup> Lardner’s views are discussed in more detail in Chapter 3.

<sup>8</sup> *Ibid.* Emphasis.

<sup>9</sup> Lardner refers to one ‘Mr Stratford, the conductor of the Nautical Almanac’ with no further identification, *Works*, Vol. 2, p. 134. This is evidently Lt. W. S. Stratford, Superintendent of the Nautical Almanac, 1831-1853. See *Superintendents of The Nautical Almanac & Heads of HM Nautical Almanac Office* [<http://www.nao.rl.ac.uk>].

had already been verified by comparison ‘with great care and attention’ by Stratford, recomputed the position of the star, and himself ‘obtained precisely *the same erroneous numerical result*’.<sup>10</sup> The occurrences could have resulted from the same flawed procedures being correctly followed by different computers, or from the same incorrect starting values or erroneous auxiliary tables being used by the independent computers. However, Lardner does not offer any explanation for these coincidences. In drawing attention to the unreliability of verification by double computation he appears to be more concerned to advertise the deficiencies of non-mechanised methods than to analyse possible causes.<sup>11</sup>

To further reduce the risk of error the separate computers were sometimes given computationally different but mathematically equivalent formulae to find the same result.<sup>12</sup> If mathematical relationships are expressed in different but equally valid ways, the process of evaluation follows a different computational process: the order of operations may be different as might the specific arithmetical functions used. Babbage gave an example in which an algebraic expression arranged one way required thirty-five multiplications and six additions. Arranged differently, the same result was produced using five multiplications and one addition.<sup>13</sup> The context of his observation was computational efficiency rather than psychology. In the context of error detection, the purpose of using different arithmetical procedures helped to disrupt shared mental and operational patterns that might lead to the same incorrect result. The technique reduced the risk of error but could

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<sup>10</sup> Lardner (1834), *Works*, Vol. 2. p. 134. Italics original.

<sup>11</sup> Chapter 3 argues that it was in Lardner’s interests to magnify the problem of tabular errors.

<sup>12</sup> See Grattan-Guinness (1990), p. 180.

<sup>13</sup> Babbage (1837), *Works*, Vol. 3, p. 60. In 1956 the Nautical Almanac Office defined computation as ‘the art of obtaining a *correct* numerical result . . . with the *minimum* amount of calculation’ (italics original). See *Interpolation and Allied Tables*: HMSO, 1956, p. 6. Babbage predicted the need for a new branch of mathematical analysis dedicated to optimising computational efficiency. These ideas are explored further in Chapter 3.

not eliminate errors arising from flawed data common to the different operational procedures.

### Self-verifying Features of Subtabulation

There are self-verifying features of subtabulation that helped the table makers. As discussed in Chapter 1 the method of differences allowed successive values of a function to be calculated by repeated addition. Beginning with a starting line of values the computer calculated a value in the table by a series of simple additions. The next value was calculated using the line of values from the immediately previous calculation and successive values were progressively generated in this way. An inherent feature of this process is that each new value depends on all prior values. Babbage pointed out that if an error occurred it would propagate and corrupt all subsequent calculation and 'any such error would have rendered the whole of the rest of the Table untrue'.<sup>14</sup> He argued that subtabulating using differences, though easier, was therefore more liable to error than direct computation of each tabular result. However, the incremental dependence that cascades a single error through the whole train of subsequent calculations also provides built in means of verification: since each value depends on all its predecessors it follows that the correctness of the last value gives a high degree of confidence in the correctness of all preceding values. This feature was well known. Babbage wrote:

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<sup>14</sup> Babbage (1864), p. 50.

If we calculate its last term directly, and if it agree with the last term found by the continual addition . . . we shall then be quite certain that every term through-out is correct.<sup>15</sup>

Verifying a long run of subtabulated values was therefore reduced to checking the correctness of only the last value, by comparison with an independent computation, and this substantially reduced the labour of verification.

There is a further self-verifying feature of subtabulation that involves using the pivotal values as a check. As described in the last chapter the process of subtabulation provides, by interpolation, the intermediate values for finer increments of the argument between widely-spaced pivotal values. If subtabulation is continued one value beyond the last value of the run then the next pivotal value is reached. If there are no errors then the subtabulated value should be identical to the precalculated pivotal value, and agreement between the two is an independent verification of correctness.<sup>16</sup> The pivotal values themselves could be calculated using the method of differences and thereby benefit from the advantages of last-value verification described above. In the absence of worksheets used by the computers or the comparers there is no conclusive evidence that these verifying techniques were used, say, by de Prony. However, the self-verifying features of subtabulation using differences were part of well-established computational practice and it may be assumed they were routinely used by table makers.<sup>17</sup>

There is at least one verification technique that can be applied *post factum* to already calculated tabular values. The technique is based on differencing the tabulated values, in effect reversing the process of numerical integration used in

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<sup>15</sup> *Ibid.*

<sup>16</sup> See Wilkes (1991), p. 142-43.

<sup>17</sup> Sang criticizes Vlacq for adopting procedures that forfeited the advantages of self-verification. See Sang (1873-74), p. 373.

subtabulation. The technique is highly sensitive and involves only repeated subtraction. First differences are taken by subtracting successive pairs of tabular values; each pair of tabular values produces one new difference. Second differences are taken by differencing the first differences, and so on. For functions subtabulated by polynomial approximation, the highest order difference converges to zero or fluctuates around zero within small limits. For correctly tabulated functions lower order differences tend to be roughly equally spaced and the presence of aberrations can often be detected by visual inspection and simple mental arithmetic. Disruption of the smoothness in the spacing of lower order differences signals that something is amiss, and higher order differences can then be tabulated for closer inspection. The effect of even small errors in a tabular value is to produce wild fluctuations in the higher order differences and these deviations are readily apparent even by visual inspection.<sup>18</sup>

Babbage makes no mention of verification by differencing. Wilkes is scathing and incredulous:

The use of higher order differences for checking, however, was something that never occurred to Babbage and his circle, who believed that the only way of checking a value in a table was to make an independent computation . . . Today it seems almost incredible that Babbage should not have realized that differences could be used for checking when he so thoroughly understood their use for subtabulation.<sup>19</sup>

Wilkes later softened his view slightly but is no less damning:

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<sup>18</sup> For a worked example see Wilkes (1987), p. 205. For a clear statement of the value of the technique and a worked illustration of the effect of an isolated error see *Interpolation and Allied Tables*: HMSO, 1956, p. 9.

<sup>19</sup> Wilkes (1987), p. 204-5.

A close examination of the writings of Babbage and his contemporaries reveals that they were strangely unfamiliar with the use of differences for checking, or at any rate, with the use of differences beyond the first. They appeared to proceed on the assumption that the only way to find errors in a table was to repeat the calculations, or, ideally, to compare the table with one computed entirely independently.<sup>20</sup>

Wilkes' dismissal of Babbage's contemporaries is harsh. Verification by differencing was used in the late eighteenth century by the comparers for the Nautical Almanac's prepared by Nevil Maskelyne.<sup>21</sup> Airy recorded in 1856 that the technique was used to check lunar tables at the Royal Greenwich Observatory, and in 1891 Derrécagaix described elaborate differencing methods used to check for printing and calculation errors in proofs of his eight figure logarithm tables typeset from de Prony' tables.<sup>22</sup> This suggests that the technique remained in more or less continuous use during the century, or at least that the technique was not lost. Airy went further when he proposed that the calculating engines be adapted to use the technique, and moreover, that the use of calculating engines might well lie in their ability to verify existing tables by repeated subtraction (differencing) rather than to generate new tables by repeated addition. But this came too late to make any difference to the fate of the machines.<sup>23</sup>

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<sup>20</sup> Wilkes (1991), p. 143.

<sup>21</sup> See Croarken (2002, unpublished.)

<sup>22</sup> Airy (1856), p. 225. The French technique involved differencing results summed in groups both vertically and horizontally. See Derrécagaix (1891), p. III-IV.

<sup>23</sup> Airy (1856), pp. 225-6. Airy's views on differencing by machine is discussed in Chapter 5, pp. 254, 266, 281.



## Checking by Comparison

Whether verifying independently calculated results or checking for transcription errors, the major technique of error detection was by comparing two sources. There is little contemporary reference to any special practices for checking manuscripts against each other, nor to the frequency of errors revealed by the method. However, De Morgan described best-practice procedures for checking a *printed proof* against a manuscript and many of these techniques will have applied equally well to manuscript sources.

De Morgan refers to comparers working together in reader-listener pairs, or working alone using visual comparison only. Both eye-only and eye-and-ear verification appear to have been in common use, and preference appears to have been a matter of personal choice.<sup>24</sup>

As mentioned in Chapter 1 no contemporary analysis has come to light indicating that routine analysis of the frequency of errors in tables during their preparation, or of the differential vulnerability to error of the separate preparatory stages. However, the two independently computed sets of manuscripts from de Prony's project survive, and the nature and frequency of the corrections provide an indicator of the error rates, and also of the effectiveness of comparison as a method of error detection. Both manuscripts sets are in Paris but not under the same roof: one set is held at the *Observatoire de Paris*, the other at the *Bibliothèque de l'Institut*.<sup>25</sup> The physical separation of the two sets rules out direct visual

<sup>24</sup> De Morgan (1842), p. 500; (1861), p. 1015. Later work raised doubts about the effectiveness of the 'reader-listener' arrangement. See below p. 95, ft. 61.

<sup>25</sup> The set currently at the *Institut* was found in 1858 in the possession of a descendant of de Prony. (Grattan-Guinness, (1990), p. 180). Each set comprises eighteen volumes one of which contains the *Exposition des Methodés* and some sample tables. In the *Institut* set the *Exposition* is identified as Vol. 1. This is listed as Introduction in the *Observatoire* set. The custodianship of the *Observatoire* set is dismaying: in June 1998 only volumes 1-16 could be found. The rest could not be accounted for.

comparison; detailed comparison is further hindered by the fact that the folios of the *Observatoire* volumes have no page numbers. However, a survey of corrections does provide revealing pointers.

The first observation to make is that the number of corrections is very large, and it is clear from this that the contribution of errors from the combined processes of calculation and transcription is substantial. Three distinct categories of correction are in evidence: contemporary corrections in which single digits of a number were overwritten in original black ink, a practice that on occasions renders the entry unreadable; cut-and-paste corrections in which a panel spanning several columns, and in some instances the whole width of the page, has been pasted in, and results re-entered in the original hand; finally, corrections in red ink probably dating in the *Institut* set from 1862.<sup>26</sup>

The relative frequency of the different categories of corrections varies. Corrections to single entries by overwriting are numerous but episodic, that is, their distribution does not follow any observable pattern. By far the largest number of corrections is to systematic errors. Typical of these is the overwriting of one digit in the same position for all entries on the same page. A common form of this would be to increment, say, all the third digits in a column of entries by '1' down the whole page. The worst case found was a run of forty seven double-page openings with either two or three columns of each page overwritten in one digit position incrementing the original digit by '1'.<sup>27</sup> Without the computers' worksheets it is impossible to tell whether these are systematic calculation errors correctly transcribed, or systematic transcription errors from correctly calculated values.

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<sup>26</sup> For an example of a cut-and-paste panel which extends across the page see results for log 14791-log 14800 in *Institut* Vol. 1 (pages not numbered). The date of the corrections is inferred from an inscription, 8 May 1862, in the *Institut* set, 'Collated in conformity with the conventions of the archives of the Observatoire'.

<sup>27</sup> See tabular entries for log 14050 to log 18790 in the *Institut* set (pages not numbered).

Overall, the large number of corrections confirms the value of checking by comparing independently executed processes.

The table-makers would resolve discrepancies between the two sources and make corrections to produce a single corrected source for delivery to the printers for typesetting.

### Typesetting

Typesetting was a well-established trade for literary book production. Setting type was carried out by a compositor, who read from the authored source and set each individual digit in loose metal type. In the case of numerical tables groups of digits formed the number values, a fixed number of these groups formed a line of type, and a block of lines formed the page.

Attempts were made to automate typesetting but success was neither immediate nor complete. The first patented cold-metal composing machine for typesetting was by William Church in 1822. This machine, like all its cold-metal successors, held ordinary moveable type in magazines above the machine, and type was released a piece at a time, one piece for each character, by an operator depressing keys on a keyboard. The process of selecting type by operating a keyboard was still manual; it was the positioning of the type in lines, blocks and frames that was automated.<sup>28</sup>

The drawback of cold-metal machines was that line justification (the alignment of text to produce straight margins at the edges of the page) was still manual. A further drawback was that they used precast type which needed to be

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<sup>28</sup> Gaskell (1972), p. 274. Babbage met Church and viewed plans for his machine. For further discussion see Chapter 3, p. 133. Also below p. 89.

distributed for reuse by reloading the magazines. Cost savings on labour were not great and few nineteenth-century printers used automated typesetters.<sup>29</sup> Those that did used them primarily for periodicals and newspapers where speed was more important than the niceties of typography.<sup>30</sup>

In the last decade of the century hot-metal composing machines revolutionised the efficiency and economics of the printing trade. The development of self-contained hot-metal machines that cast type from molten metal as it was composed, was slow and complex. Linotype machines, which cast a line at a time and which became synonymous with the process, were not produced in large numbers until the 1890s.<sup>31</sup> However, it is unclear whether or when they were used for typesetting numerical tables. For most of the nineteenth century, and certainly during the period in which the utility of calculating machines was contested, traditional manual procedures remained the norm.

Reading from the authored source, compositors retrieved individual digits of type from boxes filled with samples of the same character, and set each digit of each result in loose type to form the groups, lines, and blocks of numbers to make up the page. The correct selection of type was guided by the habitual location of the box in the matrix of boxes, without the face of each sample being verified as it was used. Errors arose when typesetters selected type from the wrong box ('mental lapse') or from the incorrect stocking of the boxes with appropriate type before retrieval ('foul case').<sup>32</sup> Babbage was especially concerned about the

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<sup>29</sup> For a detailed analysis of the economics of book production in the nineteenth century see the recent study by Weedon (2002).

<sup>30</sup> Gaskell (1972), p. 274.

<sup>31</sup> *Ibid.*, p. 276.

<sup>32</sup> A 'case' was a large wooden tray divided into compartments with one sort of type character in each box. See Gaskell (1972), p. 34. For a discussion of compositing errors see *ibid.* pp. 347-48.

hazards of foul case. His anxieties led him to devise ways of verifying that only type of one species was resident in each of the ten boxes. He proposed a mechanical solution in which each piece of type of the same character had a slot in a position common to that character allowing a wire to be threaded through. If the type intended for one box was lined up, rogue type was detectable by obstructing the smooth passage of the wire.<sup>33</sup> It would not be possible from an analysis of printed errors to distinguish between errors of arising from mental lapse or from foul case, and it is impossible to know whether Babbage's remedial device, described at an early stage of his designs, was an inventive response to a pseudo-problem by a gentleman-outsider to the trade, or whether his remedy would have made any useful contribution had it been implemented.<sup>34</sup> However, Gaskell confirms the prevalence of foul case:

No doubt it would have been hard to find a case of type in any printing house that had positively no letters in the wrong boxes, but some cases were fouler than others. If a case was carelessly overfilled it was easy for type to spill over from one box to another, usually into the box immediately below. But ordinary distribution could get wrong type into almost any box, either because a compositor made a mistake at the beginning of a word he was distributing and so dropped all its letters successively into wrong boxes, or because he allowed type to spill into the case . . .<sup>35</sup>

Ensuring the correctness of type using Babbage's method may have offered some advantage but verifying the type before distribution would add an extra stage to the process.

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<sup>33</sup> Babbage (1822), *Works*, Vol. 2, p. 31. Also Babbage (1864), p. 44-45.

<sup>34</sup> Babbage's early speculations date from 1821-2 and are based on attempts to automate typesetting on the assumption that loose type was the medium. His plans to use loose type were superseded in the mid-1820s by designs for automatic typesetting using print-wheels. The new apparatus eliminated both loose type and the human compositor. For further discussion see Chapter 3, p. 134, ft. 64.

<sup>35</sup> Gaskell, p. 347-348.

Manually typesetting numbers rather than language text might appear to pose special difficulties: letters make up recognisable words, whereas numbers have no immediate meaning, and the compositor had no intuitive sense of whether one digit has any sensible relationship to the one before or after. Surprisingly, typesetting does not appear to have been a source of undue anxiety as a source of error, at least by the 1840s. De Morgan commented on how, in the case of literary texts, a reputable compositor would improve the integrity of a manuscript source and professed surprise at the accuracy with which ‘first-rate London printers can turn out their proofs, even where the manuscript is criminally bad’.<sup>36</sup> It is clear that this tribute was not confined to literary texts, but included mathematical tables. Indeed, the standards of top printers appear to have been disconcertingly high, to the extent that the absence of error led to a sense of insecurity about the quality of the checking:

We have frequently looked at page after page of table-matter more times than we should otherwise have thought necessary, merely because the total absence of detected error left it an unsettled point whether it was the excellence of the proof, or a temporary suspension of our own quickness of perception, which caused the absence in question.<sup>37</sup>

In his continuing eulogy to the London printers de Morgan recounted an anecdotal instance in which the Nautical Almanac of 1845 with 500 octavo pages of numerical tables was reset from scratch and proof printed in seventeen working days. The apparent absence of any error convinced a different printer that the tables had not been reset at all but reprinted using the original type frames.

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<sup>36</sup> De Morgan (1861), p. 1015. This is a variant of the same claim carried forward from the version of the article published in 1842.

<sup>37</sup> De Morgan (1842), p. 501.

Attestations from the typesetters and their overseers were secured to convince him otherwise.<sup>38</sup>

One incentive to encourage high standards of typesetting from printers was for the author to insist that error-checking was carried out only when the whole volume was printed for proofing rather than verified piecemeal as pages were set. De Morgan encouraged authors to insist on this in their negotiations with printers.<sup>39</sup> His recommendation was based on established practices in the trade: piecework compositors (but not apprentices) were obliged to correct mistakes at their own expense.<sup>40</sup> If pages were checked as they were set then the knock-on effect from major errors could be preempted and the burden of spotting the errors fell on the table-makers. It was quicker, and therefore more profitable, for the typesetters to work in this piecemeal way than to bear the responsibility and cost of correcting a whole section or volume that might be spoiled by early errors cascading through the whole work. Insisting that proofing was done all at once when the section or volume was finished placed a greater burden of responsibility on the printer and was intended as an incentive for the typesetter to work with greater care.

While typesetting does not itself appear to have been a major source of anxiety for table makers, at least when reputable printers protective of their reputations were engaged, loose type set in frames was vulnerable to accident and subsequent derangement. If more than one piece of type fell out before proof printing it was a puzzle as to which digit belonged where, without reference to a source. If the frames were stored for later editions, reference sources may not be to hand at the time of any mishap. Babbage cited an error in Adrian Vlacq's tables

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<sup>38</sup> De Morgan (1861), p. 1015, ft. Subsequent verification revealed thirty-three errors in the whole volume.

<sup>39</sup> De Morgan (1842)

<sup>40</sup> Gaskell (1972), p. 348.

printed in 1628 from which he surmised that a '4' and '8' had fallen out from two separate lines and been transposed in careless replacement.<sup>41</sup> De Morgan gave a more alarming example in which a line of type fell out and was replaced at the top of the compartment instead of the bottom. This shifted the numbers and misaligned the column of results in relation to the argument. 'The consequence was twenty six gross errors, of a far worse kind than the author could have made, unless he had tried'. The contribution to the error count was substantial: only ten other errors were found in the entire work of 1,020 large quarto pages filled with figures.<sup>42</sup> De Morgan concludes that when it comes to correctness he inclines to the view that of all the agents party to the final printed product, 'the printer is the most important of all'.<sup>43</sup>

It was remarked on in Chapter 1 that unlike literary texts, the preparation of calculable tables involves exact mathematical rules that determine the precise sequence of digits, and that embodying such rules in mechanism created the unique opportunity for mechanised authorship. As part of their Utopian ideal the engine advocates proposed to couple the calculating section of the engine directly to an automatic typesetting machine, and in so doing eliminate the need for manual transcription and manual typesetting. The integration of calculation with typesetting was part of Babbage's earliest conception of his engines, and his speculations

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<sup>41</sup> Babbage (1827), p. 66. *Works*, Vol. 2, p. 69. This same example is used by Lardner who ascribes the removal of the type to the loose type being drawn out by adherence to the inking balls. See Lardner (1834), *Works*, Vol. 2, p. 134-135. Babbage mentions the dangers of 'drawing the type' in his unpublished paper written in 1822 (*Works*, Vol. 2 p. 30) but does not cite the Vlacq transposition as an example of this and Lardner could be taking a liberty in using the error as an instantiation of the generic dangers of 'drawing type'. Given that Vlacq's tables were printed at Gouda in 1628 it is difficult to see how Lardner's suggestion can be anything but speculative. Edward Sang cites errors arising in Taylor's tables when 'moveable types had been drawn out by the inking dabber and erroneously replaced by the pressman'. See Sang (1874-75), p. 424; also Chapter 1, p. 50.

<sup>42</sup> De Morgan (1861), p. 978, ft.

<sup>43</sup> *Ibid.* p. 978



dating from 1821-2 include schemes for automatic mechanical typesetting.<sup>44</sup> His earliest descriptions used loose type, and his interest in Church's machine was clearly to learn how this might be done. In an unpublished manuscript from 1822 Babbage mentioned automatic stereotyping using copper plate. But his experiments returned him to loose type and in 1824 he opted for automatic typesetting using print wheels.<sup>45</sup> Here he exploited the fact that numerical texts required only ten distinct characters (the decimal numbers '0' to '9') instead of the full set of alphanumeric characters plus punctuation required for literary texts. Each print wheel had the ten numerals embossed or 'slugged' around the circumference and 'type selection' was carried out by the mechanism rotating each wheel so as to position the digits of the result in a line. Each cycle of the engine would set up a new multi-digit result on the wheels. These would be freshly inked automatically for each new result and a printed impression transferred by pressure to paper.

The single printed copy could not be reproduced automatically (except by rerunning the calculation) and was intended for checking and record purposes only. The engine advocates proposed to meet the need of multiple printed copies by automatically stereotyping the results to produce moulds from which printing plates could be cast. In Babbage's most complete design, in addition to transferring the result to the printwheels for inked copy on paper, the same result was transferred to a set of wheels with number punches around the circumference.<sup>46</sup>

Each cycle of the machine would lower the punches into soft material (lead,

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<sup>44</sup> For a summary description of Babbage's designs for typesetting and printing see *Passages*, p. 47. For a brief description of the historical development of Babbage's ideas on typesetting and printing see *LEC*, pp. 26-7, 28-31.

<sup>45</sup> *Works*, Vol. 2, pp. 16, 29. The Babbage papers held in the Biblioteca Dell'Accademia Delle Scienze di Torino, Turin, most of which were left by Babbage during his visit in 1840, contains printed samples from experimental copper stereotypes. For reference to Babbage's experimental 'co-ordinate' machine for stereotyping on copper see *Passages*, p. 46.

<sup>46</sup> For a technical description of a fully designed automatic printing and stereotyping apparatus by Babbage see Swade (1996, *Science Museum*). See Illustration 15, Appendix III for image of stereotype punches made to Babbage's design.

copper, or soft card) to receive an impression of the each new multi-digit result. A tray of such indentations served as a mould from which to cast the printing plates.<sup>47</sup> With the punches captive there was no possibility of them being displaced or removed as with loose type. Further, with 'type selection' dictated and controlled by the outputs from the calculating section, the arrangement would eliminate errors of transcription, foul case, and mental lapse to which manual typesetting using loose type was susceptible.

While Babbage produced detailed and elaborate designs for automatic printing and stereotyping apparatus, only small experimental versions were produced in his lifetime and his schemes remained largely speculative.<sup>48</sup> However, the detail and, in at least one case, the completeness of the designs demonstrate the seriousness with which he prosecuted the idea of an integrated calculator-and-printer, and at the same time explored the viability in principle of its mechanical realisation.<sup>49</sup>

### Stereotyping

Once the tables were typeset, proof sheets were run off for a further stage of checking. The inherent vulnerability of moveable type to corruption during the

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<sup>47</sup> See Illustration 16, Appendix III for image of stereotyping in Plaster of Paris.

<sup>48</sup> Babbage made a small stereotyping apparatus which survives as part of the experimental model of the Mill of the Analytical Engine that was under construction at the time of his death in 1871. The model is on part of the Science Museum collections (Inventory No. 1878-3). For illustration see Swade (1991, *Science Museum*, p. 33). A twenty-eight digit stereotype apparatus is included in the four-function calculator based on the Analytical Engine design and built by Babbage's son, Henry Prevost, and completed in 1910. (Science Museum Inventory No. 1896-58. See *ibid.* p. 37 for illustration). For reports on several printing and stereotyping experiments by Babbage see *Passages*, pp. 45-6.

<sup>49</sup> A complete printing and stereotyping apparatus built to an original Babbage design dating from 1847-9 was completed at the Science Museum in 2002. The apparatus automatically prints an inked copy of each thirty-digit result and produces stereotype moulds in plaster.

printing proof sheets, and subsequently during a print run *after* proof checking, created understandable insecurities for the table makers. The practice of stereotyping substantially relieved their anxieties. Stereotyping involved making a printing plate (called a 'stereotype') from moulds taken from the type already set in frames.<sup>50</sup> Taking cast impressions from type represents a near-immutable form of information capture. There was a variety of techniques. Stereotyping using plaster moulds was reinvented in the 1780s in Scotland and in France and up till the 1830s plaster was the only material used for casting moulds.<sup>51</sup> A material called 'flong' was later used to provide more flexible moulds. Here a damp laminate of blotting paper and tissue paper was beaten onto the face of the type to form a mould. It was then dried and hardened *in situ* before being lifted and used to cast the printing plate. Flong was first used in Lyon in 1829 and patented in England a decade later.<sup>52</sup>

Stereotyping had significant attractions for table makers. Printing from stereotyped plates removed the risk of loose type being corrupted during the printing process and ensured that all copies in a given print run were identical. There were other advantages. Particular editions of tables acquired their cachet for reliability over decades, and reputable volumes of early tables were kept in print, sometimes for centuries. Because type was the costliest part of a printer's equipment, set type was rarely retained after the printing order was completed and it was common practice to reset the type from scratch when later editions were called for. However, resetting the type for a new edition broke the line of integrity

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<sup>50</sup> The word *cliché* is thought to come from the French *clicher* which literally meant 'stereotype', i.e. a block of print used repeatedly. See Kirkpatrick (1996), p. v.

<sup>51</sup> For a history of the origins of stereotyping see Gaskell, p. 201.

<sup>52</sup> *Ibid.*, pp. 201, 203.

with the original edition. De Morgan captures the concern and stresses the benefit of eliminating the mediation of the compositor in new editions:

A second edition derives no authority from the goodness of the first, because the printer, who is, as already observed, as important a person as the author in the matter of tables, has again stepped between the latter and the public.<sup>53</sup>

Storing the stereotypes was an economical way of preserving investment in the labour of typesetting and verification. Moreover, the use of stereotypes guarantees immunity from new errors arising from displacement of loose type in later printings.

The benefits to the printers were more ambiguous, and early stereotyping suffered the prejudices of compositors and typefounders protective of their trades. Resetting a new edition from scratch was welcome new work, and committing expensive type, once set, to storage for future editions increased trade for typefounders. However, for the printers, tying up type in storage represented unused capital and the cost of type acted against holding excess stock. Few books were kept set up in type and stereotyping offered the welcome liberation of type stock which could then be redeployed in new work. A more subtle benefit to the printer was the reduction in wear in the same pieces of type. Continuous redeployment of type spread wear more uniformly across the stock, prolonged its useful life, and deferred the costs of replacement so raising yields on the printers' capital.<sup>54</sup> For a combination of economic and information management reasons stereotyping became the preferred technique for reducing the risk of corruption in the press and for keeping standard books in print.<sup>55</sup> Stereotyping became a hallmark of accuracy and, in de Morgan's extensive annotated bibliographies of

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<sup>53</sup> De Morgan (1842), p. 500; (1861), p. 1014.

<sup>54</sup> Gaskell (1972), p. 201.

<sup>55</sup> *Ibid.* p. 205.

printed tables, if an edition was stereotyped this was noted as an implied recommendation. The benefits of stereotyping to table-making were such that de Morgan recommends enforcement of its use.<sup>56</sup>

### Proofreading and Fatigue

Proofreading is an exacting and tedious process and the table-makers evolved maxims, nostrums, and idiosyncratic aids to maintain mental alertness and fight the fatigue that dulled the mind during checking sessions and lifted error rates. The demands on concentration and precision were reflected in the high rates for such work.<sup>57</sup> Checking the proof sheets was the last opportunity to identify errors before production printing.

It is curious that while de Morgan urged meticulous care for each stage of the process, he reserved the most rigorous stipulations for checking the stereotyped proofs. He counsels that 'the strictest investigation should take place in the proof which is taken from the stereotype, ordinary pains being taken with the previous proofs'.<sup>58</sup> The reason for this uncharacteristic relaxation of the highest standards of care throughout is that however scrupulous was the checking of proofs from type, the integrity of the printed result depends on the moveable type remaining entirely undisturbed during the whole printing process, and there were clearly anxieties about the vulnerability of type to derangement between the printing of the first

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<sup>56</sup> De Morgan cites Callet's edition of Gardner's tables, published in 1795, as stereotyped (1842, p. 498, 499; 1861, p. 1001). Campbell-Kelly identifies this edition as 'the first tables ever to be stereotyped rather than being printed using moveable type'. (Campbell-Kelly (1988), p. 163).

<sup>57</sup> Babbage reports in 1822 that the rate for checking proofs was three guineas a sheet. See Babbage (1822), *Works*, Vol. 2. p. 13, ft.

<sup>58</sup> De Morgan (1842), p. 500; (1861), p 1014.

proofs and producing the stereotype plates. A further reason was that any errors found could still be corrected in moveable type and a new stereotype made.<sup>59</sup>

Checking the stereotype proofs was the safeguard of last resort, and this is reflected in the variety, subtlety and ingenuity of the practices and ploys devised to counter fatigue and ensure effective error detection.

The checking process advocated by de Morgan involved three people, one to read aloud from the manuscript, the other two to check, without collaboration, identical printed proof copies from the stereotypes. The *modus operandi* he recommended involved a reader and multiple listeners where possible, but he offered special advice for those unable to afford the luxury, and who needed to proof-check solo.

He advised that the manuscript and the proof copy be brought into close proximity so that the two numbers being compared were contained in the same unbroken field of view, and he is indifferent to physical wear on the manuscript, recommending that it is folded as frequently as every two or three lines 'so as always to have both manuscript and proof under the eye in one position'. The wisdom underpinning this practice was articulated by Glaisher who wrote that 'it is well known that the number of errors . . . is proportional to the distance the eye has to carry the numbers'.<sup>60</sup> De Morgan warned against the tendency to mistake double figures (744 for 774, for example) and against transposition (012 for 102). If either proof or manuscript was harder to read, he counselled reading the easier first, 'for the mind is apt to allow knowledge derived from the more easy to give help in interpreting the more difficult'. He further recommended alternating the

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<sup>59</sup> Stereotypes represented an immutable form of information capture. Minor changes to stereotypes were possible depending on the nature of the correction. Babbage (1835), *Works*, Vol. 8, p. 52, Section 94.

<sup>60</sup> Glaisher (1874), p. 135. Quoted in G&F, p. 32. Said of transcription.

datum for each reading, that is, reading a result from the manuscript, making the comparison with the proof, then taking the next reading from the proof and so on. Different checkers appear to have had different preferences, some favouring visual checking in silence, others by vocalising the numbers, so combining ear and eye.<sup>61</sup> Whatever the preference, the checker was advised to break patterns to relieve tedium. Shifting bodily position, moving hands and feet, and varying the pitch or tone of voice in the spoken repetitions were recommended practices.<sup>62</sup>

Another technique used, especially by inexperienced checkers, was the use of 'author traps'. This involved the printer being requested to make, at his discretion, a fixed number of deliberate mistakes in every page. The location of these is concealed from the author-checker but carefully registered by the printer.<sup>63</sup> Failure by the checker to find the prescribed number of errors per page during checking served as a warning of faltering attention or fatigue, and acted as a quality control alarm. Clearly, the hazards of fatigue and tedium were many and real, and however ingenious the measures, insecurities remained:

It is hardly credible, to those who have not tried, how much the perceptions are dulled by the monotonous comparison of one column of figures with another, how many and how gross errors both eye and ear, when tired, will suffer to pass.<sup>64</sup>

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<sup>61</sup> Later practice questions the use of reader-listener pairs. In relation to work at the NAO in the 1920s and 1930s L. J. Comrie wrote:

All proofs are compared with original calculations. Each reader works single handed, i.e. compares his copy and proof. The method of one reader and one listener working together is not employed at all, partly because it takes two persons as long to do the work in this way as one can do it by himself, and partly because the mental comparison of an aural image and a visual image is not satisfactory in the case of figures.

Comrie (1933), p. 16.

<sup>62</sup> *Ibid.*

<sup>63</sup> De Morgan (1842), p. 500; (1861) p. 1015.

<sup>64</sup> *Ibid.*

Rewards were offered for the detection of errors after publication, both as an advertisement of confidence in accuracy and as a way of improving subsequent editions through ‘user-testing’ in the field. Glaisher reports that Vega offered a reward of a ducat for every error found in his table of 1794, including errors of a unit in the last digit. It turns out that Vega was fortunate that no contemporary checked and took up the challenge. An analysis of the relative frequency and magnitude of last-digit errors published in 1851 by Gauss would have cost Vega dear: there are large numbers of hereditary errors from Vlacq on whose tables Vega’s are based.<sup>65</sup> It is also possible that a few deliberate errors were left as a protection against unauthorised copying. Later, in the 1940s, L. J. Comrie knew of less than five errors in tables he had scrupulously checked, which he left uncorrected as ‘an uncomfortable trap for any would be plagiarist’.<sup>66</sup>

### Printing, Typography, and Clarity

The typography of printed tables was fiercely debated amongst table-makers and users of tables. In the Preface to *Logarithms*, Babbage formulates twelve ‘Rules’ that are the outcome of a survey of tables carried out with his friend and collaborator, Thomas Colby, to whom the volume is dedicated.<sup>67</sup> The purpose of

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<sup>65</sup> Glaisher (1874), p. 138-139. De Morgan reports that no mistake has ever been found in Lalande’s logarithms of 1831 though it is rumoured that rewards were offered for detection of errors. See de Morgan (1842), p. 499. The practice of offering rewards for discovered errors was still current in the twentieth century. In the 1940s L. J. Comrie had a graded scale of reward. He paid 2d for every error found in first proofs and charged 6d for every error missed; 5s was offered for errors in plate proofs. So confident was he in the final outcome that he offered £5 for errors exceeding a particular threshold. See Croarken (1990), p. 103.

<sup>66</sup> Croarken (1990), p. 103.

<sup>67</sup> Babbage (1827), *Works*, Vol. 2, p. 76-81.



the survey was to discover ‘on what typographical circumstances perspicuity depended’ and his findings reflect the interplay of typography and psycho-perceptual factors affecting clarity, legibility and ease of use.<sup>68</sup> These ‘best practice’ rules include recommendations on font selection, layout, the use of lines as separators, the optimal intervals and weight of separators, character height, line height, the proportions of spacing between characters, separation of results into groups leaving blank lines, the transparency and opacity of paper, and, finally, the use of coloured paper in preference to standard white. He included several of his own innovations in the 1827 edition of *Logarithms*, and also in examples of tables in his book on life assurance published in 1826.<sup>69</sup>

The choice of typeface was an issue of seemingly inexhaustible contention. The contribution or otherwise to legibility, of serifs, the evenness of heads and tails, thickened stems, and uniform depth were endlessly argued, with subjective preference and habituation ensuring that agreement was rarely reached. Superior elegance was traded off against clearer differentiation between similar characters, with the ability to discriminate readily between ‘3’ and ‘8’, or a ‘9’ from ‘6’ or ‘0’ used as criteria. Comrie charges Charles Hutton with the introduction of ‘modern face’ which featured equal height figures. This was favoured in America and its use persisted well into the twentieth century.<sup>70</sup> De Morgan regarded figures with heads

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<sup>68</sup> *Ibid*, *Works*, Vol. 2, p. 76.

<sup>69</sup> For samples of Babbage’s logarithm tables see Babbage (1827), *Works* Vol. 2, pp. 92-107. For examples of Babbage’s assurance tables see *Works* Vol. 6, pp. 112-129. Features include the use of three different character heights and dots under the last digit to indicate rounding up.

<sup>70</sup> Comrie (1948), p. vi.

and tails as superior and viewed with regret the temporary adoption of equal height figures by German editors.<sup>71</sup> Comrie, writing much later, sides with de Morgan:

A very convincing testimony to the superior legibility of old style figures came from the compositors and proof readers of these tables, who emphatically agreed that the figures here used were less fatiguing and so less liable to misreading than modern face figures.<sup>72</sup>

The introduction of new character styles was mostly opposed and there was a general preference for classical antiquarian figures amongst tables specialists.<sup>73</sup>

The intricacies of connoisseurial typography were thoroughly baroque, but appear perhaps less bizarre when fine distinctions in the style of character spacing could spare proofreaders and other heavy users headaches from eyestrain, not to mention the interest the table-makers had in reducing errors through clarity and legibility. There is evidence that the table-makers and table-users feuded with booksellers, printers and publishers whom the first accused of being only concerned with the aesthetics of appearances rather than with legibility.<sup>74</sup> The tension between book producers and consumers was not reserved for books of tables, but extended to literary texts. The trade edition of Edward Gibbon's twelve-volume *The Decline and Fall of the Roman Empire*, published in 1820, is cited by de Morgan as 'one of the most legible books we know of' but which 'is considered by the booksellers themselves to be very badly executed.'<sup>75</sup> Comrie would doubtless agree. He wrote that 'figures have to be read one by one, not in groups

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<sup>71</sup> De Morgan (1861), p. 978.

<sup>72</sup> Comrie (1948), p. vi.

<sup>73</sup> De Morgan (1842), p. 496; (1861), p. 977.

<sup>74</sup> De Morgan, 1861 only, p. 978.

<sup>75</sup> De Morgan (1861) p. 978.

like words', and he favoured any typographical feature that would break uniformity.<sup>76</sup>

Any presentational feature that might affect clarity, legibility and ease of use, came under scrutiny. This included the colours of paper and ink, their contrast and relative brightness. It seems that maximising the contrast between ink and paper was counterproductive. The deepest black on the whitest white was not recommended:

It is also a mistake to suppose that great blackness in the ink, combined with great whiteness in the paper, is favourable to the reader. Every increase of the contrast, over and above what is necessary to perfect legibility, is injurious to it: jet upon snow would in time destroy the strongest eyes'.<sup>77</sup>

De Morgan summarised his views on colour of paper for printing tables:

We are satisfied after many trials and comparisons, that a dull paper, of a whitish-brown character, too thick to be seen through, and an ink which is of a dull-brown black, as it were the very deepest shade of the colour of the paper itself, are the things which are permanently agreeable to most eyes. Those who try it should remember that the first page read is not as good a test as the hundredth.<sup>78</sup>

The issue of ink colour and backing paper was one that preoccupied Babbage to a curious degree. In an elaborate experiment to find the least fatiguing combination of ink and paper Babbage printed the same two sample sheets of his logarithm tables using every combination of colour and hue of ink and paper available in London – a total of thirteen different inks on 151 different colours of

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<sup>76</sup> Comrie (1848), p. vi.

<sup>77</sup> De Morgan (1842), p. 978.

<sup>78</sup> *Ibid.*

paper including black ink on black paper and green ink on green paper.<sup>79</sup> Shades of the same colour of ink were used including light and dark blue, light and dark green, olive, shades of yellow, light and dark red, purple and black. The experiment extended to printing in gold, silver and copper on vellum. A single complete set of variations was bound in twenty-one volumes in 1831.<sup>80</sup>

In his *Cyclopaedia* articles de Morgan discusses aspects of typography and he explicitly acknowledged Babbage ‘to whose large and rare collection of tables we have been much indebted’.<sup>81</sup> The acknowledgement supports the speculation that Babbage was the source of at least some of de Morgan’s recommendations. It is difficult to know how seriously others took Babbage’s views on colour, and whether de Morgan’s inclusion of Babbage’s views in the *Cyclopedia* was the indulgence of a friend, or even a professional *quid pro quo* for assistance and access to Babbage’s extensive library of printed tables. Campbell-Kelly suggests that Baily may have been making gentle fun of Babbage in remarking:

Would it not be desirable to have impressions on papers of various colors: – for instance, I should, myself, like *yellow* paper by daylight, and *pink* or *blue* by candlelight.<sup>82</sup>

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<sup>79</sup> There are inconsistencies in the reported number of colours. Babbage reported that he chose 140 differently coloured papers and ten different colours of ink. (Babbage (1831), *Works*, Vol. 2, p. 115). Van Sinderen, (1980), p. 178 gives 151 as the ‘correct’ figure; Williams (1981), p. 239, reports an extended set using thirteen colours of ink and 151 colours of paper. See also Campbell-Kelly (1988), p. 163.

<sup>80</sup> The set, bound by Fellowes in London, is at the Crawford Library of the Royal Observatory. A small selection of loose sheets including shades of green ink on green paper donated by the Rev. William Barton Babbage in the 1990s is in the Science Museum Library. For additional bibliographic details see also editor’s note, *Works*, Vol. 2, p. 115. The first twenty volumes consist of ten duplicated volumes so that comparisons could be made by laying them open side by side opened at different pages. See Babbage (1831), *Works*, Vol. 2. p. 116.

<sup>81</sup> De Morgan (1842), p. 497. This was not the only case of de Morgan disseminating Babbage’s ideas. In an encyclopaedia article published in 1836 de Morgan extended Babbage’s mathematical ideas on functional equations – the only substantial use made of this area of Babbage’s work. See Grattan-Guinness (1992), p. 39, ft.

<sup>82</sup> Baily to Babbage, 8 November 1826, BL Add Ms 37183, f. 357. Quoted in Campbell-Kelly (1988), p. 163.

Certainly Babbage took the issue of colour seriously. Following his own advice, he printed the first edition of his *Tables* on yellow paper.<sup>83</sup> Later editions and reprintings used green, white, fawn, pale yellow and grey paper.

The issue of coloured paper surfaced from time to time for decades after, and Babbage's experiment features in ophthalmology debates years after his death. In *The Causes and the Prevention of Blindness*, published in 1885, Ernst Fuchs, Professor of Ophthalmology in the University of Liege, includes a note confirming that the issue remained controversial. Based on Babbage's findings a resolution was passed at the International Congress of Hygiene at Turin in 1880 urging governments to print schoolbooks on yellowish tinted paper. The Society of Hygiene at Lausanne also came out against white paper recommending a bluish grey as less injurious to the eyes.<sup>84</sup>

The adoption of coloured papers by table-makers was not widespread. At the same time the use of coloured paper was not entirely ignored. Callet's sine tables were printed on yellow paper in France to accompany Babbage's *Logarithms* published in 1827. The paper colour was poor and the 'experiment' was discontinued.<sup>85</sup> Charles Nagy, a Hungarian mathematician and astronomer, wrote to Babbage in 1833 asking for a quotation for 1,500 to 2,000 copies of his tables to

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<sup>83</sup> There were at least twelve editions and reprintings of Babbage's tables between 1827 and 1915 on various coloured papers. The 1841 edition and later printings were on plain white paper. See editor's note, *Works*, Vol. 2, p. 73, ft. For summary table of editions and reprintings see Campbell-Kelly (1988), p. 166. For additional bibliographical information see van Sinderen (1980), p. 176. For eye-witness description of surviving volumes see Williams (1981), p. 239. Babbage lists the publication date of the first edition as 1826 rather 1827 which is the accepted date. (See Babbage, Henry Prevost (1889), unpaginated, "List of Mr. Babbage's Printed Papers", item 33). Williams has carried this apparent error, Williams *ibid*.

<sup>84</sup> Fuchs (1885), p. 236. I am indebted to Judit Brody for drawing my attention to this reference and to the work of David Roth who is the likely link with the moderator whose comments in Fuch's book address the issue of paper colour in the context of blindness.

<sup>85</sup> de Morgan (1842), p. 499; (1861), p. 1003-1004.

be printed on 'very light green coloured paper'.<sup>86</sup> Nagy visited Babbage in London three times between 1830 and 1834 and Campbell-Kelly suggests that Nagy's preference for green paper was at Babbage's suggestion.<sup>87</sup>

The image of Babbage as a volatile eccentric is suggested by Mabeth Moseley in the title of her biography of Babbage, *Irascible Genius*, and it is easy to see Babbage's preoccupation with colour combinations as part of his notorious oddity.<sup>88</sup> However, his apparent obsession with colour may have had its roots in a medical condition that has only now come to light. Babbage was an inveterate inventor and delighted in instruments and mechanical contrivances of all kinds.<sup>89</sup> In 1847 he constructed and demonstrated an instrument for examining the inside of the eye. He published no details but took his instrument, the first ophthalmoscope, to a leading eye specialist, Thomas Wharton-Jones.<sup>90</sup> Wharton-Jones saw no value in the device and Babbage did not pursue the invention. Four years later Hermann von Helmholtz was credited with the invention of a similar instrument and Babbage was denied official credit. In 1854 Wharton-Jones was asked to report on Helmholtz's ophthalmoscope and he owned up to his blunder.<sup>91</sup>

It has always been a puzzle as to what drew Babbage to investigate the inner eye and, in the absence of any directive clues, the device has been regarded, perhaps uneasily, as a product of exuberant invention with no further significance. However, Richard Keeler, an historian of ophthalmology, has found evidence that

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<sup>86</sup> Campbell-Kelly (1988), p. 166.

<sup>87</sup> *Ibid.* I am indebted to Magda Vargha (Konkoly Observatory) for details of Nagy's visits. Personal correspondence, 27 February 1998. Derrécagaix's eight-figure logarithm tables published in 1891 appeared on pale yellow paper. Derrécagaix (1891), p. III.

<sup>88</sup> Moseley (1964).

<sup>89</sup> For a compilation of Babbage's inventions See Swade (1991, "Mechanical Contrivances".) Also *CWB*, pp. 177-184.

<sup>90</sup> Keeler (1997), p. 140.

<sup>91</sup> Flick (1947), p. 246. See also *CWB*, pp.181-182.

Babbage suffered from a vision impairment – bilateral monocular diplopia – a condition in which there are two foci in the same eye, resulting in double vision when looking through either eye singly.<sup>92</sup> Keeler suggests that Babbage might have been motivated to devise the ophthalmoscope to investigate the physiology of the eye in an attempt at self-diagnosis.<sup>93</sup>

Babbage nowhere mentions the condition, or its effect on him, in any of his published writing. However, he described the effects of the impairment in correspondence with P. Prevost who published the description in 1832.<sup>94</sup> Babbage wrote that when viewing an object through both eyes or singly, he saw two images, one above the other with the upper image the weaker and slightly offset. Images of nearby objects were not doubled but were indistinct at their edges. Remote objects, especially the distant horizon, were invariably doubled and stargazing was frustrated by the impossibility of determining which were primary images and which secondary. The condition worsened in ill-health. Babbage developed various coping strategies. To lose the weaker image and see more clearly he frequently looked through a small hole in a card, or through an aperture made between fingers and thumb, or through a concave lens. He also took to leaning his head back and narrowing the field of view by lowering his eyelids, and found that frowning helped, but this required effort.

As Keeler suggests, a visual impairment suggests a solution to the puzzle of Babbage's interest in ophthalmic instruments. It may also provide the underlying motivation for the extreme lengths to which he went to explore optimal legibility of tables using all permutations of colours of ink and paper. Perhaps there is an

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<sup>92</sup> Keeler (1997), pp. 1456-1457.

<sup>93</sup> Keeler's original source is Albert and Edwards (1996) see p. 185.

<sup>94</sup> Prevost, (1832) see Section IV, pp. 212-214. Prevost's account was reported in 1835 in, Mackenzie (1835), p. 856.

unnoticed clue when he asked in 1827 whether 'the state of general health of the observer may not affect the organ of sight, and render different colours agreeable in different states of health'.<sup>95</sup> Perhaps more tellingly he wrote in 1831 after his experiment with coloured papers that one of his findings that invited further study was 'whether the colours of paper and ink which are least fatiguing will not depend on peculiarities in the organs of sight of the individuals who make the judgements'.<sup>96</sup> It is curious that Babbage nowhere discloses any connection between the impairment and his experiments in colour combinations. However, the thesis of visual impairment offers itself as an alternative to idiosyncrasy, to which he was anyway disposed.<sup>97</sup>

### Errors, Accuracy and Blame

The risk of errors in each of the processes in the chain of production was a continuing source of anxiety for the table makers. Errors in tables were part of the facts of life for table makers and users of tables. As many errors as possible were corrected prior to printing, and the tables were progressively improved in a continuing process after publication, through the distribution of errata sheets and the incorporation of discovered errors into later editions and revisions. Knowing which tables were reliable was an issue of connoisseurship, and publishers sought

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<sup>95</sup> Babbage (1827), *Works*, Vol. 2, p. 81.

<sup>96</sup> Babbage (1831), *Works*, Vol. 2, p.117.

<sup>97</sup> Another feature of Babbage's notorious eccentricity was the vigour of his campaigning protests against street musicians. Babbage great-great-grandson, Neville F. Babbage has suggested a medical explanation for his intolerance of organ grinders. Based on an analysis of Babbage's autopsy report. Neville Babbage suggests that Babbage's recorded heart condition is associated with deterioration in the inner ear leading to hypersensitivity to noise. See N. F. Babbage (1991), p. 759.



testimonials from eminent mathematicians to preface new editions.<sup>98</sup> The response of the table makers and users to errors was to correct them. They would decry the need to do so, be unsparing in their criticism, and then knuckle down to the tedious business of verification, bemoaning their fate as they adjusted their spectacles and uncorked the red ink. Errata sheets would be published, reviews written, bibliographies annotated – all within the confines of the scientific community.

There is at least one notable instance in which deficiencies in tables overflowed into the political arena. The case in question is the decline of the Nautical Almanac under John Pond. Pond succeeded Neville Maskelyne as Astronomer Royal in 1811 and the appointment carried with it responsibility for the annually issued Nautical Almanac. Pond made distinguished contributions to astronomy but appears to have been little interested in the practical problems of navigation, leaving the Almanac to poorly supervised assistants.<sup>99</sup> Pond, who is said to have lacked Maskelyne's administrative abilities, failed to establish an effective new team of computers and comparers when Maskelyne's team retired.<sup>100</sup> The reputation of the Almanac declined, and by 1818 so battered was its credibility that it became an embarrassment to English science, especially on the Continent. The state of the Nautical Almanac was debated in parliament, probably at the instigation of Davies Gilbert, in 1818, the year Thomas Young was appointed as Superintendent.<sup>101</sup> Francis Baily and James South made concerted attacks on the

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<sup>98</sup> For example see de Morgan's testimonial at the beginning of Herschell Filipowski's tables. Filipowski (1849).

<sup>99</sup> See Hall (1984), p. 11-2. Airy succeeded Pond as Astronomer Royal in 1835. A contemporary account of Pond's achievements can be found in Airy's testimonial (*Biog.*, pp. 127-9) in support of a pension for Mrs. Pond who was financially distressed on the death of her husband in 1836. Airy's testimonial is regarded as charitable given the decline of the NA under Pond. See Ronan (1967), p. 144.

<sup>100</sup> Wood ((1954), p. 341-2.

<sup>101</sup> *Ibid.*, p. 342. Also (1984), p. 11; Hoskin (1989), p. 177. In 1814 Babbage advised Herschel against seeking an appointment under Pond because of the declining reputation of Pond's operation. Babbage wrote in favour of forcing Pond's resignation. Hall (1984), p. 224, n.24.

‘defective state’ of the Nautical Almanac in 1822, and Baily renewed his attack in 1829.<sup>102</sup>

The role of errors in the attacks on the Nautical Almanac requires clarification. The ‘defective state’ of the Nautical Almanac is often loosely associated with errors in its informational content, and the deficiencies of the Almanac are sometimes identified with errors in the published tables. Errors did feature in the attacks. But the number of known errors was modest and these were quickly corrected after 1818 under Young’s energetic superintendence. It is significant that the ‘causes’ of the errors were portrayed as deficiencies of superintendence, poor leadership, or negligent recruitment of appropriate staff – organisational shortcomings in the execution of established practice – but the basic practices themselves in the use of human agency were taken as given. Though errors featured in the criticisms they were not the main thrust of the attacks, but were used as part of a larger political movement in which the increasingly vociferous community of scientists, which included the astronomers, sought to prevail on government for improved financial support for scientific activities.<sup>103</sup> The purpose of the attacks was to have the scope of the Almanac extended to better cater for the needs of astronomers and not just navigators for whom it was primarily intended. Wood comments that alleging inaccuracy of the Almanac, which was funded by the Admiralty, ‘was an effective tactical weapon in a campaign for reform which sometimes did not stop short of unscrupulous methods’.<sup>104</sup>

Discrediting the Almanac was a tactical move intended to effect reform of the

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<sup>102</sup> See Wood (1954), p. 309-10. The phrase in single quotes is Baily’s and appears in the titles of his two published attacks. See Baily (1822, 1829).

<sup>103</sup> See GoS, p. 42. Errors featured in only one of four of Baily’s objections. The NA of 1818 had fifty-eight errors, forty eight of which had the same source. See Wood (1954), p. 312-3. For a summary of Baily’s criticisms, and Young’s replies see *ibid.* p. 312

<sup>104</sup> *Ibid.*, p. 345.

Almanac in the form of increased government funds for astronomical and scientific activities. The campaign backfired to the dismay of its architects: the Board of Longitude was abolished in 1828, and with its demise went two decades of regular government funds for research in the physical sciences.<sup>105</sup>

Apart from this minor guest appearance in a political tussle for resources between scientists and government, errors in tables do not appear to have been a political or scientific issue outside the context of the engine debates.<sup>106</sup>

Having discussed the train of processes from calculation to printing, with special reference to errors, the question arises as to just how bad the tables were. The publication that has had a defining influence on perception that tables were riddled with errors is Lardner's article 'Babbage's Calculating Engine' published in the *Edinburgh Review* in 1834.<sup>107</sup> The picture is a bleak one. Lardner cites a survey of a collection of tables and uses published errata as a barometer of accuracy:

We have before us a catalogue of the tables contained in the library if one private individual, consisted of not less than one hundred and forty volumes. Amongst these there are not duplicate copies; and we observe than many of the most celebrated and voluminous tabular works are not contained amongst them. They are confined exclusively to arithmetical and trigonometrical tables; and consequently, the myriad of astronomical and nautical tables are totally excluded from them. Nevertheless, they contain an extent of printed surface covered with figures amounting to above sixteen thousand square feet. We have taken at random forty of these tables, and

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<sup>105</sup> *Ibid.*, Also Gleason (1991), p. 168-9; For an account of the abolition of the Board of Longitude and the reaction see Hall (1984), p. 42-3. For further political background see Ashworth (1994).

<sup>106</sup> A search of *Abstracts of the Papers Printed in the Philosophical Transactions of the Royal Society of London, 1800-1830* shows no articles on tabular errors outside an occasional reference to the defective state of the Nautical Almanac. Lindgren conducted a similar vain search of sources in Sweden and the USA. See G&F, p. 370, Note 69.

<sup>107</sup> Lardner (1834), *Works*, Vol. 2, pp. 118-186.

have found that the number of errors *acknowledged* in the respective errata, amounts to above *three thousand seven hundred*.<sup>108</sup>

Lardner proceeds to chronicle the specifics of known deficiencies in tables, growing more triumphant as he progressed. He cites Baily's detection of 500 errors in the solar and lunar tables of the Nautical Almanac; Hutton's forty errors in a single page of multiplication tables prepared for the Board of Longitude; the 1,000 errors in the first edition of supplementary tables to the Nautical Almanac issued by the Board of Longitude, and so on. He cites seven folio pages of errata to supplementary Almanac tables which themselves contained more than 1,100 errors. The errata were corrected by further errata.<sup>109</sup> His joy is unconfined at finding that the Nautical Almanac of 1836 would require erratum of the erratum of the errata.<sup>110</sup>

Lardner's article and the point at which errors in tables begin to do rhetorical work are treated in more detail in the next chapter. At first sight at least, the evidence of published errata is self-incriminating, and Lardner's demonstration that table-making practices left much to be desired notwithstanding the best efforts of the table makers, typesetters, proofreaders and printers, appears, to be a compelling one.

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<sup>108</sup> Lardner (1834), *Works*, p.129. Emphasis original. Campbell-Kelly suggests that the private individual is almost certainly Babbage. Campbell-Kelly (1988), p. 162. Williams states that Babbage's collection of tables was 'one of the most complete collections . . . in existence, perhaps second only to that of the Royal Society' (Williams (1981), p. 239). Lardner collaborated closely with Babbage on the article and the circumstances in which it was written make it highly likely that the tables were Babbage's. For a detailed discussion of the context and circumstances of Lardner's article see Chapter 3.

<sup>109</sup> *Ibid.*, p. 132, 133.

<sup>110</sup> *Ibid.*, p. 138.

It seems then that for the table makers errors were an unavoidable fact of life. Babbage comments on errors that occurred 'in works where neither care nor expense were spared' and exonerates the table makers:

It is, however, but just to the eminent men who presided over the preparation of these works for the press to observe, that the real fault lay not in them but *in the nature of things*.<sup>111</sup>

The manner in which the seemingly intractable difficulties of producing error-free tables was used by the engine advocates is discussed in greater detail in the next chapter.

## Summary

Each manual process in the chain of production was vulnerable to error and tables were checked for correctness using a variety of techniques. The process of subtabulation was to some extent self-verifying and this helped error detection.

The dissemination of tables in print medium presented a new series of difficulties. Typesetting, proofreading, and printing were to a greater or lesser extent dependent of human agency and therefore vulnerable to error. Manual typesetting by a compositor using loose type was prevalent for most of the century. Stereotyping offered compelling advantages for table-makers. The technique removed the risk of disturbing type once set, an inherent danger in the use of loose type. Storing stereotypes was also an economical way of preserving the investment in typesetting and proofreading, and secured the integrity of the information for later editions.

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<sup>111</sup> *Passages*, p. 138. Italics original.

The main technique of checking tables was by comparison of two sources, and elaborate practices were devised to fight fatigue and ensure effectiveness in detecting discrepancies. Typography was keenly debated, including font selection, the use of separators, and the optimum colour of paper and ink. Babbage went to extraordinary lengths to investigate combinations of ink and coloured paper. It has recently come to light that he had an eye defect and it is possible that his interest in typography and aspects of visual presentation were prompted by this.

Despite the best efforts of the table makers errors were unavoidable. The deficiencies in the overall chain of processes from calculation to printing were compellingly demonstrated by Dionysius Lardner in 1834 through a survey of published errata of printed tables, in which he identified over 3,700 errors in a random selection of forty volumes of tables. Lardner's survey has had a defining influence on the perception that tables were riddled with errors.

The engine advocates proposed to replace fallible humans and manual methods with machinery and thereby to eliminate all sources of human error in the production of printed mathematical tables. Babbage produced detailed designs for mechanical apparatus that integrated calculation, typesetting and stereotyping and that would function without the mediation of human agency. The next chapter discusses the expectations and aspirations of the engine advocates, and examines the rhetorical role of errors in the promotion of the engines.

### Chapter 3: Babbage's Expectations for his Engines

*All the dull monotony and numerical computation is turned over to the untiring action and unerring certainty of mechanical agency.*

– Dionysius Lardner, 1834.

#### Introduction

The question of errors in published tables dominates historical accounts of automatic calculation in the nineteenth century. The publication that has had a defining influence on the perception that errors in tables were a central issue is Lardner's article 'Babbage's Calculating Engine' published in the *Edinburgh Review* in 1834.<sup>1</sup> In this sixty-five-page article, the most comprehensive contemporary account of Babbage's engine project, Lardner cites the results of a survey of forty randomly selected volumes of printed tables. Using published errata he reveals a count of over 3,700 acknowledged errors in just this set.<sup>2</sup> He presents this as a shocking and outrageous statistic.

Lardner uses the deficiencies of tables to argue the benefits of mechanisation:

We trust that we have succeeded in proving . . . that more effectual means are necessary to obtain such tables suitable to the present state of the arts, sciences, and commerce, by showing that the existing supply of tables, vast as it certainly is, is still scanty, and utterly inadequate to the demands of the community – that it is rendered inefficient, not only in quantity, but in quality, by its want of numerical correctness; and that such numerical correctness is

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<sup>1</sup> *Works*, Vol. 2, pp. 118-186.

<sup>2</sup> Collier describes the article as 'the most competent and extensive description of both the background and the actual operation of the Difference Engine published up to the present day'. See *LEC*, p. 87. For Lardner's survey see Lardner (1834), *Works*, Vol. 2, p. 129. For quotation and extended details see Chapter 2, pp. 107-8.

altogether unattainable until some more perfect method be discovered, not only of calculating the numerical results, but of tabulating these – of reducing such tables to type, and of printing that type so as to intercept the possibility of error during the press work. Such are the ends which are proposed to be attained by the calculating machinery invented by Mr. Babbage.<sup>3</sup>

Lardner is emphatic that machinery is the only solution to the problem of errors.

Immediately following his triumphant tableau of known errors in the *Nautical Almanac* he wrote:

If proof were wanted to establish incontrovertibly the utter impracticality of precluding numerical errors in works of this nature, we should find it in this succession of error upon error, produced, in spite of the universally acknowledged accuracy and assiduity of the persons at present employed in the construction and management of the Nautical Almanac. It is only by the *mechanical fabrication of tables* that such errors can be rendered impossible.<sup>4</sup>

The deficiencies of tables presented here are two-fold: the scope of existing tables was inadequate; and tables were inaccurate. The deficiencies in tables are portrayed as the 'problem' and machinery as the 'solution' and the parade of errors, and the need for new tables, is paired with an unabashed promotion of the potential benefits of Babbage's engine.

Lardner discounts the prospect of improvement through correction. He argued that because of the difficulty of detecting errors, each discovered error implies a larger number still concealed:

When the nature of a numerical table is considered – page after page densely covered with figures, and with nothing else – the chances against the detection of any single error will be easily comprehended; and it may

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<sup>3</sup> Lardner (1834). *Works*, Vol. 2, pp. 139-140.

<sup>4</sup> *Ibid.*, p. 138. Emphasis original.



therefore be fairly presumed, that for one error which may happen to be detected, there must be a great number which escape detection.<sup>5</sup>

The evidence of published errata established incontestably that the manual production of tables, certainly of newly computed editions, was highly fallible despite the best efforts of the table makers. But Lardner went further when he implied that tables were so flawed as to be substantially immune to correction. He presented endemic errors as a terminal condition, irremediable by progressive correction after publication. The argument can be seen as an attempt to pre-empt the use of the same evidence (published errata) to reverse the force of the argument, that is, that the greater the number of known errors documented in errata sheets or corrected in subsequent editions, the fewer the residual errors and purer the host tables became. Further, that the problem was diminishingly small the longer the tables remained in use.

The difficulty was less the known errors than unknown ones, and in an increasingly utilitarian society in which quantification and number were the hallmarks of truth, in which the culture of measurement prevailed, and in which science offered new certainties, the case that there were large numbers of undiscovered errors was an irksome reminder of imperfection. Worse still, establishing the number of residual errors was not something that could be done by appeal to anything measurable. Airy, for one, argued that requisite accuracy was achievable without machinery, and at significantly lower cost.<sup>6</sup> Babbage was agnostic: he observed that 'the multitude of errors really occurring is comparatively little known'.<sup>7</sup> Babbage was a mathematician and a perfectionist. In relation to

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<sup>5</sup> *Ibid*, p. 132.

<sup>6</sup> Airy's views are discussed in detail in Chapters 4 and 5.

<sup>7</sup> Babbage (1864), p. 138.

tabular errors, there were no half measures. A table was to be perfect, and known to be perfect, or useless.<sup>8</sup> Herschel captured the insecurity and the dangers of latent errors in his letter to Henry Goulburn, Chancellor, when he wrote in 1842 that 'an undetected error in a logarithmic table is like a sunken rock at sea yet undiscovered, upon which it is impossible to say what wrecks may have taken place.'<sup>9</sup> Experts disagreed and empirical method was powerless to provide resolution.

Lardner's pairing of errors as 'problem' with engines as 'solution' encourages the notion that the elimination of errors was the essential purpose of automatic calculating engines. While he stops short of attributing these motives directly to Babbage he does nothing to discourage anyone else from so doing.

There is a second seminal account that serves to reinforce the perception that the elimination of errors was the primary purpose of Babbage's engines. This is the vignette of Babbage and Herschel in 1821 poring over newly calculated tables for the Astronomical Society when, dismayed by the large number of discrepancies, Babbage exclaimed 'I wish to God these calculations had been executed by steam'.<sup>10</sup> Babbage relates how he was seized by the notion of

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<sup>8</sup> In 1821, when Babbage conceived of his engine, his main experience and interest had been in mathematics. His published output to that time consisted entirely of mathematical papers, thirteen of which were published between 1813 and 1821. Lindgren implies that Babbage's perfectionism, associated with mathematical thinking, was at least partly responsible for his downfall. He also argues that the quest for the 'perfect' table was part of the drive to realise calculating engines. He observes that parts for Difference Engine No. 1 were made to a higher precision than was practically necessary and that the same precision was wastefully extended to decorative features. See *LEC*, pp. 262-3.

<sup>9</sup> Herschel to Goulburn, September 1842. Royal Society Herschel Archive, Box 27, Item 51.

<sup>10</sup> There are three known accounts of this episode written by Babbage: 1822, 1834 and 1839. Quoted in *LEC*, 14-8. The quotation cited is taken from the third account which appears in the Buxton memoir. (See next footnote). The first account leaves it open as to whether it was Babbage or Herschel that made the suggestion. In the second and third accounts Babbage claims ownership for himself. All three accounts refer to steam as the agent of redemption. The third account is the most dramatised and is the only one to include direct speech. Peter Ackroyd describes Babbage's exclamation as 'one of the most wonderful sentences of the nineteenth century'. See Ackroyd (1994), p. 116.

calculating engines immediately after this episode and how his near obsession with the "Calculating Engine" made him ill in the following days and weeks.<sup>11</sup>

Lardner's article, combined with Babbage's account of his mechanical epiphany, has had a defining influence on historical accounts of automatic computation, and the perception that Babbage's primary purpose was the elimination of errors is one that has consolidated over time. It has the appeal of monocausal simplicity and features increasingly in historical accounts to this day.<sup>12</sup> While errors have been emphasised in historical accounts it should not be assumed that the elimination of error was the sole purpose, or indeed the enduring purpose, or necessarily even the main appeal of the machines to Babbage simply because errors were the trigger to his deliberations and subsequent efforts.

This chapter argues that while the elimination of errors features in Babbage's own justification for his engines, it does so alongside several other reasons, benefits and justifications, and far less prominently, than Lardner's article and historians since have suggested. Secondly, that Lardner's simplification of purpose by an emphasis on errors can be seen as a product of the need to appeal to lecture hall audiences. Finally, that the political context of the collapsing fortunes of the engine project disposed Babbage to accept and even endorse Lardner's simplification even if it represented a less than complete picture.

Lardner's article was published in the July 1834 issue of the *Edinburgh Review*, thirteen years after Babbage's first conception of the machine, and after many of the arguments for and against the utility of the machines, their funding and construction had been played out. Whatever effect the article has had on historians

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<sup>11</sup> Babbage's account of the aftermath of his mechanical epiphany is related in Buxton's memoir. See Hyman (1988), pp. 46-48. Buxton, a junior colleague of Babbage, was entrusted by Babbage with the task of writing his biography using manuscript sources he supplied. The memoir was written between 1872 and 1880 but not published until 1988. See Hyman (1988), editor's Introduction, p. xiii. The political background to the commissioning of these tables is discussed below. See below pp. 125-6.

since, it had no material influence on the earliest formulations of the purpose and benefits of the machines. To uncouple contemporary accounts from the backward projection of later historical accounts it is necessary to start with Babbage's earliest papers on the utility of the machines written before Lardner framed utility the way he did.

### Babbage's Early Writings

The documents that are most revealing of Babbage's earliest notions of the capacities and potential benefits of the machine date from the period between 14 June and 13 December 1822. During this time he wrote five papers, four published close to the time of writing, and one manuscript (Item 3 below) that remained unpublished during his lifetime:

1. "A note respecting the application of machinery to the calculation of astronomical tables." *Memoirs of the Astronomical Society* 1 (1822): 309. [Dated 2 June 1822. Read 14 June 1822]
2. "A letter to Sir Humphrey Davy, Bart., President of the Royal Society, on the application of machinery to the purpose of calculating and printing mathematical tables." . London: Cradock and Joy, 1822. [Dated 3 July 1822].<sup>13</sup>
3. "The science of number reduced to mechanism." November 1822 [Buxton MS].

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<sup>12</sup> See for example Stan Augarten's account which is typical of many. Augarten (1984), p. 40.

<sup>13</sup> Reprinted in *Parliamentary Papers*, 1823, Vol. 15, pp. 9-14. See editor's note, *Works*, Vol. 2, p. 6, ft. a. An excerpt of the letter was published in *Edinburgh Philosophical Journal*, Vol. VII, p. 274. See editor's note, *Works*, Vol. 2, p. 38.

4. "Observations on the application of machinery to the computation of mathematical tables." *Memoirs of the Astronomical Society* 1 (1822): 311-314. [Read 13 December 1822].
5. "On the theoretical principles of the machinery for calculating tables." *Edinburgh Philosophical Journal* 8 (1823): 122-128. [Dated 6 November 1822].<sup>14</sup>

The suite of papers was written after the first trials using a small working model completed in Spring 1822 but before Babbage showed any serious ambition to build an engine.<sup>15</sup> The papers derive much of their material from the results of the first experiments and the theoretical speculations stimulated by them. With the exception of the manuscript (Item 3 above) all the papers are short. The notice to the Astronomical Society (Item 1) is barely 300 words long, and the Items 4 and 5 run to only a few pages and are narrowly mathematical.

Though Babbage's writing is voluminous and polymathic the four published papers in this suite, plus an additional one read on 3 May 1824 to the Cambridge Philosophical Society, are his only published writing on the Difference Engines apart from fragmentary references in later works.<sup>16</sup> While he returns repeatedly in his published work to the collapse of the engine project, the protracted negotiation with successive administrations, and his grievances over the Government's final

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<sup>14</sup> This paper is a published letter from Babbage to David Brewster.

<sup>15</sup> The exact date the model was completed is not known. In a diary entry dated 10 May 1822 Babbage wrote 'my calculating engine is nearly finished' (Babbage Papers, Waseda University). Collier narrows the date to 'near the end of May 1822' (*LEC*, pp. 30, 32). On 14 June 1822 Babbage refers to the 'engine just finished' (*Works*, Vol. 2, p. 3). This first model has never been found nor any plans for it. It is known in the literature as 'DE0' to signify that it preceded Difference Engine No. 1. See Taylor (1992); Tee (1994).

<sup>16</sup> For a detailed annotated Bibliography of Babbage's printed works (six books and some eighty six papers) see van Sinderen (1980). For a complete list of known publications see Bibliography, *Works*, Vol. 1, pp. 34-45. The paper read to the *Cambridge Philosophical Society* in May 1824 was published in 1826 (Babbage (1826), *Works*, Vol. 2, pp. 61-68). The most substantial of Babbage's fragmentary writing on the engines is in *Passages*.

withdrawal in 1842, there is little additional material that expands on his early views on the utility of his machines.

Though Babbage was ordinarily a prolific correspondent there are few private letters to supplement these published works, or to provide personal, political or professional context.<sup>17</sup> The surviving technical archive of his work shows that his unpublished work is as rich as his published work is sparse: his Scribbling Books or Sketch Books – a form of ‘laboratory’ record of notes, diagrams and jottings – run to between 6,000 and 7,000 pages; there are some 300 large design drawings showing the mechanical details of the mechanisms, as well as about 600-700 sheets with detailed ‘Notations’ written in the special symbolic language he invented for this purpose.<sup>18</sup> However, this material is technical, highly internalist and does little to illuminate the context and rationale of his expectations of his engines in any but purely mechanical terms. The Scribbling Books were intended for his private use and the writing makes few concessions to readers; some of the crucial sheets are undated and the comments and diagrams are often cryptic, fragmentary or presuppose a line of thought now lost.<sup>19</sup>

Third party accounts are few and, with one notable exception, insubstantial: Babbage gave no lectures in England though he gave countless private demonstrations that by all account captivated his audiences.<sup>20</sup> His ‘seminar’ on the

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<sup>17</sup> Babbage's correspondence in the British Library (BL Add Ms 37982-37201) alone runs to twenty volumes.

<sup>18</sup> Babbage's ‘Scribbling Books’ were renamed ‘Sketchbooks’ after October 1837. See Roberts (1989, unpublished), p. 30. The source of these figures is Bromley (2000), p. 4. The figures represent a downward revision from his earlier estimates. See Bromley (1991), p. 11. For description of Babbage's Mechanical Notation see *CWB*, p. 119-122.

<sup>19</sup> This technical material was not studied in any detail until the 1970s by Wilkes, Bromley and Collier in separate studies.

<sup>20</sup> For an eyewitness account by George Ticknor, 12 July 1835 see Jennings (1995), p. 196. Quoted in *CWB*, p. 80-1. For an account of Babbage's abilities to transfix his audience see Lyon Playfair's account of his visit to Babbage see *PC*, p. 217, quoted in *CWB*, p. 81. Playfair was engrossed for seven hours and missed his lunch appointment. The notable exception is Lardner's extensive account of Difference Engine No. 1. See Lardner (1834).

Analytical Engine in Turin in 1840 is the only recorded technical address by Babbage to a scientific audience and was primarily concerned with the later Analytical Engine rather than the Difference Engine.<sup>21</sup> For the most part, and especially after 1834, he appears to have worked in near-complete isolation, and third party accounts tend to be anecdotal reminiscences by those who witnessed or were treated to private demonstrations. Maurice Wilkes, among the first in the modern era to assess with any authority Babbage's technical work wrote:

Ever since going through Babbage's notebooks, I have been haunted by the thought of the loneliness of his intellectual life during the period when, as he later tells us, he was working 10 or 12 hours a day . . .<sup>22</sup>

Four of the five papers listed are essentially technical: their audience was the scientific community including mathematicians and astronomers, many of whom were Babbage's colleagues and friends. These papers focus primarily on the mathematical principles and prospects of the engines, and technical features of their design. Their purpose is less rhetorical than informative and philosophical. The exception to this is the open letter to Sir Humphry Davy, President of the Royal Society at the time.<sup>23</sup> This is the most 'political' of the five papers and has a promotional agenda that goes beyond the workings and principles of the machines, though these are present as well. Of the five papers it is the only substantial piece that seeks to justify the prospective benefits of the invention as distinct from describing its capabilities. Lindgren cites evidence that Davy had helped Babbage with the formulation of the letter, and this would confirm that the letter was written with wider consumption in mind, as the start of a campaign for a

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<sup>21</sup> For an account of his visit to Turin see *CWB*, pp. 128-133.

<sup>22</sup> Wilkes (1971), p. 8; *Passages*, p. 115 for reference to working habits.

more ambitious machine.<sup>24</sup> It also suggests that the terms in which Babbage frames utility in the letter begin to reflect the value of the machines as perceived by others.

The declared purpose of the letter to Davy is to seek scientific endorsement. Babbage wrote that he was 'naturally anxious, in introducing it to the public, to appeal to the testimony of one so distinguished in the records of British Science'.<sup>25</sup> He says that his concern in seeking such endorsement is to establish the credibility of the invention for those to whom the possibility of a calculating machine might seem improbable. There is also the suggestion that the letter, and Davy's 'testimony', would serve as a form of informal patent establishing his priority for the invention.<sup>26</sup>

Babbage does not make an overt bid for financial support but hints that he might be prevailed upon to undertake a larger machine depending on the 'nature of the encouragement' he might receive. At the same time he says that the larger undertaking is one that he felt 'unwilling to commence, as altogether foreign to my habits and pursuits'.<sup>27</sup> Babbage's intentions are at first sight unclear: on the one hand he appears reluctant to undertake a large project; on the other, he appears receptive to inducements, though whether these are moral or financial is left open.

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<sup>23</sup> Davy was president of the Royal Society from 1820-1827. *CDNB*.

<sup>24</sup> Lindgren (1987), p. 46.

<sup>25</sup> Babbage (1822), *Works*, Vol. 2, p. 13.

<sup>26</sup> Babbage was not the first to propose a printing calculator, nor the first to suggest the method of differences as a suitable principle on which to base mechanised tabulation. This distinction goes to Johann Helfrich Müller, a German engineer and master builder, who described the idea in a letter in 1784. A booklet published two years later mentions Müller's idea, thirty five years before the genesis of Babbage's scheme. Herschel translated key sections of the published account for Babbage. It is unclear whether Babbage knew of Müller's work in 1821 before his own conception of the idea. For details of Müller's ideas and a discussion of precedence see *G&F* 64-70. Babbage vigorously opposed patents on the grounds that the wish to profit restricted the free access to the benefits of science which should be universally disseminated. See Babbage (1830), *Works*, Vol. 7, pp. 66-7; *CWB*, pp. 183-4.

<sup>27</sup> *Ibid.*, p. 14.



It could be that he was lobbying in the capacity of 'inventor' for support for development by others, and this reading allows his own reluctance to be reconciled with a coded bid for funds.

The letter to Davy was privately printed and circulated to colleagues and friends as well as those who might have influence in securing financial support for a full sized machine.<sup>28</sup> It is clear that however genuinely ambivalent Babbage was about his willingness to undertake a larger engine himself, the letter is a directed act of persuasion and was intended to do rhetorical work. Since the letter consciously seeks to frame features of the machine as benefits, it can be seen to be more revealing of the terms in which utility was perceived in the wider scientific and political communities, than the more technical material. In this respect it stands apart from the other public accounts in this set of papers.

Babbage does not refer in any consistent or systematic way to the benefits of his machines. His written papers do not present a unified or structured advocacy, nor do the arguments build on each other in a progressive way. The papers tend to present aspects of his current preoccupations, and it is often difficult to unbundle compound statements of multiple benefits, or to establish their relative importance in his arguments. However, overall, the terms in which Babbage frames the utility of the machines can be seen to fall into two implicit categories: remedies for known deficiencies in tables; and new practical and theoretical implications of automatic computation. Lardner broke down the deficiencies of tables into two further categories: quantity (the supply was 'inadequate to the demands of the community'); and quality ('its want of numerical correctness').<sup>29</sup> Babbage implies in his arguments that solving the issue of supply required at least two essential

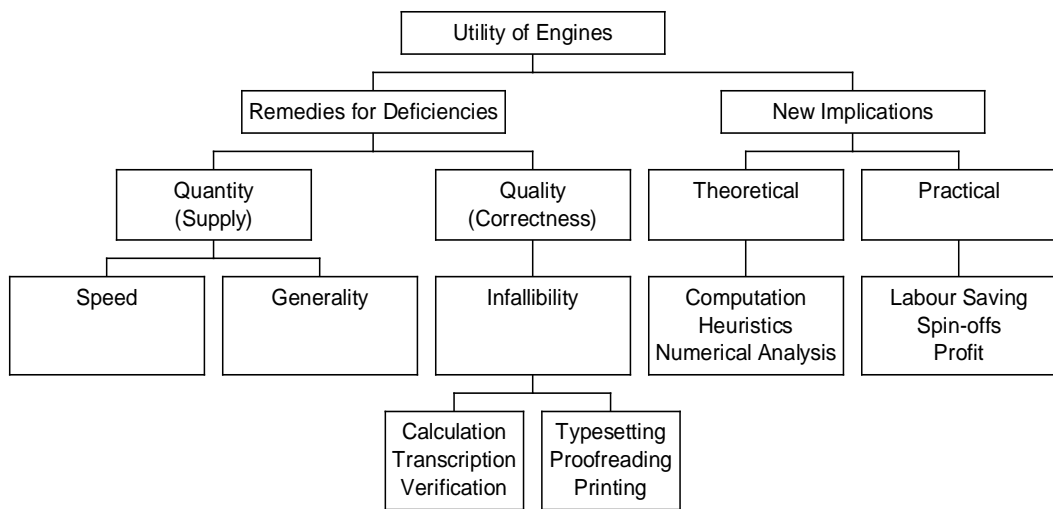
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<sup>28</sup> See *LEC*, p. 34, 36.

<sup>29</sup> *Works*, Vol. 2, p. 139. The full quote is given above see p. 111.

features: speed (the machines need to be at least as fast or faster than human computers), and generality of application (the machines need to be versatile enough to cater for a variety of tables).

The diagram below is presented as a form of navigation aid. It illustrates the logical structure of what follows and is intended as a device to situate Babbage's compound arguments in a map of utility derived from his own advocacy, and that of others, based on his views:



### Remedies for Deficiencies

#### Quantity: Engines as Factory

In the brief notice to the Astronomical Society, dated 14 June 1822, Babbage wrote that the machine 'by the application of a moving force may calculate any

tables that may be required.<sup>30</sup> The method of finite differences was introduced by way of reassurance that claims for the generality of the invention were not farfetched:

Although it might at first view appear a bold undertaking to attempt the construction of an engine which should execute operations so various as those which contribute to the formation of the numerous tables that are constantly required for astronomical purposes, yet to those who are acquainted with the method of differences the difficulty will be in a considerable degree removed.<sup>31</sup>

It is clear that the first statement of need was not that of remedying the inaccuracies of existing tables, but the supply of new tables for astronomy, the requirements for which are stated as given. There is no reference to any examples of tables for which there was perhaps some pressing need. Nor is there any attempt to argue the priority of astronomy as a scientific pursuit, as a cultural *desideratum*, or as a utilitarian tool for trade and commerce.

The question of supply is one that has been overlooked in the main historical accounts. When Babbage was dismayed by the errors in tables during his meeting with Herschel in 1821 he invoked steam as a remedial agent. The figurative associations with 'steam' and of steam engines, are those of force, motive power and endurance, rather than of accuracy or precision. In his accounts of the episode Babbage mentioned the tedium of repetitive calculation and checking, as well as 'discordances' or errors. So in specific terms the appeal to 'steam' might be read

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<sup>30</sup> *Works*, Vol. 2, p.3. Babbage wrote to Davy that in the machine 'may be made to move uniformly by a weight' (*Ibid.*, p. 9). No plans or physical relics of this first machine have survived. It is not clear whether the model was driven in this way, or whether it was operated by hand.

<sup>31</sup> *Ibid.*

as an invocation of an untiring agent, immune to fatigue.<sup>32</sup> There are other senses that can be read into the word. The early decades of the century were a period of industrial expansion and unprecedented engineering ambition. The symbolic agent of change in the social and economic upheaval of the Industrial Revolution was the steam engine. In one of his earliest references to the machine, in a letter to Herschel dated 20 December 1821, Babbage refers to his 'arithmetical engine'.<sup>33</sup> Lindgren observes that Babbage's choice of the word is unusual and may be a conscious or unconscious claim that the calculating machine, by association with the steam engine, would have 'the same profound and revolutionary social consequences as its power-source counterpart'.<sup>34</sup> By this reading 'steam' can be seen as a reference to the unlimited prospects of a new technology-driven age that Babbage wished to extend to the problem of tables. In reference to the zeitgeist of the period Carlyle described the period as 'The Age of Machinery, in every outward and inward sense of that word'.<sup>35</sup> In specific cultural terms 'steam' can thus be seen as a metaphor for machinery in general, reflecting an obsession with contrivances, mechanical invention, and contraptions, that prevailed for most of the century.<sup>36</sup> But there is a specific and deeper meaning suggested by 'steam' that embraces the notion of 'production', and the circumstances that led to the meeting in 1821 between Babbage and Herschel, during which the idea of a calculating

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<sup>32</sup> Lardner refers to the 'untiring action . . . of mechanical agency'. Lardner (1834), Works, Vol. 2, p. 169.

<sup>33</sup> Babbage to Herschel, 20 December 1821, Quoted in *G&F*, p. 38.

<sup>34</sup> *G&F*, p. 39, 41.

<sup>35</sup> See Partington (1998), p. 180.

<sup>36</sup> For an illustrated compendium of Victorian inventions reported in all seriousness in scientific journals see de Vries (1971). The outlandish impracticality of many of the contrivances conveys the obsession with mechanical contraptions. For a selection of both serious and quirky inventions that were patented during Queen Victoria's reign see van Dulken (2001).

engine was first mooted, suggest that this was the essential underlying force of the term.

As discussed in Chapter 2, attempts to reform the Nautical Almanac by Baily, South, Herschel and others, in the early 1820s, were part of a movement to secure government funds for the physical sciences, primarily for astronomy, meteorology and magnetism.<sup>37</sup> The attacks on the Nautical Almanac were intended to advertise its deficiencies, with a view to securing funds to improve and expand the scope of the Almanac to better serve practicing astronomers. The general context was that astronomers were pressing for more extensive tables of the stars for which published information, intended for navigation, was not best suited. A contributory factor to the astronomers' dissatisfaction was the growing number of 'new small planets' (asteroids) that were being discovered, the motions of which they were impatient to have tabulated.<sup>38</sup> The founding of the Astronomical Society in 1820, independently of the Royal Society, can be seen as part of the reforming group's further efforts to secure greater representation for active astronomers, many of whom were influential and wealthy figures.<sup>39</sup> In this climate, a Council meeting of the newly formed Astronomical Society was held on 30 November 1820 at Baily's house to consider a request to the Admiralty for accurate star tables for a set of

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<sup>37</sup> See Chapter 2, pp. 105-7. Also *GoS*, p. 42.

<sup>38</sup> See Forbes (1965), p. 398.

<sup>39</sup> The idea for an astronomical society was first proposed by William Pearson who wrote a prospectus in 1816. A meeting was held on 12 January 1820 at the Freemason's Tavern in London to consider forming such a society. Those present included Baily, Herschel, Babbage, South, Thomas Colby, and William Pearson. For an account of the origins of the Society, and its relationship to the Royal Society see Gleason (1991), p. 52-5. Joseph Banks (1743-1820), President of the Royal Society (1778-1820), was hostile to the formation of the new Society, which he saw as threatening the ruin of the RS through competition. Banks died on 19 June 1820. Babbage visited him about two weeks before his death and records Banks' opposition. See Babbage's diary 1820-1825, Babbage Papers, Waseda University, Tokyo. Also Wood (1954), p. 308. For a summary biography of Banks see Chambers (2000), pp. xiii-xxi.

Greenwich stars.<sup>40</sup> The request ran aground, apparently because of the 'indolence' of the Board of Longitude.<sup>41</sup> Shortly afterwards, the Astronomical Society commissioned, at its own expense, a set of tables for the use of its members. It is unclear whether commissioning the tables was a provocation, or simply a means for the Society to meet its own needs. Either way, Herschel and Babbage, founders and active members of the Society, were charged with verifying the calculations by comparing the two independently computed sets of results.<sup>42</sup> It was the abundance of errors revealed in this process that prompted the conception of 'calculating by steam'.<sup>43</sup>

This background indicates that the need for new tables was a pressing preoccupation for astronomers, and that Babbage's involvement was a direct consequence of this known need. The weight of the term 'steam' in Babbage's appeal can therefore be seen as that of industrial production, as a solution to a problem of supply. This reading is consistent with why, in the earliest public announcement of his invention (which was to the Astronomical Society) Babbage appears to perceive the primary benefit of the engine, not as a means of ensuring error-free tables, but as a means of providing the 'numerous tables that are constantly required for astronomical purposes'.<sup>44</sup> The metaphor of industrial production was echoed by Lardner who refers to the 'mechanical fabrication of

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<sup>40</sup> Wood (1954), p. 308. Babbage's indicates that it was the maverick James South who made the request. See Babbage's diary entry for 30 November 1820. Babbage Papers, Waseda, University, Tokyo.

<sup>41</sup> Wood (1954), p. 308.

<sup>42</sup> The tables in question reduced the observed positions of over 3,000 stars to their true positions. See *Quarterly Review* June (1826), p. 163.

<sup>43</sup> *Ibid.*

<sup>44</sup> *Works*, Vol. 2, p.3. Quoted above, see p. 123.

tables', and also by Babbage's son, Benjamin Herschel, when he described the Difference Engine as 'emphatically a machine for manufacturing tables'.<sup>45</sup>

A requirement for the generic production of tables was the capacity for generality. As mentioned above, the method of differences was the simplifying principle on which the generality of the machine was founded. In his letter to Davy, Babbage wrote:

In order to satisfy the condition that the calculating part should be capable of computing every species of table, it was necessary to found it on some great and comprehensive mathematical principle. The method of differences is the only one that possesses this extensive range and, although there are some few tables of rare occurrence to whose calculation it is not commodiously adapted, yet I hoped that by certain modifications I might apply it even to these.<sup>46</sup>

As discussed in Chapter 1 the method allows a large class of mathematical functions (polynomials) to be evaluated, and the generality of the principle is based on the ability to express most regular mathematical functions as an expansion of polynomial terms. The technique was well known and was used as a standard method by human computers for subtabulation. The engineering advantages of the method are the same as those for manual calculation: the reduction of arithmetical operations to simple addition and subtraction which are more readily realised using gear wheels than are multiplication and division.<sup>47</sup> For Babbage the need for only

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<sup>45</sup> Lardner (1834), *Works*, Vol. 2, p. 138; B. H. Babbage (1872). *Works*, Vol. 2, p. 226.

<sup>46</sup> *Works*, Vol. 2, p. 16.

<sup>47</sup> Babbage commented that of all the arithmetical operations he attempted to mechanise 'none offered more formidable difficulties than . . . the operation of division'. Babbage (1837), *Works*, Vol. 3, p. 43.

addition or subtraction simplified the mechanism and its manufacture in that it could consist 'of a small number of different parts frequently repeated'.<sup>48</sup>

A second remedial requirement for 'deficiency of supply' was speed. Babbage reported to the Astronomical Society that in trials using the experimental model the machine produced results 'almost as rapidly as an assistant can write them down'.<sup>49</sup> To Davy he said more. He indicated that the larger the number of columns in the machine, the more effectively it could compete with a human computer: 'one remarkable property of this machine is, that the greater the number of differences the more the engine will outstrip the most rapid calculator'.<sup>50</sup> In the same letter he reports on the results of specific speed trials:

I will only at present mention a few trials which have since been made by some scientific gentlemen to whom it has been show, in order to determine the rapidity with which it calculates . . . In the earlier numbers my friend, in writing quickly, rather more than kept pace with the engine; but as soon as four figures were required, the machine was at least equal in speed to the writer.<sup>51</sup>

Since each new result was formed by an addition to its immediate predecessor, each new result replaced the previous one and the appearance of each result was transient. In the absence a mechanical method of capturing results (the printing apparatus was unbuilt) Babbage evidently used his friend as a human scribe to

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<sup>48</sup> Babbage (1822), *Works*, Vol. Vol. 2, p. 3

<sup>49</sup> *Ibid.*

<sup>50</sup> *Ibid.*, p. 7.

<sup>51</sup> *Ibid.*, p. 9.



write down the results.<sup>52</sup> For smaller values of the argument, the results had few digits and the scribe kept up, but as the tabulation progressed the machine came into its own. Babbage continues:

In another trial it was found that thirty numbers of the same table were calculated in two minutes and thirty seconds: as these contained eighty-two figures, the engine produced thirty-three every minute. In another trial it produced figures at a rate of forty-four in a minute. As the machine may be made to move uniformly by a weight, this rate may be maintained for any length of time, and I believe few writers would be found to copy with equal speed for many hours together.<sup>53</sup>

The separate claims made in these passages are that a machine with more than nominal capacity will calculate faster than a human computer; that the rate of calculation would outstrip a scribe's capacity to record results; finally, that a machine with external motive power is not bound by the limits of human endurance.

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<sup>52</sup> See *Works*, Vol. 2, p. 9. Babbage never completed a printer (see *LEC*, p. 30). Some 12,000 printer parts were made for Difference Engine No. 1. These were returned to Babbage by Clement in 1834 and later melted down for scrap (see H. P. Babbage (1910), pp. 224-5). A small stereotyping device is included in the experimental portion of the Analytical Engine under construction at the time of Babbage's death in 1871. Babbage left complete designs (1847-9) for a large thirty-digit automatic printing and stereotyping apparatus intended for both Difference Engine No. 2 and the Analytical Engine. This was built to original designs and completed in March 2002 at the Science Museum, London. The printing and stereotyping apparatus weighs an estimated 2.5 tonnes and consists of 4,000 parts. For a technical description see Swade (1996, *Science Museum*). Also, Swade (2003).

<sup>53</sup> *Ibid.* Trials on the Difference Engine No. 2 completed in 2002 showed that the machine could calculate, print and stereotype, six thirty-digit results per minute. Each result requires eight thirty-one digit additions.

## Quality: Machines and Fallibility

So far there is no mention in these early papers of machinery eliminating the risk of error in calculation, nor any explicit emphasis on the fact that the machine was automatic in that it did not rely on the informed manual intervention of the operator, as did all previous calculating devices. In neither the notice to the Astronomical Society, nor the letter to Davy does Babbage refer to machinery guaranteeing the integrity of computation, and it is clear that he did not take for granted that the act of mechanisation *per se* ensured infallibility. Though he later insisted on the highest precision possible in the factoring of parts, Babbage did not rely at this early stage on mechanical precision to ensure correct operation, and from the start he incorporated devices to prevent wheels drifting, becoming deranged, or progressively accumulating errors through disturbance, wear, or imprecise manufacture. In this he took his lesson from the first experimental model. In his letter to Davy he wrote that his first machine suffered from 'great defect in the workmanship' and that he had:

employed a principle by which any small error that may arise from accident or bad workmanship is corrected as soon as it is produced, in such a manner as effectually to prevent any accumulation of small errors from producing a wrong figure in the calculation.

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The technical drawings of his later designs show a technique for this continuous correction: at several fixed points in the calculating cycle a wedge is inserted between the teeth of all the wheels. If the gear wheel drifts into a position intermediate between integral whole numbers, the insertion of the wedge acts as a corrective if the derangement is slight, but fouls the end of the tooth on attempted

entry if the derangement is more severe. If the insertion of the wedge is obstructed in this way the machine jams. Jamming is used as a form of error detection and acts as a warning that a number is indeterminate. The arrangement ensures that what is essentially an analog mechanism operates with digital discreteness.<sup>55</sup> In a manuscript, dated 26 December 1837, Babbage expressed this feature as a 'principle' of design: 'every movement shall be of such a kind that the engine shall either break itself or stop itself or execute the intended motion'.<sup>56</sup> Lardner, who despite chronic grandiloquence often expressed more vividly points made by Babbage, wrote that 'the consequence of this exquisite arrangement is, that the machine will either calculate rightly, or not at all'.<sup>57</sup> More elegantly he wrote that 'whatever the machine would do, it would do truly'.<sup>58</sup> Babbage's designs evidence a continuing preoccupation with security devices to ensure the absolute integrity of results. Many of these devices and techniques are subtle and ingenious and their presence distinguishes his designs from those by others that followed.<sup>59</sup>

In a further indication that he did not take for granted the infallibility of machinery, Babbage commended to Davy the self-verification features of the method as a belt-and-braces check on correct working:

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<sup>54</sup> Babbage (1822). *Works*, Vol. 2, p. 9.

<sup>55</sup> For an example of this mechanism see drawing BAB[A]171. For description of operation see Swade (1996, *Science Museum*), pp. 70-1.

<sup>56</sup> *Works*, Vol. 3, p. 39.

<sup>57</sup> See *Works*, Vol. 2, p. 159.

<sup>58</sup> *Ibid.*, p. 183.

<sup>59</sup> For a description of one such see *CWB*, p. 295. The Scheutz difference engines, for example, did not include such devices and though the machines were, and often still are, regarded (falsely) as a successful realisation of Babbage's principles, derangement was a constant anxiety. See Chapter 5, p. 277. Babbage later saw some elements of his security precautions as 'optional'. See Babbage (1851, *Exposition*), p. 112. The susceptibility of the Scheutz machine to 'wilful derangement' because of the lack of security devices is specifically mentioned in a Royal Society Report. See Stokes (1855). Reprinted in H. P. Babbage (1889) see p. 265. See Chapter 5, p. 252, 279.

I would however, premise that if anyone shall be of opinion, notwithstanding all the precaution I have taken and means I have employed to guard against the occurrence of error, that it may still be possible for it to arise, the method of differences enables me to determine its existence. Thus if proper numbers are placed at the outset in the engine, and if it has composed a page of any kind of table, then by comparing the last number it has set up with that number previously calculated, if they are found to agree, the whole page must be correct: should any disagreement occur, it would scarcely be worth the trouble of looking for its origin, as the shortest plan would be to make the engine recalculate the whole page, and nothing would be lost but a few hours' labour of the moving power.<sup>60</sup>

Since each new result depends on its immediate predecessor, the last result in a run of calculations depends on all prior results. So the correctness of the final result establishes a high degree of confidence in the integrity of each result in the whole run, and verification is reduced to comparing the final result with the expected value computed independently by hand. The technique applies equally to manual and to machine calculation and is not a privileged feature of mechanisation. It is a feature of the method of differences and was used by human computers.

The first mention Babbage made of errors referred not to errors of calculation, but to those of typesetting and printing, and he seemed more concerned to give reassurances about remedial measures for these than he was to establish the mechanical integrity of the calculations. In the last passage of the announcement to the Astronomical Society he wrote:

In the prosecution of this plan, I have contrived methods by which type shall be set up by the machine in the order determined by the calculation; and the

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<sup>60</sup> *Works*, Vol. 2, pp. 9-10. See also *Passages*, p. 50. The check described is one in which the last result of a run of calculations should be identical to the precalculated pivotal value that is the start of the next run. For a description of the principle see Chapter 2, p. 78.

arrangements are of such a nature that, if executed, there shall not exist the possibility of error in any printed copy of tables computed by this engine.<sup>61</sup>

It is clear from this that in its earliest conception, machinery was to extend to automatic typesetting, mechanically integrated into the design, so as to eliminate the fallible human who would otherwise mediate between calculated results and printed outcome. This is confirmed by his description of measures he experimented with and proposed in his letter to Davy, as well as in the manuscript account written in November 1822. To Davy he wrote:

Supposing these engines [were] executed, there would yet be wanting other means to ensure the accuracy of the printed tables to be produced by them. The errors of the persons employed to copy the figures presented by the engines would first interfere with their correctness. To remedy this evil, I have contrived a means by which the machines themselves shall take from several boxes containing type, the numbers which they calculate, and place them side by side; thus becoming at the same time a substitute for the compositor and the computer; by which means all error in copying as well as in printing is removed.<sup>62</sup>

The techniques Babbage proposed were intended to eliminate the risk of transcription errors and typesetting errors by replacing the human compositor by direct mechanical transfer of results from the calculating section to an automatic typesetting apparatus. The arrangement, as Lardner later wrote, would 'substitute an automaton for a compositor'.<sup>63</sup> At this stage Babbage's plans for automatic typesetting still used loose metal type along lines then being developed by William Church, an American resident in England, who patented the first composing

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<sup>61</sup> Babbage (1822). *Works*, Vol. 2, p. 4.

<sup>62</sup> Babbage to Davy, 3 July 1822. *Works*, Vol. 2, p. 7.

<sup>63</sup> Lardner (1834), p. 119.

machine, in 1822.<sup>64</sup> To Davy, Babbage acknowledged that despite automatic typesetting there remained two sources of error inherent in the use of moveable type that still need guarding against: 'foul case' (type being distributed in the wrong boxes or magazines before typesetting); and the danger of loose type being displaced from the frame and incorrectly replaced through carelessness.<sup>65</sup> Babbage assured Davy that he has provided 'simple and effectual means' to remedy the problem of foul case and 'means of a similar kind' that render drawing the type 'impossible' but does not burden Davy with any detail.<sup>66</sup>

In the scourge of errors Babbage emphasised typesetting and printing errors rather than those of calculation. He justified the measures he has taken to ensure the integrity of typesetting and printing by reminding Davy that 'the quantity of errors from carelessness in correcting the press, even in tables of the greatest credit, will scarcely be believed, except by those who have had constant occasion for their use'.<sup>67</sup>

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<sup>64</sup> Gaskell (1972), p. 274. It seemed that Babbage met Church and viewed drawings and models of Church's composing machine but was unable to work out how it functioned. See Babbage (1822), Vol. 2, p. 29-30. Church's machine was operated from a keyboard. Type was contained in preloaded magazines and released by pressing the appropriate keys (see Chapter 2, p. 83). In the manuscript dated November 1822 Babbage mentions automatic stereotyping on copper plate as an early aspiration (*Works*, Vol. 2, p. 16) but experiments returned him to loose type (*Ibid.*, p. 29). He abandoned loose type in 1824 and finally opted for automatic stereotyping. For a technical description of a fully designed automatic stereotyping apparatus by Babbage see Swade (1996, *Science Museum*). For a brief description of the historical development of Babbage's ideas on typesetting and printing see *LEC* (1970), pp. 26-7, 28-31.

<sup>65</sup> For Babbage's description to Davy see *Works*, Vol. 2, p. 8. Incorrect preparation of type can occur during 'diffing', i.e. the redistribution of type from the frames back to the boxes for reuse after printing. Displacement of type could occur when type simply fell out of the frames before printing, or during printing when type stuck to the inking rollers or wads, especially if the ink was too thick ('drawing the type'). For Babbage's description see *Works*, Vol. 2, p. 30 *et seq.*. Also Chapter 2, pp. 84-5.

<sup>66</sup> Babbage to Davy, 3 July 1822. *Works*, Vol. 2, p. 7-8. Babbage provides additional detail in the Buxton papers dated November 1822 (*Works*, Vol. 2, p. 29-31). The techniques are also described more briefly in *Passages*, p. 45. The schemes involved drilling or slotting the type to ensure that type in any given box can be verified *en masse* by inserting a thin wire. Incorrect type would obstruct the passage of the wire and identify the errant type. Similarly, drilling or slotting all the type of one font in the same position allowed a wire to be inserted through rows of type and would prevent type escaping from the frames. For comments on Babbage's plans for automatic stereotyping to eliminate errors of foul case and displaced type see Chapter 2, p. 82-3.

<sup>67</sup> *Works*, Vol. 2, p. 8.

An attendant deficiency of existing manual methods was the drudgery of repetitive low-level calculations, and it was the relief of drudgery rather than generality, speed or infallibility that Babbage advertised to Davy as the first 'important consequence' of his invention:

The intolerable labour and fatiguing monotony of a continued repetition of similar arithmetical calculations, first excited the desire, and afterwards suggested the idea, of a machine by which the aid of gravity or any other moving power, should become a substitute for one of the lowest operations of the human intellect.<sup>68</sup>

The passage is a reiteration of Leibniz's lament of 1685 in which he refers to the labour of calculation as 'unworthy of excellent men' and more appropriate to slaves, peasants or machines.<sup>69</sup> Babbage here implies the superiority of abstract, analytical or philosophical activity over repetitive task-specific activity and the passage highlights two features promised by the engine: the saving of labour, and the elimination of drudgery.<sup>70</sup> In his manuscript account dated November 1822 Babbage's sympathies extend to the mathematicians who did the preparatory work for the computers and also, and especially, to the proof readers:

Although the larger share of that wearisomeness and disgust, which always attend the monotonous repetition of arithmetical operations, must undoubtedly fall to the lot of those to whom the details of the computations are committed, yet the preliminary calculations, and especially the subsequent comparisons and verification, usually afford a considerable trial of the patience of those who superintend them.<sup>71</sup>

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<sup>68</sup> *Ibid.*

<sup>69</sup> See Chapter 1, p.33.

<sup>70</sup> For discussion of Babbage's labour-saving arguments see below p. 146.

<sup>71</sup> *Works*, Vol. 2, p. 15.

Babbage was writing here from personal experience: the passage recounts his earliest conception of the engine when he and Herschel met to proofread, check and 'superintend' a new set of astronomical tables.

#### New Implications - Theoretical

The analysis so far is based on a close reading of Babbage's early published papers with a view to placing his early expectations of his engines in the context of deficiencies in published tables as contemporarily perceived. Focussing on the issues of errors, drudgery and the saving of labour frames the benefits of the engines in terms of technology as replacement for existing manual methods – how to do better or differently what was already being done. The major studies of Babbage tend to accept 'replacement' as the motive, and then to focus on the engineering practicalities and the political and financial tribulations of the construction project that occupied Babbage till 1834, and on the design of the Analytical Engine that occupied him thereafter. As a result his early speculations on the potential of the engines, for purposes other than replacement, have been largely overlooked, and this omission has unbalanced historical accounts of his views on the potential use and value of his machine.

It is clear that from the start the theoretical implications of his engines to mathematics exercised Babbage greatly and he appears at times to be more intrigued by this new territory than by mundaner aspects of utility. Baily hinted at this when he wrote:



But, it is not in these mechanical contrivances alone, that the beauty and utility of the machine consist. Mr. Babbage, who stands deservedly high in the mathematical world, considers these but of a secondary kind, and has met with many curious and interesting results, which may ultimately lead to the advancement of the science.<sup>72</sup>

### Computation as Systematic Method

The first mention of new mathematical implications appears in the letter to Davy, almost as a throwaway:

Another and very remarkable point in the structure of this machine is, that it will calculate tables governed by laws which have not been hitherto shown to be explicitly determinable, or that it will solve equations for which analytical methods of solution have not yet been contrived'.<sup>73</sup>

No further details are provided to Davy. But the last two papers in the set of five are exclusively devoted to the topic and it is clear from this that the mathematical implications preoccupied him at this stage.<sup>74</sup> There are two elements of his claim to Davy: the ability of the machine to calculate series for which there is no given analytical formula; the ability to solve equations with no known analytical solution. The statement signals something fundamentally new, that is, computation seen as systematic process. No plans or physical relics of his first model survive. However,

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<sup>72</sup> Baily (1823). *Works*, Vol. 2, p. 54.

<sup>73</sup> *Works*, Vol. 2, p. 7.

<sup>74</sup> The two papers in question are the letter to Brewster, dated 6 November 1822, published in the *Edinburgh Philosophical Journal*, and a notice to the Astronomical Society read on 13 December 1822. See *Works*, Vol. Vol. 2. pp. 33-43. A third paper on the subject was read on 3 May 1824 and published in *Trans. Cambridge Phil. Soc* in 1826. See *Works*, Vol. Vol. 2, pp. 61-68.

if the larger design, which followed hard on the first, is indicative then we can surmise, even if not conclusively, as to what he might have had in mind.<sup>75</sup>

The roots or solutions of an equation are the values of the independent variable at which the function passes through zero. The standard analytical technique for solving equations was to equate the expression to zero and to solve for the unknown. There was no systematic process for doing this and the success of the process depends on ingenuity, creativity, and often an ability to manipulate the problem into a recognisable form that has a known class of solution. Not only was there no guarantee of solution using such techniques, but there was no way of determining whether or not the equation in question was soluble in principle.<sup>76</sup> If analytical methods failed, then trial and error substitution could be tried. This involves substituting trial values of the independent variable and repeating this process to see if a value of the argument can be found that reduces the function to zero. But the technique was hit and miss. It was regarded as 'inelegant' by mathematicians and did not guarantee success.

What was new in Babbage's description of solving equations with machines was the use of computation as a systematic method of solution. As mentioned earlier, the solution to the equation is the value of the independent variable that reduces the value of the function to zero. Starting with an initial value of the independent variable, each cycle of the engine generates each next tabular value, and the machine has found a 'solution' when the figure wheels giving the tabular result are all at zero. Finding a solution reduces to detecting the 'all-zero' state, and the number of machine cycles taken to achieve this represents the value of

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<sup>75</sup> Babbage's fragmentary account of the first machine indicates that it was similar in several respects to his later designs, specifically, the representation of numbers by figure wheels in vertical stacks, and its automatic operation.

<sup>76</sup> These ideas pre-echo notions of definite method and the problem of 'computability' explored in by Alan Turing in a landmark paper published in 1936-7. See Davis (1988); Hodges (1983).

the independent variable, which is the solution sought. Rather than rely on visual detection of a particular tabular value Babbage incorporated a bell in his second machine that rings to alert the operator to the occurrence of specific conditions in the column of tabular values.<sup>77</sup> If the bell mechanism can be set to detect the all-zero condition, or a sign change, then 'extracting the roots of equations' involves the operator in setting the initial values, then cranking the handle to cycle the machine until the bell rings. He would then halt the machine and read off the number of cycles the machine had run (which is automatically counted on one of the registers). This number is the first root of the equation. If there are multiple roots, as there would be in most cases, the operator keeps cranking, until the bell rings again.<sup>78</sup> In the event that there are no roots, the machine continues *ad infinitum* without the zero state or sign change occurring.<sup>79</sup>

The machine here represents a new technology of mathematics which renders practical methods that would otherwise be prohibitively labour intensive. The feature of the engine that allows this is the fact of it being automatic, that is, it embodies mathematical or computational rule in mechanism, and has ability to repeat the computational operations by incurring physical rather than mental cost.

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<sup>77</sup> See Lardner (1834), *Works*, Vol. 2, p. 169.

<sup>78</sup> Clement's portion of Difference Engine No. 1, assembled in 1832, is operated manually by a crank handle. Babbage refers to DE '0' as being operated by gravity or 'some other means'. There is no reason why detection of the all-zero state should not automatically halt the machine on the detection of a solution. Babbage left detailed designs for devices to automatically halt his engines: Difference Engine No. 2, for example, has a mechanism for automatically halting the engine at the end of a printed page of results. See Swade (1996, *Science Museum*), p. 44-5. In 1842, in the context of the Analytical Engine, Luigi Menabrea made specific reference to the zero-state halting the machine automatically. See *Works*, Vol. 3., p. 109.

<sup>79</sup> In the 1930s Alan Turing formalised the problem of 'computability' in terms of the halting criterion of a notional universal machine. His notions of 'definite method' and 'mechanical process' were seminal concepts in computer science. Turing is not thought to have known of Babbage's work. See Hodges (1983), p. 109.

### The Heuristic Value of the Engines

The second feature flagged to Davy refers to the ability of the machine to calculate tables according to rules for which there is no known analytical law. In his letter to Brewster he expands on this:

I can by setting an engine, produce, at the end of a given time, any distant term which may be required; or if a succession of terms are sought, commencing at a distant point, these shall be produced. Thus, although I do not determine the analytical law, I can produce the numerical result which it is the object of that law to give.<sup>80</sup>

The power and appeal of analytical formulation derives from its generality, that is, the ability to represent, in a single statement through symbols, any and all specific instances of the relations expressed. The unspoken values of analytical science elevate generality and universality, above example and instantiation. A silent premise of contemporary mathematics and philosophy was that example is inferior to generalisation, induction inferior to deduction, empirical truths to analytical truths, and the synthetic to the analytic. Stretching the analogy to social class there are parallels with the social inferiority of trades and manual activity compared with philosophical and intellectual occupations; journeyman compared to gentleman. Calculation, which involves specific numerical example, was, in the prevailing culture, implicitly inferior to formal analysis. The existence of a series that could be produced by computational rule for which no formal law was known, fell outside the comfort zone of analytical tradition. This was new territory, and Babbage was clearly intrigued by the general question of how to find analytical

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<sup>80</sup> *Works*, Vol. 2, p. 43. Babbage makes a similar statement to Davy. See *Works*, Vol. 2, p. 7, quoted above.

laws for series suggested, or produced, by the engine. In his letter to Brewster he tabulates the first set of values and differences of a new series suggested by the engine and proceeds to derive a general expression for the  $n$ th term.<sup>81</sup> The process is essentially one of induction and Babbage acknowledged that the process is at odds with the traditions of mainstream mathematics. He wrote in 1824 that the unusual route he took in arriving at a general form for a new series was 'much more conducive to the progress of analysis, although not so much in unison with the taste which at present prevails in that science'.<sup>82</sup>

The example of the new series he described to Brewster was prompted by the layout of the engine, and represents one of several suggestive and actual instances of the heuristic value of the engine in stimulating new enquiries in mathematical analysis. In the second notice to the Astronomical Society, read on 13 December 1822, Babbage traced the process suggested by his use of the engine, that led to his speculations on the new series:

I will now advert to another circumstance, which, although not immediately connected with astronomical tables, resulted from an examination of the engine by which they can be formed. On considering the arrangements of its parts, I observed that a different mode of connecting them would produce tables of a new species altogether different from any with which I was acquainted. I therefore computed with my pen a small table such as would have been informed by the engine had it existed in this new shape and I was much surprised at discovering that no analytical method was yet known for determining its  $n$ th term.<sup>83</sup>

He then listed the series in which each second difference is given by only the units value of the current tabular value, with the larger value digits ignored. The gist of

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<sup>81</sup> *Works*, Vol. 2, p. 38-40.

<sup>82</sup> Babbage (1826), *Works*, p. 62.

<sup>83</sup> *Works*, Vol. 2, p. 34. Read 13 December 1822.

the issue is that the new series was formed by using only some of the digits in the tabular value as the basis for the calculation of each next value.

The passage quoted indicates that the idea for new series of this kind arose from the spatial representation of number in the machine. In all Babbage's calculating engines numbers are represented by figure wheels engraved with the numbers '0' through '9', arranged in vertical stacks or columns, with one wheel for each digit of the number.<sup>84</sup> The columns stand alongside each other and are coupled with internal gearing that orchestrates the motion of the figure wheels to perform the repeated additions required for tabulation by method of differences. The broadside view of the engine presents a rectangular matrix of figure wheels representing the value of the tabular value in one column, the first difference in the next column, second difference the next column, and so on. The matrix of wheels suggests that apart from the internal gearing for repeated addition, individual figure wheels can influence others by *external* connection. Specifically, by externally gearing wheels together any given figure wheel (representing units, tens, hundreds etc of a given number) could add its value to a wheel in another column. The technique allows feedback, feed-forward or cross-feeding of individual digits in a way that influences the step-wise generation of successive results. For example, only the tens wheel on the second difference column could be coupled to the hundreds wheel on the tabular value column, leaving intact the machine's internal gear train linking the two columns. Cycling the machine would produce a new series for which there was a clear computational rule by which to generate each

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<sup>84</sup> The physical organisation of the first machine appears to have been similar to that of the version of Difference Engine No. 1 a portion of which that was constructed and for which plans survive. See *LEC*, p. 28, 31-32. Babbage introduces the term 'figure wheel' in the Buxton MS dated November 1822. See *Works*, Vol. 2, p. 19.

next value, but for which there was no analytical formula necessarily known or available.<sup>85</sup>

There is no indication that Babbage physically implemented such cross-coupling on his first model, and the early exploration appears to have been a pen-and-paper speculation. This is inferred both from the passage quoted above, and also from his letter to Brewster written about a month earlier which contains a similar account.<sup>86</sup> However, the portion of Difference Engine No. 1, assembled by Clement in 1832, clearly shows the additional axes and gears that allow such cross coupling that were added when Babbage returned to these ideas, after a decade of distraction constructing the larger machine.<sup>87</sup>

Babbage's speculations on finding general analytical laws for empirically generated series are undeveloped. They were inconsequential to the development of mathematics and represent one of the topics outside mainstream analysis that he focused on, but did not pursue. However, in the history of the development of mathematical ideas, the line of enquiry represents the earliest realisations that there was a theoretical dimension to computational method that was important and unexplored, and that computation involved more than the contingent specifics of numerical example. The prospect of calculating machines elevated computation to a systematic method, and in these early papers Babbage appears to be struggling to communicate its new status. His writing is often unclear and suggestive, and tends to slide between specifics and generalities. However, he appears clearly to have sensed that there was something new and fundamental in step-wise

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<sup>85</sup> These ideas are considered by Collier in the context of the genesis of the Analytical Engine and the notion of the engine 'eating its own tail'. The reflexivity of the process is seminal to the transition from calculator to general purpose computation. See *LEC*, pp. 107-116. The present discussion is concerned more specifically with the heuristic value of the mechanical representation of quantity, and of mathematical rule.

<sup>86</sup> *Works*, Vol. 2, p. 38.

<sup>87</sup> For details of the mechanical additions see Roberts (1987), p. 211; Also *LEC*, p. 112.

mechanical process as a realisation of algorithmic procedure, and that the prospect of calculating engines invoked a new discourse of mathematical analysis.<sup>88</sup>

### Numerical Analysis

In an oblique passage to Brewster Babbage wrote:

Some kinds of equation of differences, can be adapted to machinery with much greater facility than others; and hence it will become an object of enquiry, how, when we wish to calculate that of any transcendant, we may deduce from some approximate equation the difference which may be suitable to our purpose. Thus, you see, one of the first effects of machinery adapted to numbers, has been to lead us to surmount new difficulties of analysis; and should it be carried to perfection, some of the most abstract parts of mathematical science will be called into practical utility, to facilitate the formation of tables.<sup>89</sup>

Babbage was arguing that to exploit the facilities of the engine would require a mastery over the analysis that must *precede* computation by machinery. The two elements of this analysis are: how best to approximate functions by particular series; and secondly, the preparatory analysis to ensure that the approximation remains valid to the requisite accuracy within the restricted range of the function being tabulated. Neither elements of analysis was new, and both were used by table-makers in the manual preparation of tables using the method of differences.

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<sup>88</sup> See Grattan-Guinness (1992, *Annals*) for a thematic treatment of Babbage as an algorithmic thinker. Babbage wrote that the engine's stimulus for his enquiries was 'singular in the history of mathematical science'. See Babbage (1826), *Works*, Vol. 2, p. 61.

<sup>89</sup> *Works*, Vol. 2, p. 43. A feature of transcendental functions is that they have non-constant differences. This is a technical issue in series expansions of certain functions that engaged Babbage and played a major role in the transition from the Difference Engine to the Analytical Engine. It is not materially relevant to the central concern here, which is with the terms in which Babbage framed utility.



However, the benefits of the engine articulated in this passage are twofold: the stimulus to mathematics in generalising and systematising the analysis of computational methods using differences; secondly the value of this analysis and the engine in rendering practically useful otherwise abstract mathematics.

His closing passage to Brewster predicts the importance of computing machines to the progress of science:

If the absence of all encouragement to proceed with the mechanisms I have contrived, shall prove that I have anticipated too far the period at which it shall become necessary, I will yet venture to predict that a time will arrive, when the accumulating labour which arises from the arithmetical applications of mathematical formulae, acting as a constantly retarding force, shall ultimately impede the useful progress of the science, unless this or some equivalent method is devised for relieving it from the overwhelming incumbrance of numerical detail.<sup>90</sup>

In a manuscript on the Analytical Engine, dated 26 December 1837, unpublished in his lifetime, Babbage further predicted the importance of computational method and numerical analysis as new discourses for optimising the efficiency of machine calculation. After manipulating a formula to show that one version required thirty-five multiplications and six additions to find its value, and a mathematically identical but alternative expression required only five multiplications and one addition, he wrote:

The consequences resulting from this circumstance are important and deserve the attention of those who are engaged in extending the domain of analysis, as well as those who look forward to the effects which are likely to be produced by the complete control which mechanism now gives over number. Whenever engines of this kind exist in the capitals and universities

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<sup>90</sup> *Works*, Vol. 2, p. 43.

of the world, it is obvious that all those enquirers who wish to put their theories to the test of number, will apply their efforts so to shape the analytical results at which they have arrived, that they shall be susceptible to calculation by machinery in the shortest possible time, and the whole course of their analysis, will be directed towards this object. Those who neglect the indication will find few who will avail themselves of formulae whose computation requires the expense and error attendant on human aid.<sup>91</sup>

In these two passages Babbage reasserted the future importance of numerical methods to the advance of science and anticipated the eventual dependence on machines as a determining factor in future progress.

In addition to the new theoretical implications of the engines, which clearly preoccupied Babbage, he also articulated practical implications that fall outside the category of 'remedies for deficiencies'. These include labour-saving implications, and spin-off benefits that were unforeseen.

#### New Implications – Practical

##### Labour Saving

A specific practical implication of the engines was the potential to save labour and this features prominently in Babbage's advocacy to Davy. He argued the case in more detail and greater length than for any other feature. Using de Prony's project Babbage sought to identify 'what share of mental labour would have been saved by the employment of such an engine as I have contrived'.<sup>92</sup> The case is a hypothetical one. He listed the scope and range of the tables and described to

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<sup>91</sup> Babbage (1837, Buxton Collection), *Works*, Vol. 3, p. 60-1.

Davy the organisation of labour into three classes divided according to a hierarchy of skills, following Adam Smith's principles on the division of labour. Babbage estimates that the first group (the elite of the mathematicians) would be reduced from five or six to at most two, not because of any benefit conferred by the machine, but because the preparatory work selecting the formulae had already been done for de Prony's project. The major savings would be in the third group (amongst whom were the unemployed hairdressers who performed the low-level repetitive arithmetic for subtabulation) that would be entirely replaced. But this economy would be offset by a complement of people 'to copy down as fast as they were able the figures presented to them by the engine'.<sup>93</sup> If the printing apparatus were constructed even the scribes could be dispensed with and replaced by 'a few superintendents' to 'manage the machine and receive the calculated pages set up in type'. He concludes that the overall labour force would be reduced from ninety-six to twelve.<sup>94</sup> Still further reductions in the second group would result, if fewer pivotal values were used by increasing the intervals between them to maintain the same numerical range of the tables. In a system of human labour this would increase the load on the third class of calculators. In a mechanised system longer subtabulation runs placed no additional burden on the engine (beyond physical input) and reduced the work of the second group.<sup>95</sup> Babbage suggested that de

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<sup>92</sup> *Works*, Vol. 2, p. 10. For discussion of de Prony's tables see Chapter 1, pp. 56-62; Chapter 2, pp. 79-81.

<sup>93</sup> *Works*, Vol. 2, p. 12.

<sup>94</sup> *Ibid.* It is not clear how Babbage arrived at the figure of twelve. By his own reckoning the complement of the three groups totals ninety four not ninety six.

<sup>95</sup> Longer subtabulation runs would also have reduced the number of times new initial values would need to be set and this would represent a saving of labour. Babbage does not use this argument.

Prony's use of subtabulation runs of 200 intervals could be increased three- or four-fold.<sup>96</sup>

Babbage nowhere explained the basis of these estimates which are offered as unsupported assertions, and lack of quantification is characteristic of his advocacy. However, Francis Baily, a friend of Babbage and a vigorous supporter the engine, in a largely derivative but well-argued paper, attempted to put number to benefit by estimating the quantity of mental labour that would be saved by the machine for each of twelve classes of tables. These include products of numbers, squares, cubes, powers, square roots, reciprocals, trigonometric functions, logarithms, and logarithms of hyperbolic and trigonometric functions.<sup>97</sup> In four instances Baily claimed that 'the whole of the mental labour would be saved'. In the case of trigonometric functions, for example, he estimated that the machine would reduce the effort by a factor of two thousand though he confessed that a precise estimate is 'difficult'.<sup>98</sup> In the case of logarithms he revealed the basis of his method of quantifying labour. This consisted in estimating the reduction in preparatory calculation by using more widely spaced pivotal values, and the elimination of the labour of subtabulation – this along the lines of Babbage's case to Davy in relation to de Prony's project.<sup>99</sup> In the case of hyperbolic logarithms Baily estimates a reduction in labour by a factor of 200 but confesses that this follows only 'a slight examination of the subject'.<sup>100</sup> Baily appears to be attempting to 'harden up' Babbage's largely rhetorical assertions by quantifying the labour-saving benefits of the engines, even though his calculations are self-confessedly in the nature of back-of-the-envelope estimates.

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<sup>96</sup> *Ibid.*

<sup>97</sup> See Baily (1823). *Works*, Vol. 2, pp. 46-50.

<sup>98</sup> *Ibid.* p. 48.

<sup>99</sup> *Ibid.*, p. 49.

<sup>100</sup> *Ibid.*

It is clear from this reading of Babbage's earliest writings that, while the elimination of error features in the rationale for his engines, it does so alongside several other considerations, and not as prominently as historical accounts have suggested.

The discussion of Babbage's expectations of his engines has been based so far on Babbage's earliest writing on the benefits of his engines. Given that the machines remained unbuilt in his lifetime, the supposed benefits were necessarily largely based on prospect and promise. However, there was a specific benefit that Babbage claimed in retrospect, and this is the spin-off benefit to manufacturing from the failed attempts to construct the engine, to which Babbage repeatedly drew attention.

### Spin-off Benefits

At a meeting with the Chancellor, F. J. Robinson in July 1823 Babbage secured public funds for the construction of a larger and fully engineered calculating engine.<sup>101</sup> Before embarking on the construction Babbage conducted a systematic review of mechanical manufacturing to establish whether existing techniques and processes were adequate to realise his machine.<sup>102</sup> His survey of craft and engineering took the form of a protracted tour of factories and workshops in England and Scotland that started in August 1823.<sup>103</sup> His conclusion was that that

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<sup>101</sup> The terms of the commitment were not recorded. See Collier (1970), p. 42. There is uncertainty in the date. Hyman places the meeting in June 1823 *PC* (1982), p. 52). Collier places it 'at some point in July'. *LEC*, p. 42.

<sup>102</sup> Babbage (1851). *Works*, Vol. 10, p. 105.

<sup>103</sup> For start date see Roberts (1990), p. 2. Babbage took to staying at inns and taverns to make contact with tradesmen whom he could tap for know-how. See *Passages*, p. 385; *CWB*, p. 39-40.

the intricacy and precision required would stretch the capacity of current practice and 'it would become necessary to advance the art of construction itself'.<sup>104</sup> His knowledge of British practice was supplemented by familiarity with Continental methods. During his travels between October 1827 and November 1828 following the death of his wife, Georgiana, his father and two of his children in 1827, he visited workshops and manufactories in Europe.<sup>105</sup> His survey of manufacturing served as the basis for his most substantial and arguably his most influential full-length work, *Economy of Machinery and Manufactures*, published in 1832.<sup>106</sup> The work, described by Maxine Berg as a 'brilliant and utterly original foray into political economy', is concerned with the domestic economy of factories.<sup>107</sup> It also includes an encyclopaedic record of craft and manufacturing processes based on his systematic survey of industry in England, Scotland and the Continent. In the preface he introduces the volume as 'one of the consequences that have resulted from the calculating engine'.<sup>108</sup>

By way of more direct reference to spin-off benefits Babbage repeatedly drew attention to the consequences to the manufacturing industries of the machines, tools and techniques developed to factor parts for the Engine. On the recommendation of Marc Isambard Brunel, Babbage hired Joseph Clement, a highly skilled tool-maker and machinist and also a first rate draughtsman, a rare combination of skills to find combined in one person.<sup>109</sup> An inventive collaboration

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<sup>104</sup> Babbage (1851), *Works*, Vol. 10, p. 105.

<sup>105</sup> See *CWB*, pp. 54-8.

<sup>106</sup> See editor's Introduction, *Works*, Vol. 8, p. 5. Also, Swade (1991, *ISIS*), p. 533.

<sup>107</sup> For Berg's assessment of *Economy* see *Works*, Vol. 1, p. 27-30. The work is described by Campbell-Kelly as 'a turning point in economic writing'. See *Works*, Vol. 8, p. 5.

<sup>108</sup> Babbage (1832), *Works*, Vol. 8, p. v. For similar statement see Babbage to Duke of Wellington, 23 December 1834, BL Add Ms 37188, f. 525, *Works*, Vol. 3, p. 3.

<sup>109</sup> *CWB*, p. 41. There is no known biography of Clement. The main source of biographical information is Samuel Smiles', *Industrial Biography: Iron Workers and Tool Makers*. See Smiles

developed. Clement adapted and improved existing tools and machines, and designed new ones to meet the needs of the calculating engine.<sup>110</sup> His contributions to machine tool technology were acclaimed. He was awarded the Society of Arts gold medal in 1827 for lathe improvements, and the next year the silver medal for his 'self-adjusting double-driving centre chuck'.<sup>111</sup> Most of the machines for which he was applauded were developed while he was working on the calculating engine, largely at Babbage's expense, and he thrived and expanded his establishment during the Engine project.<sup>112</sup>

What was possibly Clement's most significant contribution was not officially rewarded. This was his attempt to standardise screw threads. In the 1820s there was practically no standardisation in manufacturing. Each mechanical engineer had his own taps and dies used for cutting screws and no two were the same. Each lathe had a different master screw used as the pattern for cutting screw threads, and a screw cut on one lathe would be different from that cut on its neighbour even in the same workshop. The issues of standardisation are inextricably linked with those of interchangeability of parts. Without standardisation, a nut cut in Manchester could not be expected to fit a bolt made in London, except by happy accident. Apart from the implications to production there were issues of repair. If a machine needed a new bolt the thread cut into the machine even if undamaged, had to be drilled out and retapped to match the new component. Samuel Smiles records that Clement saw the waste and trouble of this

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(1876), Chapter XIII, pp. 236-257. For a discussion of Clement as 'the first computer engineer' see Williams (1992).

<sup>110</sup> Babbage gives an example of how the needs of the stereotyping apparatus led to the development of a general shaping machine. See *Passages*, p. 46.

<sup>111</sup> Smiles (1876), p. 245; *Ibid.*, p. 247.

<sup>112</sup> *LEC*, p. 223. For details of published descriptions of Clement's inventions and machines see *LEC*, pp. 223-4. . For description of a new shaping machine directly resulting from stereotyping

and in about 1828 settled on a fixed number of threads in a given length of screw.<sup>113</sup> The scheme was not immediately adopted by others, but in the 1850s Whitworth, one of Clement's journeymen who worked on the calculating engine between 1831 and 1833, established the eponymous Whitworth thread which became a world-wide engineering standard for the next 170 years.<sup>114</sup>

In *Exposition* published in 1851, Babbage wrote:

Frequently sketches, or new drawings, were made, for the purpose of constructing the tools or mechanical arrangement thus contrived . . . In the meantime, many workmen of the highest skill were constantly employed in making the tools, and afterwards in using them for the construction of parts of the engine. The knowledge thus acquired by the workmen, matured in many cases by their own experience, and often perhaps improved by their own sagacity, was thus in time disseminated widely throughout other workshops. Several of the most enlightened employers and constructors of machinery, who have themselves contributed to its advancement, have expressed to me their opinion that if the calculating engine itself had entirely failed, the money expended by government in the attempt to make it, would be well repaid by the advancement it had caused in the art of mechanical construction.<sup>115</sup>

The appearance of this claim in a work on the Great Exhibition is perhaps no accident. The publication of *Economy* in 1832 had established Babbage as an authority and elder-statesman of the industrial movement, and done much to repair the damage to his standing following his sarcastic attack on the Royal Society in

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experiments see *Passages*, p. 46. For a description of Clement's role in the Engine project see *CWB*, pp. 68-71.

<sup>113</sup> Smiles (1876), p. 248.

<sup>114</sup> For references to Whitworth's service on the Engine in Clement's workshop see *PC*, p. 231; *LEC*, p. 88; Williams (1992), p. 77. For reference to Whitworth and his screw threads see *LEC*, p. 224.

<sup>115</sup> Babbage (1851). *Works*, Vol. 10, p. 106. For statements attesting to the benefit to manufacturing see *Works*, Vol. 2, pp. 111, 178; Vol. 3, p. 3, 7. For an earlier statement relating to the dissemination of skill through the movement of workmen see Babbage (1843), *Works*, Vol. 3, p. 86. For claim that men who worked on the engine attracted higher wages see Babbage (1834), *Works*, Vol. 3, p. 7 ft; also *LEC*, p. 88. For a panegyric of the benefits to manufacturing in an extended tribute to Babbage see Henry (1874), p. 173.



*Decline*, which appeared to general dismay in 1830. But his reputation for public protest and unconstrained criticism of the scientific establishment seems to have endured, and he was actively excluded from the commissions and from any participation in the organising committees of the 1851 Exhibition. Babbage was sixty, and Hyman concludes simply that 'the old scientific radical was not wanted'.<sup>116</sup> *Exposition* was a bitter attack on the Commissioners and their conduct, and Babbage's wish to establish his contribution to the industrial arts by staking a claim in so robust a way can be seen a response to his exclusion from the largest extravaganza of industrial manufacturing yet staged – a form of protest against hurtful marginalisation.

His preoccupation with recognition for the indirect benefits of the engine project did not stop in the 1850s but appears to have been an enduring one. In *Passages*, published thirteen years later, Babbage reasserts his claim by quoting at length Lord Rosse's presidential address delivered in November 1854 to the Royal Society. The published address includes an assertion by Rosse:

I wrote to one of our most eminent mechanical engineers to inquire whether I should be safe in stating to Government that the expense of the Calculating Engine had been more than repaid in the improvements in mechanism directly referable to it; he replied, – unquestionably.<sup>117</sup>

The 'eminent mechanical engineer' was James Nasmyth.<sup>118</sup> Following Rosse's address, Babbage solicited testimonials from other leading lights in engineering including William Fairbairn and Whitworth, all of whom confirmed Rosse's

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<sup>116</sup> *PC*, p. 216-7. See also *CWB*, pp. 184-6.

<sup>117</sup> Quoted in *Passages*, p. 100.

<sup>118</sup> See Rosse to Babbage, 22 July 1852, BL Add Ms 37195, f. 108.

assertion differing only in the degree of fulsomeness of their tributes.<sup>119</sup> In

November 1869, two years before his death, he again registered his claim:

I have heard at different times from men I had employed in former years that amongst their own class it was frequently said that:

Mr. Babbage made Clement.

Clement made Whitworth.

Whitworth made the tools.<sup>120</sup>

There are other circumstances that support the reading that Babbage was seeking compensatory recognition. Between 1847 and 1849 Babbage designed Difference Engine No. 2, an elegant advanced design that benefited from the Analytical Engines.<sup>121</sup> In 1852 Babbage offered the plans of the new machine to Lord Derby, the newly appointed Tory PM, via his friend and supporter, Lord Rosse.<sup>122</sup> The response from Government was a resounding rejection.<sup>123</sup> Babbage had had enough. He replied to Rosse that he did not propose to 'force a generous offer upon a reluctant country' and made a reference to 'pearls before swine'.<sup>124</sup> Babbage made no further attempt to secure support. Finally, after three decades of frustration, disappointment, and some degree of humiliation, he effectively gave up any prospect of realising any of his machines in physical form. Since he could no longer hope for recognition or vindication from a completed calculating machine, his attempts to have his contribution endorsed by testimonials from eminent

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<sup>119</sup> See Babbage to Fairbairn and Whitworth, 25 June 1855, BL Add Ms 37196, f. 255; Fairbairn to Babbage, 27 June 1855, BL Add Ms 37196, f. 259. For Fairbairn's 'testimonial' see BL Add Ms 37197, ff. 257-8. For Nasmyth's testimonial see Nasmyth to Babbage, 22 June 1855, BL Add Ms 37196, f. 251.

<sup>120</sup> Letter to Pearson, November 1869. Quoted in *LEC*, p. 223. Also, *CWB*, p. 70.

<sup>121</sup> See *CWB*, pp. 174-176. *LEC*, p. 211.

<sup>122</sup> *LEC*, p. 214.

<sup>123</sup> Talbot to Rosse, 16 August 1852, BL Add Ms 37195, f. 118. Quoted in *LEC*, p. 218.

<sup>124</sup> *LEC*, p. 218.

engineers, and his repeated references to the indirect benefits of the failed engine project to the industrial arts, may well have been an attempt to salvage some dignity and secure compensatory recognition, now that the original trophy, a competed calculating engine, was no longer in prospect.

### Profit

In Babbage's advocacy for his engines he was, in general, hazy about quantifying the supposed benefits in contemporary terms. Apart from using de Prony's tables project to illustrate the quantity of labour that might be saved, Babbage rarely dignified his arguments with number.<sup>125</sup> In relation to quantifying the labour-saving potential of the engine it was Baily who came to the rescue. But nowhere did Babbage, or anyone else, attempt to justify the engines in cost terms by, say, estimating the capital costs of an engine, and comparing this with the labour costs of a manual alternative. Thomas Young and Airy later argued that investing the high capital costs of an engine and using the interest or dividend to pay computers was an economically sound alternative to building machines. But again, in an age obsessed with number, empiricism and certainties, there is no evidence that even those hostile to the engines used economic arguments founded on calculation, or indeed, anything beyond assertion or accusation.<sup>126</sup>

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<sup>125</sup> For example, in his letter to Davy, Babbage states that if engines were made and were afterwards useless 'tables could be produced at a much cheaper rate' (Babbage 1822), *Works*, Vol. 2, p. 13. But there is no evidence of a numerical estimate. Similarly, in 1823 he wrote enthusiastically to Herschel that with his engines he could produce logarithm tables 'as cheap as potatoes'. (Quoted in *PC*, p. 53). This must be taken as a figurative expression of cheapness rather than anything more serious.

<sup>126</sup> For discussion of Young's objection see Chapter 4, p. 212-3.

Babbage was himself quite clear from the outset that the construction of the engines would not yield profit to an investor. In 1822 he ended his letter to Davy commenting that the success of the engine would 'be obtained at a very considerable expense, which would not probably be replaced, by the works it might produce, for a very long time'.<sup>127</sup> The implication here is that the product was the printed table, and the engine, the means of production. In similar vein he wrote to Henry Colebrooke in January 1824 suggesting that Colebrooke, in his forthcoming Presidential address to the Astronomical Society, might wish to acknowledge

the very liberal manner in which Government have behaved and at the same time to state the principle on which the Chancellor of the Exchequer acted. That the machine was of importance to the country and could not possibly repay the contriver the sum which its products would sell for.<sup>128</sup>

Babbage evidently wished to commend government for what he regarded as an enlightened view in supporting ventures that served the public good, but that would not themselves produce a direct financial return. A decade later Babbage was still clearly of the same mind. In 1834 he wrote to the Duke of Wellington:

About 13 years ago, I undertook to superintend the construction of the calculating engine at the wish of the then administration. The grounds on which they took it up were, that it was not in its nature capable of becoming an object of pecuniary profit, that it was of the highest importance to a country, possessing an extensive marine to add to its security by the construction of an engine capable of producing astronomical and nautical

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<sup>127</sup> *Works*, Vol. 2, p. 14.

<sup>128</sup> Babbage to Colebrooke, 19 January 1824. Transcribed from photograph of original, Science Museum Photo Library reference 330/91, 331/91. Original probably in Herschel papers, Royal Society. The occasion of the address was the presentation to Babbage of the first gold medal of the Astronomical Society, for the invention of the calculating engine. See *Works*, Vol. 2, p. 57.

tables with unerring precision. I thought, and still think, those were *statesmanlike* reasons.<sup>129</sup>

The emphasis on 'statesmanlike' again signals Babbage's commendation of government securing public benefit in situations where the market failed to provide sufficient private incentive.<sup>130</sup> His own stated motives were consistently independent of financial motive. Indeed, he was stung by allegations that he had profited personally from the lavish sums expended by the Treasury on the Engine.<sup>131</sup>

By 1842 there are indications that Babbage's position had altered, even if only speculatively. In November 1842 he pressed for a meeting with Peel ostensibly to have resolved the future of the Engine project.<sup>132</sup> In preparation for the interview, which took place at short notice on November 11, Babbage outlined his case in a series of private notes itemising the progression of the argument he proposed to follow.<sup>133</sup> The main thrust of his case was his grievance at the lack of reward or recognition for his years of work, and his wish for government to place him in a position to produce the engines at his own expense by awarding him a pension or paid position, or by financing the project directly. As it happened,

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<sup>129</sup> Babbage to Wellington, 23 December 1834, *Works*, Vol. 3, p.2. Emphasis original. For a similar statement by Lardner see Lardner (1834), *Works*, Vol. 2, p. 184.

<sup>130</sup> The view that engines were not a profitable was a durable one to the Babbages, and extended to the Analytical Engine. His son, Henry Prevost, wrote in 1888: I see no hope of any Analytical Engine, however useful it might be, bringing any profit to its constructor'. See Babbage (1889), p. 337. The statement was made in a paper read at the meeting of the British Association, Bath, 12 September 1888. Reprinted in *Works*, Vol. 3, pp. 190-205. For quotation cited see p. 204.

<sup>131</sup> Babbage raised his 'vexation' at these allegations with Robert Peel on 11 November 1842. See Buxton Memoir (Hyman (1988), p. 110). Babbage hinted to Peel that a public honour would dispel public suspicion. See Babbage's meeting notes, November 1842, BL Add Ms 37192, f. 182. Lardner asserts that the allegations were 'destitute of truth'. See Lardner (1834), *Works*, Vol. 2, p. 184. Herschel publicly defended Babbage in a letter to the *Times*, 19 August 1828. See also *Exposition*, *Works*, Vol. 10, p. 102.

<sup>132</sup> The background and circumstances of this meeting are discussed in Chapter 4. See also *CWB*, pp. 150-3.

<sup>133</sup> See BL Add Ms 37192, ff .180-4.

Babbage did not get to put his proposal to Peel. Babbage misjudged the PM's mood, and the meeting went catastrophically wrong. Peel refused to acknowledge that Babbage was owed anything. They argued, and Babbage stormed out without having made his request.<sup>134</sup> However, Babbage's private notes reveal a scheme that might have delivered both the Analytical and Difference Engines if Peel had adopted it. The scheme involved Government funding the Difference Engine in three equal stage payments, the first (non-returnable) as an advance or down-payment, the second when the machine was half-finished, and the final payment on the successful printing of a table of logarithms to five or six figures. So far the scheme involved capital funding by government. There was no provision yet for financing the Analytical Engine. But Babbage then suggested that the copyright of 'all tables of log, log-sines, cos, tan, sec, Tables of interest and all ready reckoners' be assigned to him. The implication of this is that the proceeds from the sale of the tables would finance the Analytical Engine.

It is unclear whether Babbage had assessed the market and done the sums to see if revenue from the sale of tables would be adequate for his purposes. He certainly knew by then that the unbuilt engine had consumed some £17,500 and was likely to cost as much again to complete. It is possible that Babbage's scheme was unrealistic, and was no more than a gesture of recognition that the financial liquidity of government had radically altered for the worse between 1823, when the Chancellor, Robinson, had apparently made an open-ended commitment to fund the Engine to completion, and 1842, by which time reserves had plummeted, and the reintroduction of income tax was being considered.<sup>135</sup> But the proposal carries the unmistakable message that Babbage, even speculatively, regarded tables as

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<sup>134</sup> For Babbage's dramatic and detailed account of the meeting written on the same day see Buxton *Memoir* (Hyman (1988), pp. 108-111).

<sup>135</sup> See Daunton (1995), p. 520.

a saleable product to the extent that he was prepared to have considered the construction of a machine, even more ambitious than his Difference Engine, using the proceeds from such sales.

The question of profit concludes the present discussion of Babbage's expectations of his engines. Attention is now transferred to Lardner's article and the its defining influence on the received perception that errors were the dominant motive and purpose for the engines.

#### Lardner and Babbage

Lardner's standing in the scientific community veered between that of a serious disseminator of science and a clown. He was a brilliant lecturer, a prolific science writer, populariser, and publicist. He was also known for colourful predictions, many of them dramatically wrong. He apparently asserted in 1839 that trains on the broad gauge could not travel at more than forty miles an hour, and on another occasion that high speed rail travel was impossible because passengers would asphyxiate because of excessive consumption of oxygen by the locomotive on steep inclines in tunnels.<sup>136</sup> He also predicted that crossing the Atlantic by steamship was impossible because of water resistance. Brunel's Great Western made the crossing soon after in April 1838.<sup>137</sup> His personal life was similarly erratic. The diaries of William Macready, which provide a seemingly inexhaustible source of society gossip, record that Lardner, while separated from his first wife, eloped with the spouse of a cavalry captain, Richard Heaviside. Husband and

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<sup>136</sup> For the first claim see PC, p. 163. For the second see Vaughan (1991), p. 54; Cerf and Navasky (1984), p. 232. For Lardner's role in the 'gauge war' see below pp. 161-2..

<sup>137</sup> Vaughan (1991), p. 95. Quoted in *CWB*, p. 194.

father burst in on the runaway couple in a Paris hotel forcibly removing Mrs Heaviside and giving the hapless Lardner a sound thrashing. The scandal was reported in the papers on 14 April 1840 and was much tattled over. Heaviside sought damages for seduction and Lardner had to pay up. The shamed Lardner was described by Macready at the time of the scandal as 'the wretched, the deplorably wretched man'.<sup>138</sup>

Hyman is scathing about Lardner's scientific standing. He refers to Lardner as 'a scientific Falstaff . . . even now . . . occasionally mistaken for a serious figure'.<sup>139</sup> Hyman's scorn is unrelenting:

Dionysus Lardner was the comedy act of the show: he ballooned across the engineering landscape of the time sustained by an inexhaustible supply of hot air. Intensely jealous of those with real scientific and engineering knowledge, such as Brunel and Babbage, Lardner could be relied upon in any engineering situation to get hold of the wrong end of the stick.<sup>140</sup>

Babbage and Lardner mixed in the same circles and corresponded frequently during the 1820s and early 1830s.<sup>141</sup> On one occasion Babbage spared Lardner public ridicule, and on another roasted him. The first was at a meeting of the British Association in Newcastle in 1838 when Babbage, unable to prevail on George Stephenson, Bryan Donkin or one Mr. Buddle to chair a meeting on Steam Navigation to America, found himself presiding over the meeting himself. Lardner was the speaker and Babbage cautioned him before the address that he regarded

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<sup>138</sup> Toynbee (1912), p. 57.

<sup>139</sup> *LEC*, p. 148.

<sup>140</sup> *Ibid.*, pp. 147-148.

<sup>141</sup> *Ibid.* p. 148. The periods of most frequent correspondence were January 1823 - March 1832 (sixteen letters) BM Add MS 37183, and July 1833 - December 1834 (26 letters) BM Add MS 37188. See *The Papers of Charles Babbage: a Listing and Guide to Part 1 of the Microfilm Collection, The History of Science and Technology*. Marlborough: Adam Matthew Publications, 1997. p. 55.



some of Lardner's views as 'hasty' and that it would be prudent for him to admit this publicly so as to defuse hostility. From the chair Babbage defended Lardner whom he suggested had seen the error of his ways and counselled the gathered audience that 'nothing was more injurious to the progress of truth than to reproach any man who honestly admitted that he had been in error'.<sup>142</sup> The meeting proceeded without incident and 'some few who attended in expectation of a scene were sorely disappointed'.<sup>143</sup> Babbage recalls that on leaving the event one of his acquaintances remarked "You have saved that \_\_\_ Lardner".<sup>144</sup> Babbage had spared Lardner the humiliation of having his views ridiculed in public.

On the second occasion Babbage was less charitable. With the expansion of the railways in the 1830s the question of the gauge of the tracks was fiercely contested. Competition for the adoption of a single standard split the interests of Bristol and the West country, which favoured Brunel's wide gauge (seven feet), and those of Liverpool and the North, which favoured Stephenson's narrow gauge (four feet eight-and-a-half inches).<sup>145</sup> The issue came to a head in 1839 at a decisive meeting of the proprietors of the Great Western Railway.<sup>146</sup> Babbage's views had been invited on the relative merits of the broad gauge over the narrow gauge.<sup>147</sup> A committed empiricist, he resorted to experiment and a locomotive and a second-class carriage were placed at his disposal. His investigation was conducted at his own expense and his role was that of an unpaid 'consultant'.<sup>148</sup> In due course he concluded in favour of the broad gauge and he was on sure ground

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<sup>142</sup> Babbage's account appears in *Passages*, pp. 326-328.

<sup>143</sup> *Ibid.* p. 328.

<sup>144</sup> *Ibid.*.

<sup>145</sup> *Ibid.* p. 326; *PC*, p. 155; Vaughan (1991), p. 104.

<sup>146</sup> Vaughan (1991), pp. 116-118; *LEC*, p. 163.

<sup>147</sup> *Passages*, p. 320.

<sup>148</sup> *Passages*, p. 320-1; *CWB*, p. 127.

when he testified to this effect at the crisis meeting at the London Tavern on 9 January 1839.

The meeting was long and acrimonious with the 'Men of the North' pitted against the aristocratic land-owning gentlemen of London and the West Country.<sup>149</sup> Lardner had festooned the walls with technically impressive charts of data from his own experiments on the broad gauge locomotive, the *North Star*. A shareholder, Heyworth, used Lardner's data to argue that high speeds were impossible on the broad gauge. Babbage showed Lardner's readings to be worthless and demolished Lardner's findings.<sup>150</sup> Lardner was publicly ridiculed. His humiliation was complete when Brunel produced figures showing that Lardner's measurements of atmospheric resistance were irrelevant and mistaken.<sup>151</sup> Hyman maintains that Lardner never forgave Babbage for his public drubbing.

Despite his mixed reception in scientific circles Lardner was an articulate, successful and highly effective communicator, at his best when conveying the views and knowledge of others. He was in great demand. His lectures, illustrated with drawings, working models, and specimens, many specially made, attracted large audiences. He was a consummate showman and played to packed houses for which he commanded substantial fees.<sup>152</sup> He reported that his fee for the tour of northern industrial towns in 1834 was fifty guineas for a course of twelve lectures, more than double that previously paid to any lecturer – this in prospect of the large audiences his name would attract and the opportunity for his hosts to profit from

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<sup>149</sup> Vaughan (1991), p. 116.

<sup>150</sup> *Ibid.* p. 117; *PC* p. 163.

<sup>151</sup> Vaughan (1991) p. 118; *PC*, p. 163.

<sup>152</sup> Babbage writes that Lardner invented sectional models of steam engines for educational purposes. Babbage to von Humboldt, BL Add MS 37188, f. 123. Letter is undated. Placing indicates late December 1833 or early January 1834. See also *LEC*, p. 85. For evidence of large audiences see for example, Babbage to Lardner, 13 January 1834, BL Add Ms 37188, f. 156; 23 January 1834, *Ibid.* f. 176; 16 February 1834, *ibid.*, f. 208.

the gate. He was not shy to insist on reward: he wrote to Babbage that he was put out at the meagreness of the fee though the sum was exorbitant for those hosting the series.<sup>153</sup> He was an energetic workhorse who drove himself to exhaustion.<sup>154</sup> On his northern tour he ran three lecture courses simultaneously, requiring eight public lectures per week, most of them one-and-half hours, in three towns thirty miles apart.<sup>155</sup> Money aside, he appears to have relished his work. After a successful tour he wrote that 'if I had no other satisfaction, the pleasure of being instrumental in the diffusion of knowledge to such an extent would in a great degree recompense me'.<sup>156</sup> His writing and lecturing brought him wealth and celebrity, as well as ambivalence from the scientific establishment which was partly gratified by his success in raising the public profile of science, and at the same time wary, at times alarmed, by his erratic pronouncements.

#### The Lecture Tour and the Article

The earliest letter indicating that the calculating engine had attracted Lardner's attention is dated 2 June 1830, in which Lardner wrote to Babbage that he would 'be glad . . . to get your observations on the Calculating machinery with a view to an article on that subject'.<sup>157</sup> The engines, their progress, promise and funding had been in the news for some years but little of substance had yet been made

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<sup>153</sup> Lardner to Babbage, 3 January 1834, BL Add Ms 37188, f. 140.

<sup>154</sup> Lardner to Babbage, 11 January 1834, BL Add Ms, 37188, f. 154.

<sup>155</sup> *Ibid.*; Also, 16 February 1834, *ibid.*, f. 208.

<sup>156</sup> Lardner to Babbage, 29 March 1834, BL Add Ms 37188, f. 288.

<sup>157</sup> Lardner to Babbage, 2 June 1830, BL Add MS 37185, f. 206.

public.<sup>158</sup> Babbage published practically nothing in the way of technical detail of the machine, its mechanical principles or operation. He also disdained to lecture on the subject to scientific audiences even during the years of promise before the collapse of the project in 1833.<sup>159</sup> Lardner's livelihood depended on income from lectures and writing and it would seem that he had identified the calculating engine as a topic of public and scientific interest ripe for his treatment. However, three-and-a-half years later, by the end of December 1833, the article was not yet written and Lardner was scheduled to undertake a demanding lecture tour of the major northern industrial towns.

The tour was to last nearly four months and venues included Manchester, Sheffield, Liverpool, Leeds, and Bolton.<sup>160</sup> It is clear from his reports to Babbage, written while on tour, that the itinerary was not fixed in advance but venues and bookings were negotiated with the various host institutions once he was on the road. His audiences were the memberships of Mechanics Institutes, Royal Institutions, and philosophical societies, as well as the public, and bookings depended on the cultural and technical interests of prospective audiences, demand and the popularity of the presentations already given with the syllabus selected from a prepared repertoire of lectures.<sup>161</sup> His presentations were widely

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<sup>158</sup> See Buxton Memoir, Hyman (1988), p. 289.

<sup>159</sup> His 'seminar' on the Analytical Engine in Turin in 1840 is the only recorded technical address by Babbage to a scientific audience. See *CWB*, pp. 128-133. He gave no lectures in England though he gave countless private demonstrations.

<sup>160</sup> Lardner left London on or immediately after New Year's Day 1834. See Babbage to Humboldt. BL Add Ms 37188, f. 123. The letter is undated but its placement and similarity in content to a letter to Dupin suggests that it was written on 30 or 31 December. Lardner wrote to Babbage from Manchester on 3 January 1834. *Ibid.* f. 140.. He was back in London by the end of April 1834. See Lardner to Babbage, 29 April 1834, *ibid.* f. 317. Lardner mentions his venues in correspondence with Babbage during the tour. See BL Add Ms, ff. 158, 208, and 288. Babbage mentions the proposed itinerary in his letter to Dupin. He includes Manchester, Leeds and Sheffield 'and many of our great manufacturing towns'. Babbage to Dupin, 30 December 1833, BL Add Ms, f. 117.

<sup>161</sup> His lectures in January 1834 at the Royal Institution in Liverpool, for example, were restricted to 'shareholders and their families . . . the public in general were rigorously excluded'. Lardner to

reported in newspapers. Healthy attendances attracted new bookings and this was an incentive to make the lectures as dramatic as possible, a task Lardner warmed to.<sup>162</sup>

Though Lardner's first mention to Babbage of an intention to write an article on the machine dates from 2 June 1830 it appears that little was done by way of familiarisation with the machine, or preparations for lectures on it, until the months and weeks immediately preceding the tour, three-and-a-half years later. In his letter to Humboldt on the eve of Lardner's departure on or around New Year's Day 1834 Babbage wrote:

Dr. Lardner applied to me many weeks since to know whether I would assist him with drawings etc to explain the Calculating engine upon which subject he intended to lecture. Of course I was glad that my invention should thus be explained to my countrymen by one so qualified to understand even its most abstract parts and I immediately gave him every opportunity of seeing the drawings, the tools, the parts of the Engine and the portion which is already executed and calculates. He has devoted himself most unweariedly to this object and now understands it well. He has had drawings and models made of the parts and my son [Herschel] has made for him a beautiful drawing of the part which now works; in fact he has spared neither time nor expense to master the subject.<sup>163</sup>

The impression that Lardner intended to lecture on the engines by agreement with Babbage, but had not prepared fully before travelling, is confirmed by several requests to Babbage for lecture props once the tour was under way. His letter to Babbage from Manchester refers to the 'printing model' which he presses Babbage

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Babbage, 23 January 1834, Add Ms 37188, f. 176. For reference to 'syllabus' see Lardner to Babbage, 13 January 1834, BL Add Ms 37188, f. 156.

<sup>162</sup> See for example Lardner to Babbage, 3 January 1834, BL Add Ms, 37188, f. 140. Also 16 February 1834, *ibid.*, f. 208 where Lardner refers to a subscription scheme for six lectures in Bolton in response to earlier success.

<sup>163</sup> Babbage to Humboldt, December 1833, BL Add Ms, 37188, f. 123. See ft. 160 above for comment on date.

to progress.<sup>164</sup> Clearly this had been arranged beforehand and Lardner was prodding for delivery. He also refers to the cost of presenting the Notations on canvass the preparations for which had clearly been initiated before he left London, though it is not clear whether these were for his steam engine or the calculating machine.<sup>165</sup> In his letter to Dupin in Paris just before the start of the tour Babbage wrote:

Our friend Dr. Lardner at the request of the inhabitants of Manchester, Leeds and Sheffield and many of our great manufacturing towns has undertaken to give them some lectures on the philosophy of their own pursuits. The Calculating Engine having become a matter of considerable curiosity he has undertaken to explain it to them and has spared no expense in time in order to understand it and have drawings and models.<sup>166</sup>

Although he does not specifically say that the lecture tour was at least in part a response to interest in the engines or even to direct request, and that the lectures on the Engine were a prearranged part of the programme, Babbage does nothing to discourage this impression, and his comments to Dupin appear disingenuous. From Lardner's reports to Babbage it is clear that though Lardner may have intended to lecture on the engines, the lectures were *not* part of any previously agreed programme and that he proposed to interest the hosting organisations in the subject once he was on the road. Far from the lectures being a response to avid demand, Lardner's proposals to include the lectures were first opposed by his hosts.

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<sup>164</sup> Lardner to Babbage, 3 January 1834, *ibid.* f. 140. There is a second urging for this model and also drawings in Lardner to Babbage, 13 January 1834, *ibid.* f. 156. See also, 29 March 1834, *ibid.*, f. 288.

<sup>165</sup> One of Lardner's props was Babbage's Notation applied to a steam engine. See Babbage to Humboldt, December 1833, BL Add Ms, 37188, f. 123.

<sup>166</sup> Babbage to Dupin, 30 December 1833, BL Add Ms 37188, f. 177.

On his arrival in Manchester at the start of the tour Lardner wrote to Babbage that he had spoken with the secretary of the Mechanical Institute:

I broached the subject of the machine, I was however a good deal surprised to find that he did not seem at all satisfied that it would be an attractive subject for lectures. He says that the mechanics want to hear [what] immediately concerns their business, and that they are not interested in the calculation of tables.<sup>167</sup>

However, within a fortnight he had contrived to introduce the calculating engine into a course of lectures for the Royal Institution in Manchester, this despite the reservations of his hosts and a wider perception of the difficulty of the material:

This will be our first essay on the subject. I find however that a very general impression prevails notwithstanding my assurances to the contrary that it is too scientific a subject for popular lectures. I had promised to give a course of lectures to the Sheffield Philosophical Society and I proposed to make it part of the course, but they likewise took exception to it as being too *hard* and *too scientific*.<sup>168</sup>

By the end of January Lardner wrote to Babbage that 'the ice is at length broken with respect to the machine'. The Royal Institution in Manchester had restricted lectures on the calculating engine to two out of nine on the grounds that the subject was 'difficult and too scientific', and the engine lectures were third and fourth in the batting order. To the surprise of his hosts, Lardner hijacked the first lecture, and devoted half the time to an account of the invention. Lardner, clearly excited by the success of his ruse and the impact of the lecture, reported:

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<sup>167</sup> Lardner to Babbage, 3 January 1834, BL Add Ms 37188, f. 140.

<sup>168</sup> Lardner to Babbage, 15 January 1834, BL Add Ms 37188, f. 158.

The room was crammed with the best classes in Manchester and I produced a very unequivocal and (to the committee a very unexpected) impression. In fact I have no doubt now that the lectures next week will excite all the attention that can be wished. I send by post a newspaper which contains a report of the lecture. It gives a very imperfect idea of what I said and especially, as I hit much harder touching on the apathy shewn here upon the subject compared with what is felt in every foreign seat of science and learning.<sup>169</sup>

Nonetheless, opposition to lectures on the calculating engine persisted. To Lardner's evident frustration his hosts in Liverpool preferred lectures on the steam engine, Lardner's stock in trade:

What dolts these people are! I could not beat into their skulls that the Calculating machine would have been not only the best subject but one the selection of which would have reflected the need [for] it on them. No they were afraid of it being *too scientific*. I told them, that the time would soon arrive when instead of having the subject offered to them they would be anxiously soliciting it.<sup>170</sup>

But he prevailed. He extended the material on the engine to cover its invention, tabulation, the method of differences, as well as Babbage's Mechanical Notation and complained that three lectures was insufficient to do the topic justice.<sup>171</sup> The tour was a great success, and Lardner basked in it. From Liverpool he wrote:

It is impossible to give you an idea of the state of the theatre every night – people are crowded at the door before it is opened and it is literally [?] in every night in every nook and corner an hour before the lecture begins. The crushing and squeezing and fainting of Ladies etc. etc. surpasses any thing of the kind I have ever before seen at scientific lectures.<sup>172</sup>

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<sup>169</sup> Lardner to Babbage, 23 January 1834, BL Add Ms 37188, f. 176.

<sup>170</sup> *Ibid.* Emphasis original.

<sup>171</sup> Lardner to Babbage, 16 February 1834, BL Add Ms 37188, f. 208.

<sup>172</sup> Lardner to Babbage, 23 January 1834, BL Add Ms, 37188, f. 176.



By the end of March, after three months on the road Lardner reported that over 5,000 people had attended the engine lectures 'which excited the greatest attention' and that he was scheduled to repeat the lectures in London at the Royal Institution in April and May.<sup>173</sup>

Though the lecture tour had intervened, Lardner had not forgotten the article. He wrote to Babbage on his arrival in Manchester at the start of the tour saying in a *post script* that he was 'about to sit down seriously to work at the article for the E[dinburgh] R[evue]'.<sup>174</sup> He then invited a raft of information in effect giving Babbage a briefing document, and his requests are revealing both of his state of knowledge at the time, and of his methods of interesting his readers:

You would materially assist me if you could from time to time as you may find leisure throw upon paper any interesting anecdotes which you may know respecting the history of tables and more especially any which relate to the invention of the machine. State the circumstances which first directed your attention to the subject and any other matters that may occur to your recollection. It would also be very interesting if you could state some of the difficulties moral, mechanical, mathematical which you had to overcome. Also any ingenious trials, the unsuccessful – examples of ingenious methods of *evading* difficulties – Processes such as that of centring the figures with respect to the section of the punch . . . All such matters will be highly interesting both now and in future time.<sup>175</sup>

But the pressure of the tour and of another article, that he had committed to write ahead of the piece on the calculating engine, prevented him from putting pen

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<sup>173</sup> The dates for the lectures were 30 April, 2, 8, and 15 May with an additional lecture (on the Notation) still to be scheduled. Lardner to Babbage, 29 March 1834, *ibid.* f. 288.

<sup>174</sup> Lardner to Babbage, 3 January 1834, BL Add Ms 37188, f. 140.

<sup>175</sup> Lardner to Babbage, 3 January 1834, BL Add Ms 37188, f. 140.

to paper, and towards the end of the tour Lardner wrote to Babbage that the article would be attacked only once he was back in London.<sup>176</sup>

From the end of April 1834 until the middle of July Lardner drafted the sixty-five page article, finding time in an otherwise busy schedule that included visits to Portsmouth to see block-making machinery, at least one trip to Paris and the four or five lectures at the Royal Society on the calculating engines.<sup>177</sup> During this period he pressed Babbage for more material. In this he shows a strong interest in tabular errors and the investment in the production of existing tables. He wrote to Babbage:

I wish I could find some short and forcible illustration of the quantity of labor mental and bodily which has been expended in computing and *composing* all the tables that have been heretofore computed. I mean of course a first approximation such as the number of computers working ten hours a day for a given time which they needed to employ. Other things appear puerile but I assure you they *tell*.<sup>178</sup>

He requested from Babbage copies of specific published tables including the most recent Nautical Almanac and the Astronomical Society reports on it and he invites Babbage's assistance on a list of topics he proposes to cover at length:

other attempts at calculating mechanism  
how far [these are] *general* – where they failed  
a catalogue of tables if any such exist  
an approximate estimate of the labor and expense which have been  
already lavished on the calculation and printing of tables  
as extensive as possible set of examples of detected errors.<sup>179</sup>

<sup>176</sup> Lardner to Babbage, 29 March 1834, BL Add Ms 37188, f. 288. His first letter to Babbage after his return to London is dated 29 April 1834, See BL Add Ms 37188, f. 317.

<sup>177</sup> For references to the lectures see Lardner to Babbage, BL Add Ms 37188, ff. 288, 333 (n.d.), 350 (n.d.). For references to visits see Lardner to Babbage, 12 July 1834, BL Add Ms 37188, f. 447.

<sup>178</sup> Lardner to Babbage, BL Add Ms 37188, f. 411. Emphasis original.

<sup>179</sup> See Lardner to Babbage, BL Add Ms 37188, ff. 333, 335 (n.d.).

On one topic Babbage sought the assistance of Capt. W. S. Stratford, a naval astronomer and the first serving secretary of the Astronomical Society, from whom he requested information on his experiences of errors in tables, particularly those committed by independent computers.<sup>180</sup> As the deadline approached Lardner pressed Babbage for meetings to go over the drafts all in something of a flurry as time to publication began to run out.<sup>181</sup> Lardner was evidently concerned that Babbage was consulted fully and that he had sight of the proofs before they were finally sent off to the printers.<sup>182</sup>

The events described suggest the nature and respective roles Babbage and Lardner played in the authorship of the article. It is clear that the technical material on principles, working and history of the invention was entirely supplied by Babbage, but that it was Lardner who took the leading role in determining the content through the selection, framing and treatment of the material, so as to maximise its appeal to readers. That the article was written after the highly successful tour also suggests that its content was shaped by the needs of the lecture hall on which Lardner's success and livelihood depended. Though the article does mention the new implications of the engines to mathematics, the reference is passing and brief, and this could reflect the perception of his tour hosts that the material was difficult and had limited appeal to the more practical interests of his audiences.<sup>183</sup> The overwhelming emphasis on errors as the 'problem' and

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<sup>180</sup> Stratford to Babbage, 12 May 1834, BL Add Ms 37188, f. 337. Because of the publication deadline Stratford sent the material directly to Lardner at Babbage's request. *Ibid.*

<sup>181</sup> Lardner to Babbage, BL Add Ms 37188, f. 413, 415, 417, 418, 420 (all undated).

<sup>182</sup> The covering letter accompanying the uncorrected proofs is dated 12 July 1834. See BL Add Ms 37188, f. 447.

<sup>183</sup> Lardner (1834), *Works*, Vol. 2, pp. 168-9.

the framing of engines as the 'solution' can therefore be seen as an artefact of the narrative needs of oratory.

Given that Babbage had the opportunity to comment and suggest revisions, and that the relationship throughout was one of supportive collaboration without any evidence of rivalry or tension, it is likely that Babbage approved or at least accepted Lardner's portrayal. However, between the first suggestion for an article in 1830 and Lardner's tour, the fortunes of the engine project had drastically changed and if Babbage had any reservations about the article, the new circumstances would have given him reason to put them aside.

The circumstance that altered the value of the lectures and the article to Babbage was the collapse in March 1833 of the project to construct the Engine. After over a decade of design, manufacture and substantial public expense there was little to show except some 12,000 finely made loose parts in Clement's workshop. To prop up the flagging credibility of the project the engineer Joseph Clement delivered, on Babbage's instruction, the small demonstration piece, about one seventh of the whole, assembled from some of the already completed parts. The machine which Babbage later referred to this as the 'finished portion of the unfinished Difference Engine No. 1' was demonstrated with dramatic effect at Babbage's celebrated Saturday soirées and became an attraction for savants, dignitaries, royalty, foreign VIPs, colleagues and friends whom Babbage charmed and enthralled with its workings.<sup>184</sup> The demonstration of his theory of miracles, viewed as programmed discontinuities in nature rather than violations of natural law, provided a powerful new explanatory model for the leading geologists, pre-Darwinian evolutionists, and theologians who were struggling with the seemingly intractable evidence of discontinuities in nature – sudden and extreme geophysical

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<sup>184</sup> The source of the quoted phrase see *Passages*, p. 150. For the historical significance of the machine see *CWB*, pp. 82-5. For description of Babbage's demonstrations see *CWB*, pp. 77-82.

trauma, and the appearance of new species after millennia of apparent stasis.<sup>185</sup>

The physical evidence of a machine capable of exhibiting behaviour that was discontinuous, and at the same time rule based, provided a compelling demonstration that laws did not necessarily entail uniformity, and that notions of rational order and unexpected events could be reconciled.<sup>186</sup> The physical evidence of the machine, and its cultural impact on the debates of the day, did much to restore the credibility of the engine project which till then had little to show after years of delay and expense.

But the boost to the reputation of the project was short-lived. A dispute had arisen between Babbage and Clement over compensation for moving the works from Clement's premises South of the river to a specially built fire-proof workshop near Babbage's house in Dorset Street.<sup>187</sup> Both were convinced of the justice of their own position. The dispute spiralled into impasse. Clement put his men on notice. Babbage was unmoved. In March 1833 Clement fired the men working on the calculating engine (Joseph Whitworth included) and downed tools.<sup>188</sup> The breach was final and work on the Engine was never resumed.

Babbage was left stranded without the specially made tools and jigs (which by custom and practice remained Clement's property), the 12,000 parts already made, and more importantly, his drawings.<sup>189</sup> The settlement with Clement was protracted and gruelling and Babbage wrote that he was 'almost worn out with

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<sup>185</sup> *Ibid.*, p. 75-6.

<sup>186</sup> *Ibid.*, p. 79. A major debate of the time was that between the uniformitarians and the catastrophists. See Beggren & Van Couvering (1984). Also Swade (2003, DNB, in press).

<sup>187</sup> For an account of the complex exchanges between Clement, Babbage and the Treasury see *LEC*, pp. 77-85. For summary account see *CWB*, pp. 66-7.

<sup>188</sup> H. P. Babbage (1889), p. 340.

<sup>189</sup> For reference to the protocols of ownership see *PC*, p. 125.

annoyance and disgust at the whole affair'.<sup>190</sup> The process involved mistrustful checking of inventory and the use of intermediaries in an ill-tempered three-way negotiation between Clement, Babbage and the Treasury.<sup>191</sup> The finished parts and drawings were returned to Babbage in July 1834 and the final payment to Clement was made in August 1834.<sup>192</sup>

It is evident from this that at the time of Lardner's interest in the engines for his tour the political fortunes of the Engine project had plummeted from a delayed, but just credibly viable enterprise, to complete collapse. Lardner's reputation and abilities as a communicator, publicist and disseminator of scientific knowledge represented an opportunity for Babbage to revive the fortunes of the project by increasing its public and scientific profile, advertising the supposed benefits of the machines, and defending his position in the messy dispute with Clement. While there is no evidence that Babbage objected to the content of Lardner's article, if he had reservations there were good reasons to defer to Lardner's PR skills in simplifying the appeal of the project by emphasising the elimination of errors as the essential utility of the machines.

Lardner was constantly on the lookout for new material for his lectures and this provides at least one motive for his interest. However, there are suggestions that he sought a more specific and far-reaching arrangement: that in exchange for his promotion of Babbage's interests both in England and on the Continent, Babbage would in turn use his influence to secure government support for dissemination of scientific knowledge, and thereby increase the market for Lardner's wares. The most explicit evidence of this is provided by Lardner himself

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<sup>190</sup> Babbage to J. Stewart, 16 July 1834. BM Add Ms 37188, f. 450. Quoted in *LEC*, p. 85; *PC*, p. 132.

<sup>191</sup> *CWB*, p. 67.

<sup>192</sup> Roberts (1990), p. 10.

in a letter to Babbage, written on his arrival in Manchester at the start of the lecture tour and after his negotiations for fees with one of the hosting institutions:

I am rather dispirited at what I hear of the financial concerns of this and other Mechanics Institutes. This is the richest of all in this district and yet it appears that the sum they have undertaken to pay me (50 g[uinea]s for 12 lectures) is more than twice the largest sum they ever before have paid to any lecturer . . . Now if this be the case with so very inadequate remuneration as the present, what hope can there be for these societies unless government steps in to their assistance. They have gone to the expense of erecting an additional gallery for my lectures to enable them the better to recover the expenses they will incur. If this be the case with the Manchester institute what must the situation of the others which are struggling to offer me the compensation of visiting them. It is really *too bad* in a country like this, where the heart's blood of the manufacturing population is sucked to pamper the prostitutes of ex-ministers to see that population craving for instruction, and the government, (paid as it is), standing indifferent to the demand.

You could do nothing which would be at once attended with so much good to the public and so much popularity to yourself as to be in some way instrumental in removing this intolerable state of things and if you should do so, you may rely on it that if I live and have brains under my skull, a tongue in my mouth and a pen in my hand your candle shall not be hid under a bushel.<sup>193</sup>

Earlier in the same letter Lardner requested a large amount of material on the engine for the article.<sup>194</sup> By way of apology and incentive he adds:

This is an irksome task perhaps for you but believe me the opportunity which I now have of acting upon the public mind, not only in England but throughout Europe, is one well worth taking advantage of even at a little personal trouble.<sup>195</sup>

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<sup>193</sup> Lardner to Babbage, 3 January 1834, BL Add Ms 37188, f. 140. Emphasis original.

<sup>194</sup> See above p. 170.

<sup>195</sup> Lardner to Babbage, 3 January 1834, BL Add Ms 37188, f. 140.

The reference to Europe perhaps provides an additional clue. A few days before, Babbage had written to Alexander Humboldt in Berlin, and also Baron le Dupin in Paris, recommending Lardner's talents and industry, and soliciting invitations for lectures on the calculating engine and the Notation, to be delivered by Lardner in their respective capital cities.<sup>196</sup> These two letters, and the reference in Lardner's letter above to Europe as an arena of influence, suggests that he and Babbage had discussed an arrangement whereby Babbage would secure the engagements as a *quid pro quo* for Lardner spreading the word. In the context of the collapsed fortunes of the Engine project it is possible that Babbage was now looking to the Continent for support (as he was to do again in 1841) and the arrangement struck with Lardner was one of reciprocal self-interest.<sup>197</sup>

### Summary

A close reading of Babbage's earliest writing shows that eliminating the risk of errors in tables features less strongly as the purpose and motive for the engines than historical accounts have so far suggested. While the elimination of errors in the production of printed mathematical tables features in his expectations of the engines, it does so alongside several other expectations. While the issue of errors was the original stimulus for mechanised calculation, for Babbage the engines represented a new technology of mathematics with significant consequences for mathematical analysis, especially for the role of computation as a systematic

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<sup>196</sup> Babbage to Dupin, 30 December 1833, BL Add Ms 37188, f. 117; to Humboldt (n.d.), *ibid.*, f. 123.

<sup>197</sup> For reference to the other occasion on which Babbage solicited foreign support see *CWB*, p. 131; *LEC*, p. 174-5.



method of solution, ideas that were historically unprecedented and have received little attention.

The context of Babbage's first engagement with tables was an outcome of pressure for expanded tables of stars and tables of newly discovered asteroids from the community of astronomers. Babbage's choice of 'steam' as the metaphorical agent of salvation can be seen as signalling the role of mechanised production as a solution to the problem of supply. Following trials using his first experimental engine completed in Spring 1822, Babbage argued that the engine would have the requisite properties of speed and generality. It is clear that Babbage did not take for granted that mechanisation *per se* guaranteed infallibility. From the outset he incorporated self-correcting and security devices that would ensure the integrity of results.

One part of the advocacy for the engines was concerned with remedying known deficiencies, namely, deficiencies of supply and of correctness. The other part of the case, revealed in his early papers, was his speculations on new implications of the engines. Babbage explored the idea of computation as a systematic method of solution of equations, and this was entirely new territory. Using his notional engine as a mental model he explicitly described the repeated cycling of the machine as a systematic procedure for finding the roots of equations for which there was no known analytical solution. In this respect the engines represent a new technology of mathematics. A second feature of the engine was its potential to suggest new series for which there was no analytical law, and to generate successive terms of such series *ad infinitum*. These ideas were not developed beyond initial speculation. Babbage predicted the eventual dependence on machines for scientific computation, and foresaw new branches of numerical analysis that would be required to optimise the efficiency of machine computation.

Using de Prony's labour-intensive model of manual computation Babbage argued for the labour-saving benefits of his invention. It is clear that profit did not feature in his own motives, and he consistently dismissed the idea that the capital investment in an engine could be repaid by the sale of tables as product, though there are indications that he might have altered his views, if only briefly, in 1842. In the early 1850s, when he had finally surrendered any prospect of completing an engine, he repeatedly sought acknowledgement for the spin-off benefits to manufacturing of the failed engine project.

The publication that has had a defining influence on the role of errors in published tables as the primary purpose of the engines is Lardner's article on Babbage's Difference Engine, published in 1834. Lardner's analysis of errata in printed tables convincingly supports the thesis that manual methods of producing tables were unreliable. However, his analysis is inconclusive with respect to whether the process of progressive improvement through correction in use produced acceptable levels of reliability.

The circumstances of the publication suggest that this framing of tabular errors as the 'problem' with engines as the 'solution' was a response to the need to dramatise the engine venture for lecture hall audiences. The evidence suggests that the reduction of motive and benefit, to the elimination of errors, was a simplifying device and an artefact of the need to appeal to non-specialists. The analysis further suggests that the article, which was written immediately after Lardner's highly successful lecture tour of Northern industrial towns, was for the most part a written version of a presentational formula developed for public consumption, proven on stages and podiums during his tour.

There is no evidence to suggest that Babbage objected to Lardner's portrayal of eliminating errors as the essential purpose of the engines. The article,

and the Lardner's lectures in London and the North, coincided with the collapse of the construction project, and any reservations Babbage might have had about Lardner's portrayal would have been waived in exchange for Lardner's promotional showmanship and his gifts as a publicist. There is also evidence that Babbage and Lardner had an agreement to serve each others' interests.

## Chapter 4: Airy and Babbage's Difference Engine No. 1

*I think it likely that he lives in a sort of dream as to its utility*

– George Biddell Airy, 1842.

*I am determined to put down Sir James South, and if you and other respectable men will give him your support, I will put you down.*

– Richard Sheepshanks, 1831.

### Introduction

George Biddell Airy (1801-1892), Astronomer Royal, was consulted by Government on at least three occasions to advise on the utility of calculating engines and on the wisdom of financial support for them. As Astronomer Royal from 1835 till 1881 he occupied the highest office in civil science, and as *de facto* scientific advisor to Government his views had a determining influence on the fate of Babbage's calculating engines and on the Swedish difference engines by Georg and Edvard Scheutz.<sup>1</sup> He was also petitioned by inventors of calculating devices hopeful of official endorsement, and his views had a defining influence on whether or not these were pursued. Except for one egregious episode, Airy consistently rejected arguments advocating the utility of automatic calculating machines – this in contrast to the uniformly positive arguments of the engine advocates.

Airy was a relative late-comer to the fray. His first formal involvement as advisor to Government on the utility of Babbage's engine occurred in September

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<sup>1</sup> For a list of Astronomers Royal from 1675 to 2000 see Illingworth (2000), p. 30.

1842, some twenty years after the start of Babbage's efforts to construct his first engine, and nearly ten years after Clement downed tools. By 1842 the physical construction of Difference Engine No. 1 had been dormant for over a decade, but the project was not yet dead: the complex commitments between Babbage and a succession of new administrations had not yet been fully resolved. Airy played a crucial role in the final *denouement*.

Airy famously pronounced Babbage's engines to be 'worthless' and most commentators use this single damning pronouncement to reinforce his image as that of an unimaginative bureaucrat on whom the promise of machines was lost.<sup>2</sup> There were no public encounters or published disputes between Airy and the engine advocates and his views on calculating engines have been almost completely neglected in the literature.

This chapter describes the circumstances in which Airy was invited to give his views on Babbage's Difference Engine No. 1, and examines the stated grounds for Airy's scepticism. It also discusses Babbage's public allegations that Airy was biased against the engines through personal malice and that his advice to the Government was distorted by personal animosity. The sources include hitherto unused archival correspondence between Airy and his government masters.

#### Peel Seeks Advice

To understand the circumstances in which Airy was first approached in 1842 it is necessary to trace the history of the Engine from the time of the final settlement

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<sup>2</sup> For full quote and discussion see below p. 214.

with Joseph Clement.<sup>3</sup> Once a settlement had been reached, Clement returned the drawings, experimental assemblies and completed parts.<sup>4</sup> With this material back in his possession Babbage returned to some of his early ideas that had lain dormant during the years of construction. His re-examination started in July or August 1834 when he began to generalise the principle of feeding back multiples of numbers from one column to columns elsewhere in the machine using the 'beautiful fragment' assembled by Clement in 1832.<sup>5</sup> By a series of undocumented steps he arrived at a circular layout columns that would allow the output being fed continuously back to the input.<sup>6</sup> He referred to this folding the machine back on itself as 'the engine eating its own tail'.<sup>7</sup> He also refers to the self-generating properties of the engine as 'a locomotive that lays down its own railway'.<sup>8</sup> He later recalled:

The circular arrangement of the axes of the Difference Engine round large central wheels led to the most extended prospects. The whole of arithmetic now appeared within the grasp of mechanism. A vague glimpse even of an Analytical Engine at length opened out, and I pursued with enthusiasm the shadowy vision.<sup>9</sup>

The pursuit of the shadowy vision unfolded between summer 1834 and summer 1836 by which time Babbage had established the main principles of the new

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<sup>3</sup> For circumstances leading to Clement stopping work see *LEC*, p. 80.

<sup>4</sup> The materials were returned to Babbage on 15 July 1834. See *ibid.*, pp. 116-7.

<sup>5</sup> The relevant folios in Babbage's scribbling books are undated, hence the uncertainty in the date on which Babbage re-examination started. See *LEC*, p. 117. The quoted phrase is Henry Prevost's, Babbage's son. See H. P. Babbage (1889), Preface, p. I

<sup>6</sup> For an account to the evolution of Babbage's ideas on the Analytical Engine see *LEC*, pp. 116-21.

<sup>7</sup> *Ibid.*, p. 119.

<sup>8</sup> *Ibid.*, p. 106.

<sup>9</sup> *Passages*, p. 112. The new machine was not known as the 'Analytical Engine' until 1840 or 1841.

machine.<sup>10</sup> The period was one of intensive activity and Babbage was completely absorbed in the new possibilities. While preoccupied with these new developments, still unclear where they were leading, Babbage conducted sporadic exchanges with successive government administrations about the future of the Difference Engine. In December 1834 Babbage wrote at length to the Duke of Wellington, the new Tory PM, requesting a 'decision'.<sup>11</sup> Babbage complained of official neglect, false allegations of financial self-interest, personal and professional sacrifices, and expressed indignation at the lack of acknowledgement for his accomplishments. Having discharged his grievances he offered Wellington four options: that Clement be re-engaged to complete the machine; that a replacement for Clement be found to work under Babbage's direction; that Government find a replacement for Babbage himself; or that the project finally be abandoned.<sup>12</sup> Babbage then committed what can be seen as an honourable but fatal strategic blunder. He told Wellington that he was working on a new machine. Concealment would be morally wrong, he says, and he felt honour bound to inform the Government of these new circumstances.

His references to the 'new engine' are confusing and apparently self-contradictory. He refers to 'a totally new engine possessing much more extensive powers', maintains that it is not intended to supersede the old, but that none of the devices and contrivances of the new machine are used in the old.<sup>13</sup> It is a puzzle to untangle these assertions, and separating the engines' logical function from its mechanical implementation is only a partial rescue. What Babbage does not clarify

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<sup>10</sup> See *LEC*, p. 121.

<sup>11</sup> Babbage to Wellington, 23 December 1834, BL Add Ms 37188, f. 525. *Works*, Vol. 3, p. 8.

<sup>12</sup> *Works*, Vol. 3, p. 5. See *LEC*, p. 92.

<sup>13</sup> *Ibid*, p. 6.

is how the new engine might affect Wellington's choice of which of the four courses of action Babbage had suggested as possible options. At the time of the letter Babbage was only six months into the heady design of the Analytical Engine and not yet in a position to know where the new machine would lead. The apparent confusion in his description of the new engine reflects incipient, evolving but yet unclarified possibilities. He makes no specific request for support but concludes the letter with a request for a decision.<sup>14</sup> Wellington can be forgiven for not knowing what it is Babbage wished him to decide: proceed with a new machine with vague, unspecified and contradictory benefits, or take one of four options presented earlier in the letter.

Wellington's administration was short-lived and Babbage did not receive a reply.<sup>15</sup> In April 1835 Babbage sent a copy of his statement directly to Peel, again requesting a decision but without any further clarification of the question being asked.<sup>16</sup> Three weeks later Peel's government was replaced by Melbourne's and Babbage renewed his request by sending to the new Whig leader a copy of his letter to Wellington this time intimating obscurely that it would be 'very difficult if not quite impossible' to complete the old machine.<sup>17</sup> While waiting for a reply the project came under renewed public pressure: the Civil Contingencies fund which had bankrolled the Engine project was debated in Parliament. Several ventures, Babbage's included, were targeted for criticism as 'unprincipled waste and squandering public money'.<sup>18</sup> There were implications of financial impropriety and

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<sup>14</sup> *Works*, Vol. 3, p. 8.

<sup>15</sup> Wellington's interim administration lasted from 17 November to 10 December 1834.

<sup>16</sup> Babbage to Peel, 7 April 1835, BL Add Ms 37189, f. 74. See *LEC*, p. 94.

<sup>17</sup> Babbage to Melbourne, 4 May 1835, BL Add Ms 37189, f. 89.

<sup>18</sup> *LEC*, p. 94.



of Babbage personally profiting, a highly sensitive issue which never failed to ignite Babbage's protests. The option of abandoning the old engine was now politically sensitive for both parties.

In January 1836 Babbage finally received a reply to his letter to Wellington, over a year after its first submission. The letter came from Spring-Rice, Chancellor of the Exchequer in Melbourne's second administration.<sup>19</sup> From this it is clear that the letter to Wellington had indeed been taken as a direct request for financial support for the new machine.<sup>20</sup> By this time Babbage had a clearer idea of the powers of the Analytical Engine and his reply to the Chancellor dated 2 February 1836 compounded the confusion by reversing his earlier assertion that the new engine would not supersede the old:

it [the new engine] performs all those calculations which were peculiar to the old Engine both in less time and to a greater extent – in fact *it completely supersedes the old Engine* . . . I believe any practical makers of machinery who would bestow sufficient time on the enquiry would arrive at the conclusion that it would be more economical to construct an engine on the new principles than to finish the one already partly executed and I am quite sure that one so constructed would be a much better instrument.<sup>21</sup>

Having finally grasped the nettle that the first Difference Engine was obsolete Babbage still makes no specific recommendation for its abandonment. He dodges the issue and declares himself to be no more than the honest messenger providing government with information that might bear on their decision:

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<sup>19</sup> *CDBN*.

<sup>20</sup> Babbage to Spring-Rice, 14 January 1836, BL Add Ms 37189, f. 273. *LEC*, p. 97. In a marginal annotation Babbage denied that his letter was a bid for funds. Emphasis original.

<sup>21</sup> Babbage to Spring-Rice, 2 February 1826, BL Add Ms 37189, f. 292. The date of this letter is given (apparently incorrectly) as 20 February in the statement prepared by Sir H. Nicolas reprinted in *Passages*. See *Passages*, p. 89.

In making this report I wish distinctly to state that I do not entertain the slightest doubt of the success of the first Engine nor do I intend it as any application to finish the one or to construct the other: but I make it from a conviction that the information it contains ought to be communicated to those who must decide the question relative to the Calculating Machine.<sup>22</sup>

The authorities could scarcely be blamed for not knowing quite what to make of Babbage's insistent but oblique requests. There was no reply and Babbage let the matter rest for nearly two and a half years. In July 1838 he wrote to Melbourne in apparent desperation, 'I now appeal to your Lordship for the last time to ask for no favor but to ask for that which it an injustice to withhold from me – a decision'.<sup>23</sup> The Chancellor, Spring-Rice, replied in a well-meaning way saying that he was unclear what question Babbage wished him to decide and courteously asked Babbage to make his wishes clear – whether he wished to finish the old or commence the new.<sup>24</sup> Babbage replied on 21 October 1838 clearly stating that:

The question which I wish to have settled is whether the government require me to superintend the completion of the calculating Engine . . . according to the original plan and principles, or whether they intend to discontinue altogether'.<sup>25</sup>

But Babbage also puts Spring-Rice in an impossible position:

if it were still desirable to have an engine possessing the same powers as that already partly made, it would cost less money to throw aside the old work

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<sup>22</sup> Babbage to Spring-Rice, 2 February 1836, BL Add Ms 37189, f. 292.

<sup>23</sup> Babbage to Melbourne, (n.d. but Spring-Rice's refers to Babbage's letter of 26 July in his reply), 26 July 1838, BL Add Ms 37190, f. 496.

<sup>24</sup> Spring-Rice to Babbage, 16 August 1838, BL Add Ms 37190, f. 518.

<sup>25</sup> Babbage to Spring-Rice, 21 October 1838, BL Add Ms 37191, f. 14.

and adopt the new principles as the basis for the engine. . . but I expressly stated that I did not intend it as an application to construct such a machine.'<sup>26</sup>

Spring-Rice must have shrugged in exasperation. It seems as if Babbage was saying 'decide whether you want to waste money completing an obsolete machine'. It is possible that Babbage, principled to a fault, thought that recommending that the old be scrapped for the new would be to renege on what he took as his original obligation to build an engine and that only the government could properly release him. It seems that he was willing to fulfil the original obligation but at the same time felt honour-bound to inform government of new circumstances that might affect the decision. The letter to Spring-Rice remained unanswered. The stalemate lasted three years with each party apparently frustrated by the other's failure to make known its specific wishes.

The political volatility of the times and the musical chairs of changing administrations in Whitehall were certainly factors in the desultoriness of the exchanges.<sup>27</sup> This accounts for delayed responses and stop-start nature of the responses from Whitehall but not for the lack of clarity as to what was at issue. Babbage repeatedly declared that his purpose throughout was to clarify the nature of the outstanding obligations between the two parties. However, his letters appear obscure and in some respects self-contradictory. Sense can be made of this difficult material if the exchanges are married up with the conceptual development of the Analytical Engine and seen as part of Babbage's complex struggle to clarify his own expectations and wishes during the time that this work continuously offered new logical and technical vistas. Babbage's world was a turmoil of new

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<sup>26</sup> *Ibid.*

<sup>27</sup> Between July 1834 and April 1835 there were four different administrations, two Whig and two Tory.

prospect and possibility. The Treasury's world was fixed and pragmatically utilitarian, and its officers can be excused for not knowing quite what to make of Babbage's insistent but opaque representations.

With the second Peel administration installed at the end of August 1841 Babbage again took the initiative. On 22 January 1842 he wrote to Peel to clarify whether the Government regarded his brief to complete the engine as ended.<sup>28</sup> Peel was unable to give Babbage any attention because of the pressure of work before the opening of parliament.<sup>29</sup> Babbage wrote twice more, without reply.<sup>30</sup> At the end of the summer session of Parliament Babbage enlisted the assistance of William Follett to press the PM for an answer.<sup>31</sup> Follett was solicitor-general under Peel during his first administration (December 1834 - April 1835), and again in 1841.<sup>32</sup> He was part of Babbage's undergraduate set at Cambridge, given to playing sixpenny whist through the night.<sup>33</sup> Macready described him as 'one of the ablest advocates and acutest lawyers of the nineteenth century; but for his premature death he would undoubtedly have been the next Tory Chancellor'.<sup>34</sup> Follett was at Drayton, Peel's country seat, with ample opportunity to collar Peel. But Babbage's letter was not forwarded and Follett did not reply until 20 October, when he offered to prod Peel in the event that Babbage had not yet had an

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<sup>28</sup> Babbage to Peel, 22 January 1842. BL Add Ms 37192, f. 19. See also, *CP*, p. 190; *LEC*, p. 99.

<sup>29</sup> George Clerk to Babbage, 29 January 1842. BL Add Ms 37192, f. 29.

<sup>30</sup> Babbage to Clerk, 4 February 1842. BL Add Ms 37192, f. 37; Babbage to Clerk, 12 August 1842, *ibid.*, f. 128; Babbage to Peel, 8 October 1842. *ibid.*, f. 147.

<sup>31</sup> Babbage to Follett, 8 October 1842. BL Add Ms 37192, f. 146. (Goulburn to Babbage, 3 November 1842. BL Add Ms 37192, f. 172).

<sup>32</sup> *CDNB*.

<sup>33</sup> *Passages*, p. 36.

<sup>34</sup> Toynbee (1912). See entry for 24 April 1843, Vol. 2, p. 205.

answer.<sup>35</sup> Follett was as good as his word and nudged the Chancellor, Henry Goulburn, to hurry things along.<sup>36</sup> This was an unpropitious time to be pressuring Peel for attention: he was in the thick of the August Corn Law riots and the country was in a turmoil. He wrote to his wife of 'great rioting and confusion' and that he was 'fagged to death' with exhaustion from his parliamentary duties.<sup>37</sup> Babbage wrote again on October 8, again without reply.<sup>38</sup>

The unanswered letters were apparently not without effect. Peel began to seek advice. On 31 August 1842 he wrote to William Buckland, a leading geologist to whom Peel turned from time to time for scientific advice.<sup>39</sup> Buckland attended Peel's 'scientific weekends' at Drayton on several occasions as one of a small coterie of scientists that Peel cultivated and from which he sought advice.<sup>40</sup> He was canon of Christ Church, Oxford, and, on Peel's recommendation, was appointed Dean of Westminster. He upheld the Mosaic account of the flood and found his views on natural theology increasingly difficult to reconcile with scientific rationalism.<sup>41</sup> In his letter to Buckland, Peel's scepticism is undisguised:

What shall we do to get rid of Mr. Babbage and his calculating machine? I am perfectly convinced that every thousand pound we should spend upon it hereafter would be throwing good money after bad. It has cost £17,000 I believe and I am told that it would cost □£14, or 15,000 more to complete it. Surely if completed it would be worthless so far as science is concerned?

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<sup>35</sup> Follett to Babbage, 20 October 1842. BL Add Ms 37192, f. 162.

<sup>36</sup> Goulburn to Babbage, 3 November 1842. BL Add Ms 37192, f. 172.

<sup>37</sup> Peel to his wife, 11 August 1842. See Gash (1986), p. 338.

<sup>38</sup> Babbage to Peel, 8 October 1842. BL Add Ms 37192, f. 147.

<sup>39</sup> *PC*, p. 148-9.

<sup>40</sup> For reference to Buckland's attendance at Peel's Drayton gatherings see Gash (1986), pp. 232, 536, 676-9. Lyon Playfair was prominent and favoured in the group. For reference to Peel's 'scientific weekends' see MacLeod ([1971], 1996).

<sup>41</sup> *CDNB*. Also *PC*, p. 149.

What do men really competent to judge say *in private* . . . It will be in my opinion a very costly toy to complete and keep in repair. If it would now calculate the quantum of benefit to be derived to science it would tender the only service I ever expect to derive from it.<sup>42</sup>

Peel goes on to compare the Government's entanglement with the engine project with a current financial nightmare, the construction of the Caledonian Canal. This was haemorrhaging money and Peel was looking for ways of extricating the Government from escalating costs and interminably deferred completion. He confides to Buckland:

I fear a reference to the Royal Society, and yet I would like to have some authority for treating this calculating machine as I should like to treat the Caledonian Canal and would have treated it but that I was told it would cost £40,000 to unravel the web that we have spent so many hundred thousand pounds in weaving.<sup>43</sup>

For Peel the engine represented further risk of open-ended expense and he appears less concerned with the engine's public utility, or with the grand enterprise of science, than with finding a politically defensible excuse to cut his losses.

The Royal Society as a source of official advice was a natural port of call. Founded by charter in 1662, it was a private society, financed by contributions from members and Fellows, who elected their own officers and Council.<sup>44</sup> Though its presidents and Fellows were influential figures, many of whom had close ties to Whitehall, as a body, formally independent of government, it had the constitutional

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<sup>42</sup> Peel to Buckland, 31 August 1842. BL Add Ms 40514, ff. 223-4. Short excerpts from this letter are quoted in Turvey (1991), p. 167, and *PC*, p. 190. The transcriptions in these two sources differ. Emphasis original.

<sup>43</sup> *Ibid.*

<sup>44</sup> See Hall (1984), p. ix. The Royal Society was the first of the chartered societies. *Ibid.*, p. 162.

trappings of neutrality. Peel was particularly well disposed towards the Society, especially through his friendship with Humphry Davy, who was President from 1820-7.<sup>45</sup> The Admiralty had a long tradition of close co-operation with the Society and used its membership as a source of technical and scientific expertise. In the 1820s the Admiralty made repeated requests for assistance on a variety of navigational, maritime and astronomical problems.<sup>46</sup> The range of problems referred to the Society by government broadened: advice was sought on the granite to be used for the rebuilding of London Bridge, the removal of contagion from imported silks and cottons, gasworks, and exploration, amongst many other issues.<sup>47</sup> Until the Astronomical Society received its Royal Charter in 1831 the Royal Society was the only body recognised by government as representing British Science.<sup>48</sup>

However, if Peel was seeking to rid himself of Babbage, his aversion to fourth referral to the Royal Society was well-founded. In 1823 while serving in Liverpool's administration, he had the question of Babbage's engine referred to the Society for an opinion on the 'merits and utility of this invention' – this on the suggestion of Davies Gilbert, MP for Bodmin, and his friend and adviser John Wilson Croker,

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<sup>45</sup> See Gleason (1991), pp. 90, 92. For background and detail of the role of government grants in support of research, and an account of Peel's active patronage of science see MacLeod ([1971], 1996). Also Poole and Andrews (1972), pp. 5-9; Gummatt (1980), pp. 20-2. The amount of the grants placed with the Society was initially £1,000 per annum and later increased to £4,000. Between 1849-1914 grants assisted 938 scientists in 2,3126 projects (See MacLeod, *ibid.*, p. 324). In comparison Treasury expenditure of £17,500 on Babbage's Engine represented a massive investment in a single scientific/engineering project. By way of comparison the cost of the *John Bull* locomotive built by Robert Stephenson & Co. in 1831 was £784 7s. See *GoF*, p. 60. Large sums were made available for big research projects such as the Ordnance Geological Survey of Britain (established in 1835 under Henry De La Beche), the Board of Trade's investigations into lighthouses, and research in public health. For a study of the Geological Survey of Britain see Morrell (1988).

<sup>46</sup> The PRS was *ex officio* a member of the Board of Longitude. See Hall (1984), p. 13; Gleason (1991), pp. 90-1. Subjects included the use of coal tar instead of pitch on ships timbers, lightning conductors, and the securing of copper sheets to the underside of ships. See Gleason (1991), p. 93.

<sup>47</sup> See Hall (1984), p. 31.

<sup>48</sup> *Ibid.*, p. 43.

then Secretary to the Admiralty.<sup>49</sup> It was on the strength of the recommendation of this referral that the Treasury began bank-rolling the construction. The first payment (£1,500), one of many that followed, was made by the Treasury from the Civil Contingency Fund in August 1823.<sup>50</sup> The Treasury consulted the Royal Society for the second time when Babbage applied late in 1828 for further funds to settle an outstanding liability of some £4,500.<sup>51</sup> The Babbage Engine Committee, this time chaired by John Herschel, reported favourably on feasibility, progress and prospective utility, and further advances were made. Finally, when Babbage applied to the Treasury in 1830 for funds to construct fireproof workshops in the grounds of his house in Dorset Street, the Royal Society, duly consulted, again found in Babbage's favour and the construction costs were borne by the Government.

The support of the Committee of 1830 appears to have been unaffected by Babbage's savage attack on the Society in his explosively critical *Decline* published in that year. Expulsion from the Society was considered at a meeting of the Council on 10 June of that year as 'defamation' was a violation of the statutes.<sup>52</sup> However, Davies Gilbert, then President and an early supporter of Babbage and his engine project, prevailed, arguing that though Babbage was in breach of the statutes he was unwilling to pursue the extremity of expulsion 'in consideration of the past services which Mr. Babbage had rendered to science'. Censure was waived and the matter was dropped, though the rumblings continued

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<sup>49</sup> For quotation cited see *Passages*, p. 69. Also *LEC*, p. 38. See Peel to Croker, 8 March 1823. Quoted in full in *G&F*, p. 49-50. The letter mentions that Davies Gilbert proposed that Peel refer the matter to the Royal Society and Peel invited Croker's views. Davies Gilbert was later President of the Royal Society, 1827-30.

<sup>50</sup> Roberts (1990), p. 1; *LEC*, p. 42.

<sup>51</sup> *LEC*, p. 49.

<sup>52</sup> The meeting was reported in a letter to the *Times* published on 8 July 1830. See Hall (1984), p. 50-1. Also Hoskin (1989), p. 210, Note 131.



to the extent of being reported in the *Times*. Babbage's application to the Treasury was made within six months of the threatened censure and his favour with the Committee seems to have been unaffected by his onslaught.<sup>53</sup> The Society, it seems, chose to overlook Babbage's outburst.

The total cost to government in August 1834 when Clement received his last payment was £17,478 14s 10d.<sup>54</sup> Referrals to the Royal Society were clearly a costly business. Thus when pressed by Babbage in 1842 Peel's fears that a fourth Royal Society committee would again favour scientific enterprise at his expense were overwhelmingly justified by precedent. Any unexpressed suspicions on Peel's part that the Engine Committees were padded with Babbage's supporters would not have been misplaced.<sup>55</sup> Peel's fear that any group of Royal Society scientists would be well-disposed to Babbage through an automatic allegiance to science or through personal association with Babbage, notwithstanding his earlier attacks on the Society, may have acted as an additional deterrent to yet another referral.<sup>56</sup> Faced with the problem of Babbage and his Engine in 1842, Peel's options were to refer the issue to one of the two chartered societies, or to seek private advice. He chose the second.

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<sup>53</sup> Expulsion was considered in June 1830. Babbage applied to the Treasury for additional funds in December of that year. (The application for funds for new workshops was referred to the RS which reported to the Treasury on 13 April 1831. Weld reports that 'the Committee gave their entire concurrence'. See Weld (1849), *Works*, Vol. 10, pp.156-7). After the publication of *Decline* Babbage ceased to participate in the Society's affairs though he never resigned his Fellowship. See Babbage to Duke of Somerset, 25 February 1834, Bulstrode Collection, Buckinghamshire Record Office (NRA 11704).

<sup>54</sup> £15,288 1s 4d on development and engineering work; £2,190 13s 6d for special buildings. Roberts (1990), p. 10.

<sup>55</sup> For discussion of Airy's allegations that the Committees were biased in favour of Babbage see below p. 201 *et seq.*

<sup>56</sup> The background to Peel's choice to seek private advice is discussed below in the broader context of the shift from institutional sources of expertise to the individual 'scientific expert'. See Chapter 6, pp. 307-310.

Peel's question 'What shall we do to get rid of Mr Babbage and his calculating machine?' was perhaps only partly rhetorical – a mock lament advertising the burdens of office. But his invitation to Buckland for advice seems genuine enough. He forwarded the papers to Buckland and concludes the letter with assurances that any opinion Buckland may venture would be regarded in the strictest confidence.<sup>57</sup> For political and financial reasons Peel expressed the wish to be shot of the project though he was evidently reluctant to do so without advice. Buckland's response is not recorded and Peel appears to have charged Henry Goulburn, his Chancellor, to find out what was being said behind closed doors.<sup>58</sup>

Goulburn was a close friend and loyal colleague of Peel. He was Home Secretary in Peel's first administration and Peel's Chancellor of the Exchequer throughout his second (1841-46) term.<sup>59</sup> Gash observes that by the mid-1840s Goulburn was 'the man he [Peel] had known longer and more closely than anyone else in public life'.<sup>60</sup> Goulburn seems not to have been much of a political firecracker. Gash notes that his speeches were usually dull, his personality limited, and that he was inclined in all matters to defer to the dashing Peel.<sup>61</sup> As Chancellor, Goulburn was permanently overshadowed by Peel who was the acknowledged fiscal expert. Increasingly Goulburn's role became that of an industrious civil servant arguing the pros and cons of various options 'without coming to definite conclusions or making specific recommendations'.<sup>62</sup> Even Jenkins, in what claims to be a revisionist biography of Goulburn, describes him as

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<sup>57</sup> Peel to Buckland, 31 August 1842. BL Add Ms 40514, ff. 223-4.

<sup>58</sup> Turvey (1991), p. 167.

<sup>59</sup> *CDBN*.

<sup>60</sup> Gash (1986), p. 547.

<sup>61</sup> *Ibid.*, p. 286.

<sup>62</sup> *Ibid.*, p. 433.

'a modest man and a loyal lieutenant' and 'widely regarded as Peel's *alter ego*'.<sup>63</sup> Hyman, never one to evade an opportunity for an *ad hominem* dig at the enemy (anyone not wholly intoxicated with Babbage's genius), refers to Goulburn as a 'mediocrity'.<sup>64</sup> Given Goulburn's subdued compliance in his relationship to Peel and the fact that he appears to have brought diligence rather than brilliance to affairs of state, it is reasonable to regard him as a reliable conduit of Peel's intentions – someone without a strong agenda of his own which might skew his task. His role in Peel's quest for an off-the-record view of 'men really competent to judge' was likely to have been that of a reliable messenger.<sup>65</sup>

This was not Goulburn's first brush with the engine. In April 1829, when Chancellor in Wellington's administration, he authorised the second grant of £1,500 following the second Royal Society report.<sup>66</sup> Goulburn visited Babbage with the Duke of Wellington and Lord Ashley to inspect progress and conducted the subsequent correspondence.<sup>67</sup> The visit was in response to representations made by a group of Babbage's friends, appealing to Wellington to reimburse Babbage for the costs of the engine project incurred by Babbage and met from his own pocket.<sup>68</sup> Babbage showed Goulburn, Wellington and Ashley the first trial piece built in 1822 that he used for his first experiments, and they viewed the drawings and work in progress.

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<sup>63</sup> Jenkins (1996), ix.

<sup>64</sup> *PC*, p. 79.

<sup>65</sup> Peel to Buckland. See above p. 190.

<sup>66</sup> *LEC*, p. 55.

<sup>67</sup> Weld (1849). Babbage reprinted the relevant chapter of Weld's history of the Royal Society as an appendix to his *Exposition*. For reference to Goulburn's visit, See *Works*, Vol. 10, p. 155. The visit took place on 19 November 1829 (Drummond to Babbage, 16 November 1829. BL Add Ms 37184, f. 412. ). Lord Ashley attended at Babbage's request. Babbage cleared the etiquette of Ashley's presence in advance (Babbage to Walpole, 17 November 1829. BL Add Ms 37184, f. 415).

<sup>68</sup> See *LEC*, p. 64.

In response to Peel's request Goulburn sought the views of Sir John Herschel. It is unclear whether the choice of Herschel as confidential adviser was Peel's or Goulburn's. Herschel was by then a prominent and respected astronomer though not specifically identified with tabulation. He was already an eminent 'man of science' and had been rewarded four years earlier with a baronetcy during Melbourn's Whig administration on his triumphant return from his years at the Cape (1834-1838).<sup>69</sup> Herschel had served on all three Royal Society Engine committees, had chaired the second committee, and compiled that Committee's report.<sup>70</sup> However, in respect of continuity of committee service he was not alone. Francis Baily, Marc Isambard Brunel, Henry Kater, and Davies Gilbert share this qualification. If Goulburn had been seeking someone known to be unsympathetic to Babbage, Herschel was an unlikely choice: he was an intimate friend of Babbage's from their Cambridge days and retained a life-long friendship. Babbage later voiced allegations that Peel's advisors were motivated by jealousy and thus maliciously ill-disposed to the venture.<sup>71</sup> In choosing someone close to Babbage and a known supporter of the project the Government consultation process was fairer than Babbage seemed ready to admit.<sup>72</sup>

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<sup>69</sup> The quoted phrase is taken from "The 'Distinguished Man of Science'." See Hall (1992). For dates of Herschel's years at the Cape see *CDNB*.

<sup>70</sup> For memberships of the three Royal Society committees See Weld (1849) in *Works*, Vol. 10, p. 151 (1823), p. 153 (1829), and p. 157 (1830). Though Herschel was a member of the 1830 committee, it appears that he did not attend the committee's site-inspection at Clement's workshop, at 21 Prospect Place, Lambeth. *Ibid.*, p.157, ft 14.

<sup>71</sup> See below p. 229.

<sup>72</sup> See Turvey (1991).

## Airy's Outburst

Airy who has so far been out of the picture, now came into play. Instead of writing directly to Herschel, Goulburn used Airy as an intermediary. It is not obvious why Goulburn saw the need for a conduit to Herschel, or why this should have been Airy. Peel appears to have had considerable respect for Airy. In February 1835, during the last term of his first administration, he offered Airy a Civil List pension of £300 p.a. with the option of settling the sum on his wife.<sup>73</sup> Even allowing for the flourishes of expression characteristic of these offers, Peel's high regard for Airy seems evident, though Airy had at this time yet to perform any substantial service to government. He had accepted Spring-Rice's offer of the post of Astronomer Royal, but was at this time not yet in post, and it is possible that Peel did not wish to offend him by exclusion.<sup>74</sup> The issue was a sensitive one: Herschel was a close friend and ally of Babbage, and also a close friend of Airy. Goulburn (or Peel) may have been concerned that a private consultation with Herschel by the Treasury might trespass on Herschel's loyalties to both Babbage and Airy, and moreover inhibit the candour of his response. Using Airy as an intermediary to Herschel seems to be a shrewd choice: if the interests of science needed to be seen to be served then Herschel might be more inclined to give a candid opinion, especially if the request came from a friend and scientific colleague rather than a politician.

Goulburn's letter to Airy reveals that he was under a misapprehension about Herschel's involvement:

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<sup>73</sup> Peel to Airy, 17 February 1835. *Biog.*, pp. 106-7.

<sup>74</sup> Airy seems to have been the happy beneficiary of Whig and Tory governments outdoing each other in the public encouragement of science. Melbourne, who succeeded Peel in March 1835, promptly offered Airy a knighthood (*Biog.*, pp. 111-113), the first of three that he was to refuse (1835, 1847, 1863), finally accepting the fourth in 1872. See *Biog.*, pp. 112, 187, 254, 196.

I am given . . . to understand that the machine was originally undertaken at the suggestion of Sir J. Herschell [sic] and I feel therefore anxious if possible to know his opinion as to the probable utility of continuing to expend upon it the sums necessary for its perfection.<sup>75</sup>

It is unclear from this whether Goulburn's phrase 'originally undertaken at the suggestion of Sir John Herschel' signifies that Goulburn was under the impression that it was on Herschel's advocacy that the Government financed the engine project or whether he understood Herschel to have been an agent in the genesis of the idea for the engine.<sup>76</sup> While Herschel supported the engine project through personal encouragement, service on the Royal Society committees and lobbying Wellington in private interview, the engine project was initiated by Babbage.

Putting aside the ambiguity of Goulburn's expression, Airy appears concerned that Herschel might take exception to being held falsely responsible for two decades of barracking by Babbage, excessive costs, and indifferent progress. Airy did not forward Goulburn's letter to Herschel because, as he explained to Goulburn the following day, of the 'slight inaccuracy on one point (the original suggestion of the machine) which might perhaps have disturbed his [Herschel's] answer'.<sup>77</sup> Instead he transmitted Goulburn's request to Herschel in a letter of his own. In this he explains Goulburn's motive for the choice of Herschel and re-interprets Goulburn's problematic phrase about Herschel's involvement. Airy informed Herschel that Goulburn wished:

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<sup>75</sup> Goulburn to Airy, 15 September 1842, RGO6-427 f.63.

<sup>76</sup> Babbage left three accounts (1822, 1834, 1839) of the original meeting in 1821 with Herschel. In the first of these it is unclear whether it was Babbage or Herschel who suggested calculating by machines.

<sup>77</sup> Airy to Goulburn, 16 September 1842. RGO6-427, f. 65.

to be guided by the best information that can be procured ... With this view, it is needless to say that it was almost necessary for him to apply to you. But I gather from his letter that there is an additional reason for asking your opinion – namely that the Chancellor understands that you were consulted in an early stage of the business, and would therefore think it wrong to proceed now without endeavouring to ascertain your present views.<sup>78</sup>

'Original suggestion' is here interpreted as 'consultation at an early stage'. The reason for choosing Herschel was the combination of scientific expertise ('almost necessary' suggesting that in this respect Herschel was not unique), and long association with the progress and conduct of the venture. Airy explained his action to withhold Goulburn's letter when he wrote ten days later to acknowledge receipt of Herschel's report sent to Airy for onward transmission to Goulburn:

I have just received and forwarded your packet to the Chancellor of the Exchequer, and I *now* send for your inspection the Chancellor's letter to me. And the reason why I did not send it at first was, that it contains the supposition that *you* originally proposed Babbage's machine: and that, as I informed the Chancellor, I was confident that this was incorrect, and that your part in approving it was simply as Member of a Committee: and that it seemed therefore highly unfair to you to propose the query in that form.<sup>79</sup>

Airy was thus clear about Herschel's involvement. However, his own expression 'originally proposed Babbage's machine' retained the ambiguity of Goulburn's original formulation: it still remained unclear whether Goulburn's misapprehension as to Herschel's role consisted in seeing Herschel as responsible for the *creative* origination of the project as distinct from the *organisational* stimulus for originator of the project. Either way, Airy's first role was to remove a possible obstacle to

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<sup>78</sup> *Ibid.*, f. 68.

<sup>79</sup> Airy to Herschel, 26 September 1842. RGO6-427-f. 73. Emphasis original.

Herschel's response by formulating Goulburn's request in uncontentious terms.<sup>80</sup>

Goulburn was candid about his own position:

My own opinion is I confess adverse to any further public expenditure on this object because I cannot anticipate from its completion any public benefit adequate to an expenditure of from thirty to forty to thousand pounds and I am therefore rather disposed to give up what the Government has already expended to Mr. Babbage and to leave it to him to deal with it as may be much in accordance with his own views and means.<sup>81</sup>

Having confessed his own views he then expressed concern not to prejudice the outcome of the inquiry:

If he [Herschel] should feel as I do that the Machine is rather to be considered as illustrating the inventive and mechanical powers of Mr. Babbage than as conducive to any great public advantage I should feel no hesitation in acting upon my own judgement when so fortified though without committing him to the decision to which I might come. If on the other hand he should be able to state that he apprehended that the completion of the Machine to its full extent was likely to be a public benefit I would then proceed to examine more minutely the expense necessary to be incurred and to reconsider my present opinion.<sup>82</sup>

As well as enlisting Airy's help in securing Herschel's opinion, Goulburn concludes his letter by casually inviting Airy's views:

My object therefore is to ascertain whether you could obtain for me Sir J. Herschell's [sic] opinion of the matter. If you could add your own also it would be conferring on me an additional favor [sic].<sup>83</sup>

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<sup>80</sup> Goulburn thanked Airy for the correction. But his letter does not remove the substance of his misapprehension. See Goulburn, to Airy, 17 September 1842. RGO6-427, f. 67.

<sup>81</sup> Goulburn to Airy, 15 September, RGO6-427, f. 63.

<sup>82</sup> *Ibid.*

<sup>83</sup> Goulburn to Airy, 15 September 1842.



Airy seized on Goulburn's throwaway invitation and immediately offered his views on the history of this 'unfortunate business' and on the 'utility of the machine if completed'.<sup>84</sup> The content and tone of his response are revealing of his own attitude to Babbage and the engine venture, and give rare insights into contemporary tensions and resentments that had remained unreported.

In his letter to Goulburn, Airy gives a summary history of the engine project and clarifies Herschel's original involvement as being solely a member of the first Royal Society committee convened in April 1823, nearly twenty years earlier. Airy confessed that 'for want of original papers' his account of the history 'may be imperfect'.<sup>85</sup> Indeed he is able to recall only two committee members other than Herschel, Babbage and Dr. Young. But his perception that the committees were populated by Babbage's acolytes is explicit:

Other members were W. Penn (simply as machinist) and, I think, Dr. Wollaston: possibly some others. These persons were all private friends and admirers of Mr. Babbage: and, without laying any thing to their charge which could not be ascribed to the most honourable man living, I cannot help thinking that they were a little blinded by the ingenuity of their friend's invention.<sup>86</sup>

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<sup>84</sup> Airy to Goulburn, 16 September 1842. RGO6-427, f. 65. This letter is dated the day after that of Goulburn's request.

<sup>85</sup> Airy also states in the same letter that he was not aware whether there was any public or private inquiry into the engine question after that of Royal Society in 1823. However, his papers include printed minutes of the Council relating to the report of the 1829 committee as well as the printed report of the Committee signed by Herschel (Chairman) (See RGO6-427, ff. 59-62). It is impossible to know whether or not Airy had filed these papers before 1842. Penn is officially listed as a member of the 1829 Committee (*Works*, Vol. 10, p. 153) but not the 1823 committee (*ibid.*, p. 151), but Airy recalls him to have been part of the earlier inquiry. It is curious that he professed no knowledge of committees after the first, but still recalled Penn's involvement. (The printed 1829 Committee Report in Airy's papers has Herschel's initials as I.T. W instead of J.F.W. The reprinted version in *Works* has this corrected).

<sup>86</sup> Airy to Goulburn, 16 September 1842, RGO6-427, f. 65. The neutrality of the Royal Society Engine committees and the unquestioned acceptance of their findings has not been seriously been challenged and persists to the present time. See for example, Turvey (1991), p. 165.

Airy's allegation is not without foundation. Of the twelve members of the 1823 committee at least seven were Babbage supporters of one sort or another – close friends, men of science involved with Babbage in collaborative projects, or sharing his 'declinist' sympathies. This group consisted of Francis Baily, Marc Isambard Brunel, Thomas Frederick Colby, Davies Gilbert, John Herschel, Henry Kater, and William Hyde Wollaston.<sup>87</sup> However, support was not unanimous:

Their Report was very favourable. When the Report was discussed by the Council of the Royal Society, it was boldly stated by Dr. Young (who, with equal breadth of views on the general interests of science, possessed infinitely greater practical acquaintance with the calculations for which the machine was proposed) that, if finished, it would be useless.<sup>88</sup>

Airy's reference to Young's 'practical acquaintance with the calculations for which the machine was proposed' was almost certainly founded on Young's long service as superintendent of the Nautical Almanac, and as secretary of the reconstituted Board of Longitude.<sup>89</sup> Airy does not state the basis of Young's objection but his description of Babbage's reaction to Young's dissent is damning:

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<sup>87</sup> The committee consisted of: Davy, Brande, Combe, Baily, M. I. Brunel, Colby, Gilbert, Herschel, Kater, Pond, Wollaston and Young. See Weld (1849), *Works*, vol 10, p. 151. Kater served on a Royal Society committee for reform for which Babbage was an energetic supporter (See *PC*, p. 97); Colby collaborated with Babbage on Babbage's "Table of Logarithms" published in 1827 and the volume is dedicated to him (*Works*, vol. 2, p. 73); Babbage's diaries show frequent socialising with Wollaston whose house (1 Dorset Street) he bought in 1829. See Babbage's diaries 1820-1825, Waseda University Library for meetings; also *CP*, p. 51, 75. For purchase of Wollaston's house see Roberts (1990), p. 3.

<sup>88</sup> Airy to Goulburn, 16 September 1842. RGO6-427, f. 65.

<sup>89</sup> Young served as Superintendent of the Nautical Almanac from 1818-29. He was also Secretary of the Board of Longitude. As Superintendent he reported to the Board until 1828 when the Board was abolished. He then reported directly to the Board of the Admiralty. See *Superintendents of The Nautical Almanac & Heads of HM Nautical Almanac Office* [<http://www.nao.rl.ac.uk>]. H. M. Nautical Almanac Office, 5 December 2002 [cited 18 February 2003]. For a biographical study of Young see Wood (1954). For circumstances of Young's appointment see Hall (1984), p. 12.

For this [the statement that the engine would be useless], he [Young] was regarded by Mr. Babbage with the most intense hatred . . . Mr. Babbage made the approval of the machine a personal question. In consequence of this, I, and I believe other persons, have carefully abstained for several years from alluding to it in his presence. I think it likely that he lives in a sort of dream as to its utility.<sup>90</sup>

Airy then turns to the use of the engine and seeks to correct the false notion that the Difference Engine was intended for general purpose calculation:

An absurd notion has been spread abroad, that the machine was intended for *all* calculations of every kind. This is quite wrong. The machine is intended *solely* for calculations which can be made by addition and subtraction in a particular way. This excludes all ordinary calculation.<sup>91</sup>

He then savages the notion that the machine, capable only of specialised calculations, would be of any practical use at all:

Scarcely a figure of the Nautical Almanac could be computed by it. Not a single figure of the Greenwich Observations or the great human Computations now going on could be computed by it. Indeed it was proposed only for the computation of new Tables (as Tables of Logarithms and the like), and even for these, the difficult part must be done by human computers. The necessity for such new tables does not occur, as I really believe, once in fifty years. I can therefore state without the least hesitation that I believe the machine to be useless, and that the sooner it is abandoned, the better it will be for all parties.<sup>92</sup>

Airy's attack on the prospective utility of the machine was four-fold. He first discredited the favourable findings of the committee on the grounds that its views were less influenced by the merits of the machine than by adulation for its inventor;

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<sup>90</sup> Airy to Goulburn, 16 September 1842. RGO6-427, f. 65.

<sup>91</sup> *Ibid.* Emphasis original.

<sup>92</sup> *Ibid.*

he scotched the notion that the engine had general applicability; he then denounced its prospective benefits to current computational tasks in astronomical navigation and observational astronomy; and finally he demolished as ill-conceived the notion that there was a significant demand for new tables. His rejection of the utility of the machine is robust and complete. Even allowing for the directness that is a characteristic of Airy's writing style, his account makes no concessions to the finer feelings of those who supported the machine.

The bluntness, though not the anger, in Airy's letter is not untypical of his writing in general. For the Astronomer Royal it was grit rather than grace. This is true of his scientific writing, his business letters, as well as personal correspondence. In a 'Personal Sketch' of his father, Wilfrid Airy observes that all matters 'he kept his object clearly in view, and made straight for it, aiming far more at clearness and directness than at elegance', writing with 'great ease and rapidity'.<sup>93</sup> Of the countless letters from Airy to his wife, Wilfrid comments that 'they are not brilliantly written, for it was not in his nature to write for effect . . . but they are straightforward, clear and concise'.<sup>94</sup> Nor is vigour untypical of his response in general. Again, in the testimony of his son, 'he never hesitated to attack theories and methods he thought scientifically wrong' and in 'debate and controversy he had great self-reliance, and was absolutely fearless'.<sup>95</sup> The robust and even combative style of Airy's reply to Goulburn is certainly a factor in assessing Airy's views. Even so, the views he expresses are unmistakeably strong.

Goulburn's letter is marked 'Private and Confidential' and all subsequent exchanges are headed 'Private' by both parties. Such was the sensitivity of the

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<sup>93</sup> *Biog.*, p. 4.

<sup>94</sup> *Ibid.*

<sup>95</sup> *Biog.*, p. 12; *Ibid.*, p. 1.

matter that Herschel sent his report sealed to ensure 'perfect insulation of minions'.<sup>96</sup> Airy appears not to have used the confidential status of the correspondence as a license to express views which might otherwise have been modified by the prospect of scrutiny by others. He frees Goulburn from any constraints on the use to which his views may be put by concluding his letter with the remark that Goulburn was 'fully entitled, in courtesy as well as in right, to use my expressed opinion in any way that you shall think fit'.<sup>97</sup> Goulburn was evidently gratified by Airy's response: Airy's condemnation fortified Peel's view that the engine was a 'very costly toy' and Goulburn's expressed reluctance to proceed.<sup>98</sup> He wrote by return to Airy tendering his 'best thanks for your satisfactory letter'.<sup>99</sup>

The impression given by Airy's account in his letter to Goulburn in 1842 of Young's dissent in 1823 is that an exchange took place between Young and Babbage at the meeting of the committee, and that Airy witnessed it. However, Airy was almost certainly not present: he is not listed as a member of the Committee and he was anyway a twenty-one year old Cambridge undergraduate at the time.<sup>100</sup> It is also almost certain that Babbage was not present. He too is not listed as a member of the Committee and there is no evidence that he participated as a 'witness' or 'sponsor'.<sup>101</sup> His diary entries show that he was closely informed about

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<sup>96</sup> Herschel to Airy, 27 September 1842. RGO6-427, f. 74.

<sup>97</sup> Airy to Goulburn, 16 September, 1842. RGO6-427, f. 65.

<sup>98</sup> For quoted phrase see Peel to Buckland, 31 August 1842. BL Add Ms 40514, ff. 223-4. See above, p. 190.

<sup>99</sup> Goulburn to Airy, 17 September 1842. RGO6-427, f. 67.

<sup>100</sup> For membership of the Committee see Weld (1849). *Works*, Vol. 10, p. 151. For dates of Airy's undergraduate time at Cambridge see *Biog.*, 12.

<sup>101</sup> Babbage's Diary 1820-1825. This MS is held at the Waseda University Library. Not all of it is legible and there are long runs of entries with unclear dates. The volume is bound but there are no folio numbers. The first entry on the RS Committee is dated 21 April [1823], the date of the first meeting. The three days before the first meeting (18, 19 and 20 April) were set aside for members to examine the engine at Babbage's house. It is during these inspection visits that Babbage would have had an opportunity for to make his case.

the proceedings of the Committee but he made no reference to any personal altercation with Young. The diary account is almost certainly based on hearsay reports from his supporters, prominent amongst whom were Baily, Gilbert and Wollaston. The diary account registers Young's dissent but casts a very different light on the degree of cordiality of the exchanges than does Airy's, as it does on the level of Young's support.

Babbage records that the first meeting on 21 April was hasty because Davy, the President of the Royal Society, did not arrive until shortly before dinner, leaving only a few minutes for discussion. He continues:

All agreed as to the practicability even Dr. Young but he denied the utility of the tables when formed. Dr. Wollaston observed that there was only one objector and no objections.<sup>102</sup>

The meeting was adjourned and continued after dinner when it was agreed that they would reconvene the following Thursday (29 April 1823) to compile the report which (records Babbage) was to say that 'Mr. B's invention is highly ingenious, founded on just scientific principles and worthy of encouragement'.<sup>103</sup> Babbage's diary account of the second meeting is as follows:

Sir H. Davy proposed a report. Mr. Davies Gilbert proposed another which was not [illegible] so strong on one point (utility) as he wished but he proposed it for the sake of unanimity. Dr. Young, who was the only dissentient last time said he still did not see the utility but he had drawn up a report that he would read. It was the strongest of the three and was adopted with little variation. I was at Sir H. Davy on Saturday evening. He mentioned the conduct of Dr. Young with great surprise and said he thought the report too strong but as Dr. Y had proposed it he agreed.<sup>104</sup>

<sup>102</sup> *Ibid.* Babbage repeats Wollaston's remark a second time in the diary.

<sup>103</sup> *Ibid.* The date is inferred.

<sup>104</sup> *Ibid.*

Young is here portrayed as providing, to everyone's surprise, the most supportive report notwithstanding his earlier opposition. Babbage reported that 'Dr. Young called on Saturday. He seemed struck with the contrivance for examining the type'.<sup>105</sup> The order of the entries in the diary suggests that this visit took place on Saturday 23 April, that is, between the two Committee meetings, and what transpired may have effected Young's *volte face* as reported by Babbage. In any event there is no sign of harsh words between them in Babbage's record.

The evidence suggests therefore that no altercation took place between Babbage and Young at the meeting, and that Airy's account to Goulburn, and indeed of Babbage's reported sensitivity to criticism, while possibly real, was based on later encounters. Airy may have felt that Young had been victimised by Babbage's gang and that the merit of Young's objection (with which Airy agreed) was dismissed as a result.<sup>106</sup> What counts against this interpretation is that Airy's attack serves less to champion Young as a wronged party than to expose Babbage as irrationally defensive of his cherished project. Babbage's account was written at the time of the events, Airy's nearly twenty years later. Both appear to be based on hearsay. If defence of Young was not a motive then Airy's account to Goulburn would appear to indicate strong feeling, resentment, indignation or even anger at Babbage and/or the engines, arising from events and circumstances that post-date the first Royal Society meeting. Events involving Babbage that might have angered Airy in years to come include the ill-tempered competition between them for the Lucasian chair during which Babbage threatened legal proceedings, the generally-felt resentment against Babbage at the neglect of his duties when he did secure

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<sup>105</sup> *Ibid.*

<sup>106</sup> For Airy's reported expression of the same view see below p. 220.

the appointment, Babbage acting as an 'expert witness' in favour of Sir James South against Airy and Sheepshanks in the rancorous legal dispute about a faulty telescope mounting, and Babbage's public allegations that Airy was motivated by jealousy and malice against him.<sup>107</sup> But at the time of the first committee meeting on the Engine in 1823 there was no hint of the tribulations to come.

The time sequence therefore suggests that Airy's robust attack on Babbage in his letter to Goulburn has its roots in events that interposed between the first committee meeting and the time of Airy's letter to Goulburn some twenty years later, and that Airy harboured some resentment against Babbage, or his conduct, that found expression in the letter which is more in the nature of an outburst than a considered set of views on the utility of the machines.

Meanwhile Herschel was squirming over his brief. He was evidently perturbed by the wording of Airy's brief as formulated in Airy's letter dated 16 September 1842. The letter mentions that the 'datum' for the exercise was 'that  £16,000 has been expended by the Government ... to no effect'.<sup>108</sup> Herschel wrote to Airy asking:

whether the words "have been expended on it to no effect" form part of the Chancellor's communication to you or are merely your own words of expressing a general meaning which I clearly apprehend to be *that the practical object has not been attained*.<sup>109</sup>

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<sup>107</sup> Each of these circumstances is discussed more fully elsewhere in this thesis. For Airy's cryptic reference to legal proceedings during the Lucasian election see *Biog*, p. 70.

<sup>108</sup> Airy to Herschel, 16 September 1842, RGO6-427, f. 68.

<sup>109</sup> Herschel to Airy, 20 September 1842. RGO6-427, f. 69. Emphasis original. Turvey (1991) quotes the phrase as 'expended . . . to no object' citing Royal Society Herschel Archive HS 1.109 as the source. A letter-press copy of Airy's original survives in Airy's papers and the phrase is clearly 'expended ... to no effect' (RGO6-427, f. 68). Herschel's letter querying the source of the phrase survives in the original and also has 'expended ... to no effect' (*ibid.*, f. 69) and Airy's response (letter press copy) is the same (RGO6-427, f. 1). There are at least three other discrepancies in Turvey's quotes which raise doubts about their overall reliability.



He was clearly concerned to know whether the Chancellor was making a judgement that the moneys already spent had been wasted. If the phrase was Goulburn's then Herschel says he would take this as a given starting point; if the phrase was 'merely' Airy's he would avoid the question in his report. Herschel was evidently baulking at a difficult and unwelcome task. He wrote to Airy equivocally that he expected to respond 'if indeed I can make up my mind to write at all about a matter which I really feel to have got beyond my depth'.<sup>110</sup> Airy had himself introduced the phrase 'to no effect' reflecting a harsher view of the situation than had Goulburn's less judgemental version. Airy's reply is scrupulously honest: the problematic phrase was his not Goulburn's and that he meant no more by it than that 'the practical object has not yet been attained' exactly as interpreted by Herschel.<sup>111</sup> He nonetheless continued to withhold Goulburn's letter because of Goulburn's misapprehension as to Herschel's original involvement. Instead Airy quoted verbatim two excerpts from Goulburn's letter to reassure Herschel that he had conveyed the Chancellor's precise meaning 'beyond the chance of mistake'.<sup>112</sup> Airy received Herschel's sealed report on 26 September 1842 and he duly forwarded to Goulburn. He then finally gave Herschel sight of Goulburn's letter containing the erroneous attribution, though he requested its return at Herschel's convenience. This Herschel duly did with a covering note commending Airy's conduct though not before taking synoptic notes of its contents.<sup>113</sup>

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<sup>110</sup> *Ibid.*

<sup>111</sup> Airy to Herschel, 22 September 1842. RGO6-427, f. 71.

<sup>112</sup> *Ibid.*

<sup>113</sup> 'You have acted with your usual distinction in the matter', Herschel to Airy, 27 September 1842. RGO6-42, f. 74. Herschel's notes survive in the Herschel Archive (R. Soc. Herschel Archive, Box 27, Item 50). An excerpt of the summary is quoted in Turvey (1991), p. 171. The original is preserved in Airy's papers, RGO-427 ff. 63 *et seq.*

Herschel's letter of 27 September 1842 ended the flurried ten-day exchange. The return of the letter also ended the first episode of Airy's official involvement in the engine enterprise: in the first instance as an intermediary between Goulburn and Herschel in the Treasury's request to secure Herschel's opinion of the advisability of further expenditure; secondly, in providing a supplementary opinion at Goulburn's invitation.

Herschel's report is long and elaborate. In it he steered an uncomfortable path between loyalty to Babbage, an apparently genuine belief in the utility of the machine, and public duty. Turvey comments that 'the many corrections and revisions made to the draft clearly show that Herschel felt that this was a disagreeable and difficult task'.<sup>114</sup> In one revision Herschel added a paragraph in which he voices his discomfort:

as a private friend of Mr. Babbage I feel the utmost delicacy in forming an opinion on a point so greatly concerning his interests, and only a paramount sense of public duty . . . could decide me to enter on such a task.<sup>115</sup>

Herschel did not consult with Babbage nor with any of his own scientific colleagues and explains to Goulburn that he felt duty bound to reply only on his own behalf.<sup>116</sup> Clearly uncomfortable about reporting behind Babbage's back he ends the report by saying:

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<sup>114</sup> Turvey (1991), p. 168. It has been pointed out that frequent correction in itself is not necessarily symptomatic of discomfort or difficulty. However, Herschel does confess to the delicacy of his position.

<sup>115</sup> *Ibid.*

<sup>116</sup> *Ibid.*, p. 175.

as his [Babbage's] friend from whom I am desirous to have no concealment on a matter so nearly affecting him I can have no objection to his being made acquainted with the contents of his letter.<sup>117</sup>

The length and complexity of Herschel's response weakened his stated conviction in the benefits of the machine.<sup>118</sup> It is difficult to know how much relative influence each of Airy's and Herschel's reports had on the outcome. If Goulburn's subsequent action was influenced by either of the two reports then Airy's short clear damnation appears to have carried more weight than Herschel's carefully considered support: Goulburn wrote to Babbage on 3 November 1842 axing the project on the grounds of cost.<sup>119</sup>

Airy's case against Babbage's Difference Engine as articulated to Goulburn in 1842 contained three separate objections. The first was that the machine was not a general purpose calculating device for 'ordinary calculation' but intended for the specialised purpose of tabulation using repeated addition and subtraction as required by the method of differences.<sup>120</sup> Airy is here correcting a misapprehension in public perception rather than contradicting any claim made by the engine advocates. His second objection is that the machine was irrelevant to the ongoing computations at the Greenwich Observatory which he superintended, and for the Nautical Almanac. Airy's third objection is perhaps the most damaging. The case made by the engine advocates is premised on the claim that there was a continuous need for new tables. Airy's commented to Goulburn that 'even for

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<sup>117</sup> *Ibid.*

<sup>118</sup> The full text of Herschel's draft is quoted in Turvey (1991), Appendix, pp. 171-175. For original MS, see Royal Society Herschel Collection, Box 27, item 51.

<sup>119</sup> Goulburn to Babbage, 3 November 1842. BL Add Ms 37192 f. 172. The letter is reprinted in full in *Works*, vol. 10, pp. 161-2.

<sup>120</sup> For quoted phrase see Airy to Goulburn, 16 September 1842, RGO6-427, f. 65.

these, the difficult part must be done by human computers'.<sup>121</sup> As described in the Chapter 1, the starting point of any machine calculation relies on manually computed starting values as well as the accurate use of approximation formulae. These preparatory works are susceptible to human error and this is a weakness of the supposed 'infallibility' of subtabulation by machine that the engine advocates did not advertise. But the point made by Airy does not relate to errors, but to the 'difficulty' of these calculations. He is almost certainly referring here to the need for a high level of abstract analysis and of complex calculation to ensure that the formula used to approximate the function remains within the requisite limits of precision for a given run of machine calculations, and that the burden of this outweighs the benefits conferred by subtabulation by machine. Babbage himself calls for an entire new branch of mathematical analysis to translate symbolic formulae into computational procedure, and this implicitly concedes Airy's point.<sup>122</sup> Finally, Airy asserts that need for new tables is a rarity and 'does not occur, I really believe, once in fifty years'.<sup>123</sup>

Airy does not elaborate on the specific objection raised by Young in 1823 beyond recounting Young's pronouncement that the engine, if finished, would be 'useless'. Young's objection was recorded. Weld, in a footnote to his account of Babbage's engine in his *History of the Royal Society*, notes:

I am informed upon good authority, that Dr. Young differed in opinion from his colleagues. Without doubting that an engine could be made, he conceived that it would be far more useful to invest the probable cost of constructing

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<sup>121</sup> Airy to Goulburn, 16 September 1842, RGO6-427, f. 65.

<sup>122</sup> For a discussion of Babbage's notions about the need for new branches of numerical analysis see Chapter 3, p. 144 *et seq.*

<sup>123</sup> Airy to Goulburn, 16 September 1842, RGO6-427, f. 65.

such a calculating machine . . . and apply the dividends to paying calculators.<sup>124</sup>

Young's argument was that there was no economic advantage to building the machine. The calculation suggested by Young's objection was easy to do in that all three elements were known: the likely capital costs of an engine, the salaries or fees of computers, and the probable return on investment.<sup>125</sup> However, there is no evidence that anyone sought to use such a calculation to support the point. The benefits of the machine were more difficult to estimate. It was, for example, practically impossible to quantify the consequential costs of tabular errors, or the number of undetected residual errors in tables. Young's argument on the grounds of cost is therefore an unequal one, and exposed another weakness in the engine advocates' case. Babbage's diary account of 1823 implies that Young retracted, or at least diplomatically waived, his objection, and gave his support to the Committee's endorsement of Babbage's invention. According to Babbage's account there was therefore no argument to answer and there is no record of any debate in which the two parties contested the case. Airy held that manual methods anyway delivered all that was required and so discounted that the engines had any benefit to offer in terms of accuracy.<sup>126</sup> There is also no evidence that Airy's conception of benefit went beyond the potential of the engines to replace existing manual methods of tabulation.

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<sup>124</sup> Weld (1849). *Works*, Vol. 10, p. 151, ft. 5.

<sup>125</sup> As a rough guide the estimated total costs of the Difference Engine if completed were £35,000. Interest rates were stable at 3%. Computers were paid about £120 per annum. Airy recorded that the cost of correcting 2,560 astronomical observations made between 1830 and 1853 which were in error following a mistake found in Burckhardt's formula for parallax correction was £400. See *Biog.*, p. 214.

<sup>126</sup> See below p. 220. Also Selander's objection. See below p. 222.

## The 'Missing' Report

In his 'Autobiography' Airy later recounted the episode of his first consultation:

On Sept. 15th Mr. Goulburn, Chancellor of the Exchequer, asked my opinion on the utility of Babbage's calculating machine, and the propriety of expending further sums of money on it. I replied, entering full into the matter, and giving my opinion that it was worthless. - I was elected an Honorary Member of the Institution of Civil Engineers, London.<sup>127</sup>

This is the most frequently quoted of Airy's condemnations.<sup>128</sup> It despatches half a lifetime's work in a brusque dismissal without supportive argument or self-justification. The throwaway mention of his election to the Institution of Civil Engineers adds insult to injury, ranking, as it seemingly does, an honorary membership of a professional institution with Babbage's grand venture. Given Airy's dim view of the utility of the engine it is tempting to read the conjunction of the two statements as a further sign of disregard. However, taken on its own the style of the passage may be misleading. The 'Autobiography' has the format of an annual diary with sections for each of the years from 1836 to 1891, and abrupt transitions between unrelated events are not uncommon. The record for 1826, for example, reconstructed by Airy from his personal papers, memorabilia and the quires which served as a form of scratch-pad for his daily workings, juxtaposed a record of the tutor's stipend (£50) and the ellipticity of heterogeneous spheroids.<sup>129</sup> A starker non-sequitur from the same year juxtaposed Airy's

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<sup>127</sup> *Biog.*, p. 152.

<sup>128</sup> See for example, *G&F*, p. 130; *LEC*, p. 101.; Turvey (1991), p. 167; Campbell-Kelly, Martin, ed. *Charles Babbage: Passages from the Life of a Philosopher*. new edition ed. London: William Pickering, 1994, see "Introduction", p. 28.

<sup>129</sup> *Biog.*, p. 66.

commencement of tuition in Italian, his witnessing a murderer guillotined in the Place Martroi, and investigations into pendulums and the calculus of variations.<sup>130</sup> Perfunctoriness is evidently to some extent an artefact of the genre. Airy's son, Wilfrid, edited and published the 'Autobiography' after his father's death. The work qualifies loosely as an 'Autobiography' (hence the apologetics) in that the sources on which it is based were largely, but not exclusively, by Airy's hand, edited and supplemented by Wilfrid.<sup>131</sup> In the case of any given example of disjointedness it is impossible to know whether this is a jump-cut by Airy himself or an artefact of Wilfrid's editing.

But the passage recounting Airy's pronouncement that Babbage's engine was 'worthless' still beckons. Airy stated that he replied 'entering fully into the matter'. If this is a reference to his short damning letter to Goulburn then to regard it as having 'entered fully into the matter' is charitable. The phrase suggests some other more substantial document, a reasoned report perhaps, that might reveal the grounds for his otherwise outright rejection of the engine's utility. No such report has come to light.<sup>132</sup> The absence of the report is more significant in Airy's case than for someone less obsessed with order, filing systems and the systematic retention of all written records. In the personal sketch of his father, Wilfrid observed that 'the ruling feature of his character was undoubtedly Order'.<sup>133</sup> On his practice of scrupulously archiving records Wilfrid observed that 'he seems not to have destroyed a document of any kind whatsoever' and that 'preserving and arranging

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<sup>130</sup> *Ibid.*, p. 69.

<sup>131</sup> *Ibid.*, p. vii.

<sup>132</sup> There is no reference to such a report in the RGO Archives. Allan Chapman who, has researched Airy's life with some thoroughness, has no knowledge of such a report (personal communication).

<sup>133</sup> *Biog.*, p. 2.

the manuscripts of the Observatory . . . was always regarded by Airy as a matter of the first importance'.<sup>134</sup> In his pursuit of order and efficiency he invoked the assistance of office technology. In 1836, a few months after his appointment as Astronomer Royal, he introduced at Greenwich a letter copying press to retain facsimile impressions of all outgoing manuscript documents and observed that from then on 'my correspondence, public and private, is exceedingly perfect'.<sup>135</sup> In 1837 he contrived an information coding and retrieval system whereby each sheet of paper was identified with a series of up to four holes.<sup>136</sup> Papers were arranged in packets and subordinate packets and presumably selected and retrieved by category by the insertion of needles. Holes were punched by hand using a pre-punched card as a template until Ransome and May provided him with a punch press in 1843.<sup>137</sup> This system remained in operation from the time of its introduction until at least 1871.

Airy's son portrays his father's preoccupation with filing and order as a dispositional trait. However, there is an instance that reveals another dimension to Airy's insistence on exhaustive record keeping. A medical man, one Dr. Leitch,

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<sup>134</sup> For first quotation see *Biog.*, p. 2. For reference to preserving Observatory papers see *Biog.*, p. 280. See also p. 324. Wilfrid records that his father 'retained counterfoils of old cheque books, notes for tradesmen, circulars, bills, and correspondence of all sorts'. See *Biog.*, p. 2.

<sup>135</sup> The process was patented by James Watt in 1780. Taking a copy involved placing a dampened sheet of thin paper over the original and applying pressure by means of a flat plate in a press. The dampened paper absorbed some of the ink from the original to produce a facsimile image of the manuscript. The thinness of the copy paper and its moistness allowed the dissolved ink to soak through the copy paper to be visible from the side not in contact with the original. This removed the need to laterally invert the image with a mirror. The letter copying book and press was slow to be adopted. But by 1875 its use was standard practice in commercial concerns and was used until relatively recently. It was still used by the Law Society in the late 1950s as letter press copies were accepted in evidence as true copies of the original (Science Museum, Technical File). The process required some skill for readable results. If the copy paper was too moist, the ink blurred, and the image was illegible especially if the handwriting was small (as was Airy's); if insufficiently moist the image was too faint; if unevenly dampened, a mix of both deficiencies was the result. Airy's voluminous papers contain excellent examples of all these variants. For quotation see *Biog.*, p. 123.

<sup>136</sup> *Biog.*, p. 131.

<sup>137</sup> *Biog.*, p. 158.



petitioned Airy to exert some influence on his behalf for a patronage appointment as a medical officer in the Crimea, and a post for his wife as a nurse.<sup>138</sup> Airy duly wrote to Sidney Herbert, War Secretary, on Leitch's behalf.<sup>139</sup> It appears that Leitch had forwarded correspondence about his ambitions to Airy in which a Dr. Davy counselled the medical hopeful to ensure that the contractual terms of any government appointment were made clear.<sup>140</sup> Airy, by now a vastly experienced civil servant, seized on this. He urged Leitch that it was imperative not only to have a clear understanding of the terms but to have these officially confirmed in writing because, he argued, the 'numerical mass of appointments is so great and the interruption of personality in the supreme command is so frequent, that *there exists no memory except upon paper*'.<sup>141</sup> During Airy's tenure as Astronomer Royal (1835-1881) there were thirteen changes of government: two Tory, two Whig, one Peelite, five Liberal and four Conservative.<sup>142</sup> Airy secured, in writing, freedom from political allegiance to Peel's Tory administration of 1835, as a condition of his acceptance of the civil pension Peel offered.<sup>143</sup> As an astute careerist the practical side of Airy's near-obsessive record-keeping may well have been as an insurance against the vicissitudes of patronage in a politically volatile age.

Given Airy's rigorous documentation practices, if a report did exist, it is extremely likely to have survived. Despite this, the failure so far to find the report

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<sup>138</sup> Leitch to Airy, 18 December 1854. RGO6-375, p. 378. Leitch had helped Airy with pendulum experiments conducted in Harton Colliery, Durham, by effecting introductions to secure access to the shafts. *Biog*, p. 220. For a description of Airy's attempts to weigh the earth using pendulum timings in mine shafts see Chapman (1993).

<sup>139</sup> Airy to Herbert, 21 December 1854. RGO6-375, p. 380. Airy admits to Herbert that he cannot testify to Leitch's medical competence but that 'Dr. Leitch is a man of good private fortune and good education ... and is in every way fitted to support his position, as equal among gentlemen and as director of subordinates'.

<sup>140</sup> Airy to Leitch, 22 December 1854. RGO6-375, f. 384.

<sup>141</sup> *Ibid.* Emphasis original.

<sup>142</sup> Whitaker's Alamanack 1997, p. 235.

<sup>143</sup> Airy to Peel, 18 February 1835. *Biog*, p. 107.

cannot be regarded as in itself conclusive with respect to whether or not such a document existed. A more compelling argument against the existence of a report is the fact that Airy made no reference to any supplementary document in his letter to Goulburn. If the letter to Goulburn was in the nature of a covering note with an additional document as an attachment, it is difficult to imagine why the letter contained no reference of any kind to it. If we therefore conclude that no such report existed, Airy, otherwise consistently scrupulous in all matters, in stating that he had 'entered fully into the matter', is claiming more than was his due, which is uncharacteristic of this pragmatic and direct man.

There is another explanation that reconciles the absence of a report with Airy's claim, and this relates to respects in which the 'Autobiography' is a problematic source. The fact that much of the material is in the first person, and the format, as mentioned, is largely that of an annual diary, creates the strong impression that the entries are contemporaneous with events. However, by Airy's own admission many of the accounts post-date the events by up to thirty years, even though the notes, reports and other sources from which Airy himself worked were contemporary. In the entry for the calendar year 1828, for example, Airy observed that his determination of the Longitude of the Cambridge Observatory (which as Plumian Professor he superintended *ex officio*) in October of that year 'has been used to the present time (1853)'. There was evidently a gap of twenty five years between the account and the events recorded.<sup>144</sup> There are at least four other instances in which references to the time of writing indicate large lapses of time.<sup>145</sup> In addition to these Airy states in 1872, 'I have therefore lately employed

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<sup>144</sup> Care of the Cambridge Observatory was attached to the Plumian Professorship. See *Biog*, p. 78. The full title of the chair was the Plumian Professor Astronomy and Experimental Philosophy. See Howse (1989), pp. 77, 177.

<sup>145</sup> The entries for 1822/3 were written in 1848 ('It was the duty of scholars by turns to read Grace after the Fellow's dinner and supper, and at this time (1848) I know it by heart' *Biog*, p. 36); entries

some time in drawing up a series of skeleton annals of the Observatory . . . and have carried it through the critical period, 1836-1851' – indicating a gap of between twenty one and thirty six years.<sup>146</sup> The 1838-9 entries were written in 1871, and the 1846 entry in 1872. Since the format of the 'Autobiography' gives the strong impression that it was written in date order by year, and in view of Airy's highly methodical way of working, it is reasonable to infer that the entry for 1842, the year of the Goulburn-Herschel episode, was written some thirty years later in 1871 or 1872 by which time his recollection of events may have blurred.

Airy produced another more systematically detailed assessment of a calculating difference engine. This was a substantial official report, submitted to the Treasury in 1857, on the utility of the Scheutz difference engine that the General Register Office proposed to purchase for the preparation of the English Life Table of 1864.<sup>147</sup> The findings of the report were similarly negative though less damning in their formulation. Airy consulted others (Hind at the Nautical Almanac Office and George Graham at the GRO) on the benefits, if any, of deploying the machine at their respective offices. Airy would have been well-justified in referring to the process of consultation, the collation and analysis of responses, and the production of a fairly substantial report, as 'having entered fully into the matter'. It is possible that at a distance of some thirty years, in claiming to have 'entered fully into the matter' in 1842 Airy had confused his response to the Goulburn-Herschel consultation with the later Scheutz report, and that consequently, no report on Babbage's engine beyond his letter to Goulburn existed.

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for 1837, 1838 and 1839 were written in 1871 (*Biog*, pp. 131, 133, 138 resp.); the entry for 1846 was written in 1872 (*Biog*, p. 295).

<sup>146</sup> *Biog*, p. 295-6.

<sup>147</sup> Airy to Trevelyan, Secretary of the Treasury, 30 September 1857. T1/6098B/19264, PRO (Kew). The context and content of this report are discussed in Chapter 5, below.

## Airy's Private Opinion

The Goulburn-Herschel consultation of 1842 was the first known occasion on which Airy entered the fray. His role was that of an intermediary rather than an official consultant and he seized the opportunity to express his views when casually invited to do so by Goulburn. The engine project had by then been dragging on for nearly two decades with the major practical efforts to construct a machine at a standstill since 1833. However, this was evidently not the first occasion on which Airy had made his opposition to the engines known. A letter from Thomas Romney Robinson, Irish Astronomer, writing to Babbage from Armagh Observatory in 1835, hints at some of the intrigue surrounding Babbage and his engines and it is clear that Airy was outspoken, at least in private, about his position much earlier than the occasion of his first official involvement:

The opinion which I (and Beaufort simultaneously) formed respecting Airy's being privy to some plan of attacking you, arose from manner and look than anything which he actually said. Indeed of the conversation I distinctly remember but two points, one his saying "that the persons who had recommended the construction of the machine would shortly find themselves in a very unpleasant predicament" the other "that in his opinion the machine was useless, for that if the money spend on it had been applied to pay computers, we could have had all that is wanting in the way of tables ["]. Airy I do not think is likely to have been the mover in this, but wherever it comes from, let me entreat you not to despise the attack as unimportant because it is contemptible.<sup>148</sup>

It seems from this that Airy shared Young's views about the economics of the engine as reportedly expressed by Young at the meeting in 1823. Airy appears to

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<sup>148</sup> T. R. Robinson to Babbage, 20 December [1835], Armagh Observatory. BL Add Ms 37189, f. 220. The year is inferred from the sequence before and after the relevant folio.

have been uninhibited in making his dim view of Babbage's engine known in social circles. This is confirmed through a piece of contemporary reportage by the actor William Macready who made the following diary entry in 1837 referring the Babbage's engine: 'Professor Airy says the thing is a humbug. Other scientific men say directly the contrary'.<sup>149</sup> Robinson's letter also suggests that Airy was not above intrigue. This in turn suggests that the impression created by Airy's son in the 'autobiography' of his father, and by Airy's meticulously archived official papers, of a professionally objective and dutifully neutral servant of government was only one aspect of his agency.

From the dates of these reports it is evident that Airy's scepticism appears to have been known well before the consultation in 1842. This raises the question of whether Airy was approached by Goulburn because of his known opposition to the machines and that his overtly even-handed brief to Airy was disingenuous. If Robinson's and Macready's reports are correct, then Airy was a promising choice to secure the desired result of axing the engine. The weakness of this argument is that Airy was, officially at least, only a conduit to Herschel, and Airy's views were invited in an apparently off-hand way. To give Airy an opportunity to damn the engines while going through the motions of securing a predictably equivocal assessment from Herschel would have required prior collusion between Goulburn and Airy, or some highly subtle manipulation by Goulburn. And this, at face value at least, seems far-fetched.

Robinson, perhaps as a form of moral support, portrayed Airy as isolated in his opposition to the machine: 'If they [Members of Parliament] go into the merits or use of the invention itself I cannot suppose they will find a man in Europe but Airy

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<sup>149</sup> Toynbee (1912), 17 September 1837. Vol. I, p. 410. Quoted in *G&F*, pp. 130, 274; *LEC*, p. 101; Moseley (1964), p. 82. Lindgren notes (*G&F*, p. 333 ft. 126) that the source of the statement was first noted by Moseley (1964), p. 82. Collier and Lindgren quote "the thing is a humbug" and omit the second part of the hearsay report. Moseley has omitted the word "directly" in her version.

to gainsay it.<sup>150</sup> However, Airy was not alone in his opposition to the engines. Nils Selander, a Swedish astronomer argued that:

Logarithmic, trigonometric and other tables, which are purely mathematical in character, and do not rely on information from experimental results or observational have already been computed with such accuracy that they leave little to desire; the improvement of astronomical, physical and similar tables depends primarily on access to more exact observational data.<sup>151</sup>

Babbage's engines were designed for a digit precision of sixteen, thirty, fifty and hundred figures. Practical measurements were limited to three or four digits at best. While Selander conceded the need for higher accuracy in generic tables his point was that the priority was improvement in experimental measurement not in computational precision. He also directly rejected the case that manual tabulation was intractably error-ridden. Selander's comments contain a deeper implicit criticism: that tables are only tools that serve some higher aim, and in that they are in themselves mundane. By implication, tabulation engines, even if successful, were no more than instrumental aids to the production of tables, and the venture to mechanise tabulation was not a great or elevated pursuit of intellect or of human aspiration, but was equally mundane. This deflates Babbage's more grandiose portrayal of the significance of his machines, especially in the context of his speculations on new implications for mathematical analysis which, in fairness, appear to have interested him more than the mundaner aspects of routine tabulation.

The emphasis placed on tabular errors as the primary purpose of the machines, by Lardner in particular, exposed the engines to criticism from credible

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<sup>150</sup> T.R. Robinson to Babbage, 20 December [1835], Armagh Observatory. BL Add Ms 37189, f. 220.

<sup>151</sup> Selander, 13 November 1844. Quoted in *G&F*, p. 133.

scientific figures that was difficult to defend against. Because of Babbage's ineptness as a publicist, and Lardner's difficulty in giving an accessible account of the aspirational promise of the machine, Babbage's higher aims were never effectively championed, and the engine advocates were defending their ambitions from a position of weakness. In this respect, Lardner's 'dumbing down' of the significance of the invention, because of the difficulty of producing an accessible account for his public audiences, was counterproductive.

Selander was not the only European who shared Airy's scepticism for the engines. Giovanni Plana, an Italian mathematician, was equally dismissive, even after he had attended Babbage's lectures on the Analytical Engine in Turin in 1840.<sup>152</sup> A friend of Babbage, Fortunato Prandi, warned:

Plana will not write anything about the Engine. He seems to think that you delude yourself, that the engine, if it ever executed, will be a great curiosity, but perfectly useless . . . He expressed great friendship and regard for you, but this is in substance what he told me concerning the engine . . . Pray say nothing of this. I tell you all I hear without restraint but you must take care that you do not compromise me.<sup>153</sup>

### Babbage's Public Allegations

Until 1856 Airy's views on automatic calculating engines (as distinct from mechanical calculating aids) were recorded in private correspondence, some of it

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<sup>152</sup> See *CWB*, pp. 128-33.

<sup>153</sup> Prandi to Babbage, 4 January 1842, BL Add Ms 37192, f. 4. For others in England and on the Continent who shared Airy's scepticism see *G&F*, p. 276.

highly confidential.<sup>154</sup> Babbage would not have been privy to the correspondence, and until 1856 his only access to Airy's views will have been through intrigue, gossip and hearsay reportage from colleagues, friends and associates. While Babbage would have been alerted to Airy's opposition and even hostility to the engines, he would not have been exposed to the specifics of Airy's case or to the technical detail of his arguments. Aware of Airy's opposition but unable to engage with his arguments Babbage resorted to conspiracy theory. In *Exposition*, published in May 1851, Babbage accused Airy of being party to a personal vendetta against him and of influencing government against the engines as part of a deliberate scheme to discredit him. In *Exposition*, he made explicit his suspicion of sinister forces:

many persons . . . have at the same time expressed to me their doubts that some occult agency was at work to prejudice the government, and have asked who were its scientific advisers on such an important subject, during the long period in which the Difference Engine was in abeyance.<sup>155</sup>

He also expressed frustration at being kept in the dark about the findings of any official assessment:

There are I am aware, other channels than those of official reports, by which the Government may have been influenced. I do not therefore, expect to find any formal report denying the practical utility of the calculating engines, or the possibility of constructing them. If there is any such, I claim as a matter of justice, that it be published. The Difference Engine and the Analytical Engine, are questions of pure science. If the Astronomer Royal has maintained that they are either useless or impracticable, then the grounds of that opinion

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<sup>154</sup> Airy volunteered his views on the Scheutz difference engine in 1856 in a letter to the Editors of the *Philosophical Magazine*. This was the first known occasion on which he went public with his views. The circumstances of the letter and its contents are discussed in Chapter 5, pp. 252-7.

<sup>155</sup> *Expo., Works*, Vol. 10, p. 93.



*must* have been stated, and, if published, the solidity of those grounds might be examined.<sup>156</sup>

The statement that 'the Difference Engine and the Analytical Engine are questions of pure science' is highly revealing in that it confirms that the basis of Babbage's conviction of the value of the engines was abstract and scientific, rather than pragmatic and narrowly utilitarian, notwithstanding the years he spent on the detail of their implementation. The statement also helps to explain Babbage's apparent indifference to founding his arguments for the engines on strong practical grounds. He evidently regarded their essential value as lying elsewhere, and this is supported by the findings in Chapter 3 which suggest that the intellectual dimension of computational machines was his central preoccupation, certainly at first, and continued to serve as the primary criterion of worth. His challenge to Airy to publish any objections based on the engines' lack of practical utility demonstrates his frustration at being unable to engage with such arguments in ignorance of their detail. A revealing feature of Babbage's attack is the imputation that Airy's pragmatic view of utility disqualified him from acting as a true arbiter of worth. Babbage's exasperated assertion that the question of the engines was one of 'pure science' exposes the basic and underlying tension between the engine advocates and the sceptics, and the polarisation between a visionary view and a pragmatic one is reflected in the dispositions of the two antagonists.

Collier provides an insight that supports the view that Babbage's essential interest in the engines was more intellectual than practical:

[Babbage] did not invent his calculating machines for profit, or to solve some practical problem; rather he found the process of inventing them to be its own

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<sup>156</sup> *Ibid.*, p. 100. Emphasis original.

reward, as a purely intellectual exercise. To be sure, it was the unsatisfactory character of manually calculating and checking mathematical tables that first directed his attention to the desirability of machines to do the job, but once he began considering them, their perfection became an end in itself, and the tables from which he had begun were no longer important . . . Thus it is vitally important in understanding Babbage's character that he was not a technologist who happened to have a scientific background, but rather a scientist and intellectual who happened to be working on technical problems.<sup>157</sup>

*Exposition* is ostensibly a continuation of his earlier Declinist attack on the conduct of science and the attitudes of officialdom to science in England. But it is more in the nature of an angry assault in which his sense of injustice at the outcome of his labours is compounded with bitterness and humiliation at his exclusion from the organisation of the Great Exhibition. The mix is one of unmistakable misery. While preparing *Exposition* Babbage was advised by friends to avoid *ad hominem* attacks, or not to publish. In the Introduction to *Exposition* he openly defied them.<sup>158</sup> The work has been described as 'the diatribe of a disappointed man . . . disfigured by personal allusions, in giving utterance to which, he wronged his better nature'.<sup>159</sup>

In *Exposition* he took the Commissioners to task accusing them of weakness in yielding to retailers' self-interest by omitting to display the price of manufactured goods on exhibition. He portrayed the refusal to display prices as an affront to the principle of free trade, the basis of which was competition, to which price comparison was central, especially on the occasion of what he calls 'the world's

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<sup>157</sup> LEC, p. 276-7.

<sup>158</sup> *Expo.*, *Works*, Vol. 10, p. vi.

<sup>159</sup> Clerke (1985), *DNB*, p. 778. See *Expo. Works*, Vol. 10, p. vii for Babbage's reference to 'disappointed man' – one of three accusations he claims are used to discredit or intimidate 'honest' men.

great bazaar'.<sup>160</sup> He objected to the location chosen for the Crystal Palace, arguing for a site 1,300 yards away to the east. In a characteristic appeal to the authority of number he supported his case with sums to show that with an estimated four million visitors, a total of five million unnecessary miles will have been travelled at a cost of £13,333.<sup>161</sup> Apart from the undertone of protest and indignation there is nothing outrageous in these arguments. But in his personal attack on Airy he abandons any pretence of being a pundit or patriarch of the industrial movement acting in the interests of the public good.

In a section called "Intrigues of Science" Babbage accused Airy of being part of a vendetta against him and influencing government against his engines through personal allegiance to Babbage's enemies. The villain of the piece (according to Babbage) is the Reverend Richard Sheepshanks, an astronomer with an early training in law and a close friend of Airy.<sup>162</sup> The circumstances are convoluted and the embroidery is not always easy to unpick. Babbage alleges in *Exposition* that Sheepshanks was twice thwarted by Sir James South in the politics of scientific affairs. Babbage had supported South on both occasions. On the second occasion, a meeting at the Admiralty about the Nautical Almanac, Sheepshanks, a belligerent stirrer who loved nothing better than a scrap, threatened Babbage as they left the meeting room: 'I am determined to put down Sir James South and if you and other respectable men will give him your support, I will put you down'.<sup>163</sup>

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<sup>160</sup> *Expo.*, *Works*, Vol. 10, p. v.

<sup>161</sup> *Ibid.*, p. 34-5.

<sup>162</sup> Sheepshanks became a Fellow of Trinity in 1817, studied law, was called to the bar and then took orders in the Church of England. Hoskin (1989), p. 178.

<sup>163</sup> Babbage recorded that the episode at the Admiralty took place in 1831. See *Expo.*, *Works*, Vol. 10, p. 95. Sheepshanks was known for sarcasm and belligerence. Hoskin comments that Sheepshanks 'was never happier than when crucifying an opponent'. Hoskin (1989), p. 178.

The third confrontation between the two men occurred during the notorious court case between South and the instrument makers Troughton and Simms over an allegedly defective telescope mounting supplied by the company for South's Campden Hill Observatory. The *cause célèbre* split the scientific community and the 'astronomers' war' was one of the bitterest of the century.<sup>164</sup> The ever-combative Sheepshanks was prominent in the hostilities and volunteered to act for Troughton. Babbage was lined up on the other side and testified in 1834 as an 'expert witness' in favour of South who had a reputation as an 'unpleasant maverick' and is described by Hall as 'irascible almost beyond the bounds of sanity when engaged in controversy'.<sup>165</sup> After Babbage had been cross-examined he found himself alone with Sheepshanks in the courtroom. Babbage alleged that Sheepshanks threatened him and attempted to intimidate him before the cross-examination which was to continue the following day. He alleged that Sheepshanks said that Babbage's allegiance to South made it necessary to discredit him and that 'he would at some future time, *attack me publicly on another subject* because of his support for South.'<sup>166</sup> Babbage took the 'other subject' to mean his calculating Engines. Babbage then completed the chain with the link to Airy. He asserted that through Airy's friendship and allegiance to Sheepshanks, Airy had become party to Sheepshanks' scheme to discredit him and that Airy was one of the dark forces behind the obstructions that had been placed in his path:

During the many years I have frequently found, in my communications with members of government on subjects connected with the calculating engines, difficulties on their part which remained entirely unexplained – unseen

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<sup>164</sup> For a detailed account of the 'astronomers' war' see Hoskin (1989)..

<sup>165</sup> For description of South as a 'maverick' see *ibid.*, p. 177. For Hall's description see Hall (1984), p. 30.

<sup>166</sup> *Expo., Works*, Vol. 10, p. 96. Emphasis original.

obstacles which were never alluded to, but whose existence could not be doubted . . . I have now traced the connection of the Rev. R. Sheepshanks . . . through his friend the Astronomer Royal with the Government. According to the Astronomer Royal's own statement, he was their adviser, on all scientific subjects. The Government had no other official adviser, and would scarcely have ventured to decide upon points connected with some of the most profound questions of mathematics, on their own responsibility.<sup>167</sup>

In each of the confrontations Babbage was alone with Sheepshanks. With Babbage's reputation for indignant outbursts and intemperate public protest there may be the uncharitable suspicion that resentment, anger or paranoia were the author of these episodes rather than verifiable events. However, when later challenged, Sheepshanks openly admitted the incident writing that he 'felt great contempt for Mr. Babbage's conduct, and for his mechanical and astronomical ignorance; and I expressed it very openly, and to himself'.<sup>168</sup> The following passage conveys the flavour of Sheepshanks' belligerence. During the hearing, counsel for Troughton, Starkie, had asked Babbage how often he had dined with South – this in an attempt to imply that Babbage's testimony was influenced by a personal friendship with South. Sheepshanks admits:

This irritated Mr. Babbage . . . and he came up angrily to Mr. Starkie and me, after the meeting was over, to complain. Now at the moment I too was full of wrath . . . I told him that he cried out before he was hurt, and that his cross-examination would give him far more reasons for complaint. That he had disgraced himself that evening doubly; by his mechanical ignorance, and supporting a person whom he knew to be a charlatan . . . After a little more snarling, I told him that I would expose his ignorance, and show him up; and to his reply, that he did not care, I told him that I would make him care . . . But I was most unfeignedly surprised, when at our next meeting for his cross-examination, Mr. Babbage appealed to the arbitrator for protection.<sup>169</sup>

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<sup>167</sup> *Ibid.*, p. 100.

<sup>168</sup> Hoskin (1989), p. 190.

<sup>169</sup> *Ibid.*, p. 190-1.

Sheepshanks maintained that the 'other subject' on which he threatened to attack Babbage was not the calculating engines but Babbage's failure to fully discharge his duties as Lucasian professor of mathematics at Cambridge.<sup>170</sup>

Babbage did not reside in Cambridge during his occupancy of the chair which he held between 1828-39, nor did he teach.<sup>171</sup> Resentment of what was perceived as an abuse was widespread and Airy evidently shared in this.<sup>172</sup>

In 1839 the court found against South in favour of Troughton.<sup>173</sup> The enraged South smashed the telescope and made an ostentatious show of auctioning the pieces.<sup>174</sup> Sheepshanks died in 1856. Even then this was not the end of it. So bitter was the feud that the enraged South published an attack on the Astronomical Society's obituary.<sup>175</sup>

The logic of Babbage's case against Airy reduces to a tortuous allegation that Sheepshanks was conducting a vendetta against Babbage because of Babbage's support for South (for whom Sheepshanks had expressed loathing) and that Airy was personally hostile to Babbage out of sympathy with Sheepshanks, Airy's close friend.<sup>176</sup> The loop is closed with Babbage's allegation that Airy's grudge influenced his judgements against the engines, and his advice to Government was therefore malicious.

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<sup>170</sup> *Ibid.*, p. 190. Boas Hall refers to 'the total neglect of his duties'. See Hall (1984), p. 46.

<sup>171</sup> For dates of incumbency see *CDNB*. There appears to have been a loophole in the regulations that allowed this degree of non-participation. See, Rouse Ball (1889), p. 125-6

<sup>172</sup> See, Airy to Whewell, 22 October 1830. Quoted in *GoS* p. 52.

<sup>173</sup> Hoskin (1989), p. 191.

<sup>174</sup> *Ibid.*, p. 193.

<sup>175</sup> *Ibid.*, p. 175.

<sup>176</sup> *Ibid.*, p. 179.

When it came to a shoot-out between Babbage and Airy it was no contest. By 1851, the year of the published attack, Airy's position was unassailable. He had occupied the highest office in civil science for sixteen years, had rendered countless services to government, been offered knighthoods in 1835 and 1839 (again in 1853 declining each time but accepting at the fourth time of asking in 1872), and was arguably the most eminent consultant engineer in the land.<sup>177</sup> Babbage, at sixty, ten years older than Airy, was seen as a difficult outsider, though an eminent gentleman, given to volatile outbursts, and the architect of an elaborate experiment to build calculating engines that failed at spectacular public expense.

Airy, completely secure, brushed off the attack, disdaining to make any public defence. Privately he called Babbage's book 'dull' and 'likely to command very little public attention'.<sup>178</sup> Airy's supporters rallied round. Prof. James Forbes wrote that *Exposition* was 'most damaging' to Babbage himself.<sup>179</sup> One J. B. Pentland wrote that in the first week of publication less than forty copies of Babbage's book were sold and expressed disgust that Babbage had 'vented his feelings of rabid disappointment in abusing men whose names every upright individual and every lover of Science must honour and venerate'.<sup>180</sup> Babbage's grievance against Airy was evidently sordid linen to wash in public and the squabble did him no good at all.

Airy was also amused by the attack. Astute and logical, he saw how tenuous was the chain of guilt traced by Babbage to connect a personal grudge with his

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<sup>177</sup> For offers of knighthoods see *Biog.*, pp. 112, 187, 254, 196. For evaluation of Airy's professional standing see Chapman (1988, 1992, 1993).

<sup>178</sup> Airy to Barlow, 14 June 1851, RGO6-373, f. 13.

<sup>179</sup> Forbes to Airy, 30 May 1851, *ibid.*, f. 81.

<sup>180</sup> Pentland to Airy, 6 June 1851, *ibid.*, f. 255.

professional advice to government via the convoluted route of personal allegiances and the litigation over a telescope. He listed eight links in the chain in a letter to John Barlow and parodied the drunken progression of Babbage's argument in an eight-verse prose poem called "This is the Engine which Charles Built" written along the lines of 'The House that Jack Built'.<sup>181</sup> If Airy has so far appeared as something of a dry stick – brilliant but methodical in the exercise of his professional duties – the humour and irony of the verses show him in a more human light. He was entirely secure and could afford to be amused and even mischievous.

At a political level Babbage's attack was undignified and ill-advised. But the action has an emotional dimension that perhaps makes it more explicable. *Exposition* contains passages that are more revealing of Babbage's emotional state than elsewhere in his writing, even his 'autobiographical' *Passages*, published thirteen years later. In *Exposition* he wrote of solitude and loneliness and describes the despair to which his efforts, personal sacrifices and lack of recognition had at times reduced him. He lamented the death of his wife, Georgiana, who died in 1827:

Perhaps another and yet dearer friend might exist, the partner of his daily cares, the witness of his unceasing toil; whose youthful mind, cultivated by his skill, rewards with enduring affection those efforts which called into existence her own latent and unsuspected powers. When driven by exhausted means and injured health almost to despair of the achievement of his life's great object – when the brain itself reels beneath the weight its own ambition has imposed, and the world's neglect aggravates the throbbings of an overtaxed frame, an angel spirit sits beside his couch ministering with gentlest skill to every wish, watching with anxious thought till renovated

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<sup>181</sup> Airy to Barlow, 14 June 1851, *ibid.*, f. 81. For full text of the verses see Appendix I. The title is given to the eight steps listed in Airy's letter to Barlow, 14 June 1851, *ibid.*, f. 13. The document is dated 20 June 1851. See, RGO6-452, ff. 284- 292.



nature shall admit bolder counsels, then points the way to hope, herself the guardian of his deathless frame.<sup>182</sup>

He also paid tribute to his mother and frames his quest for recognition as a means of vindicating and rewarding her trust in him:

He may look with fond and affectionate gratitude on her whose maternal care watched over the dangers of his childhood; who trained his infant mind, and with her own mild power, checking the rash vigour of his youthful days, remained ever the faithful and respected counsellor of his riper age. To gladden the declining years of her who with more than prophetic inspiration, foresaw as woman only can, the distant fame of her beloved offspring, he may well be forgiven the desire for some outward mark of his country's approbation.<sup>183</sup>

Despite despair and depression he appears never to have relinquished the conviction that he has accomplished something of significance and takes solace for the ills of the present from the prospect of the recognition of posterity:

The certainty that a future age will repair the injustice of the present, and the knowledge that the more distant the day of reparation, the more he has outstripped the efforts of his contemporaries, may well sustain him against the sneers of the ignorant or the jealousy of rivals.<sup>184</sup>

These passages are uncharacteristic of his other writing which is generally energetic, mischievous and witty, though at times pompous and wooden. These passages appear, of all places, in a book supposedly on the Great Exhibition. Babbage was in pain, and his *Exposition*, criticised as ungracious and unworthy of

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<sup>182</sup> *Expo., Works*, Vol. 10, p. 147.

<sup>183</sup> *Ibid.*, p. 146-7.

<sup>184</sup> *Ibid.*, p. 146.

him, is a rare public testament to the darker moments of his struggles. It is in the context of Babbage's emotional trajectory that his otherwise bizarre public attack on Airy becomes more intelligible.

The impression given by Airy's professional conduct from his vast correspondence, which he took extraordinary pains to catalogue, duplicate and preserve, is of a scrupulously professional conscientious civil servant, forthright and objective in all his dealings.<sup>185</sup> Certainly his 'user survey' sent to Hind at the Nautical Almanac Office and to Graham at the GRO on the potential use of the Scheutz difference engine has the appearance of coming from someone open to the views of others.<sup>186</sup> However, while framing Babbage's attack in an emotional context may make his actions more explicable, there remains the question of whether Airy was as free from prejudice as his official correspondence would indicate.

Babbage was not alone in questioning Airy's impartiality in scientific conduct. David Brewster made similar allegations. The process of refereeing papers for publication by the Royal Society was anonymous, following the introduction of the practice in 1832.<sup>187</sup> Brewster was outraged by the rejection of a paper he had submitted and suspected Airy of being the anonymous referee. He wrote to Brougham:

Altho' I am one of the oldest members, and have received all their Medals and have contributed about thirty six Papers to their Transactions, without one of them having ever been called into question or rejected yet, the Council have lately rejected an original and valuable Paper, without assigning any

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<sup>185</sup> For a description of the lengths to which Airy went to organise and preserve his papers see above pp. 215-6.

<sup>186</sup> Airy's role in the Government's purchase of the Scheutz engine is discussed in the next Chapter 5.

<sup>187</sup> Craik (2002), p. 192.

reason, and refusing to mention the name of the Reporter on whose authority this was done. The bulk of this Paper contains results and views hostile to the Undulatory Theory [of light] which seems now to be the Creed of the Society, I believe *Airy* is the person who has reported on my Paper, and who has done this entirely from personal feelings . . .<sup>188</sup>

In October 1841 Brewster had written to Henry Fox Talbot making a similar protest: 'believing as I do that Airy is the Reporter, & that his Report is influenced by personal feelings, I can see no alternative, but that of leaving the Society.'<sup>189</sup> Craik comments that the offended Brewster made no further submissions of papers to the Royal Society following this episode.<sup>190</sup> It is difficult to separate issues of personal bias with roots in *ad hominem* dislike, from professional judgement. Brewster expressed the belief that Scots were ill-treated by the Royal Society and his accusation that Airy was influenced by personal feelings may have its source in the fraught political climate of the Royal Society reforms. Morrell and Thackray comment that 'neither Babbage nor Brewster possessed great patience or affability'.<sup>191</sup> Babbage and Brewster shared Declinist views and their separate accusations against Airy would carry more weight if neither had reputations for quarrelling as a routine mode of conduct.

While Airy portrays himself as a public servant of impeccable impartiality and who, with one late exception, appears offhand in his interest in the calculating machines, a letter from William Whewell to Herschel written in October 1822, shortly after Babbage's first announcement of his invention to the Astronomical Society, signals what may have been the seeds of later rivalry:

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<sup>188</sup> Brewster to Brougham, 14 December 1841. Quoted in Craik (2002), p. 192. Emphasis original.

<sup>189</sup> Brewster to Fox Talbot, 5 October 1841. See Schaaf (1994), p. 34.

<sup>190</sup> Craik (2002), p. 192.

<sup>191</sup> *GoS*, p. 47.

You have of course heard from Peacock about Airy, a pupil of his, and certainly a man of very extraordinary talents. The reports about Babbage's machine have, it seems, excited him to attempt something of the same kind. He and another man have made a machine to solve cubic equations, but besides this he has, so far as I can make out, invented a good deal in the way of Babbage's contrivance. He is not here at present, but his friend tells me that it has got toothed wheels, working in one way for the differences and in another for the digits. If it be a similar invention to that, it is probably an independent one; for I do not know any way by which he has got any lights about Babbage's affair.<sup>192</sup>

Airy's scratch-pad journal shows a 'sketch for a computing machine (suggested by the publications relating to Babbage's), sketch of a machine for solving equations'.<sup>193</sup> However, though Airy's inventiveness and interest in mechanical engineering, instrumentation, locks and other contrivances remained active throughout his career, there is no evidence that he developed his early ideas, or that he had any ambitions of his own to design or build calculating engines, either as an intellectual exercise or for practical purposes. Apart from his later role as a consultant on issues of their utility it seems that his personal interest in the engines started and stopped with a doodle on his student notepad while he was a Cambridge undergraduate.

## Summary

Airy was first drawn into the engine debates in 1842 to help resolve a protracted and unsatisfactory negotiation between Babbage and successive Governments

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<sup>192</sup> Whewell to Herschel, 17 October 1822. Todhunter (1876), pp. 50-1. For Airy's late partial *volte face* see Chapter 5, p. 275 *et seq.*

<sup>193</sup> *Biog.*, p. 37.

about the fate of the unfinished Difference Engine No. 1. Peel sought private advice, first from William Buckland, and then from John Herschel. Airy was not consulted directly but was used purely as an intermediary in the approach to Herschel. However, in his brief to Airy, Henry Goulburn, Peel's Chancellor, casually invited Airy's views as 'an additional favour', and this seemingly throwaway invitation produced from Airy an uncharacteristic outburst of feeling and condemnation of Babbage and his engines.

Airy's revelations raise doubts about the neutrality of the Royal Society committee convened in 1823 to report to the Treasury on the probable usefulness of the machines. He portrayed Babbage as deluded about the potential benefits of the engine and hypersensitive to the point of irrationality when it came to the defence of his invention. He robustly rejected the notion that there was any need for new tables, pronounced that the engine was 'useless', and recommended that the project be abandoned. In contrast to Airy's short sharp rejection, Hershel's report was favourable, but weakened by well-meaning equivocation, length and elaborateness. The upshot was that Government finally abandoned the project on the grounds of cost.

Airy's opposition to the engines was known much earlier than the occasion on which Goulburn first invited his opinion in 1842. In the mid-1830s Airy's unsolicited pronouncements, which were damning, were recorded in hearsay reports to Babbage and others. Airy's known opposition to the machines, and Peel's expressed wish 'to get rid of Mr. Babbage and his Calculating Machine', raise the question whether the Government was disingenuous in involving Airy at all.

Airy was not alone in his opposition to the engines. Amongst those who shared his scepticism was Nils Selander, a Swedish astronomer, who argued that

the engines were unnecessary given that the important mathematical functions had already been calculated, and to exemplary levels of reliability. Further, that the priority for science was the improvement in the exactness of observational data not more accurate or more extensive tables. Selander implicitly identified the utility of the engines with the production of error-free tables and in doing so discounted the engines' theoretical and mathematical promise promoted in Babbage's early papers, but largely overlooked since. Selander's views suggested that tables were mundane instruments of science and, by implication, that engines dedicated to their production were equally mundane. His sober and authoritative views deflate the more grandiose aspirations of the engine advocates.

In 1851 Babbage published allegations against Airy accusing him of deliberately prejudicing government against the engines as part of a personal vendetta. Babbage alleges that Airy opposed the engines in sympathy with Richard Sheepshanks, Airy's friend, who was determined to discredit Babbage for siding with James South against Sheepshanks (and Airy) in a legal dispute over a defective telescope mounting. The logic of the allegations is convoluted and the publication in which they appeared, Babbage's *Exposition of 1851*, was regarded as an ill-judged act of frustration and bitterness that did Babbage more harm than good. Airy's professional standing was unassailable by this time and he brushed off the attack. He was also amused by the complexity of the chain of blame and wrote an eight-verse parodic prose poem ridiculing the drunken logic of Babbage's case.

In *Exposition* Babbage demanded that Airy to publish the grounds on which he opposed the practical utility of the engines. More importantly he declared that the question of the engines was one of 'pure science' and implied that Airy, in using practical usefulness as the sole criterion, was blind to their essential value. In

emphasising pure science Babbage was asserting that Airy's pragmatism disqualified him from acting as an arbiter of worth.

The views expressed by Airy to Goulburn in 1842 were in the nature of irritable and even angry assertions rather than reasoned technical arguments or a professional assessment and, on the strength of Airy's responses in this consultation alone, it is impossible to come to a conclusion as to whether or not his professional opinion was coloured by personal feelings. Airy was consulted on at least three subsequent occasions, and the question of whether his professional views were influenced by personal animosity is revisited later in the light of the case studies discussed in the next chapter.

## Chapter 5: Airy , the Scheutzes, Fowler and Bell

*Its arithmetical music had to be elicited by frequent tuning and skilful handling*

– William Farr, 1864

### Introduction

Airy was consulted by government on three further occasions for his views on automatic calculating machines. He was also petitioned by individuals seeking scientific endorsement for new mechanical calculating aids and devices. In the case of the three government consultations the objects of interest were the difference engines by Georg Scheutz (1785-1873), a Stockholm printer, publisher and journalist, and Edvard (1821-1881), his son.<sup>1</sup> Airy's views solicited in these consultations are particularly revealing of the reasons for his opposition to calculating devices, Babbage's included. With one exception his pronouncements were consistently damning.

The middle decades of the century represent a period of prolific invention, and inventors and entrepreneurs seeking scientific endorsement approached Airy for his views on new manual calculating aids, that is, devices that partially automated the process of calculation. Two such devices are considered here, one by Thomas Fowler, that came to Airy's attention in 1840, the other by William Bell who approached Airy in 1849 for an opinion on his calculator for continuous floating-point multiplication and division.<sup>2</sup> Neither of these devices features in

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<sup>1</sup> For a biographical account of Georg Scheutz's life see *G&F* pp. 82-97; for a brief biography of Edvard see *ibid.*, p. 116.

<sup>2</sup> I am indebted to Mr. Murray Laver who first drew my attention to Thomas Fowler in 1989. As far as I am aware the first description of the Fowler episode in the modern canon appears in my



standard accounts of the history of mechanical calculation: Fowler's calculator did not progress beyond a few experimental prototypes, and Bell's machine made little impact. However, Airy's views, recorded in correspondence with the hopeful petitioners, give insights into the generic criteria by which he judged the utility of machine-assisted calculation.

Lindgren's *Glory and Failure* provides a detailed study of Scheutzes' machines, namely, the prototype completed in 1843, the fully engineered version completed in Stockholm in 1853, and the copy made for the General Register Office in London, in 1859.<sup>3</sup> His study excels in its technical and economic analysis and in the use of Swedish sources in addition to those in England, particularly the Treasury papers at the PRO (Kew), and the account below draws heavily on Lindgren's work.<sup>4</sup> However, his analysis did not benefit from relevant material from two additional major primary archival sources, namely, Airy's papers in the Greenwich Royal Observatory archive at Cambridge University Library, and the Letter Books and the Registrar General's Private Letter Book at the PRO (Kew), which record the detailed inward and outward correspondence between the GRO and the Treasury.<sup>5</sup> These new sources bring to light two new revealing episodes: Airy's unsolicited views on the utility of the first Scheutz engine exhibited in London in 1854-5, and an instance of Airy's enthusiasm for the second Scheutz engine built

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essay "It will not slice a pineapple: Babbage, Miracles and Machines". See Swade (1996). For reference to Fowler see pp. 39-40.

<sup>3</sup> The convention adhered to here is to refer to the 1843 machine as 'the prototype', the first fully engineered version built in Stockholm as 'the first Scheutz engine', and the GRO copy as 'the second Scheutz engine'.

<sup>4</sup> Lindgren's main sources are the Treasury papers T1-series. This source has been severely weeded by PRO staff. See *G&F*, p. 331, Note 104.

<sup>5</sup> The inward and outward Letter Book references are RG29-1 through RG29-8; the Registrar General's Private Letter book is RG29-17, 27 October 1837 - 16 June 1850, PRO (Kew).

for the GRO that represents a sudden reversal of his consistently hostile attitude to the machines.

This chapter provides accounts of the occasions on which Airy was consulted on the Scheutz, Fowler and Bell machines, and completes the case studies in which Airy expressed his views on mechanised calculation.

### The Scheutz Prototype

Georg Scheutz, like Babbage, was a liberal reformer, committed to free trade and the social benefits of technology. He edited, translated and wrote for journals and technical magazines to disseminate information in attempts to influence social, political and economic development.<sup>6</sup> He was also an innovator who improved on existing technology and filed for several patents for new inventions. His innovations include an advanced high-speed printing press, chemical dyes, a steam turbine, an optical instrument for copying, a drawing apparatus, and a method of brick making.<sup>7</sup>

Inspired by Lardner's article on Babbage's Difference Engine, in the *Edinburgh Review*, July 1834, Scheutz started to design his own difference engine. The machine was based on the same mathematical principle as Babbage's (the method of finite differences) but the mechanisms were of his own design, as Lardner's article did not include specific details of Babbage's. The prototype was completed in 1843 by his son Edvard who started the project in 1837 while a

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<sup>6</sup> See *G&F*, p. 92.

<sup>7</sup> For details of Scheutz's inventions see *Ibid.*, pp. 93, 95, 318.

fifteen-year-old engineering student at the Royal Technological Institute in Stockholm.<sup>8</sup> The machine had three orders of difference and printed results to five decimal places. More significantly, it incorporated an integral printer mechanically coupled to the calculating mechanism as intended by Babbage, and the whole apparatus ranks as the first automatic printing calculator. Unlike Babbage's Engine, which had the benefit of the best of British engineering, the first Scheutz prototype was constructed in a wooden frame and built using hand tools and an elementary lathe by a young inexperienced engineering technician.

Airy was drawn into the affair when the Scheutzes attempted to sell their invention to the English Government in October 1843, just under a year after Peel had decisively axed Babbage's engine project.<sup>9</sup> Using a testimonial from three members of the Swedish Royal Academy of Sciences as a quasi-official endorsement, Scheutz offered a fully engineered difference engine to the English Government.<sup>10</sup> It is unclear whether the fate of Babbage's engine was a factor in the timing of Scheutz's initiative. It was certainly widely known on the Continent that the construction had been at a standstill since 1833. Scheutz's proposal was

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<sup>8</sup> Merzbach (1977) gives February as the completion date (see p. 9); Lindgren gives the end of summer (*G&F*, p. 123). For an account of the history and construction of the Scheutz prototype see *G&F*, pp. 112 *et seq.* The wooden-framed machine was rediscovered in the collections of the Nordiska Museet, Stockholm, by Lindgren in December 1979 (*G&F*, pp. 372-378). Until Lindgren's *G&F*, Merzbach's monograph (1977) was the standard reference on the Scheutz machines.

<sup>9</sup> Georg Scheutz's letter of offer is dated 31 October 1843 (*G&F*, p. 128). Babbage's final meeting with Peel was on 11 November 1842.

<sup>10</sup> The report is dated 18 September 1843 and is signed by the three Academy members. Lindgren emphasises (*G&F*, p. 126, 329 Note 81) that the three signatories did not constitute an official Academy committee. Despite this their findings have repeatedly been attributed to the Academy (*LEC* p. 228; Merzbach (1977), p. 9; *Specimens* see *Works*, Vol. 2, p. 199). The testimonial was more in the nature of a private pronouncement solicited by Georg Scheutz to establish the priority of the device and as a promotional endorsement. For English translation from Swedish of the full report see *G&F*, pp. 124-5. The machine offered to the English Government featured seven orders of difference and seventeen figures. See *G&F*, p. 128.

forwarded to the Home Secretary, Sir James Graham (brother-in-law of George Graham, Registrar General) via the Swedish ambassador in London.<sup>11</sup>

After the protracted debacle of Babbage's engine that had come to a head the year before, James Graham sought advice. He appointed an advisory committee to consider the Swedish offer and Airy was approached to officiate. The committee included Sir William Symonds (Surveyor of the Navy), Airy (Astronomer Royal) and an 'unidentified professor' at London University.<sup>12</sup> Lindgren speculated that the unknown professor may have been Babbage and conducted a literature search of the Babbage correspondence without result.<sup>13</sup> Babbage did not hold office at London University though he was offered the chair of Higher Mathematics and Mathematical Physics in 1827. He declined (as did Herschel who was offered the post shortly after) and took the Lucasian chair at Cambridge the following year. The successful candidate for the first mathematics chair of the new university was Augustus de Morgan who served with distinction from 1828 for thirty years (excluding 1831-1836) and de Morgan was almost surely the mystery professor.<sup>14</sup>

Official records of the proceedings of this committee and of its findings appear to have been destroyed and the only account relies on an autobiographical entry in a dictionary of Swedish scientists where Scheutz recorded that his offer was rejected by the Treasury on the grounds that Parliament would be unlikely to

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<sup>11</sup> George Graham was the recipient of a patronage appointment as Registrar General in 1842, succeeding Thomas Lister. He served from 1842 to 1879. See Eyler (1979), p. 46. The original proposal appears to have been disposed of during a weeding process at the PRO (Kew) The date of the letter was taken from an annotation on the envelope that did survive. See *G&F* p. 331, note 102, and Note 104 for Lindgren's efforts to locate the missing papers.

<sup>12</sup> William Symonds (1782-1856), Surveyor of the Navy, 1832-1847. *CDNB*.

<sup>13</sup> *G&F*, p. 331, Note 104.

<sup>14</sup> For an account of de Morgan's appointment and service see Rice (1996, "Inspiration or Desperation?").

support a foreign invention of the same kind as the English difference engine, which had already proved so costly. In the absence of any official record of the committee's findings, or official or personal correspondence, Airy's view of Scheutz's proposal remains unknown and his influence on the outcome equally so.<sup>15</sup>

This episode offers little insight into Airy's views on calculating engines. However, it is worth noting that the source of expertise drawn on by government was not the Royal Society as in the first three Treasury consultations on Babbage's engines, or an individual, as in the case of the fourth, but an *ad hoc* advisory committee. Further, that Airy was among the first in country to learn of the Swedish difference engine, and his involvement may have served as a marker for the substantial role he was subsequently to play.

### The Third Government Consultation

The third occasion on which Airy was consulted by government is uniquely revealing of his role as arbiter of utility. The question at issue in this case was whether the Treasury should fund the purchase of a Scheutz difference engine for use by General Register Office (GRO) and for possible use in other government departments.<sup>16</sup> Airy's opinion was sought by the Treasury in July 1857 and the

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<sup>15</sup> Lindgren's search of the Peel-Graham correspondence yielded no reference to the committee of its findings. See *G&F*, p. 332, Note 111. The likelihood of de Morgan being the 'unidentified professor' suggests further searches for exchanges between Airy, de Morgan and Symonds.

<sup>16</sup> For a detailed history of the Swedish engine see *G&F*. Accounts in *LEC* and Merzbach (1977) do not have the benefit of Lindgren's Swedish sources.

episode has several distinctive features.<sup>17</sup> In the Herschel-Goulburn episode Airy was an intermediary and an informal supplementary advisor. In the case of the first Scheutz offer in 1843, he was member of an advisory committee and operated in collaboration with others. In this third consultation, though he polled two others for their professional opinions, he was the sole consultant, and his report to the Treasury was neither mediated nor blurred by the authority of others. Further, in the two previous consultations the identity of the eventual users of the machine was never entirely clear: in the 1842 consultation it was at no stage specified under whose superintendence or under whose auspices Babbage's engine would operate and which specific constituencies of the scientific or government community would benefit. Little is known of issues invoked by the first Scheutz offer in 1843 but the likelihood is that the supposed benefits would be the familiar ones rehearsed by Lardner: the importance of error-free tables for astronomical navigation, mixed in with rhetoric about the grand enterprise of science, but with the identity of the specific beneficiaries obscured in the virtue of the appeal. However, in the case of the 1857 consultation, Airy was invited to comment specifically on the potential use of the engine in three named Public Offices: the General Register Office, the Nautical Almanac Office, and the Royal Observatory for which he was responsible as Astronomer Royal.

In the case of Babbage's engine the relationship between government and the scientific community, represented through the Royal Society, was culturally complex. Government was in one sense servant of the public good, but master in that it held the purse strings. The gentlemen of science who populated the Royal Society, on the other hand, were 'masters' in the sense of being part of the social,

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<sup>17</sup> For details of Airy's brief see Trevelyan to Airy, 8 July 1857. RGO6-454, f. 426.

political and intellectual elite whose class populated both houses at Westminster, but were financial supplicants when it came to funding grand projects. The ambiguity of the master-servant relationship in the representational politics of the Babbage episode was absent in the 1857 consultation: as Astronomer Royal, Airy was a paid servant of government, the three candidate beneficiaries of the engine were government departments, and the paymaster was the Treasury. The reporting structure and the lines of accountability were part of a well-defined civil service hierarchy, free of the structural ambiguities of society overall.

To connect Airy and the second Scheutz engine it is necessary to bridge the eleven-year gap between 1843 and 1854 and relocate from Stockholm to London. The failed attempt by the Scheutzes to secure the sale of a difference engine to the British Government in 1843 was followed by a similar failure to secure support from the Swedish government to build an extended engine in metal.<sup>18</sup> After six years the Scheutzes renewed their attempts to secure state support for a new engine. In a submission to King Oscar on 28 January 1851 Georg Scheutz appealed for a relatively modest sum (the equivalent of £283), about a third of his earlier bid, to enable him to build a serviceable prototype, and to cover travel while promoting the machine to foreign institutions that had a mandate to produce tables on a routine basis.<sup>19</sup> The proposal became enmeshed in political tensions between the palace and the Swedish parliament – the four-chamber Riksdag. As a result of a leakage between the worlds of science and politics Scheutz's application became an issue of contention in the power struggle between conservative factions in the Riksdag,

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<sup>18</sup> See G&F, pp. 130-133.

<sup>19</sup> G&F, p. 159.

largely supportive of the King, and liberal factions close to Scheutz's.<sup>20</sup> The proposal was successfully championed by radical liberals and the funds were sanctioned.

However, there was a set of binding conditions jointly imposed by the Palace and the Swedish Academy of Sciences that contrast starkly with the vague and amateurish terms in which Babbage received government support thirty years earlier. The Swedish machine would need to be completed and operational by a fixed deadline (the end of 1853) or the grant would need to be repaid; instalments would only be paid subject to the Academy satisfying itself that prior payments had been expended to their proper purpose; and that payments only be made against personal security signed for by Scheutz himself. The safeguards protecting this relatively trifling capital investment appear excessive, particularly when compared to the offhand manner of the British Governments' grants to Babbage: these were vast in comparison; the commitment was financially open-ended with no completion deadline; and the payments were not conditional on the success or otherwise of any physical result.

The uncompromising conditions of the grant had the desired effect. The engine was completed by the end of October 1853 in the workshop of Johan Wilhelm Bergström, a prominent Stockholm engineer-craftsman, and one of the fifteen burghers who stood security for Scheutz.<sup>21</sup> The machine was an expanded version of the 1843 prototype, but this time professionally engineered, in metal. It

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<sup>20</sup> For a description of the circumstances and complex dynamics of this episode see *G&F*, pp.162-166.

<sup>21</sup> See *G&F*, pp. 168-170



calculated with four orders of difference to fifteen decimal places and printed results to eight figures.<sup>22</sup>

The bid for state funding had included provision for promoting the machine abroad and with the engine complete the Scheutzes set about finding a foreign buyer. For the Scheutzes England represented a mature market: the British Government's generosity towards Babbage was seen abroad as official acceptance of the value of calculating engines, and it was well known in Europe that the English project was long since defunct.<sup>23</sup> Funded by a Swedish nobleman, Pehr Sparre, who had a commercial interest in the machine, the Scheutzes and their engine arrived in London on 30 September 1854 and they applied for an English patent for the machine on 17 October.<sup>24</sup> The machine was exhibited in Bermondsey at the workshops Bryan Donkin & Co. an established engineering firm known for the manufacture of papermaking machinery.<sup>25</sup> On the urging of the engineer, William Gravatt, who examined the engine while it was on display at Donkin's establishment and who became an avid advocate and demonstrator of the machine, the engine was transferred to the Royal Society, Somerset House, shortly after 16 November 1854 where it remained, again at Gravatt's request, after the Scheutzes' return to Stockholm early in 1855.<sup>26</sup>

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<sup>22</sup> *Ibid.*, p. 171.

<sup>23</sup> See *G&F*, 185; Merzbach, p. 17. Lardner's influential article reported in 1834 that the construction project was at a standstill. Also Babbage's lectures on the engines at the symposium in Turin will have covered the state of progress. Babbage had a wide network in Europe and was honoured by many of its academies. A bound volume of certificates and awards in the Science Museum Library has thirty certificated honours of this kind. (See Swade (1991), *Science Museum*, p. 43). News of the English engine project was disseminated through Babbage's network of foreign correspondents.

<sup>24</sup> For date of arrival see Scheutz (1857), p. xii. For date of patent see *G&F*, p. 186.

<sup>25</sup> Scheutz (1857), p. xii.

<sup>26</sup> For transfer to the Royal Society see *G&F*, p. 187; Merzbach, p. 19; *Specimens*, xiii. None of these accounts cite the date of the transfer.

Here the machine became a learned curiosity for the dignitaries of the scientific establishment, to whom it was demonstrated and expounded upon by the willing Gravatt. The Scheutzes recorded with evident pride that viewing parties, hosted by Gravatt, included luminaries of the day as well as officers and Fellows of the Society. He specifically mentions Colonel (later General, Sir) Edward Sabine, Roget (physician, savant and originator of the eponymous Thesaurus), Faraday (famous electrician), John Russell Hind (astronomer and superintendent of the Nautical Almanac), George Stokes (Lucasian Professor of Mathematics), Charles Wheatstone (physicist and inventor), and William Hallowes Miller (mineralogist and crystallographer).<sup>27</sup> The engine was also newsworthy to the wider public. Prince Albert was received at the Royal Society by Donkin and Gravatt on the morning of Monday 25 June 1855 and the engine was royally inspected.<sup>28</sup> The occasion was covered in *The Illustrated London News* of the following Saturday in a near-full page spread featuring a large engraving of the full engine, a smaller engraving of a single addition and carry mechanism, and a fulsome description of its wonders, one of which was that the device required such modest power so as to be within the physical compass of a small turnspit dog.<sup>29</sup>

Babbage was an early contact for the Scheutzes through a letter of introduction provided by Wheatstone.<sup>30</sup> The Scheutzes had had misgivings about the reception they might expect from Babbage whom they feared may see them as

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<sup>27</sup> *CDNB*; *G&F*, p. 189.

<sup>28</sup> The date of Monday 25 June 1855 is taken from the date of *The Illustrated London News*, Saturday 30 June 1855, p. 661 which reported that the visit had occurred on the previous Monday. *Specimens* (p. xiii) gives the date as 29 January 1855, and this is repeated by Collier (*LEC*, p. 232).

<sup>29</sup> The large engraving was used as the frontispiece of *Specimens* and is acknowledged on p. xv.

<sup>30</sup> Lindgren fixes the likeliest date of the first meeting between Scheutz and Babbage as 30 November 1854. *G&F*, p. 188.

competitors or even intellectual plagiarists.<sup>31</sup> To the Scheutzes' apparent relief Babbage received them warmly, showed them his house, his workshops and partially completed engine, spent two days exploring the Scheutz invention, and energetically promoted their interests thereafter.<sup>32</sup>

Airy has his Say

Airy is so far curiously absent. He was by now well-established as Astronomer Royal after twenty years in the post, was director of the Greenwich Observatory, the instrumentation and published output of which had become the envy of its European counterparts. He was, in addition, scientific advisor to Government, pre-eminent consultant engineer, Fellow of the Royal Society since 1836 and fully deserving of top billing on the Scheutzes' client list as the most prized potential customer.<sup>33</sup> However, his name is not included in Scheutz's roll of honour naming those who came to pay their respects at the altar of invention. He was also not included in the four-man committee appointed by the Council of the Royal Society to examine the Scheutz machine relatively early in the engine's presence in London, before its transfer to *l'Expositions Universelle* in Paris in August of that year (1855).<sup>34</sup> It is highly likely that that Babbage, in his capacity as intellectual host

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<sup>31</sup> See *LEC*, p. 238.

<sup>32</sup> *G&F*, p. 189.

<sup>33</sup> Babbage later commented that it was 'extraordinary . . . that the Astronomer Royal did not become the most enthusiastic supporter of an instrument which could render such invaluable service to his own science'. *Passages*, p. 140.

<sup>34</sup> For membership of the committee see Stokes (1855), p. 268. Reprinted in H. P. Babbage (1889). Stokes chaired the committee. In Paris the machine won a gold medal apparently by unanimous decision of the jury. See *G&F*, pp. 193, 196; Merzbach (1977), p. 20; Farr (1864), p. cxli;

and strategic adviser to Scheutzes' mission to market the engine, had warned the Scheutzes of Airy's known antagonism to the engines and contrived with Gravatt to ensure that Airy was kept well out of the way.

Airy had not yet featured overtly in the celebration of the engine nor in the official exercise to evaluate its capabilities. But he had by his own account examined the engine and was moreover moved to register his views in print. The *Philosophical Magazine* for July 1856 carried a transcription of the report of the Royal Society committee appointed to examine Scheutz's engine. Airy took the opportunity of making his views known in the form of a letter to the Editors and his unsolicited comments are highly revealing of the technical grounds for his consistently expressed scepticism for practical utility of calculating engines.

The Royal Society report, submitted by George Stokes, the Committee's chairman, is soberly factual, mathematically detailed and free of glorifying rhetoric. It commends the machine as working 'with the greatest freedom and smoothness' but warned that because moving parts relied on friction to retain their positions, the machine was susceptible to 'wilful derangement'.<sup>35</sup> The report concludes that the main use of the machine would be for mathematical tables but that 'the most important of such tables have long since been calculated' implying that the machine was largely redundant.<sup>36</sup> However, the closing statement of a respectful but unenthusiastic report, redeems the machine in a concessionary way:

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*Specimens*, p. xiii (which reports that the medal was presented by Prince Charles on 21 April 1856). Also "The Swedish Tabulating Machine of G. & E. Scheutz." *Annals of the Dudley Observatory*. Albany, 1866. 116-125. Vol. I, p. 117.

<sup>35</sup> H.P. Babbage (1889), p. 265. Quoted in *LEC*, p. 190. Babbage's designs incorporated security and locking devices to prevent derangement. See Chapter 3, pp. 130-1.

<sup>36</sup> For similar view expressed by Selander see Chapter 4, p. 220.

It has been suggested to us too, and we think with good reason, that the machine would be very useful even for the mere reprinting of old tables, because it could calculate and print more quickly than a good compositor could set the types, and that without risk of error.<sup>37</sup>

That the Report took the machine seriously was perhaps its strongest endorsement. Airy, possibly encouraged by the reservations of the Committee, volunteered his views by writing to the editors of the *Philosophical Magazine* and his letter was published in the August issue.<sup>38</sup> Having established that he had examined the engine 'shortly after its arrival in this country' he states:<sup>39</sup>

I cannot refrain from expressing my general admiration of the beauty of its arrangements and my assurance that ... it can be constructed at small expense. I am also impressed with the ability and accuracy of the Committee's Report. But I wish to guard myself from giving an opinion on the utility of the machine; remarking only that, as I believe, the demand for such machines has arisen on the side, not of computers, but of mechanists.<sup>40</sup>

The allegation is clear: the stimulus for the machines derived not from deficiencies in existing computational methods but from the enthusiasm or entrepreneurial motives of technologists. Airy suggests with implied disdain that the engine advocates were ignorant of the real needs of tabular calculation:

<sup>37</sup> H. P. Babbage (1889), p. 268. Quoted in *LEC*, p. 190.

<sup>38</sup> Airy (1856). A copy is filed in RGO6-813, f. 193-4.

<sup>39</sup> Airy inspected the engine on Tuesday 5 December 1854. His diary entry for that day reads 'I went to Donkin's factory, Bermondsey, to see the Swedish Machine'. See *Astronomer Royal's Journal*, Jan 1848 – December 1861, RGO 6-25. Airy makes at least two other references to his first inspection of the engine: Airy to Trevelyan (Treasury Secretary), 30 September 1857 (T1/B/19264, PRO (Kew), letter press copy in RGO6-454 ff. 485-492) where he wrote that he 'had the advantage of very carefully examining the Machine with the assistance of Mr. Gravatt, when it was lodged at Messrs. Donkin's factory' and that 'he was personally well-acquainted with it'. Also, Airy to Graham, 31 August 1859, RGO6-456, f. 268.

<sup>40</sup> Airy (1856), p. 225.

Permit, me however, to point out to you a course of computations, of a most mechanical and monotonous kind, which is going on frequently in every place of extensive and systematic calculations, and to which I think the attention of constructors of mechanical computing machines might advantageously be directed. An immense number of computations consists of parts following each other in the following order: – <sup>41</sup>

He then listed the four stages entailed in many manual computations and the respects in which each stage might or might not benefit from mechanisation. The first stage involved independent manual calculations for the widely spaced pivotal values, and the example he cited is for lunar tables. These he states ‘can never be dispensed with’ making the point that the starting values of a machine computation are irreducibly dependent on manual methods. The second stage was verifying the pivotal values by taking differences, that is, checking by repeated subtraction of successive pairs of values. This process Airy conceded is ‘well adapted to mechanical action’ and he suggests that the machine might be modified for this purpose:

Speaking (for the present) without mature consideration, I should conceive, that, by reversing the order of the figures on some of the wheels, and with some other instrumental changes, and by feeding the proper wheels continually with new computed numbers instead of new differences, the additive operations of M. Scheutz’s engine might be made subtractive; and the new final result in every case might be a fourth difference instead of a number formed by fourth differences . . . If this could be done, I really believe that a very important benefit would be conferred on computers. <sup>42</sup>

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<sup>41</sup> *Ibid.*

<sup>42</sup> *Ibid.* p. 225-6.

Airy was suggesting that the machine be used not to generate new tabular values by repeated addition as suggested by the engine advocates, but to feed existing tabular values and then verify their correctness by repeated subtraction and thereby to use the machines for error-detection for already computed tables rather than tabulation *ab initio*.<sup>43</sup> Babbage nowhere mentioned the idea of verification by mechanical differencing and Wilkes is incredulous that Babbage could have been blind to this application of his engine.<sup>44</sup> Airy's suggestion was both revolutionary and, in the light of later twentieth century practice, as it happened, farsighted.<sup>45</sup>

The third process mentioned is the preparation of first and second order differences for subtabulation. This Airy says could be done by machine but the simplicity of the calculation barely warrants it. Finally, the differences from the third stage were used for subtabulation. Airy concedes that these 'would be effected by a very simple difference-engine' implying that the Scheutz engine was more elaborate than necessary.<sup>46</sup>

With the exception of Airy's new suggestion of verification by mechanical differencing, his assessment was that the benefits of the machine were marginal and barely worth the fuss, and in general his views are hostile to the Utopian notion of a handle-cranking solution to the error-free production of mathematical tables.

Airy's letter to the editors of the *Philosophical Magazine* published in 1856 represents the first public expression of reasoned arguments for his opposition to

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<sup>43</sup> Airy repeated the proposal to use difference engines for verification in a letter to William Gravatt (Airy to Gravatt, 2 September 1859. RGO-456, f. 272.). See below p. 281. Also in a letter to J R Hind. See below p. 266.

<sup>44</sup> For Wilkes' criticism of Babbage missing this see Chapter 2. pp. 79-80.

<sup>45</sup> Comrie pioneered the verification by mechanical differencing using commercially available desktop calculators at the Nautical Almanac Office in the 1930s. See Comrie (1931)(1933). The process was major development in automated tabulation.

<sup>46</sup> Airy (1856), p. 226.

the engines, this some fourteen years after his largely unargued condemnation of Babbage's engine to Goulburn. It is difficult to say to what extent the clarity of the views expressed in the published letter underpinned the views he expressed to Goulburn, or whether his technical arguments were developed to support a prejudice against the machines.

In his letter to the editors Airy appears concerned to signal that he was writing in his official rather than his private capacity. The evidence for this is in the conventions he adopted as signatory for his various works. His printed papers number 518 items in all, though this tally includes official reports, parliamentary returns, committee evidence and lectures.<sup>47</sup> While the majority of his printed papers cover scientific topics, primarily astronomy and astronomical instrumentation, there are several that are outside his professional interests and remit. These include works on bible interpretation ('Exodus of the Israelites', 'The Deluge', 'The Land of Goshen'), constitution of Cambridge colleges, the Roman invasion of Britain, marriage odes, translation of Virgil, the position of the Blue-coat Girls' School (Greenwich), a speech by Nelson, and an interpretation of an obscure passage in the Koran.<sup>48</sup> In his scientific writing he invariably identifies himself as Astronomer Royal and when his name appears in such papers it is usually in the form 'G. B. Airy'. However, when writing in his private rather than his official capacity, he took to reversing his initials and signed himself 'A.G.B'.<sup>49</sup> In his letter to the editors expressing his views on the Scheutz engine Airy identifies himself as 'G. B. Airy,

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<sup>47</sup> For itemised breakdown by category see *Biog.*, p. 372. For full listing by title and date see *Biog.*, pp. 373-403.

<sup>48</sup> See *Biog.*, "Printed Papers by G. B. Airy", pp. 373-1. For bibliographic details of non-scientific papers mentioned above see (in order), *Biog.*, pp., 380-1, 392, 385, 386, 390, 389, 391, 398, 402.

<sup>49</sup> See for example Airy (1858), p. 587.



Esq., Astronomer Royal' evidently wishing to register that he was not writing in his private capacity but that his views reflected his official position.<sup>50</sup>

The second Scheutz engine was transported from London to Paris in 1855, and its official reception in France was as jaundiced as that given to it in London. After an initial success at the Paris Exhibition where it won a gold medal it was transferred, in December 1855, to the Paris Observatory by order of Napoleon III for evaluation in day-to-day use.<sup>51</sup> On the recommendation of the Observatory's director, Urbain Jean Joseph Leverrier, the purchase of the engine was rejected by the French Government. He wrote that the machine 'only did by itself a fifth of the work necessary for the calculating the tables, and this fifth, it did less quickly than an ordinary calculator'.<sup>52</sup> At the end of July 1856 the engine was returned to London and located in Gravatt's home.<sup>53</sup> The Scheutzes continued in their efforts to market the machine and prepared a promotional booklet, *Specimens of Tables, Calculated, Stereomoulded, and Printed by Machinery*, containing sample tables, printed from stereotyped plates produced by the machine, for wide circulation to potential customers.<sup>54</sup> The engine itself was finally sold for £1,000 in January 1857 to the Dudley Observatory in Albany, New York, with funds provided by an Albany businessman, John F. Rathbone.<sup>55</sup> The machine arrived in Albany in April 1857

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<sup>50</sup> Airy (1856), p. 225.

<sup>51</sup> G&F, pp. 193, 196; Merzbach (1977), p. 21.

<sup>52</sup> Quoted in G&F, p. 197.

<sup>53</sup> *Ibid.*, p. 203.

<sup>54</sup> Scheutz and Scheutz (1857). The booklet was prepared by Edvard Scheutz, Gravatt and Babbage (G&F, p. 203); its authorship is cited as 'anon' in *Works*. For a brief discussion of the distribution of *Specimens* see G&F, p. 208. For distribution list see G&F, Appendix 2, pp. 379-388.

<sup>55</sup> G&F, pp. 203, 205, 206. The dollar price was \$5000. See "The Swedish Tabulating Machine of G. & E. Scheutz." *Annals of the Dudley Observatory. Albany*, 1866. pp. 116-125. Vol. I, p. 117.

and reports vary as to whether it was ever put to productive use.<sup>56</sup> In any event the machine had departed the European arena and was effectively consigned to trans-Atlantic oblivion.

### William Farr and the Second Scheutz Engine

The hole left by the engine in London was soon filled by a new initiative to provide an engine for the General Register Office that had responsibility for analysing official records of births and deaths, as well as data for decennial national censuses.<sup>57</sup> The main protagonist of the new initiative was William Farr, who went by the unusual title of 'compiler of abstracts' and superintendent of the Statistical Department.<sup>58</sup> He was *de facto* the GRO's chief statistician, and the processing and tabulation of data were his daily fare.

Farr's background was impoverished. After a medical training in provincial Shrewsbury and then in Paris, he returned to practice in England. The medical profession was hierarchical and competitive, and without wealth, connections or

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<sup>56</sup> *G&F*, p. 209. Merzbach provides details of the machine's transatlantic afterlife at Albany and discussions about its use for tabulation under contract to government (Merzbach (1977), pp. 27-28). Farr observes: 'no account has reached us of any work executed by it in America' (Farr (1864), Appendix, p. cxxxix, footnote). Lindgren notes that the machine was not used after Gould's dismissal from the Dudley Observatory in July 1858 (*G&F*, 227). Grant noted in 1871 that the Albany machine 'has been but little used' (Grant (1871), p. 1, RGO6-459, ff. 275-277); Comrie notes, 'there is no record of any useful work being done by it' (Comrie (1931), p. 6). The Albany Scheutz machine, after being lodged with the Felt and Tarrant Manufacturing Company, was eventually transferred to the Smithsonian Institution where it resides to this day. For a revisionist account of the use of the engine at the Dudley Observatory see *G&F*, p. 282-4.

<sup>57</sup> For contextual background to the increasing pressure on the GRO for expanded tables following the Registration Act of 1836 see Chapter 1, pp. 36-7. For role of vital statistics and the development of the census in Britain see Glass (1973).

<sup>58</sup> Eyler (1979), p. 9. He was permanently appointed on 10 July 1839 though he appears to have been in charge of the statistical department before this date. See *ibid.*, pp. 47 and 207 Note 49.

gentlemanly graces, Farr struggled.<sup>59</sup> He turned instead to medical journalism and statistics, and finally abandoned medicine to join the General Register Office in 1839, where he served as a career civil servant for forty years.<sup>60</sup> He was committed to the use of statistics as an instrument of social reform and was tireless in his efforts to alleviate the plight of the poor through improved public health.

Farr was an imposing figure, forthright and dedicated, with a formidable reputation as England's foremost medical statistician. Such was his influence that he was often mistaken for the Registrar General. By the mid-1850s Farr had vast experience in the preparation of tables of life expectancies, annuities, insurance premiums, interest payments, as well the collation, statistical analysis and tabulation of census data following the 1836 Registration Act.<sup>61</sup> He had plans for an ambitious expansion of the English Life Table, and the Scheutz engine represented a means of preparing the information in time without the need for additional clerks and computers. The needs of the GRO represented a new problem of production and supply, and Farr's advocacy for the Scheutz machine extended the engine question beyond pure science and into the practicalities of statistics and civil information management. The engine question acquired a new seriousness outside the community of astronomers and mathematicians to which it had been confined.

The distribution list of the fifty-page promotional booklet, *Specimens* includes Farr's name.<sup>62</sup> The list, compiled by Babbage and Edvard, contains some four hundred addressees, including major international observatories, academic and military institutions, savants, prominent individuals, astronomers, aristocrats,

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<sup>59</sup> Eyler (1979), pp. 1-2.

<sup>60</sup> *Ibid.*, p. 9.

<sup>61</sup> See Cullen (1979), p. 37 *et seq.*

<sup>62</sup> *G&F.*, p. 207.

diplomats, patrons of arts and sciences, and sundry interested and/or influential personages. Farr's inclusion on the mailing list was not his first connection with differencing engines. In 1843 he had registered the potential of Babbage's engine to relieve the tedium of repetitious calculations performed in the preparation of his first life table fifteen years earlier.<sup>63</sup> He had also written to Babbage in 1852 suggesting that an extensive life table he had prepared might be verified by Babbage's machine, and enquiring at the same time whether Babbage's engine was capable of tabulating joint lives.<sup>64</sup>

While the second Scheutz engine was on display at the Royal Society before its transfer to Paris in August 1855, Farr had the attendant, probably Gravatt, run a test calculation and stamp the results on a stereotype mould.<sup>65</sup> The results were to Farr's stated satisfaction.<sup>66</sup> As a working statistician at the business end of statistical computation, Farr was unimpressed by ornamental curiosities without practical benefit. Reflecting retrospectively on the use of the Scheutz engine at the GRO he wrote:

Now there are, besides the thousands of machines in the clouds of inventors' brains, many ingenious and beautiful machines in exhibitions of no practical use whatever. How can the spectator know whether they will execute genuine work at all? ... A watch to look at is sometimes not a watch to go, according to common observation.<sup>67</sup>

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<sup>63</sup> Cullen (1979), p. 218 Note 3.

<sup>64</sup> *G&F*, pp. 212, 353 Note 3.

<sup>65</sup> *G&F*, p. 193.

<sup>66</sup> Farr to Graham, 20 August 1857. RGO-454, f. 463.

<sup>67</sup> Farr (1864), Appendix, p. cxxxix.

The production of *Specimens* signalled tangibly, as it was intended to do, the completion of a technically viable machine for calculation and tabulation that demonstrated that the Scheutz's engine was not one of the thousand impractical fictions in inventors' minds. The simultaneous sale of the Swedish machine to the Dudley Observatory, a new flagship institution with ambitions to become the 'American Greenwich', was a forceful endorsement by a wealthy scientific institution of the machine's supposed capabilities.<sup>68</sup> As Farr subsequently recounted, these events encouraged him to believe that the time had come to put the unfulfilled promise of such machines to the test:

Here were calculating machines in which everybody was interested . . . in which money had been invested for nearly forty years, and which in the completed state had hitherto realised none of the expectations which the country naturally entertained; so it did seem that the time had come for substantial work rather than for exhibition and appeals even to legitimate curiosity.<sup>69</sup>

Farr wrote on 16 May 1857 to Edvard who was still in London promoting the engine following the publication of *Specimens*.<sup>70</sup> On 3 July 1857 Edvard wrote to Sir George Lewis, Chancellor of the Exchequer under Palmerston, offering to supply to a machine 'similar to that exhibited at Somerset House in 1854' for the sum of £1,200.<sup>71</sup> Edvard included a copy of *Specimens*, referred to the favourable report by the Royal Society Committee, and drew attention to the unanimous award of a

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<sup>68</sup> This expression is used by Lindgren (*G&F*, p. 205) citing James (1983), p. 2. The Dudley Observatory was 'incorporated' in 1852. See Merzbach, p. 23.

<sup>69</sup> Farr (1864), Appendix, p. cxxxix.

<sup>70</sup> *G&F*, p. 213.

<sup>71</sup> Scheutz to Lewis, 3 July 1857. T1/B/19264 (PRO (Kew)). A clerk's copy is preserved in Airy's papers (RGO6-454, ff. 427-429). Advances in manufacturing between the 1820s and 1850s were a significant factor in the low cost of the Scheutz machine in comparison with Babbage's.

gold medal at the Paris Exhibition for the second Scheutz engine.<sup>72</sup> Edvard had already met with George Graham and presumably with Farr. In his letter to Lewis, Scheutz mentions that at the interview with Graham:

it was conceived that such a machine would be of great advantage on constructing Life Tables at the General Register Office as well as tables in some of the other Offices of the Government.<sup>73</sup>

Graham had already primed Lewis ahead of time. He had written about a fortnight earlier, suggesting that the Swedish machine might be of use to the work of the GRO but that at a cost of £1,200 he did not feel justified in asking the Treasury to purchase it for the exclusive use 'of this subordinate office'.<sup>74</sup> He suggested that the machine might be useful 'to other Departments, such as the Astronomer Royal or the Framer of the Nautical Almanac'.<sup>75</sup> He alerted Lewis to expect a submission by Scheutz and urged that before rejecting the offer outright the two offices mentioned be invited to report.

Graham's letter to Lewis, anticipating Scheutzes' offer, appears to be part of a strategy planned in advance by Graham, Farr and Scheutz. The timing of the letters, the pattern of priming visits ahead of correspondence, and the suggestion that the machine might be useful in several government departments seems

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<sup>72</sup> This was the second copy sent to Lewis. The first was sent to him as part of the large mailing made on publication in April 1857. Lewis's name appears in the original MS distribution list for *Specimens*.

<sup>73</sup> Scheutz to Lewis, 3 July 1857. T1/B/19264 (PRO (Kew)). A clerk's copy is preserved in Airy's papers (RGO6-454, ff. 427-429).

<sup>74</sup> Graham to Lewis, 17 June 1857. Graham's handwriting is not always clear. The original letter (T1/B/19264, PRO (Kew)) is difficult to decipher in at least three places. Comparison with the clerk's copy preserved in Airy's papers (RGO6-454 f. 429) resolves this. Lindgren cites the date of the letter as 7 June 1857 (*G&F*, p. 354, Note 15). The original is dated 17 June 1857 and this is confirmed by the dating on the clerk's copy in Airy's papers.

<sup>75</sup> *Ibid.*

designed to pre-empt outright dismissal.<sup>76</sup> It is possible that Lewis may have been deliberately targeted as the recipient of the bid because of his known sympathy with Farr's values. In 1833 Lewis had been assistant commissioner to inquire into the condition of the poorer classes in Ireland. He was a poor-law commissioner for England and Wales from 1839-47 and a Liberal MP in 1847.<sup>77</sup> The Registration Act of 1836 made provision for collecting data for poor-law administration and Farr, who by all accounts was genuinely concerned for the plight of the poor, may well have seen Lewis as someone sympathetic to his own cause.

#### Airy's Considered Opinion

If the suggestion in Graham's letter to Lewis to consider possible benefits to other Departments was a ploy to avoid outright rejection, it worked: the case was referred to Airy. Lindgren, working from PRO Treasury records, writes that Lewis forwarded Scheutz's letter to Airy with the request 'that he [Airy] will favour this'.<sup>78</sup> The surprising implication here is that Lewis appeared to be steering the outcome of Airy's forthcoming deliberations and thereby compromising the independence of the consultation. However, inspection of the original letter with the Treasury action-note annotated by Lewis allows a different reading. The letter was folded in such a way as to create separate panels bounded by the folds. The sentence quoted by Lindgren was written on one of the panels and examination of the original shows

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<sup>76</sup> There was a further priming meeting: Edvard Scheutz called on John Russel Hind, superintendent of the Almanac, alerting him to the possibility that he [Hind] might receive an official Government communication about the engine. Hind to Airy, 11 August 1857, RGO6-454, f. 433.

<sup>77</sup> *CDNB*.

<sup>78</sup> *G&F*, p. 213.

that a panel containing the crucial continuation was concealed on the panel hidden by the fold. The missing part of the sentence appears after the solidus below. The full text of the action note signed by Charles Trevelyan, assistant secretary to the Treasury, reads:

Transmit a copy of this letter and the letter from the Registrar G[eneral] and the accompanying Specimen tables to the Astronomer Royal, and request that he will favour this/ Board with his opinion upon the subject.<sup>79</sup>

The addition of the missing phrase revises the construction of the brief. Reference to Airy's papers, amongst which Trevelyan's original letter to Airy survives, confirms that the correct version is the full-length one. This rereading removes any imputation of tendentiousness in the Treasury's brief to Airy.<sup>80</sup>

Airy did not reply for over a month.<sup>81</sup> Instead a holding letter was sent to Trevelyan by the First Assistant, Robert Main, advising that Airy would be away for several weeks.<sup>82</sup> Airy wrote to Trevelyan on his return acknowledging the brief, assuring him that the inquiry would receive immediate attention but informing him

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<sup>79</sup> Trevelyan to Airy, 8 July 1857, T1/B/19264, PRO (Kew). Solidus indicates fold.

<sup>80</sup> Trevelyan to Airy, 8 July 1857. RGO6-454, f. 426.

<sup>81</sup> From June 27 to August 5 Airy was travelling in Scotland with his wife and two eldest sons and then visited his mother-in-law at Brampton (*Biog.*, p. 231).

<sup>82</sup> Main to Trevelyan, 9 July 1857. RGO6-454 f. 431. Airy made it a condition of his appointment as Astronomer Royal in 1835 that the incumbent First Assistant of the Greenwich Observatory should be removed from office. He appointed Robert Main in his stead on the recommendation of Mr. Hopkins, a well-known private tutor at Cambridge. Airy states openly that he was determined 'to have a man who had taken a respectable Cambridge degree', (*Biog.* p. 109). Main ran the Observatory, deputising for Airy during the last quarter of 1835 (*ibid.*, p. 110). Airy was then still at Cambridge, formally holding two jobs (Astronomer Royal and Plumian Professor), while repairs and alterations to the Greenwich buildings were undertaken in preparation for his transfer (*ibid.*). In 1853 there was competition between Airy and Main for the editorship of the Nautical Almanac left vacant by the death of Lieutenant Stratford. Airy was willing to take it at a low rate to supplement his Greenwich salary. In the event John Russell Hind was appointed (*Biog.* p. 216). Main served loyally as Airy's First Assistant until June 1860 when he accepted the office of Radcliffe Observer at Oxford (*Biog.*, p. 238)



of a short delay to 'procure necessary information'.<sup>83</sup> On the same day (August 11) Airy wrote to Hind, superintendent of the Nautical Almanac, asking whether the Government had written to him separately to solicit his views on the Swedish engine.<sup>84</sup> Hind replied saying that Scheutz had already called on him to let him know him of the possibility that an official communication might be forthcoming, but that none had yet materialised.<sup>85</sup> Airy then set about the serious business of official consultation. By way of clarifying his brief he wrote to Graham on 17 August 1857 asking him whether he wished the view expressed in Graham's letter to Lewis – that the Swedish engine 'might be of considerable practical use' to the GRO – to be taken as a datum for the inquiry, or whether Graham was willing for Airy to address more specialised questions on 'the frequency and facility of the use of such a machine 'with the view of making my Report more perfect'.<sup>86</sup> Graham replied by return agreeing to the more detailed brief.<sup>87</sup>

Airy had already begun to compile a set of ten specific questions on the use to which the machine might be put in the Nautical Almanac Office and the GRO, and he wrote two near-identical letters, one to Hind and one to Graham.<sup>88</sup> The letters differ only in minor details where the questions have been varied to apply to the specifics of the two Offices.<sup>89</sup> Each of the letters enclosed a non-returnable copy of the Royal Society committee's report of 1855 that served as a form of

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<sup>83</sup> Airy to Trevelyan, 11 August 1857. RGO6-454, f. 432.

<sup>84</sup> Airy to Hind, 11 August 1857. RGO6-454, f. 433.

<sup>85</sup> Hind to Airy, 11 August 1857. RGO6-454, f. 434.

<sup>86</sup> Airy to Graham, 17 August 1857. RGO6-454, f. 451.

<sup>87</sup> Graham to Airy, 18 August 1857. RGO6-545 f. 451.

<sup>88</sup> Airy to Hind, 18 August 1857. RGO6-454 f. 454 *et seq.*; Airy to Graham, 19 August 1857. RGO6-454 f. 458. For transcriptions of the letters see Appendix II below.

<sup>89</sup> Questions 1, 4, and 7 are variants.

operational specification to convey 'an authentic account of the peculiarities and of the powers of the machine'. Airy itemised ten questions to which he solicited specific responses. He asked about the frequency with which specific mathematical functions were generated using differencing techniques of the fourth order; about the need for the immediate production of stereotype plates; whether the loss of intermediate results (a feature of the machine-technique) was a disadvantage; the frequency with which temporary as distinct from permanent tables were required; whether if the machine were infrequently used, it would be preferable to employ a computer or someone to maintain the machine; what mechanical aids (slide rules, adding machines and the like) were currently in use, and so on. The set of questions constitutes the most specific and detailed survey of the operational utility of a calculating engine that has come to light.

Airy had an afterthought and wrote again to Hind on the same day, inviting him to comment on Airy's piece in the *Philosophical Magazine* the previous year, in which he suggests that a differencing engine could be used for verification rather than straight tabulation by adapting it to generate fourth differences from already computed tabular results.<sup>90</sup> Hind had not seen the published letter and wrote two days later saying that he would take the earliest opportunity to do so.<sup>91</sup> On the same day Hind responded separately to the survey with concise individual replies to Airy's ten questions.<sup>92</sup> The answers are terse and the overall response is negative. In response to Airy's question as to whether in preference to an infrequently used calculating engine Hind would 'prefer to employ a computer in the

<sup>90</sup> Airy to Hind, 18 August 1857, RGO6-456, f. 456. For reference to Airy's published suggestion for verification using differencing see above, p. 254.

<sup>91</sup> Hind to Airy, 20 August 1857, RGO6-454, f. 460. There is no recorded response from Hind to Airy's question about verification by mechanical differencing.

<sup>92</sup> Hind to Airy, 20 August 1857, RGO-6-454, f. 461.

ordinary way with pen and paper', Hind replied that his preference is certainly to employ a pen-and-ink human because 'very few error are committed and the work is performed quicker than a machine would turn out'.<sup>93</sup>

Airy wrote again to Hind on August 24 with a new question and then again the next day clarifying his letter of the day before by providing the relevant mathematical formulas and worked examples.<sup>94</sup> Airy wished to know which of two methods of interpolation Hind used at the Almanac Office to subtabulate values (hourly values of the Moon's position, for example) from fixed known pivotal values (12-hourly positions). He also asked whether the availability of a machine would induce him to abandon a non-mechanisable method in favour of one amenable to machine computation. Hind replied negatively, again asserting that 'any practised computer, with the aid of Tables we now use, would beat such a machine as that constructed by Mr. Scheutz in a [. . . period] of four or five hours'.<sup>95</sup> There was a further flurry of exchanges during which Airy attempted to clarify Hind's methods of differencing, using figures for the Moon's declination for 1861 taken from the Almanac which Airy, curiously, did not have to hand.<sup>96</sup> Clearly, Airy was not stinting in diligence in his public service to the Treasury.

In the meantime Graham was preparing his responses to Airy's survey. His reply was hampered by Farr's imminent departure for Vienna to attend the International Statistical Congress.<sup>97</sup> Airy's specific questions were confined to past and current practice at the GRO. However, Farr wished to use the engine to relieve

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<sup>93</sup> *Ibid.*

<sup>94</sup> Airy to Hind, 24 August 1857. RGO6-454, f. 465, f. 467.

<sup>95</sup> Hind to Airy, 27 August 1857. RGO6-454, f. 473.

<sup>96</sup> Airy to Hind, 28 August 1857. RGO6-454, f. 474; Hind to Airy, 31 August 1857. *Ibid.*, f. 475.

<sup>97</sup> Graham to Airy, 26 August 1857, RGO6-454, f. 459.

the computational burden of a new series of tables of joint lives not yet computed, and a point-by-point response to Airy's questions would not have given him the scope to articulate his plans. Farr therefore made two submissions that Graham forwarded to Airy: a concise point-by-point response to the ten questions, and a separate submission in the form of a letter to Graham.<sup>98</sup> The letter was an authoritative account of the proposed application of the engine for the calculation of joint life tables and was highly favourable to the prospective use of Scheutz's engine. The credibility of his advocacy was strengthened by the fact that he had already started the computation of joint lives using two computers 'of good capacity' and was able to quantify the labour involved.<sup>99</sup> Farr concludes his case with a strong recommendation in favour of the machine:

I have taken a good deal of trouble about the matter, and am satisfied that a *well constructed machine* would be of the greatest use in this Department and would enable us to construct Tables rapidly, which we cannot otherwise attempt or complete without extra hands, that would cost the country much more than the machine, even if they worked by the quantity, and I should never have the same confidence in Tables so constructed as in the Tables constructed by Scheutz's machine.<sup>100</sup>

While the response from the Nautical Almanac to Airy's ten-point survey was uniformly negative, that from the GRO was determinedly positive. In the last of the ten points Airy asks whether any mechanical aids (slide rules or adding machines, for example) were used in the work of the GRO. Farr replied that 'various

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<sup>98</sup> For Farr's representations to Graham see *ibid.*, f. 463. For Graham's onward transmission to Airy see *ibid.* f. 469. The original of Graham's response to the ten questions is not to be found in Airy's papers, only a clerk's copy listing the questions with answers alongside. The copy is undated. See RGO6-454 ff. 470-471. For transcription see Appendix II below.

<sup>99</sup> Farr to Graham, 20 August 1857. RGO6-454 f. 463.

<sup>100</sup> Farr to Graham, 20 August 1857, RGO-454, f. 463. Emphasis original.

contrivances have been tried; but none of them are applicable to the work of the Office – except – Mr. Scheutz's machine'.<sup>101</sup> Farr's technically informed, well-argued and credible case favourable to the engine flew in the face of Airy's long-held scepticism about the usefulness of the engines, which was now confirmed by Hind's similarly disparaging views. Airy was now at a loss to resolve the conflicting recommendations of the two Offices without further consultation with Farr. Clearly impressed by Farr's submission he wrote to Graham that:

Dr Farr's expression of opinion on the utility of the machine is so distinct and so important that I shall willingly delay my Report as long as possible, in order to have the opportunity of speaking with him on the subject.<sup>102</sup>

Three weeks later Airy showed signs of becoming anxious. He wrote to Graham clearly unwilling to proceed without further consultation with Farr, reminding him that he had delayed his report to the Treasury for this purpose and that the GRO's case was 'a most important element' in the Treasury's decision. He hoped, he wrote, that now that the Vienna conference was ended, Farr would not be long returning.<sup>103</sup> Graham was away and the senior clerk, Thomas Mann, replied on 22 September saying that Farr was expected hourly and that any communication from Airy sent to the GRO would reach him immediately.<sup>104</sup> Airy wrote by return emphasising the importance he attached to Farr's opinion, because 'it is from your office alone [the GRO] that I receive a distinct expression favourable to the

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<sup>101</sup> RGO6-454, (n.d.), f. 470. It is clear from Airy's letter to Graham acknowledging the GRO's response to the survey that Airy assumed that it was Farr not Graham who had provided the answers. See Airy to Graham, 27 August 1857. RGO6-454 f. 472. Also, Airy to Farr, 23 September 1857, *ibid.* f. 480.

<sup>102</sup> Airy to Graham, 27 August 1857. RGO6-454 f. 472.

<sup>103</sup> Airy to Graham, 18 September 1857. RGO6-454 f. 477.

<sup>104</sup> Mann to Airy, 22 September 1857. RGO6-454 f. 479.

presumption of practical utility of the Engine.<sup>105</sup> Airy confessed that he was at a loss as to how to proceed. To Farr he wrote:

I scarcely know what I can ask you that has not been already asked. But probably you have, with practical views, turned the matter over well in your mind. The two elements into which every consideration must resolve itself seem to be *cheapness* and *accuracy*. And it might be that you can present some remarks on these which have not occurred to me.<sup>106</sup>

Airy offered Farr the option of a personal interview and the absence of a reply from Farr in Airy's meticulous records suggests that this is what occurred.<sup>107</sup>

Airy finally reported to Trevelyan on 30 September 1857, some two-and-a-half months after the original Treasury brief.<sup>108</sup> The eight-page letter is a considered assessment of the utility of the engine in the three Offices proposed. Airy wrote:

In the Royal Observatory, the Machine would be entirely useless . . . During the twenty two years in which I have been connected with the Royal Observatory, not a single instance has occurred in which there was a need of such calculations.<sup>109</sup>

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<sup>105</sup> Airy to Farr, 23 September 1857. RGO6-454 f. 480.

<sup>106</sup> Airy to Farr, 23 September 1857, RGO6-454, f. 480. Airy had expressed the same sentiment to Graham. Airy to Graham, 18 September 185, *ibid.*, f. 477. Emphasis original.

<sup>107</sup> That Airy consulted with Farr after his return from Vienna is confirmed by Airy's letter to Trevelyan (see next note) in which Airy excuses the long delay, for which he cites three reasons: his own absence, the time for his inquiries, and the 'absence of an officer to whom it was especially important to refer, at the Statistical Congress of Vienna'. Airy confirms that he had 'taken the earliest advantage of the return of this gentleman to obtain from him the information which I required'. That they met is supported by Airy's statement in the same letter that he had consulted Farr by 'written and oral communication'.

<sup>108</sup> Airy to Trevelyan, 30 September 1857. The eight-page original is preserved in T1/6098B/19264. A copy-press facsimile is preserved in Airy's papers. RGO6-454 ff. 485-492.

<sup>109</sup> *Ibid.*, paragraph 5.

His conclusions as to the use of the machine by the Almanac Office were similarly dismissive. He reports that there was, *prima facie*, 'ample occasion' for the use of the engine and proceeds to identify four specific applications. Three of these he rules out as being beyond the assistance of the machine. The fourth he allows was 'entirely within the province of the Machine' but:

[The superintendent's ] opinion is clear and unhesitating, that no advantage would be gained by the use of the Machine, and that he would prefer the pen-computation of human computers, in the way in which it has hitherto been employed.<sup>110</sup>

But Farr was not so easy to dismiss and it is possible that the quality of Farr's advocacy had seeded some genuine curiosity about the utility of the machine. Airy quoted verbatim from Farr's letter, which pressed the point that the GRO could not hope to complete the proposed new tables without extra hands, the expense of which additional labour would exceed the cost of the machine.<sup>111</sup> Further, that machine-generated results would warrant greater confidence in their accuracy. Airy registered a difference of opinion but he defers to Farr:

My own opinion differs in some respects from Dr. Farr's. I am inclined to believe that the expence [sic] of computation by pen would not exceed that of application of the Machine (interest of investment included), that pen-and-paper calculations are more convenient, and that by use and correction of stereotypes, with a trifling premium on the detection of errors, accuracy may be made certain. But I am scarcely at liberty to urge this opinion strongly in opposition to that of Dr. Farr, who has with a practical view given his earnest attention to the matter.<sup>112</sup>

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<sup>110</sup> *Ibid.*, paragraph 7.

<sup>111</sup> Farr to Graham, 20 August 1857, RGO-454, f. 463. Emphasis original.

<sup>112</sup> Airy to Trevelyan, 30 September 1857. T1/6098B/19264, paragraph 9.

His respect for Farr was such that he was unwilling to override Farr's case despite his own reservations. Airy then conceded the positive value of taking a risk: the practical benefit of using the GRO as a limited-liability experiment, and the related benefit of resolving a matter of public interest:

There is a consideration, not connected with an immediate and certain pecuniary advantage, which I think merits to be taken into account. The preferred introduction of a Calculating Machine into an active office for real practical use is a new thing: which, if it succeeds, will be an important gain, and, if it fails, will entail no further loss. Moreover, it is a proposal upon which the Scientific Public have for several years looked with great interest, but which could not be tested except in a public office were there is a large demand for a peculiar class of computations.<sup>113</sup>

Finally, Airy recommended that the machine be acquired at public expense for use in the GRO in the first instance, but takes care to note that this conclusion was founded on Farr's recommendation notwithstanding his own reservations.

Airy's recommendation to proceed with the purchase was not enough to secure Treasury approval. The Treasury's response was curt. An action note on Airy's report instructs that the report should be transmitted to the Registrar General stating that:

unless it can be distinctly shown that the use of the Machine will be productive of a saving of expense, Mr. Scheutz will be informed that H.M. Govt. does not propose to purchase it.<sup>114</sup>

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<sup>113</sup> *Ibid.*, paragraph 10.

<sup>114</sup> *Ibid.*



A brisk letter to this effect was sent by Trevelyan on 9 October 1857.<sup>115</sup> Graham's response was fulsome: it ran to fifteen pages.<sup>116</sup> He concedes at the outset that the one and only way of effecting savings is to lose clerks but that he was opposed to this. The crux of his argument is that the needs and ambitions for a new series of tables could not be met without the machine, or a substantial increase in staff. The programme to expand the range of tables produced by the GRO included Farr's programme to extend the existing tables of life assurance to include joint lives, to compute tables of interest and annuities for rates other than 3%, and to accommodate Farr's determined wish to compute life tables by district to identify the deleterious or wholesome effects of urban and rural environments on health measured by mortality, Farr's favoured index.<sup>117</sup> Graham concluded simply, arguing that Scheutz's machine would save the expense of the additional computers required to deliver the proposed programme of work.

Graham's marathon submission evidently did the trick. An internal Treasury minute dated 12 November 1857 authorised the purchase.<sup>118</sup> No provision was made for a down-payment and the assumption is that payment was to be made on delivery. The notice to Graham included a proviso that though the machine was to be deposited at the GRO as the site affording the most benefit, the Treasury

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<sup>115</sup> Trevelyan to Graham, 9 October 1857. RG29-5 f. 420.

<sup>116</sup> Graham to Secretary of the Treasury, 19 October 1857. The original letter, not in Graham's hand but signed by him, is preserved in Treasury papers T1/6098B/19264 PRO (Kew). A clerk's copy is included in the Outward letter book for 1836-1863: RG29-1 ff. 538-545. There are minor discrepancies between the letter book version and the original. There are also revealing deletions in the original concerning Brunel's recommendation for additional expenditure on Babbage's engine. The variants do not alter the sense in any material way.

<sup>117</sup> *Ibid.*

<sup>118</sup> T1/6098B/19264 PRO (Kew). The action note requested that a copy of the Treasury minute be sent to Airy and the clerk's copy can be found in RGO6-454 f. 495. The case was handled internally on 27 October 1857. A minute apparently signed by George Lewis refers the case to a Mr. Wilson, giving him authority, if he concurred with the recommendation, to proceed and authorise the purchase.

reserved the right to deploy it in other Public Departments should its transfer 'appear to promise greater public advantage'.<sup>119</sup> Scheutz, who received the order while in France, confirmed the order in a letter to the Treasury on 7 December 1857, undertaking to deliver the new machine made by Donkin & Co. within eighteen months of the order date, that is by the 14 May 1859.<sup>120</sup> However, Scheutz evidently ran into financial difficulties and appealed to Graham for an advance.<sup>121</sup> An up-front payment of £300 was granted by Trevelyan on application from Graham on Scheutz's behalf, subject to the condition that Donkin stood security by guaranteeing to repay the advance in the event that the engine was not delivered by the due date.<sup>122</sup>

Scheutz and Donkin overran the delivery deadline by a few weeks: Donkin had run into difficulties possibly arising from design modifications introduced by Georg Scheutz.<sup>123</sup> The engine delivered to the GRO was a direct copy of the second Scheutz engine built by Bergström in Stockholm and sold to Albany, but with minor modifications.<sup>124</sup> The precise date on which the machine was physically

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<sup>119</sup> Trevelyan to Graham, 14 November 1857. RG29-5 f. 421.

<sup>120</sup> Scheutz to Treasury, 7 December 1857, T1/6098B/19264, PRO (Kew). Lindgren, working without the benefit of the GRO letter books, cites the contracted delivery date as 12 May 1859 (*G&F*, p. 216), i.e. eighteen months reckoned from 12 November 1857, the date of the Treasury minute authorising the purchase. The letter to Scheutz placing the order is not on record. However, the Treasury letter to Graham confirming the order is dated 14 November (RG29-5 f. 421 (PRO (Kew))) and it is highly probable that the letter to Scheutz was written at the same time. Trevelyan's letter to Graham of 24 March 1858 (RG29-5 f. 423) specifically confirms that the date of the order was 14 November 1857 (RG29-5 f. 423). The due delivery date was therefore the 14th of May 1859.

<sup>121</sup> *G&F*, p. 217.

<sup>122</sup> Trevelyan to Graham, 24 March 1858. RG29-5 f. 423.

<sup>123</sup> See *G&F*, pp. 216, 223-4.

<sup>124</sup> See *G&F*, p. 216. Airy's inspection report notes 'two or three small changes, which are in all cases improvements', Airy to Graham, 31 August 1859. RGO6-456, f. 268. Farr reported that Donkin stated that the engine consisted of 4,320 parts, of which 2,054 were screws, 364 compose the chain, and 902 other parts. The machine without the protective case was estimated to weigh 10 cwt. Farr (1864), Appendix, p. cxi, ft.

transferred from Donkin's works to George Graham at Somerset House is not known.

### *Airy's Volte Face*

Airy does not appear to have taken any part in the proceedings during construction of the machine. But with the engine complete Graham requested Airy to perform an acceptance inspection and provide written confirmation that the engine satisfied the original proposal.<sup>125</sup> He wrote to Airy confirming that the machine had been delivered and requested that he inspect it at the Astronomer Royal's convenience.<sup>126</sup> Airy was abroad and Robert Main, the Observatory's First Assistant, again sent a holding letter.<sup>127</sup> A month later Donkin had still not been paid and Farr was becoming anxious. Farr wrote on Graham's behalf pressing for an appointment on Airy's return.<sup>128</sup> It took a further four increasingly importunate exchanges from Farr before Airy agreed to inspect the engine on 30 August 1859, subject to no other urgent claim on his time.<sup>129</sup> Airy seemed in no rush to sign the engine off. Having taken seven weeks to pin Airy down, Farr was evidently concerned not to let pass even this qualified commitment on Airy's part, and he

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<sup>125</sup> Graham to Airy, 12 July 1859. RGO6-456, f. 261.

<sup>126</sup> *Ibid.*

<sup>127</sup> Main to Airy, 19 July 1859. RGO6-456, f. 262. Airy was travelling in France (Auvergne and the Vivarais) with his two eldest sons from 4 July to 2 August. *Biog.*, p. 238.

<sup>128</sup> Farr to Rev. R. Mayne [sic], 11 August 1859. RGO6-456 f. 263.

<sup>129</sup> For Farr's increasingly urgent requests see Airy to Graham, 12 August 1859. RGO6-456, f. 264; Farr to Airy, 25 August 1859. f. 265; Airy to Farr, 27 August 1869. f. 266; Farr to Airy, 29 August 1859. f. 267. The Observatory's First Assistant was away and Airy was holding the fort. He made his visit conditional on not being 'detained by accident'. Airy to Farr, 27 August 1859. RGO6-456, f. 266.

returned to London from the country specifically to meet Airy at Somerset House at the appointed time.<sup>130</sup> The bluntness of Farr's letter confirming the arrangement suggests that either the trip was inconvenient or that he was aggrieved at Airy's apparent reluctance, and the consequences to Donkin of the delay in payment.<sup>131</sup>

The need for Airy to inspect the engine was not a stipulation of the original agreement with the Treasury. Graham nonetheless seems to have felt the need for expert confirmation before requesting authority to settle Donkin's outstanding bill. It is also possible that Graham had not had sight of Airy's report to the Treasury recommending purchase and wanted Airy to confirm that the engine complied with expectations to which only Airy and the Treasury had been privy. Graham also requested the Royal Society to attest to the satisfactory completion of the machine and the same four-man committee that had convened under George Stokes' chairmanship to report on the second Scheutz engine was reconvened to report on the third.<sup>132</sup>

Airy reported to Graham the day after his inspection.<sup>133</sup> He commended the workmanship of Donkin's copy as superior to the original.<sup>134</sup> The report is uniformly favourable and Airy concludes:

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<sup>130</sup> Farr to Airy, 29 August 1859. RGO6-456, f. 267.

<sup>131</sup> *Ibid.*

<sup>132</sup> Graham to Treasury, 12 September 1859. RG29-1, f. 576.

<sup>133</sup> Airy to Graham, 31 August 1859. RGO6-456, f. 268.

<sup>134</sup> Lindgren's physical comparison of the two engines has led him to dispute Airy's judgement here. Lindgren observes that in Donkin's copy of the second Scheutz engine there were parts carelessly made, and lubricating points omitted. (*G&F*, p. 250 *et seq.*). He also observes that the print quality of the GRO machine was worse than that of the first Scheutz engine made Bergström (*G&F*, p. 233). Lindgren speculates that Airy's defective eyesight may have been responsible for this flawed judgement and cites Meadows (1975). Airy suffered from severe astigmatism (*CWB*, p. 182). But the imputation that his corrected eyesight was deficient is contradicted by his son Wilfrid who maintains that his father 'saw extremely well' using a choice of three sets of spectacles with lenses ground to Airy's own specification (*Biog*, p. 1).

I wish to express that I am entirely satisfied with the construction of the Machine, and that I think that Mr. Donkin is fully entitled to the immediate satisfaction of all claims which the agreement of the Government might authorise him to urge on the completion of the work.<sup>135</sup>

On the strength of both Airy's and Stokes' reports Graham wrote to the Treasury on 12 September that:

The correct working of the Machine has been severely tested, and that it is found to have been admirably constructed and to be as perfect as human hands can make it.<sup>136</sup>

As well as enclosing copies of both reports, Graham enclosed a bill from Donkin for the sum of £900 still outstanding. More disturbingly, he also enclosed two letters from Donkin dated 3 September stating that the build-cost of the engine was £623 10s 6d in excess of the contractually agreed sum, that Donkin was out of pocket by this amount and had taken nothing by way of labour costs or indeed profit.<sup>137</sup> The engine had been built at a thumping loss. Graham requested that the Treasury pay Donkin the overspend in addition to the £900 outstanding, pointing out that the total sum of £1,823 10s 6d would have produced a machine 'perfect and complete' which over £17,000 expended on Babbage's engine had failed to do.<sup>138</sup> The Treasury authorised the payment of the £900 but declined the extra payment.

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<sup>135</sup> Airy to Graham, 31 August 1859. RGO6-456, f. 268.

<sup>136</sup> Graham to Treasury, 12 September 1859. RG29-1, f.576.

<sup>137</sup> *Ibid.* Lindgren notes that Donkin wrote to Scheutz over a week earlier (24 August 1859) advising him that there was an overspend. Lindgren cites the lower figure of £615 for the overspend. See *G&F*, p. 225, p. 357 Note 71. Graham to Treasury, 12 September 1859. RG29-1, f.576.

<sup>138</sup> *Ibid.*

However, they allowed that should the engine go on to prove itself they would consider an *ex gratia* payment in recognition of the improvements to the engine not covered by the original estimate.<sup>139</sup> Though Donkin was aggrieved at the loss and at the delay in payment, he continued to maintain and repair the machine as well as provide training for GRO clerks in its use and operation.<sup>140</sup> Donkin was perhaps still living in hope of a Treasury gratuity to make up his loss. It is unclear when Donkin withdrew support for the engine. But some twenty years on, when the engine needed repairs, Donkin refused because of his unrecovered loss.<sup>141</sup> Graham managed to track down Donkin's former Foreman, White, who had superintended the operation of the machine and instructed the clerks when the machine was first delivered, and engaged him privately in Donkin's stead.<sup>142</sup>

The third Scheutz engine was used, as Farr intended, in the production of the English Life Table of 1864. But the hoped-for benefits of mechanised tabulation were not realised. The machine did not include any of Babbage's security mechanisms to safeguard against derangement and was temperamental. Farr described the difficulties and bemoaned the large developmental gap between the conception of an invention and a proven working device:

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<sup>139</sup> Hamilton to Graham, 26 September 1859. RG29-5, f. 438. The state grant (the equivalent of £283) for the engine built by Bergström in Stockholm also did not fully meet manufacturing costs. On satisfactory completion in 1853 the grant to the Scheutzes was doubled by Royal dispensation 'as compensation and reward'. See *G&F*, pp. 171-2. It is possible that Donkin was encouraged by this to hope for similar treatment by the British government.

<sup>140</sup> For Donkin's 'harsh' letters to Scheutz see *G&F*, p. 227. Graham's requests for the Treasury to meet maintenance, repair and training costs are documented in the letter book records of inward and outward correspondence with the Treasury. See RG29-1, ff. 599, 624. RG29-5, ff. 451, 463. One letter refers to experiments with Indian Rubbers, lead, Gutta Percha, and *papier mâché* in the search for a suitable soft material for stereotyping results. (Graham to Treasury, 8 August 1860, RG29-1, f. 599. Original in T1/6257B/12792).

<sup>141</sup> Graham to Treasury. RG29-2, f. 250. The entry is undated but appears to be included in an entry about Arithmometers dated 16 March 1877.

<sup>142</sup> *Ibid.*

The machine required incessant attention . . . Of the first watch nothing is known, but the first steam-engine was indisputably imperfect; and here we had to do with the second Calculating Machine as it came from the designs of its constructors and from the workshop of the engineer. The idea had been as beautifully embodied in metal by Mr. Bryan Donkin as it had been conceived by the genius of its inventors; but it was untried. So its work had to be watched with anxiety, and its arithmetical music had to be elicited by frequent tuning and skilful handling, in the quiet most congenial to such productions.<sup>143</sup>

In the event the machine made only a slight contribution to the 1864 Life Table. Of the 600 pages of printed tables in the volume only twenty eight pages were composed entirely by the machine; 216 pages were partially composed and the rest were typeset by hand.<sup>144</sup> This was not all. The hoped-for economies from automatic stereotyping also evaporated. It was expected that if the machine produced stereotype plates for printing, the costs of typesetting (which was the largest proportion of costs), as well as checking, would be saved.<sup>145</sup> Not so. HMSO, which produced the fat volume, stated baldly that had the entire volume been automatically typeset by the machine it would have made a saving of only ten per cent over conventional methods.<sup>146</sup> Despite Farr's grand hopes and valiant campaign, the Scheutz engine failed to deliver any significant technical or financial benefit. Airy, vilified by Babbage for his hostility to the engines, seems to have

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<sup>143</sup> Farr (1864), p. cxi. Babbage referred to the 'the lavish rejection of inventions' in the development of apparatus. Babbage (1837), *Works*, Vol. 3. p. 40.

<sup>144</sup> See J. R. McCulloch (Comptroller, HMSO) to G. A. Hamilton (Treasury), 26 October 1864, RGO29-6, f. 6.

<sup>145</sup> The total cost of 1,000 clothbound copies of the 1864 Life Tables excluding the cost of corrections was estimated by HMSO as £222 13s 9d. The breakdown of costs was: Typesetting and Stereotyping (50%); Presswork (5%); Paper (19%); Binding (26%). See McCulloch to Hamilton, 23 April 1860, T1/6257B/12792 (PRO, Kew).

<sup>146</sup> *Ibid.*

been vindicated by events, at least insofar as practical usefulness and cost savings were concerned.

But there is one final twist. Airy had consistently expressed general scepticism about the utility of automatic calculating devices. Despite this sustained and apparently intractable opposition, he appears, late in the day, to have undergone a partial conversion. It had taken Farr and Graham seven weeks to prevail upon Airy to make the time to inspect the engine after its delivery to the GRO, Somerset House. He finally examined the machine with some thoroughness on 31 August 1859. Something appears to have happened during the inspection. After decades of opposition Airy suddenly saw a use for it. He wrote immediately to Donkin to explore an application of the machine at the Nautical Almanac Office. He asked, in effect, whether the machine could be converted from decimal to sexagesimal use to accommodate the minutes and seconds of arc characteristic of astronomical calculations.<sup>147</sup> He also asked whether the printing apparatus could be modified to provide vertical and horizontal separation of tabulated results, grouping the printing wheels to provide separation between columns, and modifying the mechanism to leave a blank line between groups of lines.<sup>148</sup> His enquiry was evidently not an idle one. Having described the new outputs, he

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<sup>147</sup> Airy to Donkin, 1 September 1859. RGO6-456, f. 270.

<sup>148</sup> *Ibid.* Babbage had already considered these formatting issues. The printing and stereotyping apparatus he designed for use with Difference Engine No. 2, and with the Analytical Engine, allows programmable formatting. The selection of pairs of 'pattern wheels' from a set of eight provides a range of formatting options including variable line height, variable numbers of columns, variable margins separating columns, variable numbers of lines per page and the option of line-to-line printing or column-to-column printing with automatic rewind to top-of-page at the end of a column. Provision is also made for grouping of lines together in sets leaving horizontal gaps between groups. No provision is made for the vertical separation of multi-digit results into groups of digits as requested by Airy; the printing mechanism could not be converted using optional accessories as suggested by Airy but a separate printing apparatus would be needed for sexagesimal use. The common practice at the time was to cut the stereotype mould or printing plate with a knife and position the resulting strips in the printing frame to allow the required gaps. Swade (1996, *Science Museum*).



enquired of Donkin the cost of parts and labour to provide a set of optional accessories to convert the GRO machine for use on the Almanac, and also the time and cost of effecting the changeover to sexagesimal use. Failing all this, he asks Donkin to quote for a new machine built for the purpose.<sup>149</sup>

Airy was clearly taken with the new possibilities for the engine. He wrote the next day to William Gravatt pondering a design difficulty with the conversion, that is, getting the mechanism to count in modulo-360 arithmetic, and he asked Gravatt's advice as to whether the existing machine could accommodate this or whether a new machine would need to be built from scratch.<sup>150</sup> Evidently still preoccupied with uses to which an engine might be put, he asked Gravatt to think about his suggestion that an engine capable of differencing known tabular results by repeated subtraction may 'be more valuable' than one that tabulated results from differences by adding them. In this he was repeating his earlier suggestion that a machine used to verify existing tables, or new tables manually calculated, may be of more use than those proposed by Babbage and Scheutz.<sup>151</sup>

Donkin replied that the GRO machine was already capable of sexagesimal operation and of separating columns of figures as described by Airy.<sup>152</sup> He also assured Airy that it would take only about three or four hours to convert the GRO machine from decimal to sexagesimal operation.<sup>153</sup> It is difficult to know whether Airy's sudden interest in the machine was a transient lapse or whether the GRO's

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<sup>149</sup> *Ibid.*

<sup>150</sup> Airy to Gravatt, 2 September 1859. RGO6-456, f. 272.

<sup>151</sup> *Ibid.* See above p. 254 for Airy's published suggestion of this technique.

<sup>152</sup> Donkin to Airy, 3 September 1849 [sic]. RGO6-456, ff. 274-275. The letter is written on Donkin's letter-head 'Engineer's Works, near Grange Road, Bermondsey, London' with part of the date pre-printed. The correct date is certainly 1859 as the letter is clearly a reply to Airy's of the day before.

<sup>153</sup> *Ibid.*

demands on the engine for the production of the 1864 Life Table, which monopolised the machine for the following four to five years, precluded Airy's access. Certainly, the machine was in such demand during 1862 that it could not be spared for the grand Exhibition held in London in that year and Farr took Babbage to task for suggesting that the GRO work might have continued as a visitor attraction while the exhibition was in progress, and for risking the machine in transit and thus jeopardising the important work on the Life Tables for which there was 'an urgent demand'.<sup>154</sup> Either way, after this flurry of positive interest, Airy bowed out and took no further known part in promoting the use of the machine at the Nautical Almanac Office, or indeed, elsewhere.

The submission of his acceptance report was Airy's last act in his capacity as official government adviser on the utility of calculating machines. After the use of the Scheutz engine for the preparation of the 1864 Life Table, the engine advocates went quiet. In Sweden, Martin Wiberg, produced a small, compact, and technically successful difference engine in the 1860s which he used to prepare published tables, but with indifferent commercial success.<sup>155</sup> In the United States George Barnard Grant built a large motor-driven difference engine, exhibited at the Centennial Exhibition in Philadelphia in 1876, but the fate of this machine is unknown.<sup>156</sup> Airy's papers include a published description of the early design of the Grant engine, and this tends to indicate that he maintained an interest or at least

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<sup>154</sup> Farr wrote: '[The Machine] had been shaken out of order on its way from the Factory to Somerset House. It might have been injured or destroyed on its journey to or from the Exhibition, and no one would absolutely guarantee its safety there. Under these circumstances the Machine was not exhibited ... I cannot agree with Mr. Babbage, who thinks that it [the GRO work] could have been safely carried on in the midst of the crowds of the Exhibition, with incessant interruptions for explanation, and with the possible clang of musical instruments, discordant sounds, or noises in the ears of the operators'. Farr (1864), Appendix, pp. cxxxix-cxl.

<sup>155</sup> See *G&F*, p. 271.

<sup>156</sup> See Merzbach, pp. 33-37.

may have remained identified with mechanised calculation.<sup>157</sup> In England there was no significant use of differencing machines for tabulation after the GRO-Schutz episode until the 1930's when L.J. Comrie revived the technique for use on commercially available NCR key-press machines.<sup>158</sup> The spectacular false-start of automatic general purpose computation (as distinct from special purpose difference engines) signalled by Babbage's design for the Analytical Engine similarly led nowhere with the exception of a few sporadic but developmentally sterile episodes.<sup>159</sup> The movement to automate calculation in the nineteenth century had for the most part failed and the emphasis shifted in the last decade of the century from automated calculation to mechanised methods of processing the vast quantities of information pouring in from census taking, first in the United States, and shortly afterwards in Europe.

#### Thomas Fowler's Calculator

Babbage, it seems, was not the only Devonian to invent a calculating machine. Thomas Fowler (1777-1843), a native of Torrington, shares this distinction though, until recently, he has not featured in standard historical accounts.<sup>160</sup> Fowler's family

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<sup>157</sup> Grant (1871), pp. 1-5. Copy in RGO6-459, f. 275-277.

<sup>158</sup> See Comrie (1931), pp. 8-10. Two little-known individually constructed difference engines were used with some success in the early part of the 20th century. The first was made by Hamann of Berlin in 1909. The machine was reported as stolen during the war and the drawings lost. The second was a device involving cascading a number of commercially available 'Triumphator' calculating machines. *Ibid.*

<sup>159</sup> See for example Randell (1982).

<sup>160</sup> For a biographical account of Fowler see Vass (1999). For first known reference to Fowler in the modern canon see Swade (1996), pp. 39-40. Also p. 240, ft. 2 above.

was too poor to afford him a place at university and except for the 'barest rudiments of education' he was self-taught, studying 'fluxions' (calculus) amongst other subjects, at nights.<sup>161</sup> Apprenticed to a fellmonger in his early teens, he later established himself as a printer, bookbinder and bookseller (his printing machine was of his own design and construction) and he went on to become clerk, partner, and sole manager of the town bank.<sup>162</sup> In 1875 his son, Hugh Fowler, brandishing patent documents before a meeting of the Devonshire Association of the Advancement of Science, claimed that his father had invented central heating for buildings in 1828 by a method then absolutely unknown (using a 'thermosiphon' to circulate the water through pipes) but was ruthlessly exploited through weaknesses in the Patent Laws by unscrupulous persons who made fortunes pirating his father's invention.<sup>163</sup> Not only did he fail to benefit from the invention but apparently found himself £400 in debt.<sup>164</sup> The motif of unjust neglect does not end with central heating: his father's calculating machine did not earn him the fame he believed it deserved. Advocacy for his engine was hindered by his refusal to release drawings of it following his bad experience with the commercial exploitation of his thermosiphon.

The stimulus for Fowler's calculating machine appeared to lie in the tedium of manual calculation. As treasurer to the Torrington Poor Law Union he needed to calculate the contribution of each of several parishes to yield collectively a given sum with the individual contributions in proportion to an annual assessment which

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<sup>161</sup> Fowler (1875), p. 172.

<sup>162</sup> *Ibid.*

<sup>163</sup> *Ibid.*, p. 178.

<sup>164</sup> Vass (1999), p. 13.

differed for each parish.<sup>165</sup> He found that the task was difficult using ordinary arithmetic, and little helped by the use of logarithms. He then 'happily hit on the idea that any number might be produced by a combination of the powers of 2 or 3', a plan that he regarded as 'entirely new'.<sup>166</sup> Using combinations of binary and ternary arithmetic he produced and published in 1838 a set of tables to aid the calculation of the proportionate charges on parishes.<sup>167</sup> It seems likely, given that his calculating machine worked using both binary and ternary arithmetic, that the idea had its roots in his poor law calculations.

Surviving accounts of the machine are fragmentary and no technical drawings or details of its construction have been found.<sup>168</sup> However, at least two contemporary general descriptions of the principles, appearance and capabilities of the machines have survived.<sup>169</sup> Fowler's machines were ternary logic (three-state) devices, working to twelve or thirteen figures of accuracy, and capable of direct multiplication and division as well as the calculation of logarithms and anti-

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<sup>165</sup> This example was cited by Hugh Fowler as illustrative of the calculations required of the Treasurer. *Ibid.*, p. 175.

<sup>166</sup> For first quoted sentence see *ibid.*, p. 174. For second quoted phrase see Vass (1999), p. 12.

<sup>167</sup> *Ibid.*

<sup>168</sup> Mark Glusker in the US has designed and reconstructed Fowler's calculator using computer-aided-design and manufacturing techniques. The reconstruction is based on textual descriptions of its principles and performance. See *Thomas Fowler's Ternary Machine* [<http://www.mortati.com/glusker/>], 2000. [cited 2 March 2003].

<sup>169</sup> For a brief history of the machine and a description of its appearance see *Notice, op cit.*, pp. 173-4. A description by Augustus de Morgan was communicated by Francis Baily to the Royal Society on 18 June 1840. An abstract of this communication appears in *Abstracts of the Papers of the Royal Society of London*, vol. IV, 1837-1843, pp. 243-4 though the paper does not appear in *Phil. Trans.* The complete manuscript of de Morgan's account is preserved in the Royal Society archive, and is titled "Description of a calculating machine, invented by Mr. Thomas Fowler, of Torrington in Devonshire. By Augustus de Morgan, Esq. Communicated by Francis Baily Esq. V.P.R.S.". The only other substantial contemporary document on the machine that I have found is Thomas Fowler's letter to Airy. See Fowler to Airy, 8 May 1841. RGO6-427, ff. 54-6.

logarithms.<sup>170</sup> Operating the machine involved pen-and-paper conversion of decimal numbers to ternary notation using look-up tables.<sup>171</sup> The numbers were then entered into the machine by hand and the sliders and rods of the machine were operated manually in a fixed sequence. Results of the calculation were then read off in ternary and converted back into decimal using look-up tables.

Unlike Babbage's and the Scheutzes' engines, Fowler's device was not automatic. The operation of the machine relied on the continuous informed intervention of a human operator both for the conversion and supply of initial values, for the physical operation of the mechanism, and for the transcription and conversion of the results.<sup>172</sup> There was another sense in which the machine was not automatic in that the rods were slid by hand with their physical movement constrained in accordance with the logic of ternary arithmetic. Fowler's machine was more a calculating aid than an 'engine', if by that it is intended to convey the automatic continuous generation of results from a single set of initial values.

There are other respects in which Fowler's machine differed essentially from the Babbage-Scheutz engines: the number base as well as the principle of its mechanical implementation was fundamentally different. The Babbage-Scheutz engines were decimal digital machines. They were decimal in the sense that they use the familiar decimal number system with the number-values '0' through '9' represented by the rotational position of a toothed gear wheel; they were digital in

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<sup>170</sup> Hugh Fowler, citing a letter from his father to de Morgan mentions the machine exhibited in London computing logarithms and antilogarithms to 'twelve or thirteen places'. See *Notice*, p. 174. Fowler's letter to Airy refers to a machine with fifty-five places in the ternary scale, capable of calculating to twenty-eight ternary places, i.e. the equivalent of thirteen decimal figures. Fowler to Airy, 8 May 1841. RGO6-427 f. 54.

<sup>171</sup> Fowler refers to the notational conversion using tables that he showed to Babbage and others. Fowler to Airy, 8 May 1841. RGO6-427. ff. 54-55.

<sup>172</sup> De Morgan made the observation that the double conversion process is susceptible to human error. See de Morgan (1840), p. 244.

the sense that the number-value of a given wheel was defined only when the wheel occupied one of ten discrete angular positions.<sup>173</sup> Fowler's machine, on the other hand, used as its active element not rotating wheels but sliding rods which could occupy only one of three discrete positions to remain logically defined. It is therefore inherently more 'digital' than a machine using ten-state logic as did Babbage's and the Scheutzes' machines. The advantage of reducing the number of distinct physical states from ten to three is that mechanical parts can be made less precisely. To Fowler the cost of working in metal was prohibitive and his machines were rendered in wood.<sup>174</sup> Although more cumbersome in wood than in metal, Fowler's machines were by all accounts practicable and reliable and required much lower precision in the factoring of parts than did Babbage's.<sup>175</sup> In the light of Konrad Zuse's work in Germany on mechanical and electromechanical digital devices in the 1930s and 1940s, and the near universal adoption of binary digital techniques in the electronic computer age, Fowler's machine was in essential respects, vastly more promising than Babbage's.<sup>176</sup>

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<sup>173</sup> In the Scheutz engine this is strictly true, i.e. each figure wheel is engraved with one each of the numbers 0 through 9. In Babbage's Difference Engine No. 2 each figure wheel is engraved with four decades, i.e. four complete sets of numbers 0 through 9. This is for reasons of engineering convenience (having large wheels) rather than reasons of arithmetical logic. It is nonetheless a decimal machine.

<sup>174</sup> *Notice*, p, 173. Also, Fowler to Airy, 8 May 1841. RGO6-427, f. 54.

<sup>175</sup> Fowler describes the wooden version of his machine as six feet long, one foot deep, and three feet wide, and estimated that a brass and iron version would be the size of a 'good portable writing desk'. Fowler to Airy, 8 May 1841. RGO6-427, f. 54.

<sup>176</sup> There are a few rare exceptions to the universal adoption of the binary system in the electronic age. The MIR computer developed in the Soviet Union during the Cold War was a three-state logic machine, and the ICL 'System 25' was an electronic mini-computer, still manufactured until the late 1980s, that used decimally configured hardware for commercial point-of-sale applications.

Babbage did consider number bases other than ten, including 2, 3, 4, 5, 12, 16 and 100.<sup>177</sup> He reasoned that the higher the base the larger the number of entities that need to be distinguished, and physical discrimination becomes increasingly difficult. Lower number bases make discrimination between values easier (the binary system requires discrimination between only two states). However, lower bases require more mechanical parts to represent a given number, and Babbage's choice to use the conventional decimal system was based on engineering convenience rather than any assumption about the sanctity of tens.

By his son's account Fowler's first machine was constructed in 1840 and an improved version in 1842.<sup>178</sup> By Fowler's own account the earlier version was inspected by London savants and other men of rank in May 1840 amongst whom he named the Marquis of Northampton, Babbage, Francis Baily and de Morgan.<sup>179</sup> Babbage, it was reported, carefully examined the machine and regarded it as possessing 'very great merit being of a different principle from his [Babbage's] own'.<sup>180</sup> Airy was invited to the private viewing but there is no record of his response to the invitation and it appears from his journal entry that he was not in

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<sup>177</sup> See Wilkes (1971), p. 4; *LEC*, p. 164; *PC*, p. 167.

<sup>178</sup> *Notice*, p. 173.

<sup>179</sup> Fowler to Airy, 8 May 1841. RGO6-427, ff. 54. It appears that both machines were exhibited in London, the earlier machine by private arrangement in Brunswick Square in May 1840, and the later machine at King's College where it remained until after Fowler-snr died. See de Morgan to Babbage, BL Add Ms 37200, f. 153 (undated); also *Notice*, p. 173.

<sup>180</sup> Letter to Airy. RGO6-427, f. 48. Signatory illegible and date either 20 or 26 May, year unknown. The sequence of the papers indicates 1840. In an undated letter to Babbage (BL Add Ms 37200, f. 153) de Morgan invited Babbage to a private viewing of the machine at 47 Hunter St, Brunswick Sq. Judd Street, London. In the letter to Airy ( RGO6-427, f. 48) the venue is identified only by the house number which is '47'. Since Babbage had already seen the machine by the time Fowler wrote to Airy on 8 May 1841, and since the first machine was not completed until 1840, both de Morgan's letter to Babbage and that of the illegible signatories to Airy were in all likelihood written in 1840.



London on the appointed day.<sup>181</sup> The later version of the machine was exhibited for some time in the Museum at King's College and demonstrated there to men of science who, by Fowler's son's account, commended its speed and accuracy.<sup>182</sup> Although Airy appears not to have seen the machine in London it was brought again to his attention five months later. Airy's correspondence on Fowler's machine is incomplete. However, it seems that he hoped to provide an abstract of a paper written by Fowler for the forthcoming annual meeting of the British Association but, having read Fowler's account did not understand it sufficiently well to do so.<sup>183</sup> Following his bad experience with central heating Fowler had little faith in the powers of the patent laws to safeguard the rights to his invention and had consistently refused to provide drawings of his machine.<sup>184</sup> Airy wrote to Professor Forbes repeating that he was having difficulty understanding the details of the machine and complained that 'it is quite wrong to send a description of machinery without drawings'.<sup>185</sup> Airy's complaint was evidently passed on to the Fowler camp.

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<sup>181</sup> *Ibid.* The letter mentions the proposed attendance of Lord Northampton whom Fowler mentions in his letter to Airy of 8 May 1841 as one of the distinguished persons to view the machine in May 1840. The invitation to Airy was for 'Friday Morning at Eleven' (*ibid.*). There is no reference to a visit to London in Airy's Journal entries for Friday 22 May or Friday 29 May 1840. Airy travelled to London on Saturday 23 May 1840 where he saw Wheatstone's telegraph and attended a meeting of London University. See RGO6-24, Astronomer Royal's Journal (1836-1847).

<sup>182</sup> *Notice*, p. 173.

<sup>183</sup> Airy to Phillips, 13 October 1840. RGO6-427, f. 49. This was presumably John Phillips, Assistant Secretary of the British Association (1832-1862), and professor of geology at King's College, London (1834-1841). Airy mentions that Fowler's paper belonged to 'Section A of the British Association.' In the BAAS sectional divisions (A-G), Section A covered Mathematics and Physical Sciences. See *GoS*, p. 576.

<sup>184</sup> Wheler to Airy, 13 May 1841. RGO6-427, f. 52. Fowler apologised to Airy for not supplying drawings but gave no reason. Fowler to Airy, 8 May 1841. RGO6-427, f. 55.

<sup>185</sup> Airy to Forbes, 13 October 1840. RGO6-427, f. 50. Given the year and the Scottish venue of the 1840 BAAS meeting, the likeliest professor Forbes is James David Forbes, secretary of Section A in 1836, Vice President of the Section in 1839, and President of the Section in 1840 at the Glasgow meeting. See *GoS*, p. 433. Forbes was eight years Airy's junior and had been 'bowled over' by Airy's lecture-demonstrations that Forbes attended during a visit to Cambridge in 1831 (*ibid.* p. 430). However, Trevor Wheler refers to a letter about Fowler's machine addressed to 'Sir John Forbes' which casts doubt on this identification.

Seven months later, Sir Trevor Wheler, described by Fowler's son as one of the 'kind and sympathetic friends' who attempted to help the humble and struggling inventor, wrote to Airy enclosing a letter to Airy from Fowler and by way of mitigating Fowler's refusal to provide construction drawings, illustrated the way the machine manipulated ternary indices in three worked examples, one each for addition, subtraction and multiplication.<sup>186</sup> In his role as Fowler's patron and protector Wheler invited Airy to visit him in Torrington *en route* to or from the British Association meeting later that year where Fowler hoped to exhibit the machine.<sup>187</sup>

Fowler's letter to Airy, forwarded by Wheler, is one of personal humility, as well as deference for Airy's professional standing and attainments. The subtext of his eloquent and gentle appeal is that he regarded the plaudits of noblemen and of London savants entertained by a scientific curiosity as all very well, but what he craved was the critical scrutiny of a 'first rate man of science' to thoroughly investigate the principle and detail of the machine 'before it will be laid aside or adopted'.<sup>188</sup> Fowler explained that he was fully aware 'of the tendency to overrate one's own inventions' and indicated in an elaborate way that he was not so much seeking official scientific endorsement for a device he wished to promote, but sought expert confirmation for his own wonder at the efficacy and beauty of the machine's operation in the hope that 'men of ability far superior to his own' would later improve the machine to make it generally useful.<sup>189</sup> Daunted by the prospect

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<sup>186</sup> See in order, *Notice*, p. 173; Wheler to Airy, 13 May 1841. RGO6-427, f. 52; Fowler to Airy, 8 May 1841. RGO6-427, ff. 54-55.

<sup>187</sup> Wheler to Airy, 13 May 1841. The 1841 BAAS meeting was held in Plymouth (Morrell and Thackray (1981), p. 576.). At the time of writing the meeting was scheduled for August. In his reply to Wheler, Airy wrote that he did not propose to attend the British Association meeting that year. See Airy to Wheler, 19 May 1841. RGO6-427, f. 56.

<sup>188</sup> Fowler to Airy, 8 May 1841, f. 54.

<sup>189</sup> *Ibid.*

of the British Association meeting where he, an untrained and formally uneducated countryman without wealth or social position, would find himself in the company of scientific glitterati, he hoped that Airy would help him to draw attention to the machine. He ends his letter with an appeal of touching vulnerability, for Airy's protective support:

I have led a very retired life in this Town without the advantage of any hints or assistance from anyone, and I should be lost amidst the Crowd of Learned and distinguished persons assembled at the Meeting, without some kind friend to take me by the hand and protect me.<sup>190</sup>

Airy did not address his reply to Fowler but responded to Wheler's covering letter within a few days.<sup>191</sup> He thanked Wheler for his worked examples which had helped him appreciate the advantage of using the symbols '0', '+' and '-' instead of '0', '1' and '2' for the ternary notation but confessed that 'with regard to the general construction' he was 'still in great obscurity'.<sup>192</sup> While in no way wishing to disparage Fowler's ingenuity which he applauded, Airy's reservations about the utility of the machine are telling.

Airy's objection, as expressed in his letter to Wheler, was to mechanised calculation in general and the basis of his opposition was not any inherent deficiency of calculating devices, but the expertise required of staff to derive any benefit from them, and the difficulty of educating staff in their use:

The number of persons who can use even the common sliding rule is very small. I do not mention this as tending to exclude absolutely the advantage of

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<sup>190</sup> *Ibid.*, f. 55.

<sup>191</sup> Airy to Wheler, 19 May 1841. RGO6-427, f. 56.

<sup>192</sup> *Ibid.*

such mechanical contrivances, but as tending to limit it presently. They will only be used when there is a systematic preparatory education: thus all my Assistants are instructed to use the sliding rule: and I believe that officers of the Excise are trained to use it before they undertake active duties: without this express training the sliding rule would not be used. This applies more strongly to a more complicated machine especially when the precise [?] reduction is so heavy as it must be in Mr. Fowler's.<sup>193</sup>

Airy, apparently mindful of Fowler's feelings, cushions the blow:

You will have the goodness to understand these remarks as in no degree derogating[?] from the ingenuity of Mr. Fowler's construction, which I believe is very great: nor from its utility as used by himself or persons immediately around him: but only as expressing my views at to the extent of its utility to other persons.<sup>194</sup>

There is no recorded sequel to Airy's views on the Fowler engine. Yet there is an ironical connection with Babbage's failed enterprise. Fowler's son wrote after his father's death that had the resources been provided to render the machine in metal it might still be in use, 'the mechanism, so unlike that of Babbage, being so simple and yet so effective'.<sup>195</sup> He also wrote of his sadness at the recollection of

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<sup>193</sup> *Ibid.* Airy systematised the qualifications and level of competence of various grades of assistants at the Observatory. He formalised the requirements in arithmetic, mathematics, astronomy, written expression, and foreign language conversance for four grades of office: Supernumerary Computers, Junior Assistants, Superior Assistants, and Senior Assistant. The requirements were cumulative and increasingly demanding, each grade requiring the qualifications of the lower grade as well. See RGO6-814, ff. 197 (1-4). The grading sheets are dated 12 May 1857. They are listed in 'Printed Papers by G. B. Airy' (*Biog.* p. 384) under 'Knowledge expected in Computers and Assistants in the Royal Observatory'.

<sup>194</sup> Airy to Wheler, 19 May 1841, f. 56.

<sup>195</sup> *Notice*, p. 174.

the weary days and nights, of the labour of hand and brain, bestowed on this arduous work, the result of which, from adverse circumstances, was loss of money, loss of health, and final disappointment.<sup>196</sup>

It was with unmistakeable bitterness that Fowler's son wrote:

The government of the day refused even to look at my father's machine, on the express ground that they had spent such large sums, with no satisfactory result, on Babbage's "calculating engine," as he termed it.<sup>197</sup>

Nothing came of Fowler's machine, and to young Fowler's bitterness is added pathos when he records that his father dictated details of the machine on his deathbed 'while in great suffering from the disease of which he soon after died'.<sup>198</sup>

#### William Bell's Calculator

In terms of human misery the case of William Bell's calculator is less distressing. Bell wrote to Airy in 1849 about a circular logarithm instrument capable of continuous division or multiplication made for him by the scientific instrument makers, Troughton and Simms.<sup>199</sup> The principle was that of a circular slide rule, but a novel feature of the instrument was that it included a mechanism that automatically kept track of the position of the decimal point. Bell wrote that he

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<sup>196</sup> *Ibid.*, p. 173.

<sup>197</sup> *Ibid.*, p. 174, ft. For a discussion of the negative historical utility of Babbage's failed engine enterprise see Swade (1996, *Faber*), pp. 39-41; also *CWB*, 308-313.

<sup>198</sup> *Notice*, p. 173.

<sup>199</sup> Bell to Airy, 8 August 1849, RGO6-428, f. 186.

wished to bring the instrument to the attention of the Royal Society and sought Airy's opinion of its usefulness before doing so. He wrote to Airy on 8 August 1849:

Messrs. Troughton & Simms have made a circular logarithm instrument for me with which I can continuously multiply & divide by integers or decimal fraction indefinitely - the result (to an accuracy of 1 in 2000) & *the position of the decimal point in it* being found by one continuous operation - with scarcely the possibility of mistake as the instrument checks itself. If not beneath the notice of the Royal Society I should be glad to lay this instrument before it & should therefore [be] very much obliged by an opinion from you of its usefulness. I can forward the instrument if you would be kind enough to allow me to do so.<sup>200</sup>

Airy was away and did not reply for over a month.<sup>201</sup> Robert Main, the First Assistant, sent the customary holding letter.<sup>202</sup> On his return Airy responded expressing his willingness to see and exercise the instrument, and offered, with Bell's authority, to receive one directly from Troughton and Simms with whom Airy had constant dealings on matters relating to the Observatory's instruments.<sup>203</sup> Bell sent an instrument to Airy directly and Airy examined it without delay.<sup>204</sup> Airy confirmed that the instrument performed as Bell described and that the ability to keep track of the decimal point was highly desirable. Having softened Bell up with this opening commendation he detonates his charge:

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<sup>200</sup> *Ibid.* Emphasis original.

<sup>201</sup> From 28 July to 12 September Airy was on an expedition with his wife to Orkney and Shetland. See *Biog.* p. 202.

<sup>202</sup> Main to Bell, 9 August 1849. RGO6-428, f. 187.

<sup>203</sup> Airy to Bell, 15 September 1849. RGO6-428, f. 188.

<sup>204</sup> Bell to Airy, 18 September 1849. RGO6-428, f. 189.

Nonetheless I am confident that the instrument will never be used. The steps in the use of it are too many. In the particular specimen which you have sent me, the point size[?] is also a disadvantage. But I do not consider this as a general disadvantage, however if the linear dimensions were only 1/[?] of what they are in this specimen, the accuracy would be equal to that of a common sliding rule. The multiplication of several successive numbers may be effected with much greater ease by means of a common sliding-rule which has the slides side by side. The thing that is really wanted is a machine which, with no more settings than in a common sliding rule, will give decimal points. The straight sliding rule is more easily used than a similar instrument and therefore if such a [?] could be given to the straight rule, it would be best.<sup>205</sup>

His reasons are, as with the Fowler machine, based on an unfavourable comparison with his benchmark of operational convenience and established use – the slide rule. His objections to Bell's instrument were that it required too many steps to operate, was more difficult to use than the slide rule, and offered little improvement in accuracy. Ironically he wished to use the machine not to advertise its merits but as an example of the deficiencies of mechanical calculators in general. He informed Bell that he would not endorse the machine but was willing to draw it to the attention of a scientific society as a vehicle for illustrating his 'personal opinion upon the present wants of instrumental calculators'.<sup>206</sup>

Bell completely conceded 'the justices of Airy's remarks'.<sup>207</sup> In elaborate language which appears to conceal both deference and a dignified sensitivity for the reputation of his invention he explained that he was willing for the machine to be used as an illustration of Airy's views on the deficiencies of calculating devices,

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<sup>205</sup> Airy to Bell, 24 September 1849. RGO6-428, f. 190. The letter press copy is a poor one.

<sup>206</sup> *Ibid.*

<sup>207</sup> Bell to Airy, 28 September 1849. RGO6-428, f. 192.

but that since the purpose was a negative one he preferred that Airy's views were communicated without the embarrassment of exhibiting the device which he requested be returned to him in Bristol.<sup>208</sup> He went on to explain that he had indeed made a linear version as Airy recommended, and confirmed that the operational steps were fewer, but that he was unable to preserve the floating-point feature so attractive in the circular version. Airy was sufficiently interested in exhibiting the device to request Bell to leave the machine with him for exhibition at a forthcoming meeting of the Royal Astronomical Society.<sup>209</sup>

It is impossible to know if Airy's motives in exhibiting the calculator were genuinely pedagogical or whether there was a political dimension in wishing to demonstrably reinforce the effect of his views on the deficiencies of calculating devices as a form of public vindication of his known opposition to such aids. In any event, Bell agreed and Airy exhibited the device at the meeting.<sup>210</sup> A description of the machine and a more detailed version of his theme extolling the simple virtues of the 'sliding rule' appeared in the following Monthly Notices of the Society.<sup>211</sup> Here Airy is reported to have remarked that Bell's calculator 'was too expensive and too cumbersome to be extensively used' and that its 'peculiar defect' (the need to intervene when multiplying three or more numbers) was remedied by having two sliding scales on a linear slide rule.<sup>212</sup> That Airy sought a public platform to advertise his reasons for opposition to the machine indicates that he regarded his criticism as defensible and able to withstand the scrutiny of his peers.

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<sup>208</sup> *Ibid.*

<sup>209</sup> Airy to Bell, 6 October 1849. RGO6-428, f. 193. The meeting was scheduled for 9 November 1849, and this is the date of Airy's published account in the Society's 'Monthly Notices'.

<sup>210</sup> Bell to Airy, 8 October 1849. RGO6-428, f. 194.

<sup>211</sup> Airy (1849), p. 19. (Copy in RGO6-812).

<sup>212</sup> *Ibid.*



The episode of Bell's calculator concludes the case studies of the occasions on which Airy was consulted for his views on calculating machines and mechanical aids.

### Summary

This chapter presents three case studies in which Airy was consulted for his opinion on calculating devices. He was consulted by the Government in 1857 to advise on the purchase of the third Scheutz engine for the General Register Office, and, in 1859, to provide an acceptance report on the Scheutz engine following its delivery. In 1841 and 1849 his views on manual mechanical calculating devices were sought by their inventors. His views were uniformly sceptical in all cases, though the stated grounds for his criticisms of the engines differed from those he expressed for calculating aids.

The successful construction in 1843 of a working printing calculator made in a wooden frame by a young inexperienced Swedish engineering technician, using hand tools and a simple lathe, raises questions about the need for the high degree of precision demanded by Babbage in the construction of his Difference Engine. The contrasting levels of skill and capital in the two ventures support the suggestion that Babbage's perfectionism, expressed in his insistence on the highest manufacturing precision possible, and in his quest for the 'perfect' table, contributed to his eventual failure.

In 1843 the Scheutzes offered a fully engineered difference Engine to the British Government. There is no evidence to indicate that the timing of the Scheutzes' offer was directly influenced by the final abandonment of Babbage's project by Peel's government a year earlier. Airy was a member of the committee that rejected the Scheutzes' proposal, on the reported grounds that Parliament would be unlikely to support a foreign invention following its costly and unsuccessful experience of Babbage's machine.

A fully engineered Scheutz difference engine built in Stockholm was exhibited in London in 1854-5. Airy was not invited to serve on the Royal Society Committee convened to report on the machine. However, he viewed the machine privately and volunteered his views in published form. His letter to the Editors of the *Philosophical Magazine* is the first known printed statement in which he went on record with reasons for his opposition to the calculating engines. He provided a detailed breakdown of four commonly used tabulation procedures and concluded that the benefits of the engine were marginal, that the machine was too elaborate for its purpose, and that its promotion was driven by the interests of inventors and entrepreneurs ignorant of the needs of day-to-day tabulation. He suggested that instead of generating new tables by repeated addition, the machine might be of greater benefit if adapted to verify existing tables by repeated subtraction. The use of the engine in this way was novel and is something Babbage appears to have overlooked.

At the joint instigation of the Scheutzes and William Farr, the GRO requested the Treasury to fund the cost (£1,200) of making a copy of the fully engineered version of the Scheutz difference engine to meet the needs of an expanded Life Table for 1864. Airy was consulted on two distinct occasions: firstly

by the Treasury for his opinion on the use of the engine in three government offices: the Nautical Almanac Office, the GRO, and the Greenwich Observatory; secondly, to conduct an 'acceptance test' after the machine was delivered to the GRO in 1859.

Airy conducted a detailed survey of the potential use of the machine through a questionnaire sent to the Registrar General at the GRO, George Graham, and the Superintendent of the Nautical Almanac, J. R. Hind. Airy's own view of the potential use of the machine at the Greenwich Observatory was damning; Hind's views were equally dismissive. However, Farr's defence of the machine for use at the GRO was credible and authoritative. Airy's final report was negative. Nonetheless, out of respect for Farr, Airy recommended the purchase of the machine, though he registered his disagreement with Farr's position.

The machine was built by Bryan Donkin in London. The delivery deadline of eighteen months overran and Donkin made a loss of over £600. To authorise payment to Donkin, Graham asked Airy to inspect the machine and certify its satisfactory completion. Airy duly did so in August 1859 and in the course of his examination suffered a conversion of sorts. He wrote immediately to both Donkin and the engineer William Gravatt to ask if the machine could be converted from decimal to sexagesimal working for the direct tabulation of astronomical tables in degrees, minutes and seconds of arc. He had finally seen a potential use for the machine after decades of consistent opposition. Nothing appears to have come from this late flash of enthusiasm.

The GRO machine was used in the production of the 1864 Life Table but did not realise the hoped-for benefits: the machine was delicate, temperamental and required constant care to prevent derangement. Its fragility did not guarantee

infallibility, and only a small proportion of the 600 pages in the volume were generated by the machine. The savings on typesetting did not materialise and HMSO estimated that even if the whole of the Table had been calculated and stereotyped by the machine, the cost savings on production would have been only 10% of the cost of conventional production methods. The maintenance and repair costs ran to £144 guineas a year, which was the annual salary for a clerk-computer.<sup>213</sup> The engine, the first complete fully engineered difference engine, did not realise the Utopian ideal of a once-and-for-all handle-cranking solution to the production of error-free tables in terms of either cost or performance.

As well as consultations for the Government Airy's opinion was sought by at least two private individuals seeking endorsement for calculating devices of their own invention, namely, Thomas Fowler (1841) and William Bell (1849). Both their devices only partially automated the process of calculation in that they required manual intervention for their use. In this they were not fully automatic as were Babbage's and Scheutzes' engines. Airy's criticisms of the two categories of device are different. His criticism of Fowler's machine was that the complexity of its manual operation would limit its use, and the amount of training required by operators of the machine was likely to be prohibitive. Airy's exposure to the idea of Fowler's machine predates by eighteen months his outburst to Goulburn about Babbage and his engine. In the case of the engines, Airy denied that there was any need for tables of the kind the machine could produce. In the case of Fowler's and Bell's calculators his objections were based on the complexity of their operation and the lack of additional accuracy compared to his bench-mark standard of convenience and precision, the lowly slide rule.

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<sup>213</sup> See *G&F*, p. 285.

With the exception of his brief positive interest in 1859 in converting the third Scheutz engine for use in the Nautical Almanac Office, Airy consistently rejected the practical utility of automatic calculating device in his privately and officially expressed judgements. The practical and financial experiences with the third Scheutz engine at the GRO indicate that, despite the engine advocates, his scepticism was well-founded.

The three case studies in this chapter conclude the accounts of the occasions on which George Biddell Airy, Astronomer Royal, was consulted for his opinion on the utility of calculating devices. With the exception of his brief interest in the potential use of the Scheutz second engine for the GR his views were consistently negative.

## Chapter 6: Summary and Conclusions

*I was ignorant of that which no man could foresee*

– Charles Babbage, 1834

The previous chapters rely on close reading of Babbage's early texts and a relatively detailed investigation of the circumstances in which Airy was consulted for his views on calculating machines and devices. In serving the needs of chronicle the study so far has been, to some extent at least, comparatively narrow in its cultural scope. This chapter first summarises the findings of the case studies in which Airy expressed his views, and then seeks to locate the implications of the earlier findings in a broader interpretative framework. The implications of the new material to existing historical accounts is considered, and further research suggested by the material overall is indicated.

### Summary of Consultations

The following tables list the occasions so far discussed on which Airy's views on calculating devices were recorded or reported.

Airy's Consultations		
Date	Machine	'Client'
1841	Thomas Fowler's Calculator	Private (Fowler, Sir Trevor Wheler)
1842	Babbage's Difference Engine No. 1	Government (Chancellor, Henry Goulburn)
1843	Scheutz Difference Engine 1	Government (Home Secretary, James Graham)
1849	William Bell's Calculator	Private (William Bell)
1857	Scheutz Difference Engine 2	Government (Chancellor, George Lewis)
1859	Scheutz Difference Engine 2	Registrar General (George Graham)

Airy's Unsolicited Views	
1835	Via Thomas Robinson (Babbage's Engine - 'useless')
1837	Via William Macready (Babbage's Engine - 'humbug')
1856	Scheutz Difference Engine 2 (Letter to the Editors of <i>Phil. Mag.</i> )

The following listing is a compilation of Airy's views on the automatic calculating engines and mechanical aids discussed in the case studies:

Airy and Utility
<p>Calculating Engines</p> <p>Negative</p> <ul style="list-style-type: none"><li>• Difference engines are specialised, not general purpose</li><li>• No need<ul style="list-style-type: none"><li>• Manual methods were sufficiently accurate</li><li>• Demand for new tables was non-existent or at best infrequent</li><li>• No economic advantage compared to conventional methods</li></ul></li><li>• Reliability of results still dependent on manual starting calculations</li><li>• Demand was led by 'mechanists' and entrepreneurs not computational need.</li></ul> <p>Positive</p> <ul style="list-style-type: none"><li>• Value of experimenting through field use</li><li>• Response to public interest</li><li>• Potential for error-checking existing tables by differencing</li><li>• Potential for sexagesimal subtabulation for astronomical use.</li></ul>



### Calculating Devices

- Operation too complicated compared to slide rule
- Training for general use impractical
- No improvement in accuracy.

### Conclusions

Historical accounts of Airy's role in the fate of Babbage's calculating engines invariably confine themselves to the single perfunctory record in his 'Autobiography' stating that when consulted in 1842 by the Chancellor, Henry Goulburn, he gave his opinion that Babbage's Engine was 'worthless'. A major conclusion to be drawn from the case studies discussed in these chapters is that Airy's condemnation went beyond a dismissive one-liner. Rather, his portrayal to Goulburn in 1842 of Babbage as irrationally defensive of his invention, his attack on the neutrality of the Royal Society committee of 1823 on whose recommendation the Treasury funded the machine, and his assertion that the machine was a bad investment compared to the cost of conventional alternatives, were decisive factors in the Government finally abandoning the Engine project. While historical accounts have noted Airy's dismissive comment, the case study of the consultation in 1842 reveals more fully the extent to which Airy's agency was a defining one in the fate of Babbage's Engine. The new and detailed account of Airy's views presented here represents a significant addition to the

perennially revisited question of why Babbage failed to realise a complete engine in physical form.

The case studies of the six occasions on which Airy recorded his views on mechanised calculation further demonstrate that his dismissal of Babbage's Engine was not a single irritable aberration but was supported by technical reasons that he articulated in official correspondence as well as publication. His dismissal of Babbage's Engine was part of his consistent and durable scepticism about the value of mechanically assisted computation, whether automatic as in the case of the engines, or partially automatic as in the case of calculating devices of the kind promoted by Thomas Fowler and William Bell. The reasons for his opposition to automatic machines were different from those for partially automatic devices, but his position was uniformly negative.

That Airy 'went public' with his reasons, in private correspondence and in print, is not itself sufficient to dismiss Babbage's accusation that Airy's opposition was rooted in personal hostility, and it is legitimate to ask whether his stated reasons informed, were an artefact of, or a justification for his opposition. In officially consulting the Nautical Almanac Office and General Register Office through a detailed technical survey of the potential use of the Scheutz calculating engine in those offices, Airy appears, at least on the face of it, to be demonstrating a professional open-mindedness, and this is consistent with his self-portrayal as a conscientious servant of government. In deferring to William Farr in recommending, against the direction of his own findings, the purchase of the Scheutz engine for the GRO, Airy appears to demonstrate professional impartiality. However, there is suggestive evidence that makes it difficult to completely acquit Airy. His overtly scrupulous conduct over the Scheutz engine can be seen as a

show of consultative neutrality affected in response to Babbage's public allegations of personal prejudice published six years earlier in 1851. The letter to Babbage from Thomas Robinson in 1835 suggests that Airy was not above intrigue in scientific affairs, and the war over the telescope, which Babbage alleges was at the root of Airy's hostility, was already underway about three years before Robinson's letter.<sup>1</sup> It is also the case that Babbage was joined by David Brewster in suggesting that Airy's professional conduct was not always impartial. Brewster, a fellow Declinist and supporter of Babbage, alleged in 1841 that Airy, in reviewing a paper submitted by Brewster for publication, had rejected it from 'personal feelings'. Finally, Airy's outburst to Goulburn in 1842 exhibits irritation and even anger at Babbage's conduct. The show of feeling from an otherwise controlled and pragmatic man suggests that there was more in play than only his professional judgement of the utility of a calculating machine. While Airy was probably fairer to the engines than Babbage was willing to give him credit for there must remain some doubt that Airy's professional opinion was indeed influenced by personal antagonism as Babbage alleged.

Airy's role as government consultant and arbiter of the utility of the engines raises issues about expertise and the advisory systems of government. Until 1831, when the Astronomical Society was granted its Charter, the Royal Society, founded in 1662, was the only formal body representing science to government. It was a private society financed by contributions from its members and Fellows who elected their own Council and officers. In combining constitutional and financial autonomy with service to government, the Society's allegiances were uncomfortably split between the scientific community, whose interests it served and

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<sup>1</sup> South's confidence in Troughton's telescope mounting evaporated in 1831. Sheepshanks became involved towards the end of 1832. See Hoskin (1989), p. 184.

promoted, and a generally reluctant government to which the scientific community was increasingly beholden for funds. Its alliance with the establishment and its self-imposed need to remain politically neutral appear to have inhibited its effectiveness in supporting the scientific community's demands for increased public funding as scientific societies proliferated and consolidated during the 1830s.

Until the mid-1830s the source of scientific expertise was essentially institutional. The emergence of the 'scientific expert' and of a non-institutional 'expert class' coincided with the crisis of reform of the Royal Society, fuelled by the Declinists (amongst whom Babbage was prominent) who argued that the Royal Society was ineffective in its superintendence of science and in its promotion of the interests of the scientific community. Increasingly, bids to government for research funds were being made by influential individuals as well as the emerging new scientific societies, and the Royal Society's representational monopoly began to weaken.

The extension of recognised sources of expertise from institutions to individuals is reflected in the pattern of government consultations on the calculating engines. In 1823, 1829 and 1830 the engine question was referred to the Royal Society. However, by 1842, when Babbage pressed Peel for a resolution, in preference to a fourth referral to the Royal Society Peel sought private advice from John Herschel as well as Airy. Peel was an active patron of science and of the Royal Society. It was on his encouragement that annual government grants, the only substantial public source of funds for independent research, were later placed with the Society. Peel had every reason to expect ready service from the Society. His choice to 'go private' can be seen as indicative of the shift away from institutional sources of expertise towards individual experts. It is also possible that

Peel wished to kill off the project and, to the credit of the Society, by seeking private advice he sought to bypass institutional scrutiny that would argue the benefits of the engine on acceptably utilitarian terms. Again in 1843, when the Scheutzes offered their difference engine to the British Government, the Home Secretary convened an *ad hoc* committee of three (Airy included) to pronounce on the advisability of commissioning the machine.<sup>2</sup> Finally, in 1857, when the Registrar General petitioned Treasury for a fully engineered Scheutz engine, government turned to an individual, Airy, as the chief consultant. Babbage was out of the running. He was then sixty five, was associated with a famously expensive failure, and was politically 'unclubbable', tainted by a reputation for intemperate and ill-considered protest. Airy, on the other hand, was uniquely qualified for the job. He was in his prime as the elder statesman of civil science. He had been a brilliant student, was Astronomer Royal with a track record of scientific distinction and resolute management, and had direct experience of astronomy and astronomical tabulation. He had performed innumerable advisory services for government, had sat on countless commissions, discharging his duties with diligence and some distinction, and was a model servant of the establishment. He had in effect created the role of chief scientific advisor to government, a *de facto* post that he went on to command for some four decades. Airy thus both fed, and fed from, the emerging role of the 'scientific expert' and of the paid professional scientist.

Airy's revelations to Goulburn in 1842 confirmed for government the diminished role of the Royal Society as the sole appropriate source of scientific expertise. Airy's allegation that the Royal Society committees appointed to advise on the engines were stacked with Babbage's acolytes must have damaged

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<sup>2</sup> The interests represented in the membership of the committee were astronomy (Airy), navigation (Symonds), mathematics (probably de Morgan). For discussion of this episode see Chapter 5, pp. 244-5.

government's faith in the neutrality of the Society's recommendations. The seemingly automatic endorsement of all three requests for public money in support of the engines will have fortified any suspicion that the grand enterprise of science was being served at public expense without due consideration of the utilitarian principles by which public benefit was properly judged. Airy's revelations to Goulburn will have served to cast the Royal Society as a pressure group representing the self-interest of an increasingly vociferous and populous scientific community. The engine episodes can thus be seen as part of, and as contributing to, a major realignment in the management of science and of 'science policy' in the middle decades of the century.

A central issue raised by the new material is the extent to which it does or does not invite a revision of accepted historical accounts of the calculating engines. Lindgren, in his *Glory and Failure*, has gone further than most in locating the nineteenth century engine initiatives in a broader interpretative framework. His overall conclusion is that there was no market for difference engines and that the Scheutzes were misguided in the belief that observatories and national institutions would purchase such machines. He argues convincingly that leading experts of the day in England and on the Continent consistently maintained that there was no need for the machines. Lindgren's view of Airy is based heavily on his role in relation to the Scheutz engines. There is nothing in the new material presented in these chapters that challenges Lindgren's overall findings. Rather, the new material presented here on Airy's substantial role in the fate of Babbage's engine, and his views on partially automatic calculating aids, reinforces Lindgren's findings: the material provides additional evidence of Airy's criteria of utility as well as the grounds for his consistent opposition to such devices. The new material fills in

important detail in Airy's professional posture, but does not change its shape or outline.

However, the new material does invite a revision of at least one central feature of almost all historical accounts to date, namely the role of errors as the primary purpose and motive for the machines. That errors in tables were the initial stimulus for the conception of the machine is consistently upheld by contemporary as well as subsequent accounts. However, the analysis presented here suggests that for Babbage himself errors did not feature nearly as prominently as contemporary or latter day commentators have suggested, and this new reading gives a credible account of his curiously indifferent defence of the practical utility of his machines.

This thesis suggests that it was Lardner, not Babbage, who was the chief promoter of the engines, and that it was again Lardner not Babbage who was responsible for grandstanding errors as the primary purpose and justification of the machines. The close reading of Babbage's earliest writing discussed in Chapter 3 clearly demonstrates that Babbage was less preoccupied with machines as the solution to the problem of errors than with the engines as a new technology of mathematics, specifically with the notion of machine computation as a systematic solution to analytical equations, and the heuristic value of the machines to new branches of mathematical analysis. It is only at the point at which securing resources for a larger engine became a political issue that errors begin to feature more prominently. The promise of eliminating the risk of errors can be seen as a response to the need to justify the engines to others in terms of utilitarian practicality rather than in terms of pure science.

The question arises as to how errors became enshrined in historical accounts as the central issue. The new reading suggests that there have been two main contributory factors to the false prominence given to errors. Firstly, there is the anecdotal vignette of Babbage's mechanical epiphany when, exasperated by errors, he invokes 'steam' as the agent of redemption. That tabular errors provided a jumping-off point for the engines is not disputed. However, it seems historians, endlessly charmed by the episode, have translated the initial stimulus for the conception of the machines into a permanent motive. Secondly, and more importantly, there is Lardner's article published in 1834 which, more than anything, is responsible for the subsequent portrayal of errors as the central 'problem'. The revisionist account given here suggests that Lardner filtered out Babbage's more elevated aspirations for his engines when his tour hosts found the material too difficult and 'too scientific' for popular audiences. Instead, I argue, he gave false emphasis to the significance of errors in published tables as a rhetorical device. The circumstances in which he formulated and developed his public account of the engine venture suggest that his portrayal of engines as the 'solution' to the 'problem' of errors was a response to the needs of the lecture hall. The reduction of the primary purpose of the engines to the elimination of errors originated with Lardner, not with Babbage, and was a simplifying device intended to dramatise the engine venture to increase its appeal to popular audiences, and his motives for so doing lay in the fact that his livelihood depended on successful showmanship and public demand for his lecture tours.

The evidence shows that his article, which has had a defining influence on all subsequent historical accounts, was a written version of the lectures he developed for public consumption; that he wrote the article under pressure immediately after



his lecture tour; and that featuring errors as the central problem was part of a successful formula he had tried and tested on lecture platforms in the Northern industrial towns, and in London. The circumstances also suggest that if Babbage had any reservations about the false prominence given to errors then there were political reasons why he would have waived them: shortly before Lardner's lecture tour the engine project had collapsed and Babbage needed Lardner's gifts as a publicist to revive his fallen fortunes.

Lardner's 'dumbing down' of Babbage's original interests in the engines and publicly identifying their utility with their practical value in producing error-free tables, exposed the engines to attacks that were impossible to defend against. Leading astronomers, mathematicians, engineers and scientists in England and on the Continent, Airy included, rejected Lardner's case that there was a problem of supply as well as correctness in printed mathematical tables. Contesting the engines in terms of their practical utility put the engine advocates at a disadvantage and forced them to defend from a position of weakness. Despite his undoubtedly good intentions Lardner's public relations efforts in 'spinning' his account to serve the interests of public entertainment, can be seen to have done Babbage's interests fatal damage.

The interpretation that Babbage did not fully sign up to Lardner's portrayal of the utility of the machines, but went along with it, offers at least a partial explanation for why he was mystifyingly silent in defence of his machines against the assertions that they had no practical value. He was anyway an inept publicist, disdaining to lecture or publish on his machines. The design and implementation of his machines became ends in themselves and in his obsession with the logic and detail of the mechanisms he soon lost sight of the reasons that had first launched

him on his quest. His single-minded absorption in the detail of design has done nothing to contradict historians' perceptions that he still subscribed to the original purpose of his pursuits. But tables soon ceased to be his central concern and especially so after the conception of the Analytical Engine in 1834. However, this new reading suggests that he disdained to defend the engines, at least in part, because he did not accept that their primary justification was their practical utility to tabulation. A crucial clue can be found in his exasperated public outburst in 1851 in which he proclaimed that 'the Difference Engine and Analytical Engine are questions of pure science'. In this he appears to be signalling that to assess their significance on the grounds of practical utility alone is to mistake their essential worth. In the same statement he publicly challenged Airy to state his grounds if he considered the machine 'either useless or impractical'. Babbage appears to be provoking Airy with the accusation that his known pragmatism, the ruling principle of Airy's scientific conduct, disqualified him from acting as an arbiter of the engines' 'true' worth. Certainly the narrow criteria by which Airy judged the utility of Fowler's and Bell's calculating devices do not recommend his talents as a futurist. Identifying the utility of these devices with their implications to routine practices at the Greenwich Observatory, and using the lowly slide rule as a benchmark of convenience and accuracy, are not strong indicators of a visionary imagination. Babbage's frustration seems to have had its source in the unbridgeable gap between the visionary open-ended aspirations he entertained for his machines, and the mundaneness of Airy's narrow pragmatism.

There are several features of this study that invite further attention. One such is the question of tabular errors and accountability. With England a leading maritime power the importance of astronomical tables for navigation features as a

central interest of government in the engines. The prospect of improved tables for navigation was a strong incentive behind the Treasury's massive investment in Babbage's invention. Reports of shipwrecks, with graphic illustrations and vivid eyewitness accounts were constant public reminders of the dangers of life at sea. Life-saving contraptions to aid survival, many of them bizarre, feature prominently in catalogues and scientific magazines. Loss of life and capital through maritime disaster was a public preoccupation and an official concern. However, while there are innumerable reports of shipwrecks, many of which are attributed to navigational error, I have found no record specifically identifying tabular error as the cause. A sample of court martial records of naval disasters indicates that the unreliability of charts and maps and failures to adhere to computational procedures blurred issues of accountability and it is impossible to isolate the specific culpability of tabular errors in maritime disasters.<sup>3</sup> The absence of a single specific example of a shipwreck being attributed to an error in tables is curious. This omission invites further investigation into the operational culture in which tables were used and the reputation of tables amongst navigators and ships captains who relied on tables to determine their position at sea.

On a broader canvas the engine episodes provide an inviting study to exercise theories of technological change. In the Introduction I mentioned that the starting point of the thesis was the collapse, notional or actual, of the narrow technological determinism that has dominated historical accounts of Babbage's failures. The new material presented in the case studies provides a promising vehicle in which to revisit issues of technological determinism and to relocate the engine episodes between the hard determinist's view of technology as the engine

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<sup>3</sup> See for example May (1960).

of history, and the contextualist's view that historical forces are the essential determinants of change. So on the one hand there is the question 'what can the engines tell us about the nineteenth century?', on the other, 'what can the nineteenth century tell us about the engines?'

Since Babbage has been given something of a hard time by Airy in these chapters it would perhaps be a courtesy to usher him back onto the podium and let him have the last word. Towards the end of his life he expressed his confidence in the recognition of posterity:

If, unwarned by my example, any man shall undertake and shall succeed in really constructing an engine . . . upon different principles or by simpler mechanical means, I have no fear of leaving my reputation in his charge, for he alone will be fully able to appreciate the nature of my efforts and the value of their results.<sup>4</sup>

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<sup>4</sup> *Passages*, p. 450.

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## Appendix I: "This is the Engine which Charles Built"

Royal Greenwich Observatory Archives. George Biddell Airy Papers, Cambridge.

RGO-452      Miscellaneous Mechanics, Hydraulics

f. 282-3

20 June 1851, A. G. B.

[Not to be printed]

The writer of the following pages, lamenting with some of his friends, that a work lately published by Charles Babbage, Esquire, has evidently produced a smaller effect than its Author had anticipated, has thought that he could not employ a few hours better than in conveying to the public the interpretation of one portion of the work in the form of a Paraphrase. The part which he has selected is the Section in which the rejection by the Government, of the Calculating Engine is explained. The structure of that Section being concatenated, and its spirit being practical, or rather romantic, the writer thought that he could not do better than adopt, as a general model for his versified translation, the prose lyric which combines the same characters, and with which every Englishman is familiar from his earliest youth. He relies however, on the imagination of his readers for supplying the portraits of men and things which are necessary to make the imitation strictly complete.

f. 284            This is Babbage the Censor, whose anger arises  
                       When he thinks how the nation distributes its prizes;  
                       The claims of philosophers meet no attention!  
                       Only think of the "difference" machine, his invention!  
                       He planned it completely: some money he spent:  
                       Then, backed by some friends, to the Treasury went:  
                       Sixteen thousand good pounds was permitted to touch,  
                       But found that he wanted at least twice as much:  
                       And, when the supplies from the nation were stopped,  
                       All further attempts at completion he dropped,

And abandoned the Engine  
that Charles built.

- f. 285 This is South, who once measured of stars not a few,  
But with Babbage's engine had nothing to do:  
Nor united with Babbage in the love of equations,  
Or in taste for mechanics, or long calculations:  
But, when Babbage wrote books on "Decline" of our "Science",  
To Babbage was joined in offensive alliance:  
Now they're closely united as brother to brother:  
Who opposes the one must be foe to the other:  
And all who have, dared South's designs to oppose  
Must submit to the charge (as the argument shows)

Of suppressing the Engine  
that Charles built.

- f. 286 This is Couchoix's great tube, with its object-glass splendid  
Which Sir South for a huge Equatorial intended;  
The mounting failed once - then efficient was made;  
In his dome it was mounted; the artist was paid;  
And then in a frenzy of anger and sport,  
He broke it to pieces (so goes the report);  
And the glass, ever since, (if the rumour tells true),  
has scarcely been used star or planet to view.  
Little thought the French artist, those lenses who ground,  
That in them would the true explanation be found  
Of the fall of the Engine that Charles built.

- f. 287 These are Troughton and Simms, in the City who dwell,  
And these astronomical instruments sell:  
To them was the telescope-mounting committed;  
The plan was approved, all the pieces were fitted:  
The stand, in the first trials, was somewhat unsteady,

But Troughton and Simms with their bracings were ready;  
 All was firm: still Sir South the due moneys withheld,  
 Till the arbiter's verdict the payment compelled.  
 but long, long, the reader may puzzle his brains  
 Before he will see how this lawsuit explained

The neglect of the Engine that Charles built.

f. 288 The is Clergyman Sheepshanks, of language ferocious,  
 Who terrified Babbage with menace atrocious:  
 With Troughton by long-standing friendship connected:  
 The steps of his work he had often inspected,  
 Till with mixed indignation and sorrow he learned  
 That South refused payment of money well earned;  
 Then Troughton and Simms he resolved to assist,  
 And ventured both Babbage and South to resist;  
 Through the long-delayed process so ably he pleaded,  
 That in "Troughton and Simms versus South" he succeeded.  
 The man who could meet and could conquer that knight,  
 Without doubt, (argues Babbage), will turn all his spite

To ruin the Engine that Charles built.

f. 289 This is Airy of Greenwich, who holds the Commission  
 Of Queen's Observator, by Royal permission:  
 The Government load him with many more cares  
 Than the mind of one man with impunity bears\*:  
 His opinions, in general, are quite undecided:  
 By Sheepshanks's judgement alone he is guided:  
 And when Sheepshanks will man or invention assail,  
 Airy joins in the onslaught with tooth and with nail.  
 And thus, if a judgement he's called to pronounce,  
 He is sure in the strongest of terms to denounce.



And Sheepshanks, of course, would command its rejection:  
For, when South would not pay for his telescope-stand,  
And Troughton appealed to the law of the land,  
Sheepshanks managed the suit, from beginning to end,  
And, of course, he hates South, and South's intimate friend:  
And Babbage, 'tis said, in that title rejoices,  
Since Babbage and South for "Decline" raised their voices.  
Thus the weakness of two, and the malice of one,  
Have stopp'd the construction so boldly begun;  
And greatly we fear that the world is now cheated  
Of the prospect of seeing by Britain completed

The half-finished Engine that Charles built.

## Appendix II: Airy's Questionnaires

Royal Greenwich Observatory Archives  
George Biddell Airy Papers, Cambridge

RGO6-454 Scheutz's Calculating Machine

f. 458

Airy to George Graham [Registrar General]  
Royal Observatory, Greenwich  
19 August 1857

Dear Sir

Mr. Scheutz, one of the inventors and constructors of the Swedish calculating machine, has offered to the Government to construct one of his machines for a stipulated sum of money, and the Government has inquired of me whether such a machine would be useful in the principal Offices, including the General Register Office, in which extensive calculations are made. For my guidance in answering correctly, I beg leave to place some questions before you, and to solicit your information on the points which they raise, and on any collateral points that may present themselves.

First, I remark that an authentic account of the principal peculiarities and of the powers of the machine, is to be found in the Report of a Committee of the Royal Society, printed in the Proceedings of the Royal Society, a copy of which I transmit by post (and which there is no need to return to me). You will perceive that the machine exercises the following functions only: that, when furnished with the first number of a series and with four orders of differences, it will form the successive numbers of the series by addition, and will print the results on plates of metal: but it will not perform any other calculation.

This being understood, I suggest the following queries as bearing on the applicability of such a machine to the calculations of your Office:-

1. How often do you construct new Tables of general and permanent character {e.g. Tables of Logarithms, Tables peculiar to the science of Probabilities, Life Interests, etc} by the method of preparing numbers for distance to distance and preparing the differences up to the 4th, and forming the remaining numbers by adding the differences?

2. If you construct such tables, do you desire at once to print them on plates of metal?
3. Do you consider it any disadvantage that no record is preserved of any of the intermediate steps of the work?
4. How often do you construct tables of transient character (e.g. such as are calculated afresh from year to year for each year's Reports) by the use of the 4th differences?
6. Supposing that you had at hand a 4th difference machine, not often used: would you prefer to employ a man to put the machine in adjustment and work it? or to employ a computer in the ordinary way with pen and paper?
7. Do you often use third differences or second differences in the formation of tables? And if so, would it in your judgement, be advantageous to employ a machine limited in its powers to 3rd differences or 2nd differences for the computations?
8. Can you estimate the proportion of the trouble of preparing the differences to the trouble of applying them?
9. How many successive numbers do you ever form by the use of the same fundamental differences?
10. In your present practice, do you use any mechanical aids to computation, as sliding rules, adding machines, etc?

Other points will probably occur to you on which your information would be relevant and useful.

I shall be gratified by your early answer; and I am, my dear Sir,

Your very faithful servant

G. B. Airy

f. 470

Copy of Professor Airy's questions to the Registrar General and of the replies to them. [Undated]

1. How often do you construct new Tables of general and permanent character {e.g. Tables of Logarithms, Tables peculiar to the science of Probabilities, Life Interests, etc} by the method of preparing numbers for distance to distance and preparing the differences up to the 4th, and forming the remaining numbers by adding the differences?

1. *Probably every two years or oftener.*
2. If you construct such tables, do you desire at once to print them on plates of metal?
2. *Yes - with a view to their being printed for publication.*
3. Do you consider it any disadvantage that no record is preserved of any of the intermediate steps of the work?
3. *No disadvantage.*
4. How often do you construct tables of transient character (e.g. such as are calculated afresh from year to year for each year's Reports) by the use of the 4th differences?
4. *The machine if at my disposal may be sometimes used for the Tables of transient character: but it would generally not be used for such calculations.*
5. Do you desire at once to print them on metal?
5. *Yes*
6. Supposing that you had at hand a 4th difference machine, not often used: would you prefer to employ a man to put the machine in adjustment and work it? or to employ a computer in the ordinary way with pen and paper?
6. *I should chiefly employ the machine for long series of calculation, to which the machine is particularly applicable.*
7. Do you often use third differences or second differences in the formation of tables? And if so, would it in your judgement, be advantageous to employ a machine limited in its powers to 3rd differences or 2nd differences for the computations?
7. *For all the Tables which I propose to construct four differences are required; and even five differences may be useful.*
8. Can you estimate the proportion of the trouble of preparing the differences to the trouble of applying them?
8. *Yes. The trouble of preparing the differences would be small.*
9. How many successive numbers do you ever form by the use of the same fundamental differences?
9. *Not more than 60.*
10. In your present practice, do you use any mechanical aids to computation, as sliding rules, adding machines, etc?
10. *Various contrivances of the kind have been tried; but none of them are applicable to the work of this Office - except - Mr. Scheutz's machine.*



f. 454

Airy to J. R. Hind  
 Royal Observatory  
 Greenwich  
 18 August 1857

My dear Sir

[Preamble identical to Airy's letter to George Graham. See above f. 458].

1. How often do you construct new Tables of general and permanent character {e.g. Tables of Logarithms, Tables of Variant Sines, etc} by the method of preparing numbers for distance to distance and preparing the differences up to the 4th, and forming the remaining numbers by adding the differences?
2. If you construct such tables, do you desire at once to print them on plates of metal?
3. Do you consider it any disadvantage that no record is preserved of any of the intermediate steps of the work?
4. How often do you construct tables of transient character (e.g. places in the Nautical Almanac) by the use of 4th differences?
5. Do you desire at once to print them on metal?
6. Supposing that you had at hand a 4th difference machine, not often used: would you prefer to employ a man to put the machine in adjustment and work it? or to employ a computer in the ordinary way with pen and paper?
7. It is understood that you employ third [second' deleted] extensively in preparing the hourly places of the Moon in the Nautical Almanac. Would it, in your judgement be advantageous to employ a machine adapted to third [second' deleted] differences for these computations?
8. [deleted]
10. Do you in the present practice use any mechanical aids to computation, as sliding rules, adding machines, etc?

Other points will probably occur to you on which your information would be relevant and useful.

I shall be gratified by your early answer, and I am, my dear Sir,  
 Yours very truly  
 G. B. Airy

I may perhaps propose the following queries supplemental to No.7.

8. What proportion does the trouble of preparing the differences bear to the trouble of applying them?
9. Do you ever apply the same differences to form more than twelve numbers?

[The original letter copy is difficult to read. The text has been reconstructed with reference to Airy's letter to George Graham dated 19 August 1857 (see f. 458 above) and from the clerk's copy].

f. 461

J. R. Hind to Airy  
20 August 1857

99 Montpelier Road  
Brighton

My dear Sir,

Your letter of the 18th inst. has been forwarded to me here. I will endeavour to reply briefly and clearly to the several queries you have proposed to me, with reference to the applicability of a calculating machine such as that made by Mr. Scheutz to the computations of the Nautical Almanac Office.

- (1) I think I may reply - never.
- (2) is answered by the reply to (1)
- (3) Although the work would check itself, it occurs to me there might be cases in which a record of intermediate steps would be desirable: still I should not consider the absence of such record a very serious disadvantage.
- (4) Annually; and this description of work occupies three or four computers about a month.
- (5) It would chiefly depend upon the practicability of writing the several parts so as to form a neat page or whole of which I doubt, that is, I doubt if so neat a specimen as the present Nautical Almanac page could be produced.
- (6) Certainly to "employ a computer in the ordinary way" because, from the

extensive practice of some of the Nautical Almanac Computers in differencing and the improved interpolation tables some time since introduced, very few errors are committed and the work is performed quicker than a machine would turn it out.

- (7) Fourth differences are used in the hourly places of the Moon and the facility and accuracy with which the results are produced leave little chance of any advantage being obtained by the use of the machine.
- (8) The trouble of preparing the differences that is, of taking them to the fourth order as compared with that of obtaining the various equations and producing the final results will vary: Supposing the Moon's places computed for intervals of 12 hours and required for every hour the proportion would be about as 1 to 5 or 6.
- (9) Never
- (10) None: Small special tables have always been preferred by the computers.

I do not know that I can add anything likely to be of service to these direct replies to the questions proposed in your letter.

I remain, My dear Sir,  
Yours very truly  
J. R. Hind



1. Charles Babbage (1791-1871), 1847-51.  
(Daguerreotype by Antoine Claudet) (NPG)



2. George Biddell Airy (1810-1892).  
(Illustrated London News, 4 Jan. 1868)



3. Charles Babbage, 1860.  
(Bernard Howarth-Loomes)



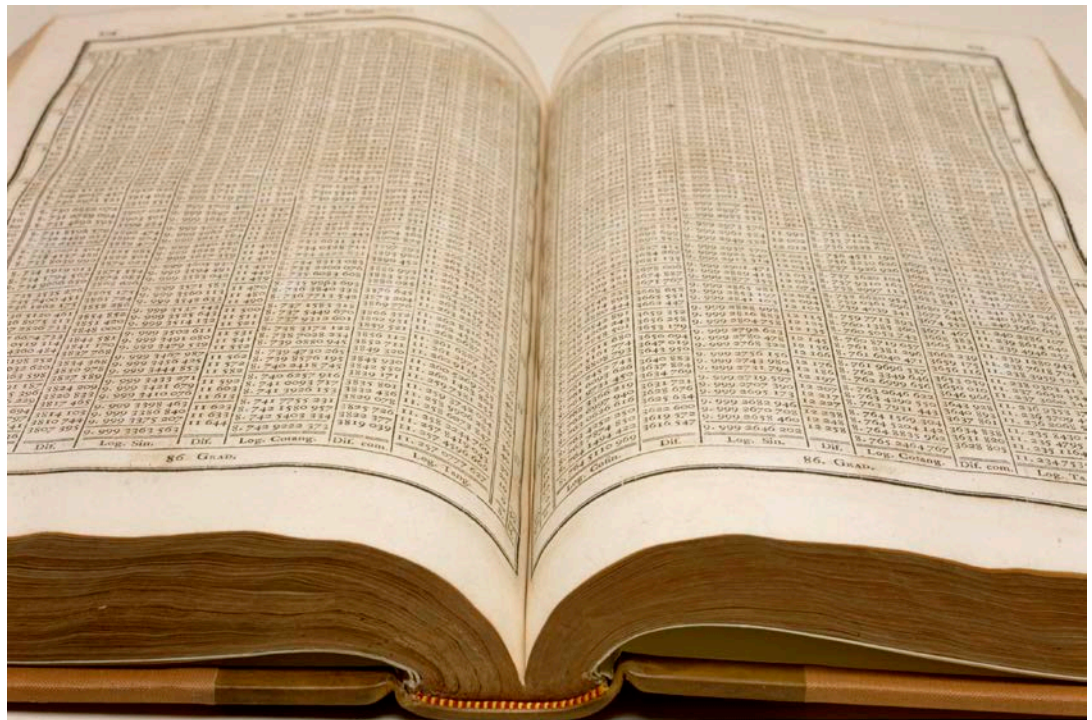
4. George Biddell Airy in old age.  
(Amati Papers, Cambridge University)



Logarithmorum vulgarium.

N.	Log.	N.	Log.	N.	Log.	N.
667	2. 824 1258 339	734	2. 865 6960 599	801	2. 903 6325 161	867
668	2. 824 7764 625	735	2. 866 2873 391	802	2. 904 1743 683	868
669	2. 825 4261 178	736	2. 866 8778 143	803	2. 904 7155 453	869
670	2. 826 0748 027	737	2. 867 4674 879	804	2. 905 2560 487	870
671	2. 826 7225 202	738	2. 868 0563 618	805	2. 905 7958 804	871
672	2. 827 3692 731	739	2. 868 6444 384	806	2. 906 3350 418	872
673	2. 828 0150 642	740	2. 869 2317 197	807	2. 906 8735 347	873
674	2. 828 6598 965	741	2. 869 8182 080	808	2. 907 4113 608	874
675	2. 829 3037 728	742	2. 870 4039 053	809	2. 907 9485 216	875
676	2. 829 9466 959	743	2. 870 9888 138	810	2. 908 4850 189	876
677	2. 830 5886 687	744	2. 871 5729 355	811	2. 909 0208 542	877
678	2. 831 2296 939	745	2. 872 1562 727	812	2. 909 5560 292	878
679	2. 831 8697 743	746	2. 872 7388 275	813	2. 910 0905 456	879
680	2. 832 5089 127	747	2. 873 3206 018	814	2. 910 6244 049	880
681	2. 833 1471 119	748	2. 873 9015 979	815	2. 911 1576 087	881
682	2. 833 7843 747	749	2. 874 4818 177	816	2. 911 6901 588	882
683	2. 834 4207 037	750	2. 875 0612 634	817	2. 912 2220 565	883

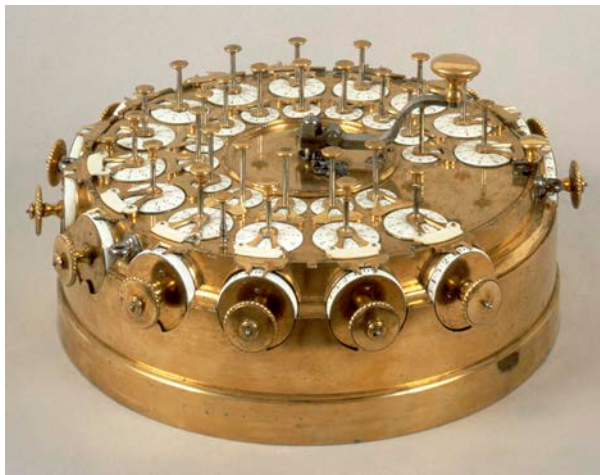
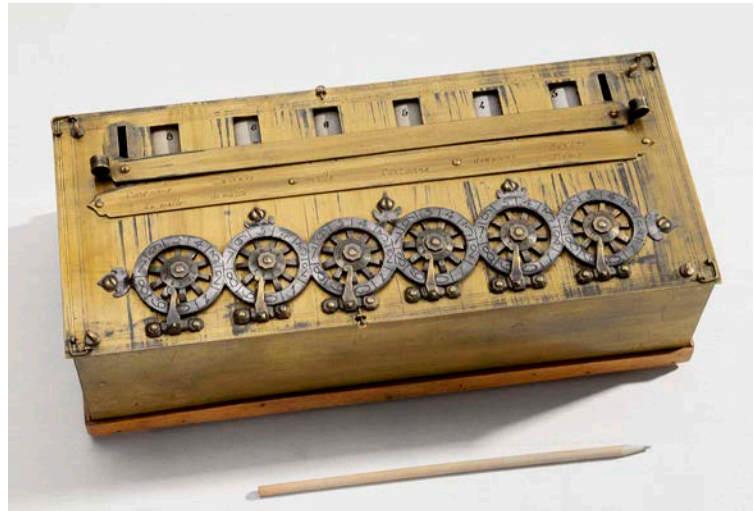
5. Page of Tables from *Thesaurus Logarithmorum*, Von Vega, 1794 (Detail). (Science Museum)



6. *Thesaurus Logarithmorum*, Von Vega, 1794. (Science Museum, London)

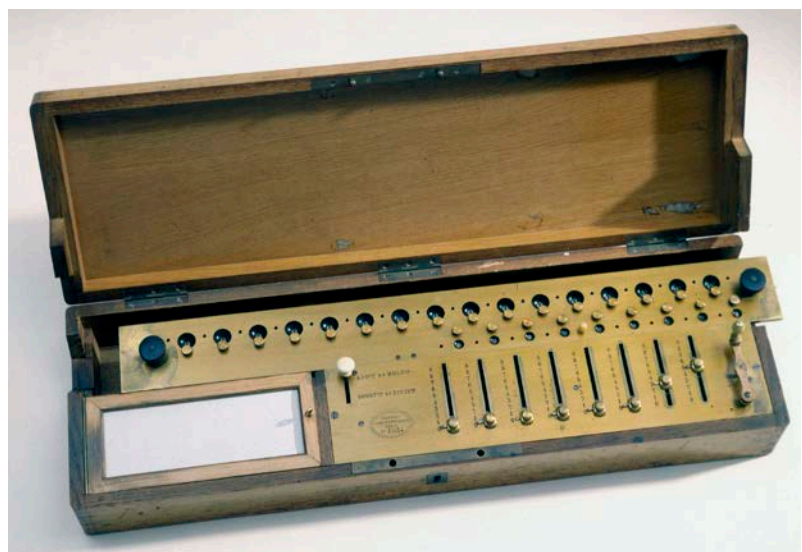


7. Pascal's Calculator  
(Replica), 1642.  
*(Science Museum, London)*



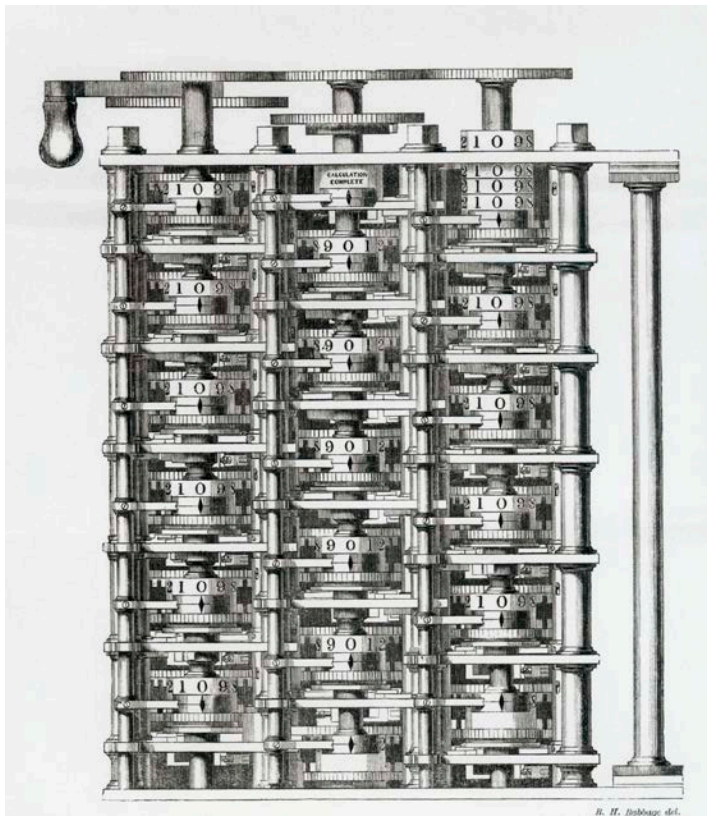
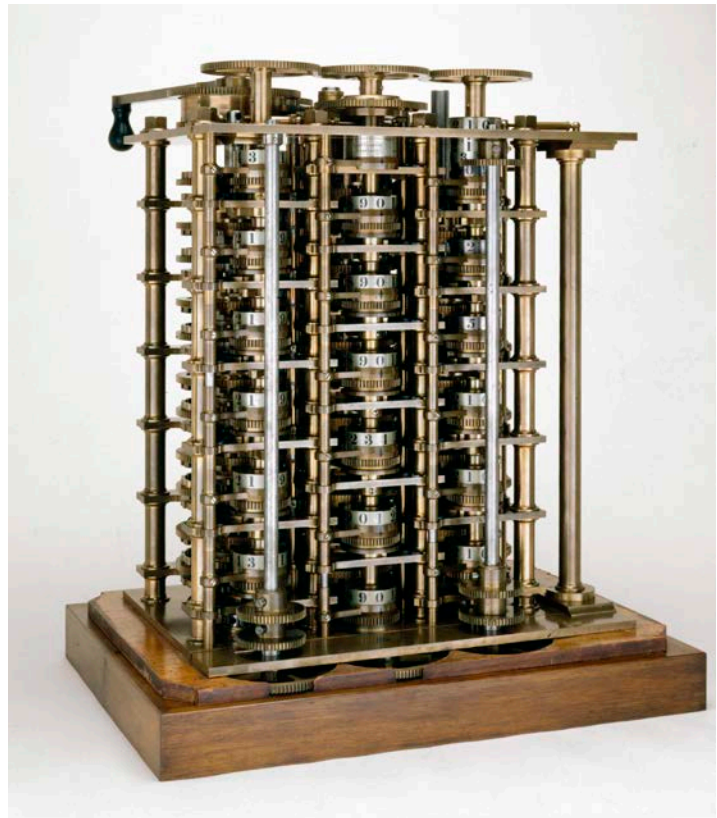
8. Johann Müller's 'Universal'  
Calculator, 1784.  
*(Hessisches Landesmuseum,  
Darmstadt)*

9. Thomas de Colmar  
Airthmometer,  
c. 1890.  
*(Science Museum, London)*



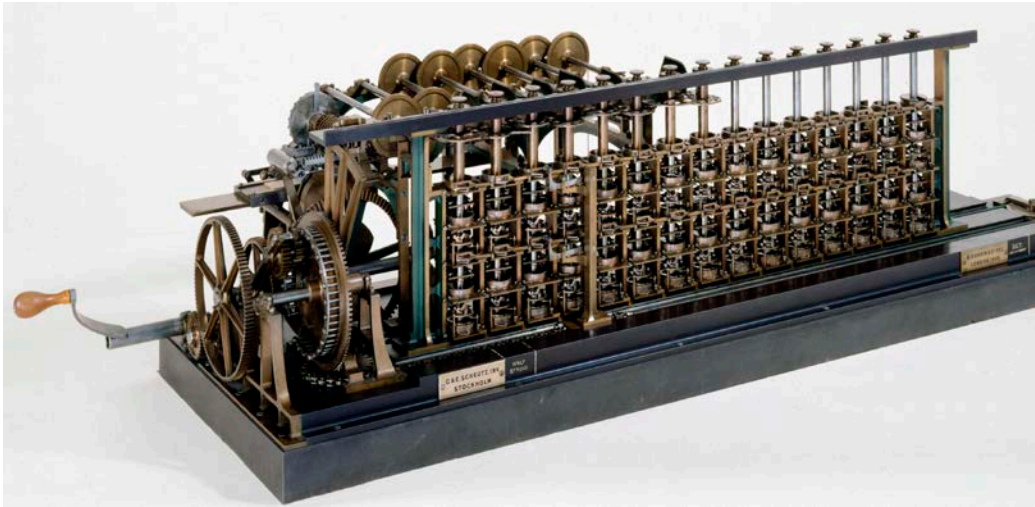
10. Difference Engine No. 1,  
Portion, 1832.

*(Science Museum, London)*

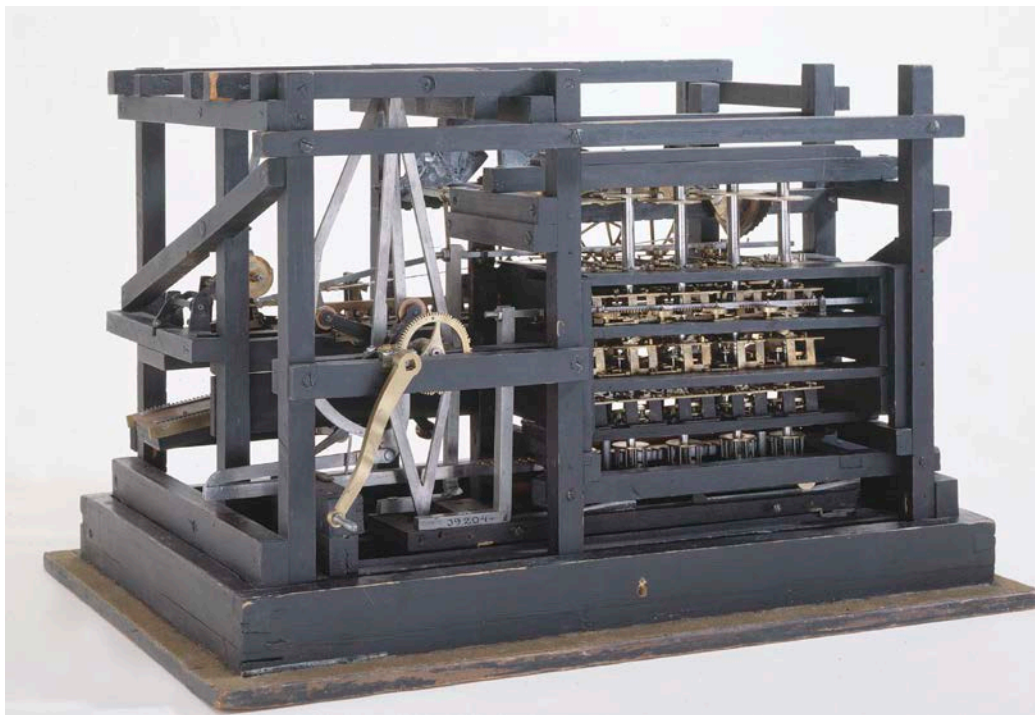


11. Difference Engine No. 1,  
Woodcut  
by B. H. Babbage, 1853.

*(Science Museum, London)*

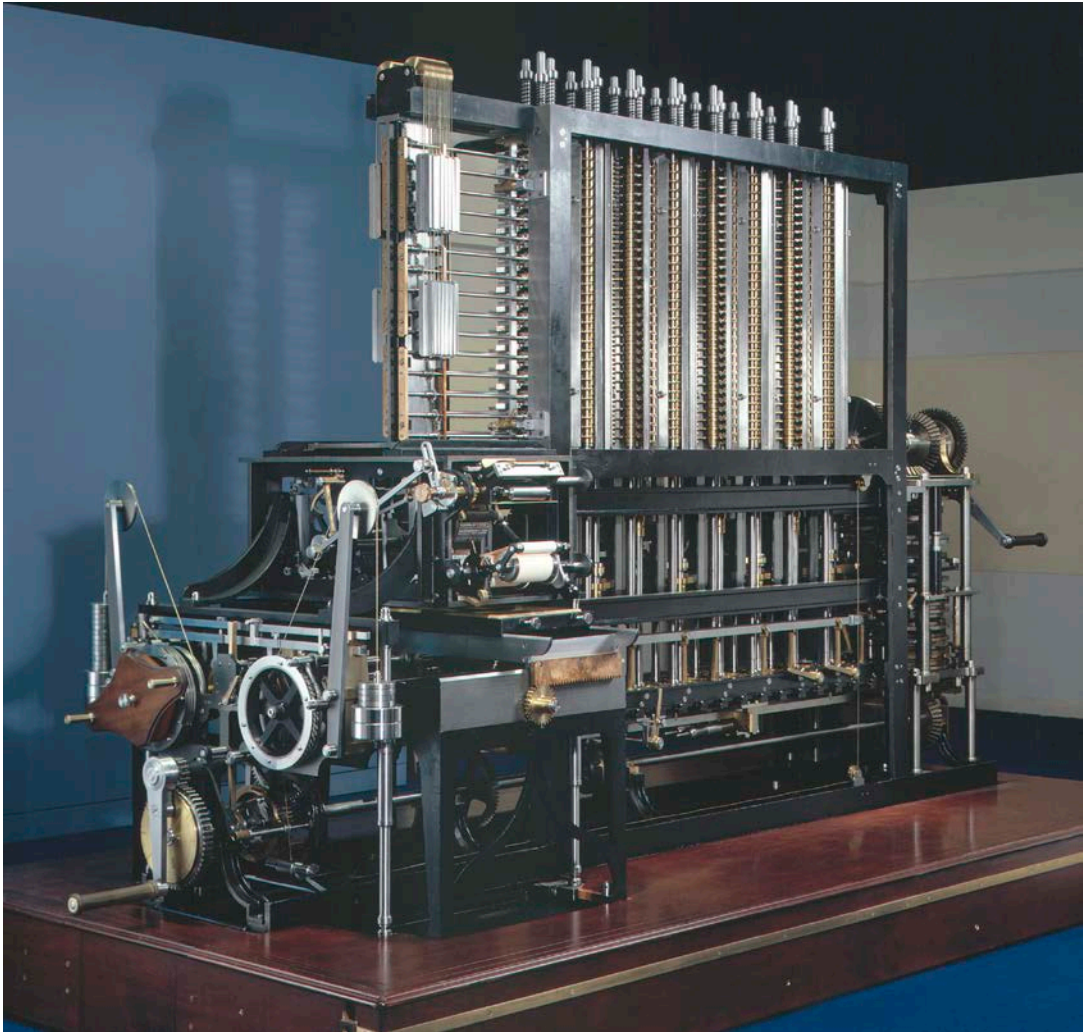


12. Scheutz Difference Engine by Bryan Donkin for the General Register Office, 1859.  
(*Science Museum, London*)



13. Prototype Difference Engine by Georg and Edvard Scheutz, 1843.  
(*Nordiska Museet, Stockholm*)





14. Charles Babbage's Difference Engine No. 2. Designed 1847-9, completed 2002.  
Size: 11 feet long, 7 feet high. Weight: 5 tonnes. No. of Parts: 8,000.  
(Science Museum, London)



15. Stereotype Punches for Difference Engine No. 2, 2002.  
(Science Museum, London)



16. Stereotype Mould in Plaster of Paris, for Difference Engine No. 2, 2002.  
(Science Museum, London)