

AN ABSTRACT OF THE THESIS OF

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Title: CHARLES BABBAGE, PHILOSOPHER, REFORMER,  
INVENTOR: A HISTORY OF HIS CONTRIBUTIONS TO  
SCIENCE

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Abstract approved

James Brookes Spencer

This history concerns the scientific contributions of Charles Babbage (1791-1871) to mathematics, to the invention of calculating machines, and machine tools, to the development of scientific ideas through a wide variety of scientific investigations and his involvement in several different scientific reforms. Babbage considered himself a philosopher in the broadest sense of the word and looked upon the whole of science as his domain of inquiry. The broad speculative and philosophical aspects of his scientific inquiries; the theoretical and technical aspects of his work relating to mathematics and calculating machines; and his ideas concerning the development of science as a profession and the role of government in the support of science will be examined in relation to his scientific career.

Babbage began his scientific career in 1812, when, as an undergraduate at the University of Cambridge, he joined with John Herschel (1792-1871), Edward Ffrench Bromhead (1789-1855), George Peacock (1791-1858), and other students to form the Analytical Society for the purpose of promoting the study of mathematical analysis and introducing the methods and notation of the differential calculus of Gottfried Wilhelm Leibniz (1646-1716) to replace the fluxional calculus of Isaac Newton (1642-1716). Babbage and his friends were actually carrying out a reform movement begun by Robert Woodhouse (1773-1827), a professor at Cambridge, whose ideas had not gained much acceptance until the Analytical Society was formed. The members of the Society published mathematical works which utilized Leibnizian methods and notation; translated and supplemented an elementary calculus textbook by Sylvestre François Lacroix (1765-1843), a French mathematician; wrote and published three books of examples to accompany Lacroix's textbook; and gained control of the mathematical content at Cambridge by becoming moderators of the annual Senate House examinations and posing examination questions which made use of Leibnizian methods and notation. Through these activities the members of the Analytical Society were successful in achieving their mathematical reform by about 1822.

Babbage and his associates continued to promote scientific reform throughout the eighteen twenties and thirties by attempting,

unsuccessfully, two reforms in 1828 and 1830 of the Royal Society; by successfully reforming the Nautical Almanac and Astronomical Ephemeris which prior to 1811 had been the foremost work of its kind in Europe; and by forming new scientific societies. After Babbage and his associates were unsuccessful in attempting the reform of the Royal Society in 1830, David Brewster (1781-1839), a scientist and Vice-Chancellor of the University of Edinburgh, assumed the leadership for the group and founded the British Association for the Advancement of Science in 1831. Babbage participated in the British Association between 1832, when he was named a permanent trustee, and 1839 when he resigned because of an intrigue by another trustee of the Association.

These reform activities and his mathematical researches form the first aspect of Babbage's career, between about 1812 and 1840. Babbage established a reputation as a first-rate mathematician through his work in the calculus of functions, in statistics, and in probability theory. As the result of his mathematical work, he was named Lucasian Professor of Mathematics, a chair which Newton had held and which Babbage held from 1828 to 1839. Babbage was also instrumental in the founding of a statistical section of the British Association in 1833. In 1834, the statistical section was established as a separate society, the London Statistical Society, and it was in this Society that Babbage remained active for the remainder of his life.

A second aspect of Babbage's scientific career began about 1820 and was devoted to the development of automatic calculating machines. His first calculating machine, Difference Engine No. 1, was designed to calculate and print mathematical and astronomical tables. Babbage received a gold medal from the Astronomical Society of London in 1823 for the invention of Difference Engine No. 1. He worked upon this engine from 1823 to 1833, during which time he received support from the government. After 1833, following a dispute with his engineer, Joseph Clement, Babbage ceased work upon Difference Engine No. 1 and began work upon a new calculating engine, the Analytical Engine. Babbage continued to negotiate with the government concerning Difference Engine No. 1 until 1842, when the government formally withdrew its support of the engine.

Babbage continued to work upon the Analytical Engine for the remainder of his life. This machine employed many of the features of modern digital computers--punched cards for data and instructions; an arithmetical-logical unit; a unit to store instructions, data, and results; and a printing device for recording the results of the calculations. In 1848, Babbage also drew up plans for a Difference Engine No. 2 based upon the improvements resulting from the development of the Analytical Engine. While none of the calculating machines were completed, the principles upon which they were based were fully demonstrated by the portion of Difference Engine No. 1 which was

assembled in 1833; by the advancement of the machine tool industry and mechanical engineering through the development of new machine tools, new standards of tolerances and mechanical drawings, and a mechanical notation; and by the advanced principles employed in the Analytical Engine which was capable of calculating any function and was therefore an entirely general calculating machine.

In addition to his reform activities, mathematical researches, and work with calculating engines, Babbage carried out many varied scientific experiments which were reported in publications throughout his career. Although he is remembered primarily for his work as a reformer and as an inventor of calculating machines, Babbage possessed broad interests and an inquiring intellect as demonstrated by such diverse publications as a book on life assurance societies, a paper on his invention of occulting signal lights for lighthouses, a paper on his experiments with magnetism, and a paper on the principles of turning and planing metals.

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**Charles Babbage, Philosopher, Reformer, Inventor:  
A History of His Contributions to Science**

**by**

**Walter Lyle Bell**

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## PROLOGUE

Charles Babbage (1791-1871) was well known throughout Europe for his contributions to science during a major part of the nineteenth century. Among the most notable of his contributions were those in mathematical analysis and in the development of universal, automatic calculating machines. He was also well known for his efforts to reform science in England. It was his hope to raise science to the status of a profession in England so that it was comparable if not superior to science in other parts of Great Britain as well as on the Continent. This dissertation is intended to provide an analysis of the above contributions to science in relation to the existing state of science in the nineteenth century. Biographical details of Babbage's life will be presented in order to provide a perspective from which to view his professional and scientific activities and his interaction with friends and associates in the scientific community throughout Europe. For example, during the years from 1814 to 1828, Babbage applied for several different positions which he hoped would provide both an income and an opportunity to pursue science as a profession, but in each instance he was unsuccessful.<sup>1</sup> These experiences served to

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<sup>1</sup> Charles Babbage, Pamphlets 1815-1864 (London: By the Author, n.d.), pp. 1-27. Babbage applied for the Professorship of Mathematics at East India College at Haileybury 1816 and for the Professorship of Mathematics at the University of Edinburgh in

strengthen his resolve to procure governmental and private patronage for science in order to encourage talented young men to pursue science as a profession.

Babbage was the only surviving son of Benjamin Babbage, a wealthy banker, and Betty Plumleigh Teape Babbage, a descendant of a well-known Devonshire family. He was born December 26, 1791 in Totnes, Devonshire and was one of four children with two brothers who died in infancy and one sister who survived him.<sup>1</sup>

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1819. In both quests he was unsuccessful. In Charles Babbage, Passages from the Life of a Philosopher (London: Longman, Green, Longman, Roberts, & Green, 1864), pp. 473-474, Babbage stated that he had no "interest," meaning support, for the 1816 vacancy and that "not being a Scot, I was rejected at Edinburgh." In Pamphlets, p. 4, he gave an impressive list of names of those who had written letters of recommendation for him: John Playfair; James Ivory; Sir William Herschel; John Herschel; Peter Roget, M.D., later author of the Thesaurus of English Words and Phrases (1852); John Leslie; William Whewell; John Pond, Astronomer Royal; Sir Joseph Banks; Richard Gwatkin, Fellow at St. John's College and Senior Moderator of the University of Cambridge; George Peacock; Rev. Dr. Milner, Dean of Carlisle, President of Queen's College, Cambridge and Lucasian Professor of Mathematics; Davies Gilbert, M.P., F.R.S., and Commissioner of the Board of Longitude; and the following members of the Institute of France: Jean Baptiste Biot; François Sylvestre Lacroix; and Marquis La Place.

<sup>1</sup>Harry Wilmot Buxton, Memoir of the Life and Labours of the Late Charles Babbage, Esq., F.R.S., Formerly Lucasian Professor of Mathematics in the University of Cambridge: Comprising a Descriptive and Historical Account of His Analytical and Difference Engines Derived Principally from His Posthumous MSS and Papers, (unpublished biography, n.d.), Museum for the History of Science, Oxford, England [hereinafter referred to as Museum, Oxford], MSS Buxton 16-17, f. 66.

Babbage's childhood was unusual due to a chronic illness. As a result of this illness, his early education was constantly interrupted, and he was forced to alternate between formal schooling and private tutors. Soon after his eleventh birthday, following a severe illness, he was placed in the care of a clergyman at Alphington, near Exeter. His parents hoped that the open air would prove beneficial to his health. Babbage was next sent to the Totnes Grammar school but was again forced to withdraw and rely upon private tutors. This period of illness lasted about two years. His illness during his childhood was at times so severe that his parents and his tutors, in their anxiety over his health, were very lax in enforcing the discipline of his studies. Harry Wilmot Buxton, a barrister-at-law and Babbage's close friend and biographer, felt that, because of Babbage's many illnesses as a child, he had been allowed to study whatever he chose and had therefore developed an extraordinary degree of independence in habits and thought and action.<sup>1</sup> Buxton's appraisal of Babbage was fairly accurate because on many occasions throughout his life he would take time away from his primary pursuits to explore something which caught his fancy such as ciphering. Babbage even acknowledged that he had spent far more time on ciphering than was justified.<sup>2</sup>

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<sup>1</sup> Ibid., f. 66.

<sup>2</sup> Charles Babbage, Passages, p. 235.



Babbage continued displaying an independence in his study habits even in a formal school setting. As he approached adolescence his health greatly improved, and about 1806 or 1807, he was sent to the academy of the Rev. Stephen Freeman at Forty Hill, Enfield, Middlesex where he remained for three years. It was here that Babbage first began to study mathematics and to read widely from a "well selected library of about 300 volumes." His tastes did not include the study of "classical writers of Greece and Rome,"<sup>1</sup> but he was an "enthusiastic admirer of the philosophy of Bacon, and ever regarded that great intellectual paradox as the founder of modern philosophy."<sup>2</sup> The following is an example of the depth, breadth and independent nature of Babbage's study at Freeman's Academy:

Being passionately fond of algebra, I had instructed myself by means of Ward's "Young Mathematician's Guide," which had casually fallen into my hands at school. I now employed all my leisure in studying such mathematical works as accident brought to my knowledge. Amongst these were Humphrey Ditton's "Fluxions," of which I could make nothing; Madame Agnesis "Analytical Institutions," from which I acquired some knowledge; Woodhouse's "Principles of Analytical Calculation," from which I learned the notation of Leibnitz; and Lagrange's "Theorie des Fonctions." I possessed also the Fluxions of Maclaurin and of Simpson.<sup>3</sup>

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<sup>1</sup> Buxton, Charles Babbage, Museum, Oxford, MSS Buxton 16-17, ff. 66-68.

<sup>2</sup> Ibid., ff. 68-70.

<sup>3</sup> Babbage, Passages, p. 26.

Babbage not only studied the mathematical works listed above, during his three years at the academy, but in 1807, about age sixteen, he began a series of independent investigations in mathematical analysis which he called the calculus of functions. A brief description of his pre-university work in the calculus of functions will be given, because this work not only marked the beginning of his contributions to science but it also represents a mastery of mathematics which is unusual at age sixteen.

A record of his investigations in the calculus of functions exists in a small manuscript volume<sup>1</sup> which contains a discussion of the ideas as they occurred to him and extracts of letters from such friends as John Frederick William Herschel (1792-1871), son of the famous astronomer William Herschel and an astronomer in his own right; Edward Ffrench Bromhead (1789-1855), a mathematician and attorney; and William Henry Maule (1790-1858), a mathematician and

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<sup>1</sup> Charles Babbage, "The History of the Origin and Progress of the Calculus of Functions During the Years 1809, 1810. . . 1817," n.d., Museum, Oxford, MS Buxton 13, f. 4, is recorded in a notebook, A New Commonplace Book: Being an Improvement on that Recommended by Mr. Locke, Properly Ruled Throughout with a Complete Skeleton Index and Ample Directions for Its Use, Equally Adapted to the Man of Letters and the Man of Observation, the Traveller & the Student, and Forming an Useful and Agreeable Companion, on the Road; and in the Closet (London: J. Walker, 1806). It should be noted Babbage wrote this "History" from copies of letters he had received from his friends as well as from copies of letters he had sent to them. Babbage wrote a note to this effect dated 1852, which indicates that his "History" was probably written many years later.

attorney. In this volume Babbage traced the development of his mathematical ideas prior to his entrance at Cambridge in October, 1810. He stated that he began to form his ideas concerning analysis about 1808 or 1809. One of the ideas he mentioned was the "well known proposition of Pappus relating to the inscription of a number of circles in a [between two] semi-circle[s]." <sup>1</sup> A consideration of a diagram relating to Pappus' theorem, such as Figure 1, <sup>2</sup> suggested to Babbage that analogous properties might be found for the parabola, the hyperbola, and for other curves.

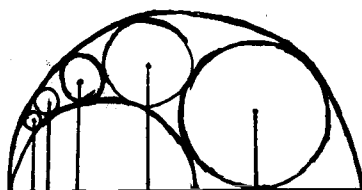


Figure 1. Diagram relating to Pappus' theorem.

One of the problems which Babbage attempted to solve concerning the hyperbola is as follows:

Supposing an hyperbola, between its asymptotes and circles, be drawn as in the annexed figure. What is the ratio of the area of the curve to the sum of the areas

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<sup>1</sup> Buxton, Charles Babbage, Museum, Oxford, MSS Buxton 16-17, f. 116.

<sup>2</sup> Babbage, "History of the Origin and Progress of the Calculus of Functions," Museum, Oxford, MSS Buxton 13, f. 5.

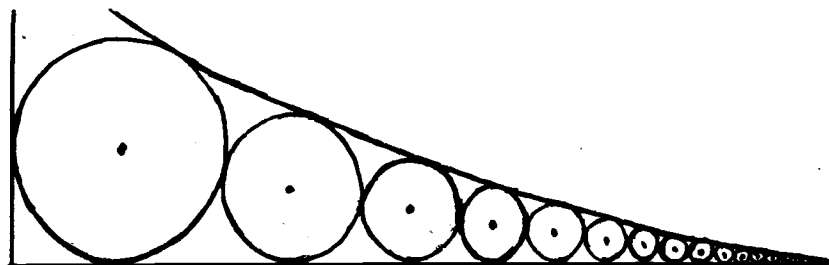


Figure 2. Hyperbola diagram.

of all the circles? and, conversely, if we suppose that area to be given or to follow any law, what will be the nature of the curves?<sup>1</sup>

Babbage spent much time on the hyperbola problem during the years from 1809 to 1810, but without the least success, and finally decided that its solution depended upon some mode of reasoning with which he was unacquainted.<sup>2</sup> As Babbage continued to develop his ideas concerning the calculus of functions, he experimented with different mathematical symbols to express those ideas. In some instances he used an old notation in a slightly different manner or invented a new symbol. For example, he employed the notation  $fx = y$  to denote the equation of the curve to be found as in the hyperbola problem above. He also used the notation  $f(a, b)$  to signify a function of  $a$  and  $b$  when he was engaged in some enquiries relative to the solution of equations.<sup>3</sup>

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<sup>1</sup>Ibid., p. 5.

<sup>2</sup>Ibid., p. 6.

<sup>3</sup>Ibid., p. 6.

As will be discussed below, such experimentation with algebraic notation was somewhat in advance of the usual practice followed in English mathematics which relied heavily upon geometrical proofs and demonstrations. Babbage noted that he was indebted to "Spence's essay on logarithmic Transcendents"<sup>1</sup> for a great addition to his mathematical knowledge and for some of his ideas concerning mathematical symbols and notation. His interest in mathematical notation and in notation in general was evinced later in life by the development of a mechanical notation for his calculating engines, a symbolic coding system to direct the functions of the Analytical Engine, and special dictionaries designed to aid him in his hobby of ciphering.<sup>2</sup>

Babbage entered the University of Cambridge in October of 1810. He continued as a university student to display the same independence in thought and action as he had as a child. For example, he continued his work on the calculus of functions even though it was not related to the mathematical studies of the university. When he found that his tutors were not interested in his inquiries, he became disgusted and with some colleagues founded the Analytical Society in order to promote the study of analysis and reform mathematics in England.

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<sup>1</sup> Samuel Spence, An Essay in the Various Orders of Logarithmic Transcendents with an Enquiry into the Application of Series (London, 1809).

<sup>2</sup> Babbage, Passages, pp. 235-241.

In 1866, near the end of his life, Babbage was severely criticized for his independence and early education. An anonymous reviewer of Babbage's book, Passages from the Life of a Philosopher, wrote:

We wish Mr. Babbage had gone to a good public school, and had then fallen in with intelligent tutors at Cambridge. He is self-taught, to a great extent, and almost wholly uneducated, in the true sense of the word. His college dons had not the discernment to see that they had a genius to deal with. Besides, the clever undergraduate took a malicious pleasure in posing them with hard problems. They, in revenge, disregarded his suggestions, and he soon became a mathematical separatist. . . .<sup>1</sup>

The reviewer was also annoyed with Babbage because he thought that he had been pretentious and vain in his account of the many inquiries he had conducted throughout his life. It was true that Babbage had wide ranging interests and had spent much time in some pursuits which were merely hobbies. His inquiries and inventions included: an analysis of humor, particularly puns; a method for punching a hole in plate glass without breaking it; the invention of a stomach pump; glaciers and the conditions under which clear ice is produced; uniform postage rates; a plan for transmitting letters along

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<sup>1</sup>"Mr. Babbage," a review of The Ninth Bridgewater Treatise: a Fragment (2d. ed.; London: J. Murray, 1838), by Charles Babbage, Esq.; and Passages from the Life of a Philosopher (London: Longmans, 1864) by Charles Babbage, Esq., M.A., F.R.S., F.R.S.E., &c., Commander of the Italian Order of S. Maurice and S. Lazarus, Inst. imp. (Acad. Moral) Paris, Corr. Acad. Amer., &c. &c. &c., in The Christian Remembrancer, LI. (January--April, 1866), pp. 12-13.

aerial wires; submarine navigation; the cause of magnetic and electric rotations; the invention of occulting signals for ships and lighthouses; a theory of isothermal surfaces to account for the geological facts of the successive uprising and depression of various parts of the earth's surface; games of skill; ciphering; methods of picking locks; principles of taxation; statistical and probabilistic theory; and the problem of three magnetic bodies as well as other miscellaneous inquiries.<sup>1</sup>

Intellectual pursuits were a source of immense satisfaction to Babbage throughout his life. His social contacts with friends and colleagues often included serious discussions of scientific topics as well as the current affairs of the day. However, Babbage also enjoyed purely leisure activities. As a boy, for example, he participated in pranks such as frightening a friend by pretending to be a ghost. On another occasion, he conducted an abortive experiment to contact the devil. On yet another occasion, Babbage and a group of friends obtained a bottle of cognac, which, at Babbage's suggestion, they mixed with a molasses called treacle. After a period of time all the participants in the scheme attended a prayer session. At the end of the session, according to Babbage, many of the boys were found asleep, others were dizzy and sick, and "some talked fast and

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<sup>1</sup> Babbage, Passages, pp. 205-486.

heroically, two attempted psalmody, but none listened." Babbage concluded his account of the incident with the following description:

All investigation at the time was useless: we were sent off to bed as quickly as possible. It was only known that Count Cognac had married the sweet Miss Treacle, whom all the boys knew and loved, and who lodged at the grocer's in the neighbouring village. But I believe neither the pedigree of the bridegroom nor his domicile were ever discovered. It is probable that he was of French origin, and dwelt in a cellar.<sup>1</sup>

During his years at the University of Cambridge (1810-1814) Babbage derived pleasure from a variety of pursuits. He was given a liberal allowance which provided him with enough money to indulge in numerous activities.. He spent his leisure time freely by playing chess, six-penny whist, and sailing with a group of distinctly non-intellectual friends to whom he referred as "strong fellows" who were needed to row the boat when the wind failed.<sup>2</sup> One of his more serious pursuits at Cambridge was his study of chemistry. He set up a laboratory which he shared with John Herschel until the latter set up a rival laboratory in friendly competition with Babbage. Babbage stated that he had never regretted the time he had spent upon the study of chemistry at the beginning of his career.<sup>3</sup> As will be discussed in detail below, he made use of his knowledge of chemistry

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<sup>1</sup> Ibid., p. 23.

<sup>2</sup> Ibid., pp. 36-38.

<sup>3</sup> Ibid., p. 38.



as an example for his argument that science had declined in England.

In June of 1814, Babbage graduated from Cambridge and married Georgianna Whitmore (1792-1827), the daughter of William Whitmore, a proprietor and descendent of a long line of proprietors. During thirteen years of married life, Georgianna gave birth to eight children of whom only three sons, Herschel, Dugald, and Henry, survived to adulthood. Four sons died in infancy and a daughter, Georgianna, died in her late teens. The deaths of his children and other members of his family had a deleterious effect upon Babbage. In 1827, for example, Babbage was so overwhelmed by a series of personal tragedies that he became ill himself. He gave the following account of this personal crisis:

I lost my father, my wife, and two children. My family, acting on the advice of my medical friends, urged me to travel abroad for six to twelve months. It was thought necessary that I should be accompanied by a servant in case of illness or accident. I objected to this on the ground that I was but just able to take care of myself, and that a servant would be a great encumbrance (to me). To satisfy, however, a mother's anxiety I proposed to take with me one of my own workmen if he liked to accompany me as an attendant.<sup>1</sup>

It was ironic that Babbage, who had been continually concerned about his financial situation throughout his married life, should have had it greatly improved shortly after his wife's death. He had earned

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<sup>1</sup> This comment was made by Babbage in a fragment concerning his travel, January 16, 1871, British Museum [to be abbreviated hereinafter as B.M.] Add MSS 37199, XVIII, 530.

some money from his scientific pursuits, and he may have received money from his father. However, upon the death of his father in 1827, he became financially independent. On February 26, 1827 he inherited assets of approximately £100,000.<sup>1</sup> He was now able to pursue science as a profession without undue concern for money. After his wife's death, he changed his mode of living considerably. He sent his two youngest sons, Dugald and Henry, to live with an acquaintance and his daughter, Georgianna, and eldest son, Herschel, to live with his mother. His visits to his children were infrequent as he became increasingly engrossed in his scientific career which included calculating machines, mathematics, and a variety of other pursuits ranging from original scientific experiments to consultant work for life assurance companies.<sup>2</sup>

Babbage used the money he had inherited from his father not only to finance many of his scientific and professional activities but also for such events as his famous evening parties. Charles Darwin (1809-1882), who was Babbage's close friend, regularly attended these evening parties. Darwin described Babbage in a way which provides some insight into his personal life following his wife's death

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<sup>1</sup> Mabothe Mosely, Irrascible Genius: A Life of Charles Babbage, Inventor (London: Hutchinson & Co., LTD, 1964), p. 84.

<sup>2</sup> Henry Provost Babbage, The Memoirs and Correspondence of Major-General Henry P. Babbage (London: William Clowes & Sons, 1910), p. 7.

in 1827. Darwin remarked that, while Babbage was always a man worth listening to, he was also a disappointed and discontented man with an expression which was often morose and sullen. He added, however, that Babbage was not half as sullen as he pretended to be. He stated that Babbage had once told him that he had invented a plan whereby all fires could be extinguished, but that he added, "'I shan't publish it--damn them all, let all their houses be burn't.' The all were the inhabitants of London."<sup>1</sup> Darwin provided another insight into Babbage's personality, when he told of a "funny dinner" in which Babbage was a guest in the house of his brother, Erasmus Alvey Darwin (1804-1881). Among the guests were Charles Lyell (1797-1875), the geologist, Thomas Carlyle (1795-1881), the writer, and some other friends. Darwin noted that Carlyle's talk was "very racy and interesting, just like his writings, but that he sometimes went on too long on the same subject."<sup>2</sup> Darwin wrote that both Babbage and Lyell liked to talk, but this time, much to the chagrin of Babbage, Carlyle

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<sup>1</sup> Charles Darwin, The Autobiography of Charles Darwin, 1809-1882: With Original Omissions Restored. Edited with Appendix and Notes by His Grand-daughter Nora Barlow (New York: W. Norton and Company, Inc., 1958), p. 108.

<sup>2</sup> Ibid., p. 112.

Silenced everyone by haranguing during the whole dinner on the advantages of silence. After dinner, Babbage, in his grimmest manner, thanked Carlyle for his very interesting Lecture on Silence.<sup>1</sup>

The foregoing sketch of Babbage's personal and public life will hopefully provide a background for a history of his scientific attainments in the nineteenth century. His scientific career falls quite naturally into two parts. During the early part of his career, he was actively involved in several efforts to reform and promote science in England. These efforts included a successful reform of mathematics at the University of Cambridge; the reform of the Nautical Almanac; the founding of new scientific societies; and an attempted but abortive effort to reform the Royal Society in order to make it an effective scientific organization which would provide the leadership and influence necessary to secure both private and governmental patronage for science. The first part of Babbage's career began about 1812 with the formation of the Analytical Society and ended for the most part with his resignation in 1839 as a permanent trustee of the British Association for the Advancement of Science. The second part of his career was concerned primarily with the invention and development of automatic calculating engines. He began this work about 1820 as a natural outgrowth of his interest in mathematics and astronomy. He was concerned that the tedious and laborious task of manual

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<sup>1</sup>Ibid., pp. 112-113.

computation would impede the progress of science in general. It was his goal to invent some means of performing the task of computation automatically and without error. When he began his quest to develop a calculating machine he had hopes of completing such a machine in the relatively short period of three to four years. Instead, he devoted the major portion of the remainder of his life to the development of calculating engines.

The following history will be devoted to a detailed discussion of the two phases of Babbage's scientific career. Part I--Individual and Collective Efforts to Reform Science in England-- will be concerned with efforts by Babbage and his colleagues to reform science in England over a period of years from about 1812-1840. The leadership for the various reform movements alternated among Babbage and his associates depending upon their particular scientific disciplines. For this reason, in some of the reform movements, Babbage assumed only a supporting role. However, as will be discussed in detail, Babbage and his colleagues were very supportive of each other and generally in agreement with the strange blend of elitist and liberal views expressed by Babbage and Edward Ffrench Bromhead. These views formed the ethos for the external coterie which, though it was not a formally organized group, was made up of Babbage and those associates whose goal was to reform science in England. The role of Robert Woodhouse is somewhat different because he worked as an

individual reformer. He is included in the discussion of the reform of science in England because he influenced the initial reform efforts of Babbage and his fellow reformers to the extent that they included many of his ideas in the mathematical reforms of the Analytical Society. Part II--Babbage's Calculating Engines--will be devoted to Babbage's efforts to develop automatic calculating engines, first with governmental support from 1823-1833; second with his own money from 1834-1871; and third with the efforts of his youngest son, Major General Henry Provost Babbage (1824-1915), to complete one of his father's calculating engines from 1874-1911. Although there is an overlap of approximately twenty years, it is still useful to think of Babbage as having begun the second phase of his career while he was completing the first phase.

The research for this study was carried out through the cooperation and support of my major professor, Brookes Spencer, of the General Science Department, Oregon State University, who critically and helpfully read through the many drafts which preceded the printed copy.

Financial support for this study was provided in part by the General Science Department through the efforts of Professors Robert J. Morris and Daniel Jones. Professor Morris also provided much assistance throughout the period of research and writing. The assistance of both men is greatly appreciated.

The source material for this dissertation was procured from many different libraries throughout the United States and Canada; from the British Museum, the Museum of the History of Science, Oxford, and the Library, St. John's College, University of Cambridge. Of particular interest were the reviews from the Monthly Review, the Quarterly Review, the Edinburgh Review, and the North British Review, because the works under review were often of secondary importance to the reviewers who commented upon the current political, social, economic, moral, and scientific affairs, and thus provided a valuable background on the life and times of nineteenth century England in particular and Europe in general.

Both manuscript and printed materials were obtained from the British Museum concerning Babbage. Among these materials were approximately twelve thousand letters of Babbage's personal correspondence which included copies of his own letters to friends, associates, and governmental officials. While these letters are an invaluable source of information, they are catalogued only to the extent that they are generally arranged in chronological order.

Another valuable source of manuscript materials relating primarily to Babbage's calculating engines is the Harry Wilmot Buxton collection at the Museum of the History of Science at Oxford. This collection includes Buxton's unpublished biography of Babbage which is largely an arrangement of Babbage's notes, comments, and

correspondence relating to the calculating engine with some commentary by Buxton; and a series of other manuscripts which consist of rough notes, drawings, and comments by Babbage concerning different parts of his calculating engines. These are very difficult to use because Babbage constantly revised his ideas concerning the calculating engines so that it is not always apparent which machine he was describing and its state of development.

The printed materials available from the British Museum include copies of Babbage's books, book reviews, reprints of some of his scientific publications, and a book by his youngest son Henry P. Babbage, editor of Babbage's Calculating Engines: Being a Collection of Papers Relating to Them; Their History, and Construction.<sup>1</sup> This is largely a collection of reprints of articles relating to the calculating engines which Charles Babbage had intended to include in a work entitled the History of the Analytical Engine.<sup>2</sup>

I wish to express my appreciation to those persons at Oregon State University who contributed their time, comments, and efforts and thus enabled me to complete this study: Professors Brookes Spencer, Robert J. Morris, Daniel Jones, and David Willis of the General Science Department; Professors Harry E. Goheen and Curtis

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<sup>1</sup> (London: E. and F.N. Spon, 1889).

<sup>2</sup> Babbage, Passages, p. 496.



R. Cook of the Computer Science Department; Professor David B. King of the History Department; Professor Howard L. Wilson of the Mathematics and Science Education Departments; Miriam S. Minnick, Reference Librarian for Inter-library Loan, whose assistance in locating source materials was invaluable; and Leona Nicholson, Administrative Secretary of the General Science Department. I wish also to express my gratitude to my wife, Luella Bell, for her assistance in typing and re-typing many drafts of this dissertation, for many helpful suggestions and for much encouragement.

CHARLES BABBAGE, PHILOSOPHER, REFORMER, INVENTOR:  
A HISTORY OF HIS CONTRIBUTIONS TO NINETEENTH  
CENTURY SCIENCE

PART I--INDIVIDUAL AND COLLECTIVE EFFORTS  
TO REFORM SCIENCE IN ENGLAND

'I'm a philosopher. Confound them all--  
Birds, beasts, and men; but no, not womankind.'--Don Juan

'I now gave myself to philosophy: but the great object of my ambition was to make out a complete system of the universe, including and comprehending the origin, causes, consequences, and termination of all things. Instead of countenance, encouragement, and applause, which I should have received from every one who has the true dignity of an oyster at heart, I was exposed to calumny and misrepresentation. While engaged in a great work on the universe, some even went so far as to accuse me of infidelity;--such is the malignity of oysters'--"Autobiography of an Oyster" deciphered by the aid of phtography in the shell of a philosopher of that race, --recently scolloped.<sup>1</sup>

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<sup>1</sup> From the title page of Passages from the Life of a Philosopher, by Charles Babbage.

## I. ROBERT WOODHOUSE, A CAUTIOUS REFORMER

At the beginning of the nineteenth century a concern was expressed that mathematics in England was inferior to Continental mathematics. The foremost proponent of this opinion was Robert Woodhouse (1773-1827) of Caius College, University of Cambridge. In 1795 Woodhouse graduated from Cambridge with a Bachelor of Arts degree and with the two highest honors given by the University-- Senior Wrangler and first Smith's prizeman. These two honors, which were awarded for outstanding achievement in mathematics, brought him immediate recognition and an opportunity to express his views. He was granted a scholarship at Cambridge, and by 1798 he had earned a Master of Arts degree and was appointed a fellow of Caius College where he began to teach mathematics. He was also invited, in 1798, to review mathematical works for the Monthly Review,<sup>1</sup> one of the leading journals of its kind since 1749.<sup>2</sup> As a

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<sup>1</sup> Benjamin Christy Nangle, The Monthly Review: Second Series, 1790-1815, Indexes, of Contributors and Articles (Oxford: Clarendon Press, 1955), pp. 74-75.

<sup>2</sup> Benjamin Christy Nangle, The Monthly Review: First Series, 1749-1789, Indexes of Contributors and Articles (Oxford: Clarendon Press, 1934), pp. v-xi, commented in some detail upon the Monthly Review. The Monthly Review was founded in 1749 by Ralph Griffiths, who remained its editor until his death in 1803. His son, George Edward Griffiths assumed full responsibility until 1825 when he sold his entire interest and gave up his editorial duties. According to Nangle, the Monthly Review was considered the leading review journal

mathematical reviewer, Woodhouse had ample opportunity to express his views regarding the state of mathematics in England. The typical reviewer of the time often devoted a major portion of his review expounding upon whatever suited his fancy. Woodhouse was no exception and began at once to comment upon the state of mathematics in England. It was an editorial policy of the Monthly Review that the reviewers remain anonymous in order to prevent them from being subjected to pleas for favorable reviews.<sup>1</sup> As the result of this policy Woodhouse was not able to lend the prestige of his name to his reviews, and was thus prevented from using his reviews as a means of promoting mathematical reform in as direct a manner as might have been possible. Woodhouse was able, however, to exert a more direct influence in the reform of mathematics as a professor at the University of Cambridge and as the author of textbooks which he used in his teaching of mathematics. For example, Babbage mentioned that, prior to his enrolling at Cambridge in 1810, he had read Woodhouse's Principles of Analytical Calculation,<sup>2</sup> from which he

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in England under Ralph Griffith's editorship. The senior Griffith maintained the right to edit his reviewers' writings which led to charges that he was a "mean-spirited tyrant, wielding his whip-lash over a miserable group of hack writers." However, it was Nangle's opinion that the reviewers were by no means inadequate but constituted in most instances the best among the literary, political, and scientific men of the times.

<sup>1</sup>Ibid., pp. ix-xi.

<sup>2</sup>(Cambridge: University Press, 1803).

he learned the notation used by Gottfried Wilhelm Leibniz (1646-1716) in his calculus. As a result of reading Principles and other English and Continental works, Babbage felt that he possessed equal facility in several notations--the dots of Newton, the d's of Leibniz and the dashes of Lagrange.<sup>1</sup> He came to Cambridge with the conviction that mathematics in England would not compare favorably with Continental mathematics until English mathematicians adopted the notation and methods of analysis of the Continental mathematicians. Prior to enrolling at Cambridge, Babbage had the idea of forming a mathematical club or society. This idea was realized, when, after enrolling at Cambridge in October of 1811, Babbage and a group of friends formed the Analytical Society in order to promote the study of analysis based exclusively upon Continental notation and methods.

Woodhouse stated that the primary reason for the inferiority of English mathematics was the self-imposed isolation of English mathematicians from Continental mathematicians. According to Woodhouse, this isolation began as the result of the celebrated controversy between Sir Isaac Newton (1642-1727) and Leibniz. The controversy began with the implication that Leibniz was a plagiarist and had no claim either to priority or to originality in the development of the calculus. The charge of plagiarism was made in 1699 by Fatio

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<sup>1</sup>Babbage, Passages, p. 26.

de Duillier (1664-1753), a Swiss mathematician who had been living in London since 1691, perhaps as a result of Leibniz having omitted Fatio de Dullier's name from the list of mathematicians capable of solving John Bernoulli's (1667-1748) problem of the line of quickest descent.<sup>1</sup> Leibniz replied to the charges, and the dispute between Newton and himself gradually widened over the years until his death to include other issues such as Newton's concept of gravity.<sup>2</sup>

Woodhouse did not attempt to revive the Leibniz-Newton controversy. However, he did think that the partisans of Newton had slighted and overlooked "the real merit of Leibniz," stating that:

The decision of the Committee of the Royal Society<sup>3</sup> is not hastily and inconsiderably to be accused of unjust partiality: but we strongly insist that those men must have had minds deplorably weak, or miserably perverted by party-zeal and national prejudice, who denied the praise of

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<sup>1</sup>J. M. Child, The Early Mathematical Manuscripts of Leibniz: Translated from the Latin Texts Published by Carl Immanuel Gerhardt with Critical and Historical Notes (London: Open Court Publishing Company, 1920), pp. 6-9. On pp. 8-9 is a translation of a portion of Fatio de Duillier's letter to Christian Huygens in 1699 in which the implication of plagiarism by Leibniz is given. Child conjectured that Duillier felt slighted when Leibniz omitted his name from the list of those capable of solving the problem of the line of quickest descent.

<sup>2</sup>H. G. Alexander, ed., The Leibniz-Clarke Correspondence, Together with Extracts from Newton's "Principle" and "Opticks" (Manchester: University Press, 1956), pp. ix-xi.

<sup>3</sup>The status of this committee is somewhat uncertain. It was formed in 1712 at the request of Leibniz to counter charges of plagiarism on the "invention of the calculus" leveled against Leibniz by the partisans of Newton. According to Augustus De Morgan the

genius to Leibnitz; who, amid a thousand other pursuits, instructed Bernouilli and rivalled Newton, not on subjects of small concern and easy comprehension, but in a science to which they were professedly, solely, and devotedly attached.<sup>1</sup>

committee of 1712 was not really an official body of the Royal Society but more of an ad hoc committee. De Morgan advanced the argument that the committee overstepped its own proper function--judicial--and, in some cases, actually altered existing documents in favor of Newton. See De Morgan's "A Short Account of Some Recent Discoveries in England and Germany Relative to the Controversy on the Invention of Fluxions," Yearbook of General Information for 1852, (London, 1852), pp. 5-20. De Morgan also published a biographical sketch entitled "Newton" in The Cabinet Portrait Gallery of British Worthies, XI (London, 1846), 78-117. This essay was, according to S  phia De Morgan, Memoir of Augustus De Morgan: With Selections from His Letters, (London. Longmans, Green and Company, 1882), p. 256, "after Francis Baily's Life of Flamsteed, [London, 1835], the first English work in which the weak side of Newton's character was made known. Justice to Leibniz, to Flamsteed and even to [William] Whiston called for this exposure; and the belief was that it did not lower the biographer's [Augustus De Morgan] estimate of Newton's scientific greatness and of the simplicity and purity of his moral character. Francis Baily's discovery of the correspondence between the Rev. John Flamsteed, the first Astronomer Royal, and Abraham Sharp, as well as between Newton, [Edmund] Halley, and Flamsteed, on the publication of Flamsteed's catalogue of stars, had thrown new light on the character of Newton. It appeared that the practical astronomer had been treated ungenerously by Newton, who failed to observe the conditions of publication agreed to by all parties; and afterwards when remonstrated with, omitted the name of Flamsteed in places where it had formerly stood in earlier editions of the Principia."

<sup>1</sup>[Robert Woodhouse], "La Croix on the Differential and Integral Calculus," review of Traite' du Calcul Differentiel: Traite' des Diff  rences et des Series, Faisant Suite au Traite du Calcul, par S[ilvestre] F[ran  ois] La Croix, Vols. I-III. Paris: De Baffe, 1797-1800. In: The Monthly Review: or Literary Journal, Enlarged, Ser. 2, XXXII (Appendix, 1800), 497-498.

Woodhouse claimed that the blind reverence which the English mathematicians had held for Newton was responsible for their neglect of the advances made by Continental mathematicians in the development of the calculus. He felt that the neglect of Continental mathematics resulted in a period of stagnation in English mathematics in the latter half of the eighteenth century. He noted that while Newton contributed much to science by his great originality, his contributions were often the result of "united efforts." "Rarely," Woodhouse wrote, "do we find that any invention is comprised within the limits of the intellectual progress of an individual. It seems not to come within the compass of the same man's power to invent and to perfect, . . ."<sup>1</sup>

In reference to the calculus, Woodhouse stated that Newton had done little more than to give simplicity and generality to the methods in use by his predecessors and contemporaries. Such an attitude clearly indicates that Woodhouse was prejudiced in favor of the achievements of Continental mathematicians over those of the English since the same statement could have been made about Leibniz. Neither man perfected his version of the calculus nor did he invent it without

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<sup>1</sup> [Robert Woodhouse], "La Grange's Theory of Analytical Functions," review of The Theory of Analytical Functions: Containing the Principles of the Differential Calculus Divested of all Reference to Infinitely Small or Evanescent Quantities, Limits or Fluxions, and Reduced to the Algebraical Analysis of Finite Quantities, by J[oseph] L[ouis] La Grange, in The Monthly Review or Literary Journal, Enlarged, Ser. 2, XXVII (Appendix, 1799), 481.



considerable debt to both his predecessors and contemporaries. Woodhouse was evidently aware that he might be criticized by his contemporaries on the same basis, since he was careful to point out that he was not trying to depreciate the character of "our great philosopher," but "Newton, like other men, must pass through futurity under the protection of his genius alone."<sup>1</sup> "If, however, the English mathematicians," Woodhouse wrote, "first adopted Newton's method from veneration to him, or from want of better information, they have persevered in it (we may almost say) against conviction."<sup>2</sup>

Woodhouse then turned his attention to what he considered were the major defects of Newton's fluxional calculus. One of the defects was the concept of motion. He agreed with Jean le Rond D'Alembert (1717-1783) that the concept of motion as used in the fluxional calculus was confusing, since speed was a relation of space to time when time was a variable. Only through the concept of limit could one gain a clear understanding of the differential of space to time.<sup>3</sup> Woodhouse's criticism of the concept of motion was indicative of a fundamental difference in English versus Continental mathematics. The concept of motion, for the English, implied the use of an argument based upon

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<sup>1</sup> Ibid., p. 483.

<sup>2</sup> Ibid., p. 488.

<sup>3</sup> Ibid., pp. 488-489.

a physical phenomenon to clarify a mathematical concept. The Continental mathematicians preferred to develop their mathematical concepts upon a purely logical and algebraic basis. Woodhouse was clearly in favor of the Continental approach to mathematics. For example, he regarded the rule for the multiplication of algebraic symbols as the true and original basis of the calculus whether fluxionary or differential. Multiplication could be used to found the reverse operations of division and extraction of roots by inspection or trial. Further, when such operations were performed on a binomial  $(x+a)^n$  the results could be expressed under a common formula called the binomial formula, whereby the second term of  $(x+a)^n$ , that is  $a$ , provided the basis from which a fluxion or differential quantity could be deduced.<sup>1</sup> Here, Woodhouse was attempting to appeal to the use of general formulas which could be applied to such particular applications as a body in motion wherein the second term of the binomial expression  $(x+a)^n$ ,  $a$ , might represent the incremental velocity of that body.<sup>2</sup> He lamented that the fluxionist pursued just the reverse method by using the principles of motion as the basis of their calculus.<sup>3</sup> In objecting to the principle of motion,

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<sup>1</sup>Woodhouse, Principles, p. ii.

<sup>2</sup>Ibid., p. ii.

<sup>3</sup>Ibid., pp. ii-iii.

he pointed out that Newton left no satisfactory explanation of it. Those who tried to explain what Newton meant by the principle of motion soon found out, according to Woodhouse, "that the very principle of the method was itself a subject of calculation and endeavored to establish it, by reasonings which fairly may be called tedious and prolix."<sup>1</sup> Such attempts to explain the principle of motion led to dilemmas such as the following:

If  $x$  be a function of the time ( $t$ ) and  $t$  becomes  $t + \dot{t}$ , then  $x$  becomes  $x + \frac{\dot{x} \cdot \dot{t}}{\dot{t}} + \frac{\ddot{x} \cdot \dot{t}^2}{1 \cdot 2 \cdot \dot{t}^2} + \frac{\dddot{x} \cdot \dot{t}^3}{1 \cdot 2 \cdot 3 \cdot \dot{t}^3} + , \&c.$  and the space described in time  $(\dot{t}) = \frac{\dot{x} \cdot \dot{t}}{\dot{t}} + \frac{\ddot{x} \cdot \dot{t}^2}{1 \cdot 2 \cdot \dot{t}^2} + , \&c.$  in which collection of partial motions,  $\frac{\dot{x} \cdot \dot{t}}{\dot{t}}$  is the space described in an uniform motion with a velocity  $= \frac{\dot{x}}{\dot{t}}$ ,  $\frac{\ddot{x} \cdot \dot{t}^2}{1 \cdot 2 \cdot \dot{t}^2}$  is the space described in a motion uniformly accelerated, by an accelerating force  $\frac{\ddot{x}}{1 \cdot 2 \cdot \dot{t}^2}$ , but the other terms as  $\frac{\dddot{x}}{1 \cdot 2 \cdot 3 \cdot \dot{t}^3}$ ,  $\frac{x^{(4)}}{1 \cdot 2 \cdot 3 \cdot 4 \cdot \dot{t}^4}$ , since they cannot be referred to any known motion are not designated by any name. . . . The first fluxion could then called the velocity, the second the tendency of the velocity, or the rate of increase of the first velocity, or the rate of increase of the first velocity supposing it not uniform, or whatever was the equivalent to the accelerating force, but the third, fourth, &c. &c. fluxions could only be called by analogy and circuitously, the tendency of the tendency of a velocity, &c. &c. phrases, to which no precise notions could be attached, and which occurring in a science, that

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<sup>1</sup>Ibid., p. iii.

ought to possess, if any other, perspicuity and accuracy, disgusted men of sound minds, and alienated them from the study of the abstruse and fine geometry.<sup>1</sup>

As can be seen from the example above, Woodhouse objected to the concept of motion because it lacked clarity when attempting to describe even first and second fluxions or derivatives and was not precise enough for higher derivatives. His goal was to base his reform on such fundamental issues as the use of algebraic operations and arithmetical processes. He maintained that a derivation based upon the ordinary operations of algebra avoided problems introduced by the use of motion and rendered analysis a purely abstract branch of mathematics. For example, differentials or fluxions could be used to describe a wide range of problems in which motion was not involved as in problems involving maxima and minima of volumes. The fluxionist had used the concept of motion to explain a limit by using terms such as a function "approaches" a limit. In order to introduce greater generality into the calculus Woodhouse thought that it was necessary to develop formulas which were independent of physical concepts. He therefore favored the practices of Continental mathematicians which involved the development of mathematical concepts which were independent of physical concepts. In this way analysis became an abstract branch of mathematics which possessed greater generality.

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<sup>1</sup>Ibid., pp. v-viii.

In attacking the foundations of fluxions and by appearing to be solely pro-Continental, Woodhouse ran the risk of alienating the very persons he wished to influence--the fluxionists. From his comments regarding the Irish logician, George Berkeley (1685-1753),<sup>1</sup> it is apparent that he was fully aware of such a possibility. Berkeley, he noted, had attacked the logical and philosophical bases of Newton's theory of fluxions with a "rich vein of wit and argument" but in so doing he had "ridiculed it into immortality."<sup>2</sup> Woodhouse therefore wisely tempered his remarks regarding the Newtonian fluxionists to some extent by attacking the Continental mathematician Lazare Nicolas Marguerite Carnot (1753-1823) on his use of infinitesimals. He maintained that Carnot had fallen into a logical trap called fallacia suppositionis, or shifting of the hypothesis. Carnot had committed

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<sup>1</sup> Berkeley, Dean of Derry in 1724 and Bishop of Cloyne in southern Ireland in 1734, attacked the logical and philosophical bases of Newton's theory of fluxions in The Analyst: or, a Discourse Addressed to an Infidel Mathematician. Wherein It Is Examined Whether the Object, Principles, and Inference of the Modern Analysis Are More Distinctly Conceived, or More Evidently Deduced, than Religious Mysteries and Points of Faith. "First Cast the Beam Out of Thine Own Eye: and Then Shalt Thou See Clearly to Cast Out the Mote Out of Thy Brother's Eye," (London, 1734), quoted in Carl B. Boyer, The History of the Calculus and Its Conceptual Development: The Concepts of Calculus, (New York: Dover Publications, Inc., 1959), pp. 224-225. Boyer stated that the "Infidel Mathematician" referred to Newton's friend Edmund Halley (1656-1742).

<sup>2</sup> [Robert Woodhouse], "La Croix on the Differential and Integral Calculus," in Monthly Review, XXXII (Appendix, 1800), 499.

this logical error when he extended the ideas of Leibniz concerning infinitesimal quantities using imperfect equations. The right and left hand members of imperfect equations were unequal but could,

by changing the infinitely small, or arbitrary quantities, be made to approach each other within any assigned limits of accuracy, and their ultimate ratio, is a ratio of equality: vanishing quantities the author [Carnot] considers as the limits of infinitely small quantities, having their relative value assigned by the law of continuity.<sup>1</sup>

Woodhouse objected that when finding the limit of a ratio derived, for example, from an expression where  $y = x^m$ , with  $x$  increased by  $i$ , and  $y$  increased by an increment  $h$ , yielding  $y+h = (x+i)^m = x^m + mx^{m-1} \cdot i +$ , etc., the limit of the ratio  $h/i = mx^{m-1} + m \cdot ((m-1)/2) \cdot x^{m-2} \cdot i +$ , etc., was obtained by setting  $i = 0$ . The limit of the ratio was therefore:  $Lh/i = mx^{m-1}$ . The specific objection Woodhouse had to this method of finding the limits of ratios was that the method

considers quantities in the state, in which they cease to be quantities; for when  $h$  and  $i$  are finite, the ratio between is sufficiently clear and intelligible; not so, when  $h$  and  $i$  vanish, or become nothing at the same time;<sup>2</sup> to conceive

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<sup>1</sup> Ibid., p. xii.

<sup>2</sup> The problem to which Woodhouse was referring was the indeterminate form of the limit  $0/0$ , that is when in the limit of  $h/i$ , both  $h$  and  $i$  simultaneously vanish. The difficulty in formulating a rigorous, logical definition of the limit was to remain a stumbling block in the calculus until 1872, when Karl Weierstrass (1815-1897) defined the limit without any reference to motion, that is the idea of a variable approaching a limit, or to infinitesimals or differential

this ratio, the mind feels considerable difficulty, and to explain it, many reasonings and illustrations are requisite.<sup>1</sup>

Thus, he objected to methods of finding limits of ratios used by both the English and Continental mathematicians because he felt that foundation for the concept of limits was not based upon logical inference. For example, the process of setting  $i = 0$  in the ratio

$$h/i = mx^{m-1} + m \cdot ((m-1)/2)x^{m-2} \cdot i +, \text{ etc.}, \text{ was}$$

produced on the express supposition, that  $i$  is some quantity if you make  $i = 0$ , the hypothesis is as Berkeley says, shifted, and there is a manifest sophism in the process of obtaining  $Lh/i$ ; since, if the hypothesis be destroyed the consequence ought not to be retained.<sup>2</sup>

Woodhouse had now attacked two basic ideas of the method of fluxions--the concept of motion and the concept of limit. He next commented on fluxional notation because he thought it was also defective and because he did not think that the fluxionary principles and

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quantities which involved metaphysical notions of fixed but infinitely small quantities. Weirstrass' concept of a limit was static and asked two questions: first, does the variable  $f(x)$  have a limit  $L$  for the value  $a$  of  $x$ ; secondly, is this limit  $L$  the value of the function for the value  $a$  of  $x$ . If  $f(x) = L$ , then the limit of the variable for the value of  $x$  is the value of the variable for this value of  $x$ , but not that  $f(x)$  reaches or approaches  $f(a)$  or  $L$ . See Carl B. Boyer, The History of the Calculus, pp. 267-298.

<sup>1</sup>Woodhouse, Principles, p. xii.

<sup>2</sup>Ibid., pp. xii-xiii.

language or notation were essentially necessary. He stated, "I had been silent of their defects, merely proposing another method, had they not received the sanction of the greatest names recorded in the annals of science."<sup>1</sup> Here Woodhouse showed that he too had bowed before the authority of a great name. This was evident again in his initial reluctance to use signs and notations of entirely foreign invention. Such acts, he stated, were in opposition to earlier habits, to feelings of national prejudice; and "to my more than rational reverence of Newton."<sup>2</sup> However, when he wrote Principles in 1803, he was bold enough to point out what he considered to be some of the obvious inadequacies of fluxional notation.

In the simplest case, perhaps, there is not much exercise of choice, and  $\dot{x}$ ,  $\ddot{x}$ ,  $x''$ ,  $\dot{x}^2$ ,  $\dot{x}^3$ ,  $\dot{x}^n$  are as neat as  $dx$ ,  $d^2x$ ,  $d^4x$ ,  $dx^2$ ,  $dx^3$ ,  $dx^n$ .

.....  
[But],  $\ddot{xy}$  or  $(xy)''$  is not so convenient as  $d^3(xy)$ .

Again, suppose  $x$ ,  $x'$ ,  $x''$ , &c. to represent successive values of  $x$ , then according to the fluxionary notation, the first, second, &c. fluxions are  $x$ ,  $x'$ ,  $x''$ ; by differential,  $\dot{x}$ ,  $d^2x'$ ,  $d^3x''$ . Which notation has here the advantage, must be determined by inspection; and if the advantage is asserted to be with the fluxionary, it is impossible to state in words any irrefragable arguments to the contrary.

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The advantage on the side of the differential is not, it may be said, very manifest in these examples, and perhaps I had adhered to the notation most familiar to me,

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<sup>1</sup> Ibid., p. xvii.

<sup>2</sup> Ibid., p. xxvii.



had not stronger reasons than what are contained in the preceding cases presented themselves, for adopting letters instead of dots as the significant symbols of operations. These reasons in a few words are: first, in the fluxionary notation, there is no simple mode of expressing the fluxionary or differential coefficients, that affect the terms of an expanded expression; thus, the form for the binomial without putting down the numeral coefficients, by

$$\text{fluxionary notation is, } (x+i)^m = x^m + \frac{x^m}{x} i + \frac{\frac{m}{x} x^m i^2}{1 \cdot 2 \cdot x^2} + \&c.$$

an awkward mode of expression certainly; and even in the differential notation, the coefficients cannot be expressed,

$$\text{except by fractions, thus } (x+i)^m = x^m + \frac{d(x^m)}{dx} i + \frac{\frac{d^2(x^m)}{dx^2} i^2}{1 \cdot 2 \cdot dx^2} + \&c.$$

but then by a slight alteration of this notation, a very commodious one is obtained; thus, using the small capital D to denote the differential coefficient, that is, putting  $Dx^m$

$$\text{for } \frac{d(x^m)}{dx}, \&c. \text{ we have, } (x+i)^m = x + Dx^m \cdot i + \frac{D^2 x^m}{1 \cdot 2} \cdot i^2 + \&c.$$

and it is commodious to employ this symbol D, long before any express and formal mention of the differential calculus.<sup>1</sup>

As seen above, Woodhouse did not claim that the Leibnizian notation was perfect, but he did think it has definite advantages and was a convenience for working problems. The patterns acquired in learning the old notation were not lightly regarded by him.

Conscious of the force of habit and partiality for our own opinions, I do not violently insist that the notation I have used, is most commodious; but, I wish entirely to dissent from those, who affirm the question concerning notation to be of small moment; I conceive it to be very important, and that most of the advantages which science has received, are to be attributed to improvements made in the language of Analysis.<sup>2</sup>

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<sup>1</sup> Ibid., pp. xxvii-xxix.

<sup>2</sup> Ibid., p. xxxi.

Woodhouse published two other books on mathematics in which he continued to advocate his methods and notation. The first book was on trigonometry<sup>1</sup> and the second on isoperimetrical problems.<sup>2</sup> He explained that his book on isoperimetrical problems was the only English work on the subject, with one by Leonhard Euler (1707-1783) being the only foreign work<sup>3</sup> on the same subject. He then made a remark about Euler's work which contrasted the English and Continental mathematicians with respect to instruction in mathematics. He did not think it would have been sufficient just to translate Euler's work on isoperimetrical problems. One reason was that some of Euler's most important processes had been superseded by more concise ones, but most important was the audience for which the work was written.<sup>4</sup> According to Woodhouse, Euler and Lacroix, as well as other Continental mathematicians, provided the student with

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<sup>1</sup> Robert Woodhouse, A Treatise on Plane and Spherical Trigonometry (Cambridge: Black and Company, 1809).

<sup>2</sup> Robert Woodhouse, A Treatise on Isoperimetrical Problems, and the Calculus of Variations (Cambridge: Black and Company, 1810).

<sup>3</sup> Leonhard Euler, Methodus inveniendi Lineas Curves proprietate maximi minimive gaudentes (Lausanne, 1774).

<sup>4</sup> Robert Woodhouse, A History of the Calculus of Variations in the Eighteenth Century (Bronx, New York: Chelsea Publishing Company, 1965), p. i. This book is a textually unaltered reprint of Woodhouse's Treatise on Isoperimetrical Problems.

"instruments of solution" such as formulas, but they did not instruct him in the object or principle of construction.<sup>1</sup> The reason for this oversight was that foreign mathematicians such as Joseph Louis La Grange (1736-1813), wrote primarily for other mathematicians and not for students and therefore stated the principles of analysis very briefly without entering into the nature of the subject. Subsequent authors followed the style set by La Grange. In contrast, Woodhouse was interested in writing books which could be used for instruction. It was his plan to combine the "historical progress with the scientific development of the subject" while endeavoring "to lay down and inculcate the principles of the Calculus, whilst I traced its gradual and successive improvements."<sup>2</sup> Thus, Woodhouse was, in reality, interested in bringing about a change in the methods of teaching mathematics as well as in conceptual and notational aspects of analysis.

During the years from 1798 to 1811, Woodhouse had expressed his views as a writer and as a teacher of mathematics, but he had not been particularly successful in bringing about the reforms he had promoted. His ideas were not received with any great enthusiasm at Cambridge and his comments in the Monthly Review were anonymous

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<sup>1</sup>Ibid., pp. ii-iii.

<sup>2</sup>Ibid., pp. iii-iv.

due to the editorial policy.<sup>1</sup> Even his own mathematical books had not been reviewed in the Monthly Review since Woodhouse was the person designated to review mathematical works and since the editor of the Review would not allow him to review his own works. It was not until 1811 that his works were finally reviewed and brought to the attention of the general public. The person who was invited to review his books was Sir William Henry Maule (1788-1858), Senior Wrangler at Cambridge in 1810. Maule was the elder brother of Frederick Maule (1790-1813) one of the undergraduate members of the Analytical Society. The elder Maule helped to promote the ideas of Woodhouse through his reviews particularly since he was in general agreement with those ideas. He also corresponded with and gave advice to Babbage concerning the affairs of the Analytical Society, and therefore provided a connecting link between Woodhouse and the Society. He was first invited to become a reviewer by Thomas Edward Dicey (1789-1853), Senior Wrangler in 1811, in a letter dated February of 1811. The letter provides an insight into both the policy of the Monthly Review and into the personal side of Woodhouse:

You may probably have heard that Woodhouse occasionally writes the mathematical articles in the "Monthly Review." As it is not unusual in these days of puffing, for an author to review his own work, his last two publications have not yet been noticed, to his no small dissatisfaction.

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<sup>1</sup> Nangle, The Monthly Review: Second Series 1790-1815, p. viii.

The editor, who is a relation of my brother-in-law, applied some time ago to him to endeavor to procure a review of his books. Accordingly, the object of my present letter is to ask you whether you are disposed to commence your career of authorship by a review of one or both of the 'Trigonometry' and 'Isoperimetrical Problems.'<sup>1</sup>

Maule consented to review Woodhouse's works and his reviews appeared in the May of 1811 issue of the Monthly Review. He commended Woodhouse for providing a treatise on trigonometry which would enable the students of the university to read with comparative ease "those inquiries into the system of the universe which they would otherwise have found inaccessible."<sup>2</sup> Maule went into more detail in his review of the Treatise on Isoperimetrical Problems. He noted that, although the mathematical sciences had made rapid and continual advances in the last century, the efforts of English writers had done little to accelerate its progress; and the "discoveries of Newton have pushed but few and feeble shoots in their native soil, while in foreign climates they have matured into fertility and expanded into luxuriance."<sup>3</sup> According to Maule, the reasons for the inferior

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<sup>1</sup> Emma Leathley, ed., Memoir of the Early Life of The Right Hon. Sir W.H. Maule (London: Richard Bentley and Son, 1872), pp. 172-173.

<sup>2</sup> [William Henry Maule], "Woodhouse on Trigonometry," review of A Treatise on Plane and Spherical Trigonometry by Robert Woodhouse, in The Monthly Review; or Literary Journal, Enlarged, Ser. 2, LXV, (May, 1811): 38-39.

<sup>3</sup> [William Henry Maule], "Woodhouse on Isoperimetrical Problems," review of A Treatise on Isoperimetrical Problems, and the

performance of English mathematicians was not as some persons thought, that "the present generation of their countrymen appears to be a degenerate and frivolous race, incapable of those efforts of study and meditation from which alone improvements in the more abstruse sciences are expected." It was not he added, because the attention of students has been diverted that "we have fewer writers because we have fewer readers of mathematics than formerly."<sup>1</sup> Students of mathematics had not increased in proportion to that of literary men in general, but Maule believed their number had not diminished and that the recent performances of contemporaries in other branches of science and literature were sufficient to vindicate them from the charge of degeneracy. According to Maule, there were several reasons for the inferior position of English mathematics compared to that of foreign mathematics. The peculiar methods by which the study of mathematics was encouraged in English universities as compared to Continental universities was given as one reason.<sup>2</sup> Maule did not elaborate, but William Smyth (1765-1849), a 1787 graduate of Peterhouse College, University of Cambridge and Regius Professor of

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Calculus of Variations, by Robert Woodhouse, in The Monthly Review: or Literary Journal, Enlarged, Ser. 2, LXV, (May, 1811), 39.

<sup>1</sup>Ibid., pp. 39-40.

<sup>2</sup>Ibid., pp. 40-41.

History at Cambridge from 1807 to 1849, gave some insight into the matter in a review for the Monthly Review. The University of Cambridge for example had patronized mathematics by encouraging professors to write mathematical works for both "teachers and learners," and had then paid the printing costs. The university paid the printing costs so that the lecture notes and mathematical manuscripts written by the professors could be shared with the students as well as with other professors. Providing works for both teachers and students was a problem because such a practice did little to extend the boundaries of mathematics. Since the writers usually "must pursue the beaten track," they "must be contented to avail themselves of what others have written on particular subjects."<sup>1</sup> Under such a system, mathematics tended to stagnate and thus gave rise to complaints by Babbage and other members of the Analytical Society that mathematics taught at the University of Cambridge did not include the advances in mathematics which had been developed on the

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<sup>1</sup>[William Smyth], "Wood's Algebra; and Vince's Fluxions," review of The Elements of Algebra: Designed for the Use of Students in the University, Vol. I, by James Wood; and The Principles of Fluxions: Designed for the Use of Students in the University, Vol. II, by the Rev. S. Vince, in the Monthly Review: or Literary Journal, Enlarged, Ser. 2, XXIII (June, 1797), 188-192. These two books were to form the first two volumes of a four volume series entitled The Principles of Mathematics and Natural Philosophy to be written presumably by Wood and Vince.

Continent. In support of Woodhouse, Maule stated that the adherence by the English to a different system of notation than was used on the Continent and the use of

methods so deficient in clearness, universality, and evidence, that their conclusions, if seldom erroneous, are almost always wanting in generality; and their formulae, though enabling them to arrive at the solution of the simplest class of isoperimetrical problems, are totally incompetent to conduct them to the more complex and less obvious, though not less interesting or less important results, which have crowned the labours of foreign geometers.<sup>1</sup>

As noted above, Maule's reviews helped to bring the views of Woodhouse to the attention of the public and, as will be shown in the discussion of the Analytical Society, to the attention of the members of the Society. However, Woodhouse would probably have not been successful in bringing about a reform in English mathematics during his lifetime even with the support of Maule. Such a reform required a group effort because of the resistance by the Cambridge professors to Continental mathematics. But, Woodhouse worked as an individual and therefore did not acquire the followers needed to effect a reform. Also, he did not offer any support to the members of the Analytical Society who were very diligent in their efforts to support and extend the ideas of Woodhouse. His primary contribution to the reform of

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<sup>1</sup>[Maule], "Woodhouse on Isoperimetrical Problems, in Monthly Review, Ser. 2, LXV (May, 1811), 40-41.



mathematics was therefore to provide a stimulus for the reform activities of the Analytical Society through his reviews, books, and teaching but not through any direct or overt support of the Society itself.

## II. THE ANALYTICAL SOCIETY--PURE D-ISM OR DOT-AGE?

The Analytical Society was formed in 1812 by a group of undergraduate students at the University of Cambridge for the purpose of promoting and improving mathematical analysis.<sup>1</sup> The Society was formed because the students were dissatisfied with the content and quality of the mathematical offerings at Cambridge. As noted above, Woodhouse provided the foundation for mathematical reform but did not attempt to initiate a group effort to carry out the reform. Such an effort was begun when Babbage enrolled in Cambridge in October of 1810 and formed friendships with other students who shared his enthusiasm for mathematics but who were not satisfied with the mathematical offerings at Cambridge. Three of his friends John Herschel, George Peacock (1791-1858) and Edward Ffrench Bromhead (1789-1855) took an active part in the formation of the Analytical Society. All four students were extremely well versed in mathematics. Babbage, as we have seen, had independently studied an impressive amount of mathematics prior to coming to Cambridge. He stated that he encountered many difficulties in his independent studies and had looked forward to having his questions answered at the University. He had even thought of a plan for a chess club and a

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<sup>1</sup>"Plan of a New Society," MS The Library, St. John's College, University of Cambridge, c.a. 1812. p. 1.

club for the discussion of mathematical subjects. However, after he was at Cambridge a short while his enthusiasm soon faded, since the many questions concerning mathematics which he had hoped to have answered were dismissed by Hudson, his public tutor, as being of no consequence. His questions were similarly received by one of the lecturers.<sup>1</sup> In 1811, an event occurred which added to Babbage's disillusionment with Cambridge mathematicians. He had heard of a book<sup>2</sup> on calculus by Silverstre François La Croix (1765-1843), a French mathematician, which he "longed to possess" and was resolved to purchase it in London on his passage to Cambridge. It was difficult to procure foreign books because of the war, and Babbage had to pay the costly sum of seven guineas. He then spent the greater part of a night looking through the book, and when his tutors dismissed his questions<sup>3</sup> on it, Babbage's disillusionment became complete.<sup>4</sup>

I thus acquired a distaste for the routine of the studies of the place, and devoured the papers of Euler and other mathematicians, scattered through innumerable

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<sup>1</sup> Babbage, Passages, pp. 26-27. Babbage did not provide any other identification for his tutor, Hudson, or any at all for his lecturers.

<sup>2</sup> Silvestre François La Croix, Traite' e'lementarie de calcul integral (2 ed. rev. et cor.; Paris: Chez Caurcier, 1806).

<sup>3</sup> Babbage, Passages, p. 27. He did not specify the nature of his questions concerning La Croix's book.

<sup>4</sup> Ibid., pp. 26-27.

volumes of the academies of Petersburg, Berlin, and Paris, which the libraries I had recourse to contained.

Under these circumstances it was not surprising that I should perceive and be penetrated with the superior power of the notation of Leibnitz.<sup>1</sup>

Babbage's earlier idea for forming a mathematical society was revived by a discussion on mathematics with a friend, Michael Slegg of Trinity College; and by an advertisement by societies formed for the purpose of printing and circulating the Bible. The societies were engaged in a fierce controversy with at least one group proposing "to circulate it [the Bible] with notes, in order to make it intelligible; whilst the other scornfully rejected all explanations of the word of God as profane attempts to mend that which was perfect."<sup>2</sup> The walls of the buildings of Cambridge were "placarded with broadsides, and posters were sent from house to house."<sup>3</sup> Slegg had left one of the posters which was in the form of an advertisement lying upon Babbage's table. Of the advertisement and society Babbage wrote:

Taking up the paper, and looking through it, I thought it, from its exaggerated tone, a good subject for a parody.

I then drew up the sketch of a society to be instituted for translating the small work of Lacroix on the Differential and Integral Calculus. It proposed that we should have

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<sup>1</sup> Ibid., p. 27.

<sup>2</sup> Ibid., pp. 27-28.

<sup>3</sup> Ibid., p. 28.

periodical meetings for the propagation of d's; and consigned to perdition all who supported the heresy of dots. It maintained that the work of Lacroix was so perfect that any comment was unnecessary.<sup>1</sup>

When Babbage saw Slegg again he showed the parody to him.

Slegg enjoyed the joke and asked Babbage's permission to show the parody to his mathematical friend, Edward French Bromhead.

Bromhead considered the idea for a mathematical society too good to be lost. He proposed that they seriously consider forming such a society for the cultivation of mathematics. He called on Babbage the next day to talk over the matter, and it was decided to hold a meeting at Bromhead's lodgings for the purpose of forming a society for the promotion of analysis.<sup>2</sup>

At that meeting, besides the projectors, there were present Herschel, Peacock, D'Arblay, Ryan, Robinson, Frederick Maule, and several others.<sup>3</sup> We constituted

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<sup>1</sup>Ibid., p. 23.

<sup>2</sup>Ibid., pp. 28-29.

<sup>3</sup>D'Arblay was the only son of Madam D'Arblay; Ryan was later the Right Honourable Sir Edward Ryan; Robinson was later the Rev. Dr. Robinson, Master of the Temple, see Babbage, Passages, p. 29. Frederick Maule was the younger brother of the Right Honourable Sir William Henry Maule, see Emma Leatherly, ed., The Right Hon. Sir W.H. Maule, p. 4. Herschel became Sir John F.W. Herschel, and Peacock became the Rev. Dr. George Peacock, Dean of Ely. In Buxton's Charles Babbage, p. 143, in a quotation from Babbage's MSS book relating to the Analytical Society, Richard Gwatkin, a Mr. Wilkinson, and John William Whittaker were also listed. Babbage gave the date of the above meeting as Thursday, May 7, 1812.

ourselves 'The Analytical Society;' <sup>1</sup> hired a meeting-room, open daily; held meetings, read papers, and discussed them. Of course we were much ridiculed by the Dons; and, not being put down, it was darkly hinted that we were young infidels, and that no good would come to us.

In the meantime we quietly pursued our course, and at last resolved to publish a volume of our Transactions. <sup>2</sup> Owing to the illness of one of our number, and to various other circumstances, the volume which was published was entirely contributed by Herschel and myself.

At last our work was printed, and it became necessary to decide upon a title. Recalling the slight imputation which had been made upon our faith, I suggested that the most appropriate title would be --

The principles of Pure D-ism in opposition to the Dot-age of the University. <sup>3</sup>

At one of their meetings the Analytical Society members drew up a "Plan of a New Society" in which they resolved to

introduce more generally the foreign notation in Analytical Science [while] regarding geometry & geometrical demonstration as contrary to its ultimate objects, yet not wholly excluding them. <sup>4</sup>

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<sup>1</sup>"Plan of a New Society," MS The Library St. John's College, pp. 1-5. A resolution was made to call the Society the "Cambridge Analytical Society" but the word Cambridge has been crossed out.

<sup>2</sup>The Transactions referred to the Memoirs of the Analytical Society, 1813 (Cambridge: J. Smith, Printer to the University, 1813), by Charles Babbage and John Herschel.

<sup>3</sup>Babbage, Passages, p. 29.

<sup>4</sup>"Plan of a New Society," MS The Library St. John's College, p. 2.

It should be noted that there was subsequently very little mention of geometry and geometrical demonstration by Analytical Society members except for those made by William Whewell (1794-1866), a fellow student at Cambridge who probably joined the Society after the resolution was made. Whewell, as will be discussed below, did not agree that geometry, geometrical demonstration, and the application of analysis to physical phenomena should be excluded. The reviewers of the mathematical works by Analytical Society members were also very critical because of the exclusion of geometry and physical applications.

In another resolution which was more emphatic with reference to notation than the one above, it was noted that

with the impossibility of expressing after the fluxionary manner many interesting and profound discoveries [the Analytical Society] will endeavor to supersede that notation as altogether inadequate to its objects and will admit no papers in which it [fluxional notation] is employed.<sup>1</sup>

Thus the Analytical Society adopted Continental notation in lieu of fluxional notation because they maintained the latter was too cumbersome for their purposes.

In the preface of the Memoirs of the Analytical Society, Babbage and Herschel discussed what they considered to be the primary deficiencies in the mathematics taught at Cambridge and in England

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<sup>1</sup>Ibid., p. 3.

as a whole. They attempted to "trace the history of the differential calculus through the cloud of dispute and national acrimony, which has been thrown over its origin, . . ."<sup>1</sup> The dispute to which they referred was that which involved Newton and Leibniz and their respective followers over who had invented the calculus. Babbage and Herschel contended, as had Woodhouse, that the followers of Newton had failed to improve upon the calculus because of their awe and respect for Newton. The result was that England had fallen behind the Continent in the study and development of the calculus in particular and mathematical analysis in general. Thus, they set for themselves the task of introducing the improvements made by Continental mathematicians into the mathematical analysis studied in England. According to Babbage and Herschel, improvements in mathematical analysis in England had been ignored "as if the soil of this country were unfavourable to its cultivation."<sup>2</sup> They further stated that "we now have to reimport the exotic, with nearly a century of foreign improvement, and to render it once more indigenous among us."<sup>3</sup>

As was noted above, Woodhouse did not offer any support of the Analytical Society. The reason for this lack of support is unclear,

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<sup>1</sup> Babbage and Herschel, Memoirs, p. iv.

<sup>2</sup> Ibid., p. iv.

<sup>3</sup> Ibid., pp. iii-iv.



but it may have been that there existed too large a communication barrier between Woodhouse as a professor at Cambridge and the group of undergraduate students who formed the Analytical Society. However, it was William Maule who provided, through his favorable reviews of the mathematical works of Woodhouse, a connecting link between Woodhouse and the members of the Analytical Society.

Maule, as a brother of Frederick Maule an undergraduate member of the Society, formed friendships with members of the Society, particularly Babbage, Herschel, and Edward Ryan (1793-1875). As Senior Wrangler in 1810 and as a reviewer for the Monthly Review Maule was well qualified for his assumed role as an advisor to the Analytical Society. An example of the advice and support which Maule offered to the members of the Society is found in a letter, dated February of 1813, to Edward Ryan. Maule wrote:

If I had been at Cambridge, I should have ventured to suggest to those members of the Analytical Society with whom I am acquainted that they should have sent their memoirs, or some of them, to Leybourne, instead of publishing them independently. By that mode of publication they would have obtained a wider circulation for their discoveries than by that which they have adopted, at a much smaller, or rather no expense; and at the same time they would have conferred on the editor of the "Mathematical Reporter" an important benefit by supplying that deficiency of original essays which is one of the principal causes of the long intervals between the

appearance of his numbers. Tell Babbage I have not yet duly considered the last letter of his, but that I hope some time to write or talk to him about it.<sup>1</sup>

Maule mentioned in this letter that he had received a letter from Babbage which concerned Babbage's interest in the calculus of functions, an interest which dated back to about 1808, or two years before Babbage entered Cambridge. Once at Cambridge he had sought the advice of Maule, who was considered to be a very promising mathematician.<sup>2</sup> Maule answered Babbage's letter as he had promised to do in his February, 1813 letter to Ryan. His letter to Babbage, on December 12, 1813, which was probably delayed because of the death of his brother, Frederick Maule, began:

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<sup>1</sup>Leathley, The Right Hon. Sir W.H. Maule, pp. 233-241. Maule in an earlier letter to Frederick, n.d., but probably written in 1812, had written a comment which gave insight into the activities of the Society members as well as his opinion of Babbage: "I am glad you are in the Analytical Society. Remember me to your brother analysts, Babbage, Slegg, &c., and to Beckett, Musgrove, &c. I shall soon, probably have an opportunity of seeing Colburn, the arithmetical phenomenon: ask Babbage if he can suggest any questions to ask him."

<sup>2</sup>Sophia De Morgan, Augustus De Morgan, pp. 386-387. Augustus De Morgan wrote a letter, dated July 1869, to Sir Frederick Pollock regarding the impact Babbage, Herschel, and Peacock had on mathematics in England, as well as the correspondence of Maule and Babbage concerning "functional equations, and all kinds of novelties." De Morgan, who along with George Peacock helped to develop the axiomatic bases of arithmetic and algebra, stated that "Maule would have been conspicuous, among the moderns, with Herschel, Peacock, and Babbage, if he had held on." Maule studied law after earning his B.A. in 1810, but he retained an interest in mathematics for a few years.

It is so long since I have met with any mathematical novelty, that I was very agreeably surprised when I received your memoirs. You will easily believe that I have not had time for more than a very hasty consideration of them. They seem to contain many curious and surprising results, original views, and bold and fortunate extensions; there are several parts on which I hope to have an opportunity of talking with you either in London or Cambridge. Am I right in guessing, from the manner in which you mention in your letter your rules for finding the indices of powers of numbers, that you obtained them by induction?<sup>1</sup>

Herschel, as had Maule, also became interested in the calculus of functions and published a paper which helped to bring to the attention of the non-University audience the activities of the Analytical Society's members.<sup>2</sup>

An anonymous reviewer wrote that he wished he could have given his readers "an intelligible abstract of this ingenious memoir,"<sup>3</sup> but the three or four pages which Herschel devoted to the defense and illustration of the "notation employed in the subsequent part"<sup>4</sup> and the general abstractness of the paper prevented him from doing so. The reviewer criticized what he felt "seemed" to be Herschel's viewpoint

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<sup>1</sup> Leathley, The Right Hon. Sir W.H. Maule, p. 270.

<sup>2</sup> John Herschel, "Consideration of Various Points of Analysis," Philosophic Transactions of the Royal Society of London, CIV, No. 1, (1814), 440-463.

<sup>3</sup> Review of a "Consideration of Various Points of Analysis," by John F.W. Herschel in The Monthly Review; or Literary Journal Enlarged, Ser. 2, LXXVI (March, 1815), 261-262.

<sup>4</sup> Ibid., pp. 261-262.

that the "present state of analysis is adequate to every purpose to which we can reasonably hope to see it applied."<sup>1</sup> He questioned whether mathematicians in general would readily subscribe to what he called Herschel's "analytical creed," that there was nothing left to "effect [in analysis] but generalization," and pointed out that though Lagrange was now dead there still remained "a Laplace, a Legendre, a Poisson, and an Ivory"<sup>2</sup> to expand the boundaries of analysis. Herschel was in reality attempting to carry out a reform of English mathematics which had been initiated by Woodhouse and supported by William Maule. The goal of this reform was to discard analytical methods which depended upon physical phenomena, such as the use of motion, and geometrical demonstration in lieu of purely algebraic methods as developed by Leibniz and his followers on the Continent.<sup>3</sup> It is not surprising, therefore, that the reviewer should question such a fundamental change.

Babbage also published a paper<sup>4</sup> in which he proposed to "present an outline of a new calculus." By using algebraic operations

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<sup>1</sup> Ibid., p. 262.

<sup>2</sup> Ibid., p. 262.

<sup>3</sup> "Plan of a New Society," MS The Library, St. John's College, University of Cambridge, p. 2.

<sup>4</sup> Charles Babbage, "An Essay Towards the Calculus of Functions," [Pt. 1], Philosophic Transactions of the Royal Society of London, CV, No. 1, (1815), 389-423.

and, in some instances, new notation, he attempted to extend the applications of the concept of a function, which could be used in its "most extended signification" to designate "the result of every operation that can be performed on quantity."<sup>1</sup> To this end Babbage employed algebraic substitution in a given function to generate a new function as in the following:

If in any function as  $\psi x$ , instead of  $x$ , the original function be substituted, it becomes  $\psi\psi x$  or  $\psi^2 x$ : this is called the second function of  $x$ . If the process be repeated, the result is  $\psi^2\psi x$  or  $\psi^3 x$ , the third function of  $x$ ; and similarly  $\psi^n x$ , denotes the  $n$ th function of  $x$ . Suppose

$$\psi x = a + x$$

then  $\psi^2 x = a + a + x = 2a + x$

and generally  $\psi^n x = na + x$

A functional equation is said to be of the first order, when it contains only the first function of the unknown quantity; as, for instance,

$$\begin{aligned}\psi ax + x\psi x - x^n &= 0 \\ (\psi x + \psi \cdot 1/x)^n - ax + x^2 &= 0\end{aligned}$$

If the second function enter, the equation rises to the second order: thus,

$$\begin{aligned}\psi^2 x &= x \\ \psi(x + \psi x) + (\psi x - x)^2 &= 0 \\ (\psi^2 x + \psi \cdot 1/x)^n &= ax\end{aligned}$$

A function of two variables admits of two second functions: thus  $\psi(x, y)$  becomes  $\psi(\psi(x, y), y)$ , and  $\psi(x, \psi(x, y))$  or they might be thus expressed  $\psi^{2, 1}(x, y)$  and  $\psi^{1, 2}(x, y)$ .<sup>2</sup>

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<sup>1</sup>Ibid., p. 389.

<sup>2</sup>Ibid., pp. 390-391.

Babbage also attempted to carry out another of the goals of the Analytical Society in his paper. He referred to geometry sparingly and then only to supply an intuitive idea of the principles he wished to explore algebraically.<sup>1</sup> He also explored the inverse method with respect to functions which he considered "by far the most difficult, and it might perhaps be added, the most useful."<sup>2</sup> By the inverse method Babbage meant the integration of a function or the extraction of a root of any number. Babbage referred to the theorem of Pappus and the hyperbole problem which he had attempted to solve prior to coming to Cambridge.<sup>3</sup> These explorations, he explained, led him to a concern with finding new methods and general solutions to functional equations. He believed that his method of treating functional equations of the first order to be "entirely new."<sup>4</sup> Equations of the second and higher orders had never been mentioned, but he thought that "it is these which present the most interesting speculations, and which are

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<sup>1</sup>"Plan of a New Society," MS The Library of St. John's College, University of Cambridge, pp. 1-5.

<sup>2</sup>Babbage, "Calculus of Functions," [Pt. 1], Philosophical Transactions, CV, No. 1 (1815), p. 389.

<sup>3</sup>Charles Babbage, The History of the Origin and Progress of the Calculus of Functions During the Years 1809, 1810, . . . 1817, n.d., Museum, Oxford, MS Buxton 13, ff. 5-15.

<sup>4</sup>Babbage, "Calculus of Functions," [Pt. 1], Philosophical Transactions, CV, No. 1 (1815), p. 391.

involved in the greatest difficulties."<sup>1</sup> Babbage concluded his paper with the observation that if his methods were to be applied to a consideration of functional equations of many variables, even greater difficulties presented themselves. An improvement in notation would be a first step in the solution of such equations. He stated that he was in possession of methods to solve equations of two or more variables, to give the general solution of equations of all orders, including symmetrical functions, to treat functional equations of the first order, of any number of variables, and to apply his new method to the solution of differential and even partial differential equations.<sup>2</sup>

An anonymous reviewer of Babbage's paper both praised and cautioned him. The reviewer noted that those readers who were well acquainted with higher analytical pursuits would comprehend and appreciate both the difficulty of the problems Babbage had presented as well as the many useful purposes which such a calculus might answer once it was well established. However, the reviewer did not consider the calculus of functions to be well established if it depended upon an artificial notation,

although it might enable the operator to exhibit a solution to the eye, of which no one could form any mental conception, or submit to any known mode of computation. In making this remark, we by no means intend to insinuate

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<sup>1</sup> Ibid., p. 391.

<sup>2</sup> Ibid., p. 423.

that any false glare is observable in the present paper; it is on the contrary, perhaps as far as the author has carried it, in the simplest form that the subject will admit: but a regret seems to be expressed that the notation is not more general; and this, we apprehend, may be followed by some attempt calculated to produce that kind of artificial and unmeaning solution to which we have referred, and against which we should wish him to guard.<sup>1</sup>

The reviewer concluded by stating that "Should Mr. Babbage ultimately succeed, his name will undoubtedly stand in the first rank of modern analysts, and we sincerely wish him every possible success."<sup>2</sup>

Herschel published a paper on exponential functions<sup>3</sup> which was a continuation of his portion of the Memoirs of the Analytical Society.<sup>4</sup> From the remarks made in the reviews it is evident that the members of the Analytical Society were beginning to attract attention which was both positive and negative. An anonymous reviewer of Herschel's paper deplored the view that the

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<sup>1</sup> Review of "An Essay Towards the Calculus of Functions," [Pt. 1], by C[harles] Babbage, in The Monthly Review: Or Literary Journal, Enlarged, Ser. 2, LXXX (May, 1816), 82.

<sup>2</sup> Ibid., p. 83.

<sup>3</sup> John Herschel, "On the Development of Exponential Functions: Together with Several New Theorems Relating to Finite Differences," Philosophical Transactions of the Royal Society of London, CVI, Pt. 1 (1816), 25-45.

<sup>4</sup> Babbage and Herschel, Memoirs, pp. 65-114.



analytical sciences in this country were rather retrograding than advancing, and that nothing strikingly new and interesting had for many years issued from the English press on those subjects; while the transactions of foreign academies, and particularly those of the French Institute, have abounded with valuable and brilliant discoveries. These, however, have not been unmixed with matters of mere curiosity and difficulty; the purpose of the writers, in many cases, being obviously to shew their own dexterity in the transformation of quantities and equations, and to make a great display of intricate and almost unintelligible formulae, without the least consideration of their application to any purpose of real utility. . . .<sup>1</sup>

The reviewer was here expressing his preference for methods of solution which used geometrical demonstration rather than purely algebraic methods. He was also making a plea that mathematical enquiry should have practical applications rather than abstractions as Continental mathematicians preferred.<sup>2</sup> He was not satisfied with the reform Herschel and other Analytical Society members were attempting to carry out.

Mr. Herschel has in two or three instances manifested considerable analytical talents, which we should be very loth to undervalue: but we fear that he is

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<sup>1</sup>"Mathematics, Astronomy, and Optics," review of "On the Development of Exponential Functions; Together with Several New Theorems Relating to Finite Differences," Pt. 1, by John F. W. Herschel, in The Monthly Review or Literary Journal, Enlarged, Ser. 2, LXXXI (December, 1816), 393.

<sup>2</sup>[John Playfair], "Woodhouse's Astronomy," review of An Elementary Treatise on Astronomy (Vol. I; Cambridge, 1812; Vol. II, 1818) by Robert Woodhouse, A.M., F.R.S., Fellow of Caius College, Cambridge, in The Edinburgh Review, or Critical Journal, XXXI (March, 1819), 375-378.

too fond of that sort of parade to which we have alluded, and which we should be glad to see him correct. We wish it also to be understood that these remarks are not so much intended to apply exclusively to the present article, as to the general character of his recent communications to the Royal Society, and to a Cambridge work<sup>1</sup> in which he is supposed to take an active part.<sup>2</sup>

Two other publications, one by Babbage and the other by Edward Ffrench Bromhead,<sup>3</sup> one of the originators of the Analytical Society, were negatively received by an anonymous reviewer. The paper by Babbage, a continuation of his previous paper, was "An Essay Towards the Calculus of Functions, Part II."<sup>4</sup> It was reviewed by the reviewer of Babbage's first paper on the "Calculus of Functions." The reviewer noted that Babbage's promise to bring a new portion of analysis to perfection was unfulfilled, that few advances had been made toward the completion of Babbage's views, and that Babbage by this time "must be well aware of the great difference

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<sup>1</sup> The reviewer is probably referring to the Memoirs of the Analytical Society by Babbage and Herschel.

<sup>2</sup> Review of "On the Development of Exponential Functions; Together with Several New Theorems Relating to Finite Differences," by John F.W. Herschel in The Monthly Review: or Literary Journal, Enlarged, Ser. 2, LXXXI (December, 1816), 393.

<sup>3</sup> It was Bromhead who took Babbage's parody for a mathematical society seriously.

<sup>4</sup> Philosophical Transactions of the Royal Society of London, CVI, Part II, 179-256.

between forming a project and carrying it into execution. "<sup>1</sup>

Bromhead's paper "On the Fluents of Irrational Functions," likewise received an unfavorable review.<sup>2</sup> The anonymous reviewer was doubtful as to whether he understood what Bromhead intended "to perform," and stated:

We have at different times endeavored to expose the absurdity of perpetually introducing new symbols, new terms, and new significations of old terms, which it seems to be the principal aim of some of our modern analysts: but in no instance have we seen it carried to so great a length as in the papers we have just examined.<sup>3</sup>

As can be seen from the reviews above, a reform movement based only upon journal publications probably would not have succeeded because of the resistance to change expressed by many of the reviewers, and because the publications were external to the reform movement in the sense that they did not directly affect students. Babbage aided the reform movement in 1813 when he returned to the idea which was one of the goals of the Analytical Society--translating Lacroix's work on the Differential and Integral Calculus. He thought that the translation of a book, which could be used by students, was a

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<sup>1</sup> Review of "An Essay Towards the Calculus of Functions, Part II," by Charles Babbage in The Monthly Review: or Literary Journal, Enlarged, Ser. 2., LXXXIII (May, 1817), 55.

<sup>2</sup> Review of "On the Fluents of Irrational Functions," by Edward Ffrench Bromhead in The Monthly Review: or Literary Journal, Enlarged, Ser. 2, LXXXIII (May, 1817), 58.

<sup>3</sup> Ibid., p. 58.

means of carrying out an internal reform at the University. The translation of this work fell to Babbage, since the other members of the Analytical Society were preoccupied with work on their degrees and examinations and were unable to undertake the task. He began the translation of the book, finished a portion of it, and laid it aside. George Peacock called on him sometime later and stated that Herschel and he were convinced that the change from dots to d's would not be accomplished until some important foreign work was translated into English. Peacock proposed that Babbage either complete the translation of Lacroix's work or that Herschel and he would complete it. Babbage suggested that they toss a coin to determine how the work would be completed, and, by the outcome of the toss,<sup>1</sup> it was decided to make a joint translation which was completed and published in 1816.<sup>2</sup>

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<sup>1</sup> Babbage, Passages, pp. 38-39.

<sup>2</sup> Silvestre François Lacroix, An Elementary Treatise on the Differential and Integral Calculus: Translated from the French with an Appendix and Notes (Cambridge: J. Smith for J. Deighton and Son, 1816). Part one on differential calculus was translated by Babbage; part two on integral calculus was translated by Peacock and Herschel. The appendix on the calculus of differences and series was replaced with an original treatise by Herschel. A series of notes followed the appendix, with the first twelve notes being written by Peacock and the remaining notes by Herschel. It is puzzling that Herschel wrote a letter to Lacroix, dated December 24, 1816, Bibliothéque de l'Institut de France, Correspondence of Lacroix MS 2396, requesting permission from Lacroix, in the names of Peacock, Babbage and his own, to translate his work. Yet, the publication date is 1816 which would indicate that they had already translated the work and belatedly asked Lacroix's permission. This material was furnished by Professor Robert J. Morris of Oregon State University.

An anonymous reviewer of the Lacroix translation commended Babbage, Herschel, and Peacock on the accuracy of their translation and for the Appendix written by Herschel and Peacock. The reviewer commented that "perhaps few mathematicians are better calculated for the due performance of such an undertaking," but he did not share their enthusiasm for French mathematics.<sup>1</sup>

The reviewer's point of view was that in matters of science "we ought undoubtedly to divest ourselves of every species of nationality; and to examine every subject, not with reference to the country in which it was produced,"<sup>2</sup> but with reference to "its actual merits and the evidence of the principles on which it is founded."<sup>3</sup> Herschel, Babbage, and Peacock, he felt, had been led "into a blind admiration of others, to the total disregard of our own merits."<sup>4</sup> The reviewer thought that Peacock, in particular, had been misled when he wrote in one of his notes to the Lacroix translation that, "in the brevity of its demonstrations, and in the facility of its applications, the

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<sup>1</sup>"Lacroix on the Differential and Integral Calculus, translated," review of An Elementary Treatise on the Differential and Integral Calculus by S. F. Lacroix. Translated from the French with Appendix and Notes; in The Monthly Review: or Literary Journal, Enlarged. Ser 2, LXXXVII (October, 1818), 180.

<sup>2</sup>Ibid., p. 182.

<sup>3</sup>Ibid., p. 183.

<sup>4</sup>Ibid., p. 183.

fluxional calculus is inferior to all other methods."<sup>1</sup> The reviewer went on to discuss why he thought such a view was naive:

This part of the note in question we unequivocally deny; for whatever may be the other defects of the doctrine of fluxions, it certainly yields to no method in those very qualities in which it is here stated to be inferior. That the mixture of the mechanical and geometrical considerations on which it is founded, is in some degree defective, we will not dispute; and that the fluxional notation has not in it that symmetry which belongs to the other, we will also concede: but is the fluxional notation an essential part of that doctrine? Might we not write  $fx$  for  $x$ ,  $f^2x$  for  $x$ , and so on, without changing in any respect the nature of the principle? Yet the author asserts that the most important distinction between this system and the differential calculus consists in a different notation: that is to say, the greatest defect in this method might be corrected by a change in the first two pages of a treatise on fluxions, by directing the reader to denote a quantity in one manner instead of another. If, then, this be the greatest defect, and this defect may be so easily corrected, how can the fluxional analysis, with justice to its illustrious inventor, be placed so low in the present writer's scale of estimation?<sup>2</sup>

The confusion on the part of the reviewer was due to Peacock, who had given a detailed analysis of why he thought the fluxional calculus was inferior to Continental methods.<sup>3</sup> He had stated, for

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<sup>1</sup> George Peacock, as quoted in ibid., p. 183. Peacock originally wrote the statement in "Note B," in, An Elementary Treatise, by S.F. Lacroix, p. 613.

<sup>2</sup> "Lacroix on the Differential and Integral Calculus, translated," Monthly Review, Ser. 2, LXXXVII (October, 1818), 183.

<sup>3</sup> George Peacock, "Note B," in Lacroix, An Elementary Treatise, p. 617.

example, that the consideration of motion, which was essential to the methods of fluxions was

foreign to the spirit of pure Analysis; and the analogy by which the name and properties of a fluxion are transferred to a modification of the difference of a function is strained and unnatural. The different orders of fluxions are also involved in considerable obscurity, and we are utterly unable to comprehend the connection which they bear to their primitive function.<sup>1</sup>

Here Peacock was echoing the sentiments of Woodhouse when he stated that the use of motion was foreign to the spirit of pure analysis. However, his insistence that "the most important distinction between this system [fluxional calculus] and the Differential Calculus consists in a different notation,"<sup>2</sup> was the reason for his reviewer's confusion. A discussion of notation was a convenient point of comparison between fluxional calculus and differential calculus. The differences in notation between the two systems were not as difficult to explain as the differences in methods, with the result being that the consideration of the relative merits of notation often overshadowed those of methods and gave the false impression that the reform Woodhouse, Herschel, Babbage and Peacock were advocating was actually rather superficial. In fact, it must be conceded that at times the reformers lost sight of their own goals.

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<sup>1</sup>Ibid., p. 618.

<sup>2</sup>Ibid., p. 618.

In 1816, there arose another opportunity to promote the goals of the Analytical Society. Peacock was elected as one of the moderators for the annual Tripos examinations for 1816-1817. Using his position of moderator, Peacock did what Woodhouse had neglected to do;<sup>1</sup> he promoted the cause of reform by introducing the Leibnizian notation into his examinations by the use of questions such as the following:

"Explain what is meant by the particular solution of differential equations. Give an instance in the equation  $ydx - xdy = n(dx^2 - dy^2)$ ." <sup>2</sup>

Peacock did not entirely forego Newtonian terminology in 1817 as the following examination question shows: "Find the fluxion of  $\sin.x$ ." <sup>3</sup>

He also drew attention directly to the differences in the methods used by Newton and Leibniz as his following examination questions will attest.

Give an account of the controversy between the followers of Newton and Leibnitz, concerning the measure of motion; and reconcile the experiments and results to which the latter appealed, with the measure assumed by the former. <sup>4</sup>

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<sup>1</sup> Cambridge Problems: Being a Collection of the Printed Questions Proposed to the Candidates for the Degree of Bachelor of Arts of the General Examinations from 1801 to 1820 Inclusive (Cambridge: J. Smith, Printer to the University for J. Deighton and Sons, 1821), pp. 3-156. Woodhouse was appointed as one of two moderators for the years 1803, 1804, 1807, and 1808, but his examination questions made use of Newtonian methods and notation only.

<sup>2</sup> Ibid., p. 338.

<sup>3</sup> Ibid., p. 341.

<sup>4</sup> Ibid., p. 357.



John White, Peacock's fellow moderator for 1816-1817, was not convinced that a reform was necessary and did not follow Peacock's example but used Newton's methods and notation exclusively.<sup>1</sup>

However, the impact of Peacock's action can be seen from a letter which William Whewell (1794-1866), a fellow mathematician at Cambridge, wrote to John Herschel on March 6, 1817:

You have I suppose seen Peacock's examination papers. They have made a considerable outcry here and I have not much hope that he will be moderator again. I do not think he took precisely the right way to introduce the true faith. He has stripped his analysis of its application and turned it naked among them. Of course all the prudery of the university is up and shocked at the indecency of the spectacle. The cry is 'not enough philosophy.' Now the way to prevent such a clamour would have been to have given good, intelligible but difficult physical problems, things which people would see they could not do their own way, and which would excite curiosity sufficiently to make them thank you for your way of doing them. Till some one arises to do this, or something like it, they will not believe even though one were translated to them from French.<sup>2</sup>

In 1817-1818 the two new moderators who were appointed, Fearon Fallows and William French, ignored Peacock's attempted reform in their mathematical examinations.<sup>3</sup>

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<sup>1</sup>Ibid., pp. 338-358.

<sup>2</sup>I[saac] Todhunter, William Whewell, D.D., Master of Trinity College: An Account of His Writings with Selections from His Literary and Scientific Correspondence (2 Vols.; London: Macmillan and Co., 1876), II, 16.

<sup>3</sup>Cambridge Problems, pp. 359-380.

Peacock was undaunted by the criticism and clamor to which Whewell alluded. His zeal and enthusiasm for the reform he was trying to effect can be seen in the extract of a letter to a friend, dated March 17, 1817.

I assure you, my dear \_\_\_\_\_,<sup>1</sup> that I shall never cease to exert myself to the utmost in the cause of reform, and that I will never decline any office which may increase my power to effect it. I am nearly certain of being nominated to the office of Moderator in the year 1818-19,<sup>2</sup> and as I am examiner in virtue of my office, for the next year I shall pursue a course even more decided than hitherto, since I shall feel that men have been prepared for the change, and will then be enabled to have acquired a better system by the publication of improved elementary books. I have considerable influence as a lecturer, and I will not neglect it. It is by silent perseverance only that we can hope to reduce the many-headed monster of prejudice, and make the University answer her character as the loving mother of good learning and science.<sup>3</sup>

As Peacock had promised in his letter, he continued in the position of moderator to promote the use of Leibnizian methods and notation. His fellow moderator Richard Gwatkin also used these methods and notation in his examination questions. The moderators for 1819-1820

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<sup>1</sup> The person's name was probably deleted in the letter by Peacock.

<sup>2</sup> Peacock was chosen moderator for this period, and he was also chosen as Senior Moderator in 1821, Proceedings of the Royal Society of London, IX (April, 1859), 539.

<sup>3</sup> Ibid., pp. 538-539.

were Whewell and Henry Wilkinson, who both used differential methods and notation.<sup>1</sup>

After completing the translation of Lacroix's Elementary Treatise in 1816, Babbage, Herschel, and Peacock began another task which was very important to the success of their reform. They prepared two books,<sup>2</sup> which were published in 1820, of problems and solutions to accompany Lacroix's book. They maintained that such books were necessary because only two of the largest colleges at the University of Cambridge had adopted the Continental methods and notation. By writing sets of problems with solutions, they reasoned that the tutors, who did not approve of the change from Newtonian methods and notation, would use the collection of problems in order to save their own time and trouble. They also thought that the sets of problems and solutions might help to motivate those persons who found it difficult to learn a new notation, because as Babbage stated

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<sup>1</sup> Cambridge Problems, pp. 403-425.

<sup>2</sup> George Peacock, A Collection of Examples of the Applications of the Differential Calculus (Cambridge: J. Smith, Printer to the University; sold by J. Deighton and Sons, 1820), and John Herschel, A Collection of the Examples of the Calculus of Finite Differences (Cambridge: J. Smith, Printer to the University; Sold by J. Deighton and Sons, 1820). Although only Herschel's name appears as the author, Babbage actually supplied the problems and solutions on the calculus of functions which he later extracted and published separately as Examples of the Solution of Functional Equations (Cambridge: J. Smith, Printer to the University; sold by J. Deighton and Sons, 1820).

It is always difficult to think and reason in a new language, and this difficulty discouraged all but men of energetic minds. I saw, however, that, by making it their interest to do so the change might be accomplished.<sup>1</sup>

Babbage added that in a few years after the publication of their books of problems and solutions, the change from Newtonian methods and notation to those on the Continent was established. The success which Babbage and his colleagues experienced in their efforts to reform mathematics was due in part to the high quality of their own mathematical works and in part to the positions which some of their members held in the University of Cambridge. The position of moderator held by both Peacock and Whewell was important to the success of the movement because it allowed them to control the content of mathematics taught at Cambridge. The moderator made up the examination given to senior students, and in this way controlled the content of the mathematics studied by the students and taught by the tutors. By providing an English translation of Lacroix's Elementary Treatise, a set of problems and solutions to accompany the book, and, finally, by writing and moderating the examinations, the reform was partially accomplished. It was possible to effect this phase of the reform partly because of the prestige and influence gained by the the Analytical Society members, particularly Herschel, Peacock, Whewell, and Babbage. For example, in 1813 Herschel graduated as

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<sup>1</sup>Babbage, Passages, pp. 39-40.

first Wrangler and first Smith's prizeman and Peacock as second Wrangler and second Smith's prizeman. These honors provided the two men with an immediate access to positions of leadership and influence of their own choosing. Both became members of the faculty at Cambridge with Herschel accepting an appointment for about a year and Peacock choosing teaching as a lifetime career. Peacock was appointed as Assistant Tutor and College Lecturer in 1815, Full Tutor in 1823, and Sole Tutor in 1835, a post which he held until his appointment as Dean of Ely in 1839, when he also took the Degree of Doctor of Divinity. Whewell was elected to a fellowship at Trinity College in 1817, appointed Assistant Tutor in 1818, and became a Full Tutor in 1823, a position which he retained until October of 1841 when he was appointed as Master of Trinity College. Both Peacock and Whewell were therefore able to maintain a long term influence over the reform by their publications, by their influence as tutors on their own students, and by continual curricular reform as University staff members which extended throughout their entire lives.<sup>1</sup>

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<sup>1</sup> Both Peacock and Whewell served on various committees for University reform, but the most important was the University Commission of 1851. See Cambridge University Commission, Report of Her Majesty's Commissioners. . ., (London: William Clowes and Sons, 1852), pp. 1-50; George Peacock, Observations on the Statutes of the University (London: John W. Parker, 1851), pp. 147-167, passim.

The influence exerted by Babbage and Herschel was of an external nature since their direct contact with the University was intermittent after 1820. Both men continued to publish mathematical works during the early part of the 1820's and both were called upon from time to time to support activities related to the University. Babbage's activities became increasingly oriented toward non-University affairs, as did Herschel's, but he was honored by being named as the Lucasian Professor of Mathematics at Cambridge in 1828. With the publication of Passages in the Life of a Philosopher in 1865, Babbage referred to his idea for an appropriate title for the Memoirs of the Analytical Society in 1812 as "The Principles of pure D-ism in opposition to the Dot-age of the University," and noted,

In thus reviving this wicked pun, I ought at the same time to record an instance of forgiveness unparalleled in history. Fourteen years after, being then at Rome, I accidentally read in Galignoni's newspaper the following paragraph, dated Cambridge: --"Yesterday the bells of St. Mary rang on the election of Mr. Babbage as Lucasian Professor of Mathematics."<sup>1</sup>

Herschel, in 1832, reminiscing about the acceptance of the works that he, Babbage, Peacock and Whewell had completed at

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<sup>1</sup> Babbage, Passages, pp. 29-34. Babbage had competed for this professorship in 1826, but his opponent George Biddle Airy, later Astronomer Royal, won the contest. Airy had been a student under George Peacock. Newton held the second Lucasian professorship, which was founded in 1663 by Henry Lucas, M.P. for the University. Babbage, who held the position from 1828 to 1839, said it was "the only honour I received in my own country."

Cambridge, offered his own humorous version of their joint success:

Students of our universities, fettered by no prejudices, entangled by no habits and excited by emulation of youth, had heard of the existence of masses of knowledge from which they were debarred by mere accident of position. They required no more. The prestige which magnifies what is unknown, and the attractions inherent in what is forbidden, coincided in their impulse. The books were procured and read, and produced their natural effects. The brows of many a Cambridge moderator were elevated, half in ire, half in admiration, at the unusual answers which began to appear in examination papers. Even moderators are not made of impenetrable stuff; their souls were touched, though fenced with sevenfold Jacquier, and tough bull-hide of Vince and Wood.<sup>1</sup>

Herschel's phrase, the "tough bull-hide of Vince and Wood," was in reference to the works of James Wood (1760-1839), Fellow of St. John's College at Cambridge, and the Rev. Samuel Vince (1749-1821), on Algebra and Fluxions respectively. The books were designed for use by students in the university and had become the standard works used by the moderators and tutors at Cambridge.<sup>2</sup> It was necessary to the success of the reform movement to replace such books, which represented Newtonian notation and methods, with books

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<sup>1</sup>[John Herschel], "Mrs. Somerville's Mechanism of the Heavens," review of Mechanism of the Heavens, by Mrs. [Mary] Somerville (London: 1832); and Mecanique Celeste, by the Marquis de la Place, &c. Translated, with a Commentary, by Nathaniel Bowditch, LL. D., &c. Vol. I (Boston, 1829), in Quarterly Review, XLVII (July, 1832), 545.

<sup>2</sup>[Smyth], "Wood's Algebra; and Vince's Fluxions," Monthly Review, 2d Ser., XXIII (June, 1797), 138-192.

using Continental methods and notation. Herschel, in reference to the moderators' use of the new notation and methods, wrote:

They were carried away with the stream, in short, or replaced by successors full of newly-acquired powers. The modern analysis was adopted in its largest extent, and at this moment we believe that there exists not throughout Europe a centre from which a richer and purer light of mathematical instruction emanates through a community, than one, at least, of our universities.<sup>1</sup>

Herschel, in looking back upon the reform in 1832, made an interesting comparison of the state of mathematics in Ireland and Scotland with that in England, during the first quarter of the nineteenth century. This comparison provides a valuable point of reference for discussing the reform. He gave credit to Woodhouse for having first pointed out the need for reform. He noted that his writings had stimulated a renewed interest in mathematical subjects and had encouraged the production of mathematical papers of a high quality, such as the one in Ireland by Dr. John Brinkley (1763-1835), the Bishop of Cloyne and President of the Royal Society of Dublin, on the exponential developments of Lagrange.<sup>2</sup> Herschel also noted that among "our Scottish countrymen, indeed the torch of abstract science had never burnt so feebly nor decayed so far as in these

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<sup>1</sup>[John Herschel], "Mrs. Somerville's Mechanism of the Heavens," Quarterly Review, XLVII (July, 1832), 545.

<sup>2</sup>Ibid., pp. 543-544.



southern abodes."<sup>1</sup> He commended John Playfair (1748-1819), a geologist, a mathematician, and a professor of natural philosophy at the University of Edinburgh who had also been critical of English mathematics, for his mathematical works which made use of Continental methods and notation. He also praised James Ivory (1765-1842), a Scottish mathematician, stating that Ivory was

the only British geometer who, at this period, seems to have possessed, not only a complete familiarity with the resources of the higher analysis, but also the habit of using them with skill and success in inquiries of moment in the system of the world.<sup>2</sup>

Thus, in Herschel's opinion the need for reform in mathematics had been greater in England, which had been "dazzled and spellbound by the first great achievement of Newton"<sup>3</sup> with the calculus, than in Ireland and especially in Scotland. However, as far as the role of the Analytical Society in bringing about reform, there were differences of opinion as to its relative importance. According to an anonymous reviewer, the members of the Analytical Society were only the "junior members of the university," and hence "the society was neglected, or discouraged, and soon appeared to be forgotten." The reviewer described the Memoirs of the Analytical Society as "evidently the production of young men, whose enthusiastic attachment

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<sup>1</sup> Ibid., p. 544.

<sup>2</sup> Ibid., p. 544.

<sup>3</sup> Ibid., p. 544.

to abstract mathematics promised, under skillful guidance, still more valuable fruits." The reviewer considered the formation of the Cambridge Philosophical Society in 1819, to be an example of one of the "more valuable fruits" of the members of the Analytical Society. The new society was the result of "some of the most active members of the Analytical Society having afterwards acquired sufficient influence to form, on a more enlarged plan, an association which the university itself was at length prevailed upon to support."<sup>1</sup>

Another anonymous reviewer commented in 1826 upon the changes brought about by the reform movement in mathematics. He was very doubtful as to whether the changes in analytical science, "so fashionable among the mathematical savans of Cambridge,"<sup>2</sup> were beneficial. He thought that innovations in analytical science which had

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<sup>1</sup>"Scientific Institutions," review of 1. Transactions of the Cambridge Philosophical Society, I. 2. Memoirs of the Literary and Philosophical Society of Manchester, 2d Series, IV, London, 1824. 3. Transactions of the Royal Geological Society of Cornwall, I and II. 4. Report of the Liverpool Royal Institution, 1822. 5. Bristol Institution. Proceedings of the Second Annual Meeting, held February 10, 1825. 6. Annual Report of the Council of the Yorkshire Philosophical Society for 1824, in The Quarterly Review, XXXIV (June, 1826), 169. Peacock and Whewell were those very active members of the Analytical Society who helped to promote the formation of the Cambridge Philosophical Society.

<sup>2</sup>"Lardner's Differential and Integral Calculus, review of An Elementary Treatise on the Differential and Integral Calculus (London: Taylor, 1826), by the Rev. Dionysius Lardner, A.M., in The Westminster Review, V (January, 1826), 130-131.

flourished under such a man as Lagrange seemed

to have entirely lost its potency when transferred to the professors and tutors of Cambridge.

Still directing our observations to Cambridge, as the centre and focus of men of science, nothing can be more true than the fact, that her mathematicians have deteriorated as these new and fashionable methods of analytics have gained currency.<sup>1</sup>

The reviewer went on to say that none of the Cambridge mathematicians could compare favorably with Vince or Isaac Milner (1751-1820), Senior Wrangler in 1774, professor of natural philosophy, master of Queen's College, and Dean of Carlisle. He stated that his sentiments regarding the professors and tutors were applicable to the students: "They are products of a hot-bed, with all the precocity, and all the delicacy and weakness, which belong to such a mode of cultivation."<sup>2</sup> He also lamented the lack of mathematical literacy among the students. By mathematical literacy, he meant that the students no longer read the standard mathematical works which had been in use since the time of Newton.

The reviewer's comments were supported by Whewell who, alone among the reformers, held misgivings about the value of the changes. He maintained that the exclusion of mathematical

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<sup>1</sup>Ibid., p. 131.

<sup>2</sup>Ibid., p. 131.

applications and geometrical reasoning in preference to analytical reasoning was particularly detrimental from a pedagogical point of view. To place Whewell's remarks in proper perspective, it should be noted that his comments about the reform in 1817 and those made in the eighteen forties, after many years of experience as a professor,<sup>1</sup> were very consistent. Also, Whewell's mathematical contributions were primarily in the field of applied mathematics, so that his ideas represent a different point of view than those of Herschel, Babbage, and Peacock who were primarily pure or theoretical mathematicians. Whewell, in a manner reminiscent of his letter to Herschel in 1817, wrote that abstract manipulation of symbols and equations did not contribute to the development of the mind and did "not possess that value as instruments of an exact and extensive discipline of reason, which the geometrical branches of Mathematics

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<sup>1</sup> William Whewell, Of a Liberal Education in General: and with Particular Reference to the Leading Studies of the University of Cambridge (2d. ed., London: John W. Parker, 1850). This book is divided into three parts with the first two parts paged separately, therefore each section will be referred to under its own pagination: Part I. Principles and Recent History, pp. 167-236; Part II. Discussions and Recent History, pp. 1-34; and Appendix, pp. 85-144. It should be noted that Whewell's first edition Of a Liberal Education in General was written in 1845. His practice was to add to an edition, alter it slightly and republish it several times. This book in particular shows evidence of having been added on to as one might add on to a building. It is therefore difficult to determine exactly the time period when he first made his remarks.

do possess."<sup>1</sup> He maintained that analytical operations in mathematics did not "discipline the reason" because they did not familiarize the student

with a chain of syllogisms connected by a manifest necessity at every link: they do not show that many kinds of subjects may be held by such chains: and at the same time, that the possibility of so reasoning on any subject must depend upon our conceiving the subject so distinctly as to be able to lay down axiomatic principles as the basis of our reasoning.<sup>2</sup>

Whewell was concerned with what he called permanent educational studies which were a part of every university student's formal education. "Our education," he stated, "would be very imperfect without Mathematics, or some substitute for that element; but mere analytical mathematics does not remedy the imperfection."<sup>3</sup> He was ready to substitute "Logic or Jurisprudence" if he were forced to contend only with analytical mathematics. He also lamented the fact that the "present" students were ignorant of ancient thinkers in mathematics and therefore the "historical character" of branches of mathematics were obscured, with the consequence being that the

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<sup>1</sup> Whewell, Of a Liberal Education in General, Pt. I, p. 49. Whewell had supported the changes advocated by the Analytical Society, but in his letter above to Herschel, dated March 6, 1817, he displayed his cautious nature and his concern for maintaining a link between mathematics and physical reality.

<sup>2</sup> Ibid., p. 49.

<sup>3</sup> Ibid., p. 50.

"value of a mathematical education" was greatly impaired by "putting out of sight" the peculiar conceptions and terms the "original mathematical explorers" had used by proceeding in geometrical terms. As he had done in a letter to Herschel in 1818, Whewell again lamented the neglect of Newton's Principia with its geometrical mode of proof.<sup>1</sup> The geometrical student, he felt, could better apply his knowledge to the "calculation of practical results and the solution of problems" because he was able to proceed step by step and to "shape his course accordingly."<sup>2</sup> In his most severe criticism, Whewell stated that he had no doubt that in any application of geometrical, mechanical, or hydrostatical principles to a problem of moderate difficulty, new to both the geometrically trained student and the analytically trained student, the former would be at a definite advantage in the solution of the problem.<sup>3</sup>

In contrast to the remarks by Whewell in Of a Liberal Education in General,<sup>4</sup> the following remarks by Peacock in 1833, were made in

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<sup>1</sup> In a letter to Herschel, dated November 1, 1818, Whewell stated that he would not be surprised "if in a short time we were only to read a few propositions of Newton as a matter of curiosity; it would however as yet be treason to breathe such an idea to most people," Todhunter, William Whewell, p. 30.

<sup>2</sup> Ibid., p. 30.

<sup>3</sup> Whewell, Of a Liberal Education in General, pp. 55-63.

<sup>4</sup> Whewell dedicated his book Of a Liberal Education in General, p. 1, to George Biddell Airy (1801-1892), Astronomer Royal, stating

reference to the impact made by Woodhouse on the mathematical studies at the University of Cambridge when he, Woodhouse, published his book Plane and Spherical Trigonometry in 1810. Peacock stated that this book by Woodhouse, more than any other work, "contributed to revolutionize the mathematical studies of this country."<sup>1</sup> Commenting in greater detail Peacock noted:

The circumstances attending the publication and reception of this work in the University of Cambridge were sufficiently remarkable. It was opposed and stigmatized by many older members, as tending to produce a dangerous innovation in the existing course of academical studies, and to subvert the prevalent taste for the geometrical form of conducting investigations and of exhibiting results which had been adopted by Newton in the greatest of his works, and which it became us, therefore, from a regard to the national honour and our own, to maintain unaltered.<sup>2</sup>

The innovation which Woodhouse introduced and which the Analytical Society adopted was the use of algebra rather than geometry in the calculus. Such an innovation was in fact a threat to those who regarded the use of geometry as a means of disciplining the mind as

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in part that, "I will further add, as another reason for offering my Reflections to you, that with regard to the kind of Mathematical Studies most suited to the University, and especially with regard to the destructive effect of mere analysis upon the mind, I know that your views agree with my own."

<sup>1</sup> George Peacock, "Report on the Recent Progress and Present State of Certain Branches of Analysis," Report of the Third Meeting of the British Association for the Advancement of Science: Held at Cambridge in 1833, III (1834), 295.

<sup>2</sup> Ibid., p. 296.

well as a mathematical method. Peacock did not concur with this point of view.

It was contended, also that the primary object of academical education, namely, the severe cultivation and discipline of the mind, was more effectually attained by geometrical than by analytical studies, in which the objects of our reasoning are less definite and tangible, and where the processes of demonstration are much less logical and complete. The opposition, however, to this change, though urged with considerable violence, experienced the ordinary fate of attempts made to resist the inevitable progress of knowledge and the increased wants and improving spirit of the age. In the course of a few years the work in question was universally adopted.<sup>1</sup>

By way of comparison, Whewell's position was representative of the thinking concerning the nature of mathematical studies at the beginning of the nineteenth century. It may have been that his expressed disapproval of Peacock's methods of introducing differential methods and notation, in his letter to Herschel in 1818, was in reality his true conviction toward the movement in general and not just toward the manner in which Peacock chose to carry out the reform.<sup>2</sup>

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<sup>1</sup>Ibid., p. 296.

<sup>2</sup>Whewell, in his letter to Herschel dated November 1, 1818, noted that in his post as assistant tutor at Trinity College he would have a "permanent and official interest" in directing the reform. He also noted that "Our moderators are Gwatkin and Peacock; in G. I have great confidence, he is reasonable, and he has been reading a good deal of good mathematics," Todhunter, William Whewell, pp. 30-31.



A much later evaluation of the reform was given by James Whitbread Lee Glaisher (1848-1928), who graduated in 1871 from Trinity College, Cambridge as second Wrangler. He was named a fellow and assistant tutor at Trinity and in 1886 became President of the London Mathematical Society. His evaluation is important because he was in a sense a product of the changes brought about by the reform and because his historical analysis of the mathematical tripos examination offers a basis for comparison with that of Whewell's in Of a Liberal Education in General. A historical discussion of the mathematical tripos necessarily involves the moderators who were responsible for administering the examination. Since both Peacock and Whewell were moderators, Glaisher's comments included a discussion of the moderators who were also members of the Analytical Society. In contrast to Whewell, he definitely looked back to the reform with favor. He noted that it was to Herschel, Peacock, and Babbage that "we mainly owe the revival of mathematics in this country and the restoration of intercourse with the rest of Europe after three-quarters of a century of isolation."<sup>1</sup> Glaisher also pointed out that the introduction of the differential notation and analytical methods and processes were a "landmark in the history of

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<sup>1</sup> James Whitbread Lee Glaisher, "The Mathematical Tripos," Proceedings of the London Mathematical Society, XVIII (November, 1886), 18-19.

Cambridge mathematics" and made possible a substantial increase in the range of subjects included in the course of study.<sup>1</sup> He also gave credit to books which influenced the advances made in applied mathematics including the publication of Whewell's "Mechanics" (1819), Whewell's "Dynamics" (1823), Coddington's "Optics" (1823), Woodhouse's "Plane Astronomy" (1821-23), and Airy's "Tracts" (1826).<sup>2</sup>

While Woodhouse began the reform movement which was carried out by the members of the Analytical Society, he did not participate in any activities of the Society. According to Whewell, Woodhouse did think that the first two decades of the nineteenth century were an "epoch period in mathematics," but added that "W. is known to have no liking for ultra-analysts."<sup>3</sup> In fact, in 1819 when Babbage applied for the professorship of mathematics at the University of Edinburgh he received no help from Woodhouse. Peacock, who was helping Babbage, wrote that "Woodhouse is a stingy fellow and will sign for no person with whom he is not personally

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<sup>1</sup> Ibid., p. 19.

<sup>2</sup> Ibid., p. 21.

<sup>3</sup> Letter, William Whewell to John Herschel, November 1, 1818, in Todhunter, William Whewell, pp. 29-30.

acquainted."<sup>1</sup> The attitude which Woodhouse displayed toward the Analytical Society members does not detract from his role in the reform, but it is doubtful that the reform would have been accomplished as rapidly without a group effort.

In conclusion, the reform initiated by Woodhouse and completed by members of the Analytical Society was successful in introducing Continental notation and methods of analysis into England. As noted, Whewell objected to the exclusion of geometry and applications of analysis to physical phenomena and thus added his own works on these subjects. The reviewers of the mathematical works of the Analytical Society members generally agreed with Whewell's point of view and thereby provided the Society with a valuable critique of the changes which had been advocated and effected. Two very important projects which helped to insure the success of the reform were the translation of Lacroix's calculus text and the writing of accompanying books of problems with solutions by Babbage, Herschel, and Peacock. Peacock was able to nurture the reform through his position of moderator of the annual Senate House examinations because he was able to control the content of the mathematics taught at Cambridge by including problems using Continental methods and notation. Lastly,

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<sup>1</sup>Letter, George Peacock to Charles Babbage, November 10, 1819, B.M. Add MSS 37182, I, 177.

the influence and prestige which the members of the Analytical Society gained through their mathematical works helped them to secure positions of influence which in turn enabled them to maintain and control the reform over a long period of time.

### III. THE EXTERNAL COTERIE--A LIBERAL SCIENTIFIC PARTY WITH ELITIST VIEWS

In 1830 Babbage published Reflections on the Decline of Science in England: and on Some of Its Causes<sup>1</sup> in which he maintained that science had long been neglected and declining in England. Babbage claimed that his opinions concerning the state of science were supported by higher authority than his own--Sir Humphrey Davy (1778-1827), the late President of the Royal Society, and John Herschel both supported his position.<sup>2</sup> Davy had begun a book on the decline of science shortly before his death.<sup>3</sup> Herschel had also deplored the neglect of science and had stated that:

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<sup>1</sup>(London: B. Fellowes, 1830).

<sup>2</sup>Ibid., pp. v-ix.

<sup>3</sup>Charles Richard Weld, in History of the Royal Society, With Memoirs of the Presidents, Compiled from Authentic Documents, (Vol. II; London: John W. Parker, 1843), pp. 331-332, stated that Davy grew disenchanted with the government because it gave him no support and was indifferent toward matters of science except when it needed some specific service which only the Royal Society could fulfill. Once the service was extended the government promptly forgot about science. Cf. "British Association for the Advancement of Science," review of First Report of the Proceedings, Recommendations, and Transactions of the British Association for the Advancement of Science. York: 1832; The British Association for the Advancement of Science, 1850, in the Palladium, No. 11, pp. 194-215, September, 1850, both in The North British Review, XIV (November, 1850), pp. 238-239. The anonymous reviewer stated that Sir Humphrey Davy's book on the decline of science was "full of feeling and eloquence, which his executors have not deemed it proper to publish."

whole branches of continental discovery are unstudied, and indeed almost unknown by name. It is in vain to conceal the melancholy truth. We are dropping behind. In mathematics we have long since drawn rein, and given over a hapless race. In chemistry the case is not much better. . . . . There are indeed, few sciences which would not furnish matter for a similar remark.<sup>1</sup>

Thus Babbage, Herschel, and Davy claimed that science in general was on the decline. Babbage and Herschel placed much blame for the decline upon the leadership of the Royal Society. They maintained that the Presidents and Councils of the Society, by not attempting to obtain governmental support for science, had not worked to promote the interests of the Fellows of the Society and of science. In particular, Babbage maintained that the Presidents of the Society had picked their own Councils with the result that the Council members were subservient to the President. The President and his Council therefore constituted what Babbage called a "party" or "coterie."<sup>2</sup> Babbage thought that if the Royal Society were to be an effective organization, then its primary function should be to maintain liason between the individual scientist and the government.

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<sup>1</sup> John F. W. Herschel, "Sound, Part I of the Propagation of Sound in General," in Encyclopedia Metropolitana; or Universal Dictionary of Knowledge, On an Original Plan: Comprising the Two-fold Advantage of a Philosophical and an Alphabetical Arrangement, with Appropriate Engravings, IV (1845), 810. The date of Herschel's remarks in above passage would have been 1830 or before since Babbage quoted them in Reflections, pp. vii-ix.

<sup>2</sup> Babbage, Reflections, p. 140.

However, it was his opinion that the Presidents and the Councils of the Society had not been interested in or were ineffective in procuring governmental support of science.<sup>1</sup>

While it was Babbage's opinion that the leadership of the Royal Society was responsible for procuring governmental support for science, he did not excuse governmental officials from responsibility for science. Both groups were responsible for the decline of science in England. Babbage had the support of others who shared his views besides Herschel and Davy. He had the support of George Peacock and Edward Ffrench Bromhead, who had been members of the Analytical Society and who had participated in the reform of mathematics in England. He was also supported by several other friends who were concerned about the state of science in England. These friends included: Francis Baily (1774-1844), a successful stockbroker and an astronomer; William Henry Fitton (1780-1861), a geologist; James South (1785-1867), an astronomer; and David Brewster (1781-1868), a scientist, editor of the Edinburgh Journal of Science, and Vice Chancellor of the University of Edinburgh. Brewster's primary contribution to the group was to broaden the scope of its concern for science in Scotland and Ireland as well. Brewster did this through his writings in the Edinburgh Journal of

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<sup>1</sup>Ibid., pp. 146-151.

Science and particularly in the Quarterly Review.<sup>1</sup>

Throughout the eighteen twenties and thirties, Babbage and his friends initiated and effected a series of reforms which were generally external to and in opposition to the officially elected leaders of the Royal Society--the Presidents and Council members. Thus the Babbage group constituted an external coterie or party which was in opposition to the coterie which governed the Royal Society; but the reference to the Babbage group as an external coterie is not meant to imply that it was formally organized as an opposition party to the leadership of the Royal Society.

The group was an external coterie in the sense that it represented an emerging generation of young scientists who were still outside the center of power of the scientific community which was represented by the President and Council of the Royal Society. As young scientists, the members of the external coterie were eager for an active and influential role in the affairs of the scientific community. The ethos of the external coterie was, in general, well represented by the mixture of liberal and elitist views held by Babbage and Bromhead. Both men thought that scientists were an

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<sup>1</sup>[David Brewster], "Decline of Science in England," review of Reflections on the Decline of Science in England, and on Some of Its Causes, by Charles Babbage, Esq., Lucasian Professor of Mathematics in the University of Cambridge, and Member of Several Academies (London: B. Fellowes, 1830), in The Quarterly Review, XLIII, No. 86 (1830), 321-331.



elite group and should therefore hold positions of influence in governmental as well as scientific circles. Babbage and Bromhead wanted the Fellows of the Society to have a greater share in the selection of the Council members and a greater participation in the affairs of the Society as a whole. Their desire to extend the influence of the Fellows in the affairs of the Council and the Society as a whole was analogous to the attempts of the liberals of the day to extend the voting franchise to a greater proportion of the citizenry--this latter movement resulted in passage of the Reform Bill of 1832.<sup>1</sup> It is significant, therefore, that Babbage was in favor of extending the voting franchise both in the case of the Royal Society and in the case of the general citizenry. It is very probable that the entire Babbage group was influenced by the general agitation for reform in the political arena which took place in Great Britain during the nineteenth century.<sup>2</sup> Babbage published a work<sup>3</sup> in 1833 which reflected an integration of his ideas toward the reform of the Royal Society and the

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<sup>1</sup> Thomas Babington Macaulay, "Parliamentary Reform," The House of Commons, March 2, 1831, Speeches of Lord Macaulay (London: Longmans Green & Co., 1875), in The English Reform Tradition: 1790-1910, ed. by Sydney W. Jackman, (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1965), pp. 52-67.

<sup>2</sup> Sophia Elizabeth DeMorgan, Memoir of Augustus DeMorgan, p. 42.

<sup>3</sup> Charles Babbage, A Word to the Wise (London: R. Clay, 1833), pp. 2-16.

extension of the voting franchise for a larger portion of the general citizenry with respect to the political affairs of the nation. From the letters written by Bromhead to Babbage, as early as 1820, it is clear that both men entertained ideas for reforming the Royal Society; that both men had a mixture of liberal and elitist ideas which were generally characteristic of the ideas included in the Reform Bill of 1832; and that both men felt that the responsibility for reforming science and the Royal Society belonged to their own circle of friends. For example, Bromhead wrote to Babbage, on June 26, 1820,<sup>1</sup> regarding the choice of a successor for Sir Joseph Banks (1744-1820), who had resigned as President of the Royal Society on May 18, 1820 because of ill health. Bromhead was interested in more than just electing a President. He was interested in total reform of the Society:

I am sure Wollaston<sup>2</sup> will not succeed, and if we cannot carry a friend of our own, our policy is to turn the scale, and at least avoid making an Enemy of the future president. If we cannot succeed now, we should not shut out ourselves from the vacancy after the present.

The Royal Society wants revolutionizing. The mode of choosing the Council is radically bad. The Majority is

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<sup>1</sup>Letter, Edward French Bromhead to Charles Babbage, June 26, 1820, B.M. Add. MSS. 37182, I, 270.

<sup>2</sup>William Hyde Wollaston (1776-1828), M.D. and physicist, became the President of the Royal Society for the five month period from June 29, 1820 to November 29, 1820. Wollaston was elected to fill Sir Joseph Banks' remaining term of office. He voluntarily stepped aside in deference to Sir Humphrey Davy.

every thing, whereas I say, that a minority forming an mth part of Society have a right to appoint an mth part of the Council. The means of doing this are too long to explain. The effect of the present plan is what universal suffrage would be in parliament; the labouring class instead of sending their share of members would send all, by being the majority.<sup>1</sup>

In the passage above, Bromhead made reference to the representation of the opinions of the minority in the affairs of the Royal Society. Both Bromhead and Babbage looked upon their circle of friends as an elite group of young scientists. They wanted the views of the external coterie represented in the affairs of the Society even though it was a minority group in 1820. Bromhead believed with Babbage that the scientific elite should be represented in government and that there should exist an Order of Bath which extended to civil and literary merit.<sup>2</sup> Their contention was that the Order of Guelph, which had traditionally been used to provide honors for such groups as politicians and military men, was too general and too politically tainted to provide any real honor or recognition for those who had distinguished themselves in fields such as science, literature, engineering, trade, and commerce. Babbage was particularly critical of governmental and Royal Society leaders because there were

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<sup>1</sup>Letter, Edward Ffrench Bromhead to Charles Babbage, June 26, 1820, B.M. Add MSS 37182, I, 270.

<sup>2</sup>Letter, Edward Ffrench Bromhead to Charles Babbage, November 22, 1830, B.M. Add MSS 37185, IV, 351.

very few professional opportunities for scientists to serve in governmental and private positions. He maintained that science therefore received very little monetary support from governmental or private sources because there were few opportunities for scientists to promote the interests of science from within government or industry. Science was primarily an activity pursued by the rich or the dedicated person who sacrificed financial security for himself and his family for the sake of science. Thus with the creation of an Order of Bath, Babbage argued that the scientific intellectual would become part of an aristocracy which would replace the hereditary peerage in the House of Lords. Babbage stated that an aristocracy based upon hereditary peerage suffered from "having little responsibility," from the "absence of objects of ambition to be acquired by knowledge or by virtue," and from "that insatiable desire for political power, which has led to acts of the most disgraceful corruption-- acts to which public opinion yet hesitates for awhile to apply the deserved epithet of infamous."<sup>1</sup> Babbage admitted that there were exceptions to his description of the hereditary aristocracy, but their contributions were very small in comparison to other classes of society:

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<sup>1</sup> Babbage, A Word to the Wise, p. 6.

During the last half-century the various classes of society in England have made very great, but very unequal, advances in knowledge and in power. Large branches of manufacturers have been created, which have given wealth to a body of capitalists possessed not only of great technical skill, but also of general intelligence of an high order, and a population has been called into existence, which depends upon those capitalists for its immediate support. Competition acting upon persons engaged in professions, as well as on those employed in manufactures and commerce, has produced a demand for information upon all subjects, which has given a stimulus to the universal mind of the country.<sup>1</sup>

In order to provide representation in Parliament for the body of wealthy capitalists who possessed scientific and technical skill, Babbage proposed that the Order of Bath be conferred upon those who had excelled in civil (including scientific) and literary merit.<sup>2</sup> Such a peerage would be conferred upon the individual for life only. The life peers would compose, as under the hereditary peerage, the upper chamber of Parliament and would in effect give political power to a scientifically educated elite:

One of the main uses of an Upper Chamber in a popular government, is to give consistency and uniformity to the more fluctuating opinion of the immediate representatives of the people. It is to the political what the fly-wheel is to the mechanical engine. It ought to represent the average, but not the extreme opinions of the people. For this purpose it is necessary that none but

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<sup>1</sup> Ibid., pp. 4-5.

<sup>2</sup> Letter, Charles Babbage to Bery Delessert, October 17, 1831, B.M. Add MSS 37186, V, 126.

persons duly qualified should have seats in the House of Lords: Peers should be elected for life:--the Peerage should not be hereditary.<sup>1</sup>

Just as Babbage reasoned that the House of Lords was the "fly-wheel" of government, he likewise reasoned that the Royal Society should play a prominent role in promoting and stabilizing the interests of science.<sup>2</sup> With a strong Royal Society and a government made up of scientific and technically oriented persons, Great Britain would be able to create an ideal environment in which knowledge would be pursued with an enthusiasm such as that which Babbage had observed on the Continent in 1828 at the annual meeting of the Congress of Philosophers in Berlin.<sup>3</sup> He stated that the existence of a "large society of cultivators of the natural sciences" which met annually in some great capital or central town of the Continent was a "circumstance almost unknown to us, and deserving of our attention, from the important advantages which may arise from it."<sup>4</sup>

It is not surprising that Babbage and Bromhead and their external coterie met with resistance from the leadership of the Royal

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<sup>1</sup> Babbage, A Word to the Wise, pp. 6-7.

<sup>2</sup> Babbage, Reflections, p. xiii.

<sup>3</sup> Charles Babbage, "Account of the Great Congress of Philosophers at Berlin on the 18th September 1828," Edinburgh Journal of Science, X (April, 1829), 225.

<sup>4</sup> Ibid., p. 225.

Society. Their zeal for reform, their attitude toward the aristocracy, the government, and the Royal Society leadership, and their contention that the British should emulate their Continental neighbors was enough to stir up a reaction. In fact, Babbage and his party encountered resistance almost at once with the leadership of the Royal Society under Sir Joseph Banks. Their initial efforts to promote science were thwarted by Banks. He gave them little hope that the Society would lend its prestige to promote science through an effective liaison with the government or through imaginative leadership. Banks, for example, opposed any attempt to divert the affairs of the Society and thus dilute the prestige and unity of the Royal Society by the formation of organizations which would promote a particular scientific discipline. In 1819, Babbage and his friends combined their efforts to form two new societies, with the result that their actions incurred the displeasure of Banks. One of these, the Cambridge Philosophical Society, was begun at the urging of Peacock, with the help of William Whewell, in November of 1819.<sup>1</sup> This society was in reality a continuation and extension of the Analytical Society.<sup>2</sup> The Society was "intended to be as comprehensive as is consistent

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<sup>1</sup> Letter, George Peacock to Charles Babbage, November 10, 1819, B.M. Add MSS 37182, I, 177.

<sup>2</sup> Letter, Edward Ffrench Bromhead to Charles Babbage, March 3, 1821, B.M. Add MSS 37182, I, 323.

with its name: comprehending pure analysis, natural history, geology, and curious points of circular geography."<sup>1</sup> Those who desired the favor of Sir Joseph Banks were reluctant to associate their names with it. Bromhead wrote to Babbage that "Woodhouse has left the Cam. Phil. Soc. in the lurch, I suppose thro fear of Sir Joseph."<sup>2</sup>

Babbage, John Herschel, and Francis Baily took the initiative in the foundation of the other society. They began in December of 1819 to gather support for the formation of the Astronomical Society of London.<sup>3</sup> The goal of this Society was "the encouragement and promotion of astronomical science."<sup>4</sup> A meeting was held on January 12, 1820 and a series of procedures and rules were drawn up to govern the proceedings of the Society. The Astronomical Society was also

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<sup>1</sup> Letter, George Peacock to Charles Babbage, November 10, 1819, B.M. Add MSS 37182, I, 177.

<sup>2</sup> Letter, Edward Ffrench Bromhead to Charles Babbage, January 19, 1820, B.M. Add MSS 37182, I, 201. Bromhead referred to Robert Woodhouse.

<sup>3</sup> "Minutes of the General Meetings of the Astronomical Society of London, Wednesday, January 12, 1820," in History of the Royal Astronomical Society 1820-1920, ed. by J.L.E. Dreyer and H.H. Turner (London: Royal Astronomical Society, 1923), p. 2.

<sup>4</sup> Letter, Edward Adolphus Seymour, eleventh Duke of Somerset to the Council of the Astronomical Society of London, March 9, 1820, in ibid., p. 9.



opposed by Banks. Edward Adolphus Seymour, eleventh Duke of Somerset (1775-1855) was asked to accept the nomination as the first President of the Society. He accepted and was elected, but he immediately resigned because of his friendship with Banks who considered the Presidency of the Astronomical Society a "hostile position."<sup>1</sup> Banks also tried to persuade John Pond (1776-1836), the Astronomer Royal, to have nothing to do with the Society but Pond "made a spirited reply" in expressing his independence of Banks.<sup>2</sup> It was Banks' opinion that all English scientific organizations should be affiliated with but subordinate to the Royal Society.

Babbage's participation in the founding of the Astronomical Society had an adverse effect upon his earning a living as a professional scientist. He had applied for a position on the Board of Longitude in 1819 and had mentioned to Banks that he wished to secure this position. When he was not successful, Banks gave him assurance that he might be more successful at some future date. When another vacancy occurred on the Board in 1820, Babbage again called upon Banks to ask him to use his influence with the Admiralty to help

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<sup>1</sup> Letter, Edward Adolphus Seymour, eleventh Duke of Somerset, to the Council of the Astronomical Society of London, March 9, 1820, ibid., p. 9.

<sup>2</sup> Letter, Francis Bailey to Charles Babbage, March 11, 1830, B.M. Add MSS 37182, I, 237.

secure this position. Banks refused to lend his support to Babbage giving as a reason the part Babbage had taken in the formation of the Astronomical Society.<sup>1</sup>

Banks' refusal to support Babbage for a position on the Board of Longitude because of his part in forming the Astronomical Society was closely related to a reform movement undertaken by the external coterie from about 1819 to 1830--the reform of the Nautical Almanac and Astronomical Ephemeris. This reform movement was particularly important to the decline of science issue which Babbage brought to the attention of the scientific world with the publication of Reflections in 1830. In Reflections, Babbage devoted much attention to the functions of the Board of Longitude because he thought that it had had the potential to become the most important scientific body of its kind in Great Britain.<sup>2</sup> The Board was important because it was a source of governmental patronage to science and was viewed by the external coterie as an agency through which scientific reform could be effected. For example, Bromhead had written to Babbage in 1819 and had suggested that they ask Banks to name twenty of the best mathematicians as honorary members to the Board of Longitude to edit and make suggestions regarding future publications of the

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<sup>1</sup> Babbage, Passages, p. 474.

<sup>2</sup> Babbage, Reflections, pp. 66-100.

Nautical Almanac. Bromhead thought that such a practice would "forcibly turn our mathematics to useful purposes, excite emulation, &c, &c."<sup>1</sup> Any hope that Bromhead entertained for persuading Banks to use the talents of the best mathematicians for the Board of Longitude vanished because of the activities of the members of the external coterie in forming societies hostile to the Royal Society. Bromhead had implied that he considered the members of the party among the twenty best mathematicians, but Banks held the power in the appointment of persons to scientific positions.<sup>2</sup> The Admiralty made the actual appointments to the Board of Longitude, but a recommendation by Banks was necessary for a scientist to have any hope of success.<sup>3</sup>

The Nautical Almanac had become a symbol of national scientific prestige through the efforts of Nevil Maskelyne (1732-1811) the fifth Astronomer Royal. Maskelyne published the first Almanac in 1767 as an extension of his very successful British Mariners'

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<sup>1</sup> Letter, Edward Ffrench Bromhead to Charles Babbage, May 7, 1819, B. M. Add MSS 37182, I, 115.

<sup>2</sup> Babbage, Reflections, p. 150.

<sup>3</sup> Dreyer, ed., History of the Royal Astronomical Society, p. 15. The Board of Longitude of 1820 consisted of the following members: J. W. Croker, F.R.S., Joseph Banks, P.R.S., Davies Gilbert, F.R.S., Robert Woodhouse, F.R.S. Cambridge, John Pond, Astronomer Royal and F.R.S., A. Robertson, S.P. Rigaud, F.R.S., Isaac Milner, Samuel Vince, F.R.S., W. Lax, F.R.S., William H. Wollaston, F.R.S., W. Mudge, F.R.S., and Thomas Young, F.R.S.

Guide.<sup>1</sup> The Board of Longitude then assumed the overall responsibility for the Almanac and appointed Maskelyne as editor of it. In the preface of the first Almanac, Maskelyne gave a general statement as to the intent of the publication:

The Commissioners of Longitude, in pursuance of the powers vested in them by a late act of Parliament, present to the public with the Nautical Almanac and Astronomical Ephemeris for the year 1767, to be continued annually, a work which must greatly contribute to the improvement of Astronomy, Geography, and Navigation. This Ephemeris contains every thing essential to general use that is to be found in any Ephemeris hitherto published, with many other useful particulars never yet offered to the public in any work of the kind.<sup>2</sup>

In the above passage, Maskelyne clearly stated that the Almanac was intended for the improvement of astronomy, geography, and navigation. He was so successful in establishing a useful, comprehensive and accurate Almanac that it became the foremost work of its kind in Europe.<sup>3</sup> When Maskelyne died in 1811, John Pond became the sixth

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<sup>1</sup> H[arry] W[alter] Maunder, The Royal Observatory Greenwich: A Glance at Its History and Work (London: The Religious Tract Society, 1900), pp. 90-92.

<sup>2</sup> Nevil Maskelyne made this statement in the preface to the first Almanac in 1767 as quoted in James South, Refutation of the Numerous Misstatements and Fallacies Contained in a Paper Presented to the Admiralty by Dr. Thomas Young, (Superintendent of the Nautical Almanac), and Presented by Order of the House of Commons: Dated 17th March, 1829 (London: J. Moyes, 1829), pp. 5-6.

<sup>3</sup> James South, Practical Observations on the Nautical Almanac and Astronomical Ephemeris: Arguments Proving that It Was Not Originally Designed for the Sole Improvement of Nautical Astronomy;

Astronomer Royal, but he did not take much interest in the Nautical Almanac even though he was the editor of it. Pond adopted the practice of sending the observations which he had made to persons called computers. Their computations were then sent to a "comparer" who was the last person to see the computations prior to the actual printing of the Nautical Almanac. The comparer was not legally responsible for any errors with the result that "errors abounded in what ought to be especially, and above all other productions, void of error."<sup>1</sup>

In 1818, the Board of Longitude was recognized at the urging of John Wilson Croker (1789-1857), F.R.S., member of Parliament, and Secretary to the Admiralty.<sup>2</sup> The responsibility for the Nautical Almanac was removed from the office of the Astronomer Royal, and Thomas Young (1773-1829), M.D., who was noted for his work in optics and in deciphering hieroglyphics, was named as Secretary to

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Reasons for Extending the Information Which It Annually Presents: Together With an Inquiry, If Before We Can Honestly Assign to It "Its Pristine Excellence," Some Improvement of It Is Not Indispensable (London: William Sams, 1822), p. 1.

<sup>1</sup> John Wilson Croker in a speech to the House of Commons as quoted in The Courier, March 7, 1818, quoted in James South, Reply to a Letter in the Morning Chronicle Relative to the Interest Which the British Government Evinces in the Promotion of Astronomical Science (London: Richard Taylor, 1829), p. 29.

<sup>2</sup> Ibid., p. 28.

the Board of Longitude and Superintendent of the Nautical Almanac.

Young had had no experience with either navigation or astronomy to qualify him for the position. This lack of experience put him in a disadvantageous position which he might have overcome except for a very stubborn nature and an unwillingness to listen to advice, especially from astronomers.<sup>1</sup> Young decided that the Nautical Almanac was intended primarily for navigation and only incidentally for astronomy.<sup>2</sup> He contended that if the Almanac included everything desired by astronomers it would become too complicated for use by the ordinary seaman.<sup>3</sup> He justified his decision by quoting a selected portion of the Maskelyne preface to the first Almanac:

Dr. Maskelyne's first preface declares, that the 'Ephemeris was made to remove the difficulty and length of calculations necessary to determine longitude at sea.' Any advantage that might be derived from it by astronomers on shore was certainly considered desirable, but at the same time as subordinate to its nautical utility, the cultivation

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<sup>1</sup> South, Refutation, p. 13.

<sup>2</sup> Thomas Young, "Report," in Refutation, by James South, p. 5. These are the earliest published comments by Young with regard to the Almanac, to be found after a very thorough search. South's work contains the "Memorandum" of a report made to the Chancellor of the Exchequer on January 28, 1828 relative to the expediency of reforming the Nautical Almanac; and a "Report on a Memorandum of a Plan for Reforming the Nautical Almanac," by Thomas Young. Young attempted to refute the charges contained in the Memorandum. South answered Young's arguments by interspersing his comments following each of those made by Young.

<sup>3</sup> Ibid., p. 64.

of abstract science being obviously of far less importance than the preservation of lives and property of seafaring persons. It follows, of course, that there must always be room for discretion with respect to the degree in which subjects purely astronomical should be admitted into a Nautical Ephemeris.<sup>1</sup>

While it was Young's opinion that the Almanac was intended primarily for nautical purposes and only secondarily for astronomers, he was clearly departing from another of Maskelyne's stated intentions. Maskelyne had stated, as noted above, that the Almanac was intended to "greatly contribute to the improvement of Astronomy, Geography, and Navigation."<sup>2</sup> Young was in reality exercising his own prerogatives as Superintendent of the Almanac in adopting the policy of restricting its contents to those items which were useful primarily to seamen. The sentence from the preface of the first Almanac which he used to justify his policy was selected out of context because Maskelyne was merely providing some examples to support his general statement as to the purpose of the Nautical Almanac and Astronomical Ephemeris.

Young was not long in office as Secretary of the Board before he began to receive criticism. In 1819 Francis Baily wrote that the Board of Longitude had the power and financial means to enlarge the original plan and design of the Nautical Almanac by including the

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<sup>1</sup>Ibid., p. 6.

<sup>2</sup>Ibid., p. 5.

features found in the almanacs published in Paris, Berlin, Vienna and other places. Such a step would "tend to retrieve the character of the work, and redound to the honour of the country."<sup>1</sup> The Board of Longitude appropriated £4000 per annum to spend for scientific purposes, a sum which Bailey had hoped would give a new impulse to the science of astronomy. He saw no reason why the Board should not take steps to encourage and direct the attention of the scientific world to particular improvements that should be made in the Almanac rather than "wait for proposals on every subject (useful or visionary) that may be laid before them."<sup>2</sup> Such encouragement, he argued, should have provided for the publication of specific rewards for the formulation of new tables of the sun, moon or any of the planets; for the best essays on practical or theoretical astronomy; for improvement in timepieces, telescopes, micrometers, or other astronomical instruments; for the best engravings of any portion of the celestial sphere; for reducing the observations of any celebrated astronomer; for the discovery of any new planet, comet, fixed star, or other

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<sup>1</sup> A. Cagnoli, Memoir on a New and Certain Method of Ascertaining the Figure of the Earth by Means of Occultations of the Fixed Stars: With Notes and an Appendix by Francis Baily (London: Richard and Arthur Taylor, 1819), p. 34. This Memoir originally appeared in Memorie di Matematica e di Fisica della Societa Italiana. Tom. vi. Verona 1792, and was translated by Baily.

<sup>2</sup> Ibid., pp. 34-35.



remarkable celestial phenomena; for the translation of any valuable astronomical treatise into the English language; and, in general, for any other object which may be "useful to science and which may deserve encouragement." <sup>1</sup> Thus Baily wanted a thorough reform of the Nautical Almanac by the Board of Longitude. He also suggested the formation of an Astronomical Society to encourage "the present contracted state" <sup>2</sup> of the science of astronomy. When Young failed to incorporate any of Baily's suggestions into the Almanac, and when he failed to urge the Board to adopt procedures which would both involve and encourage the participation of Astronomers, Baily reacted by getting the newly formed Astronomical Society of London to adopt the procedures listed above for rewarding persons who made contributions to the science of Astronomy. For example, Babbage received the Astronomical Society's first Gold Medal for his Difference Engine No. <sup>3</sup> which was designed to automatically calculate and print astronomical and mathematical tables free of errors. The number of errors to be found in the Almanac was one very important

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<sup>1</sup> Ibid., pp. 34-35.

<sup>2</sup> Ibid., p. 30.

<sup>3</sup> Henry Thomas Colebrooke, "Address of Henry Thomas Colebrooke, Esq., F.R.S., President of the Astronomical Society of London, On Presenting the Gold Medal to Charles Babbage, Esq., F.R.S., Awarded 13th July, 1823," in Babbage's Calculating Engines: Being a Collection of Papers Relating to Them; Their History, and Construction, ed. by Henry P. Babbage, (London: E. and F.N. Spon, 1889), pp. 223-224.

reason why it was scorned by astronomers.

Young continued to ignore any suggestions for changes in the Nautical Almanac throughout the eighteen twenties despite the continuing criticism of Baily and South. However, both Baily and South were undaunted in their efforts to improve the Nautical Almanac. In January of 1822, Baily published a work to show what an ephemeris ought to contain.<sup>1</sup> He stated that the tables included in the work would form a valuable addition to the annual volumes of the Nautical Almanac, as they already did in some foreign ephemerides.<sup>2</sup> Baily noted that it was erroneous to think that the Nautical Almanac was intended only for nautical purposes since its title implied another and more important object--that of astronomy. The Nautical Almanac, he stated, fell far short of what was "annually required by astronomers."<sup>3</sup> In particular, Baily wanted Young to include occultations of the moon, that is, the immersion of stars in the first quarter of the moon on the dark limb, and to include stars of the seventh, eighth, and ninth magnitudes. It was his opinion that stars of such magnitudes could be as readily observed as stars of the first, second, and third magnitudes and that such data would be preferable to observations of

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<sup>1</sup> Francis Baily, Astronomical Tables and Remarks for the Year 1822 (London: J. Richardson, 1822), p. ii.

<sup>2</sup> Ibid., p. ii.

<sup>3</sup> Ibid., p. iii.

Jupiter's satellites, or lunar eclipses, for determining longitude.<sup>1</sup> Baily thought such information should be published annually and not allowed to "moulder on the shelves" of a public or private library, because "science is not science till revealed."<sup>2</sup> Furthermore, he maintained that it was mortifying that since the time of Edmund Halley (1656-1742), the second Astronomer Royal, England had not produced a single astronomical table in which the data gathered at the Greenwich Observatory had not first been put in tabular form on the Continent and then returned to be reprinted in the Nautical Almanac.<sup>3</sup>

Baily's primary purpose in publishing his Astronomical Tables in January of 1822 was, as noted above, to show what an ephemeris ought to contain and to promote astronomy as a science in England. In May of 1822, he published another paper<sup>4</sup> in response to an anonymous critique of his Astronomical Tables. This paper was in effect his first direct confrontation with Young as Superintendent of the Nautical Almanac. He stated that he saw no impropriety in detecting and exposing error but that some anonymous writer had been offended by his remarks and had "stigmatized them with the

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<sup>1</sup> Ibid., p. iv.

<sup>2</sup> Ibid., p. vi.

<sup>3</sup> Ibid., p. ix.

<sup>4</sup> Francis Baily, Remarks on the Present Defective State of the "Nautical Almanac," (London, May 7, 1822).

epithet of 'superflous and frivolous.' "<sup>1</sup> Baily hinted that the anonymous writer was Young because of "his peculiar intimacy with the subject matter under review."<sup>2</sup> He included the critique<sup>3</sup> in its entirety in his Remarks and answered it point by point.<sup>4</sup> In addition, he included an analysis of the ephemerides of Paris, Berlin, Milan, and Coimbra and pointed out that each one was intended for the use of an observatory as well as a navy.<sup>5</sup> His purpose in including an analysis of each ephemeris was to enable Young and other interested persons such as astronomers in making a proper selection of the items to be included in an improved ephemeris. Baily also suggested that a "Committee be appointed to ascertain whether any and what alterations should be made in the Nautical Almanac."<sup>6</sup> This suggestion was made because it had been rumored that some members of the Board of Longitude maintained that the Nautical Almanac was

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<sup>1</sup> Ibid., p. 2.

<sup>2</sup> Ibid., p. 2.

<sup>3</sup> "A Reply to Mr. Baily's Remarks," in Remarks, by Baily, pp. 3-16.

<sup>4</sup> "Mr. Baily's Answer to the Reply," in Remarks, by Baily, pp. 17-72.

<sup>5</sup> Ibid., p. 19.

<sup>6</sup> Ibid., p. 35.

intended; by the Act of Parliament, for the improvement of navigation only; whilst others (who may be disposed to adopt a more liberal and enlarged view of the question) conceive that the management of it is the peculiar province of the Secretary, and that they are prevented, by a feeling of etiquette, from interfering therein by any suggestions of their own.<sup>1</sup>

By suggesting that a committee be formed, Baily hoped to provide a means of removing any influence which might prevent a careful consideration of the state of the Almanac.

In April of 1822, just one month prior to the publication of Baily's Remarks, James South published a pamphlet,<sup>2</sup> stating that the Nautical Almanac had always contained information which was of use only to astronomers and that there was, therefore, good reason for extending the items needed by astronomers.<sup>3</sup> Such an argument did not make sense, since there was reason to extend the information in the Almanac only if it were actually needed and not because it had always been included for the use of astronomers.

Young did nothing to answer the criticisms or to act upon the suggestions of the astronomers. Approximately five years passed before the astronomers were able to obtain any support from the Board of Longitude. The Board finally requested that John Herschel

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<sup>1</sup> Ibid., pp. 34-35.

<sup>2</sup> James South, Practical Observations on the Nautical Almanac and the Astronomical Ephemeris, pp. 38-39.

<sup>3</sup> Ibid., pp. 2-3.

prepare a report on the Nautical Almanac which he presented at a meeting of the Board of Longitude in April of 1827.<sup>1</sup> Herschel stated that the Almanac was sufficient for nautical purposes but defective as an astronomical ephemeris. He noted that an astronomer needed an ephemeris for two very distinct purposes: the first to prepare him for his observations; and the second, to assist him as much as possible in the reduction of the data resulting from the observations. An ephemeris, he stated, should therefore contain the apparent places of the sun, moon, stars, and all the planets as well as notices of all such phenomena which occurred irregularly or infrequently. All regularly recorded information or observations should be given for equidistant times to facilitate interpolation of data, since an ephemeris served both as an aid to contemporary astronomers as well as a historical record for future astronomers. He noted that no such information was to be found in the Almanac. He also recommended the inclusion of the apparent places of at least one hundred stars; the declinations and right ascensions of the planets with hourly recordings of the right ascension and time of the semi-diameter of the moon as it passed the meridian; and a statement of the formulas and other

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<sup>1</sup>J[ohn] F[rederick] W[illiam] Herschel, "Report on the Nautical Almanac, April 5, 1827," Great Britain, Parliament, British Sessional Papers (House of Commons), XXI (March 17, 1829), 221-223.

authoritative data used in making calculations and tables.<sup>1</sup> Herschel's suggestions were similar to those made by Baily and South, but his reputation and prestige as an astronomer were such that Young made some small improvements, which included separately publishing a supplement to the Nautical Almanac for the year 1828.<sup>2</sup> Baily was still not satisfied. In January of 1829, he published another pamphlet<sup>3</sup> on the defective state of the Nautical Almanac in which he pointed out that a supplement was not a satisfactory solution to the problem. He wanted nothing less than a complete and thorough reform of the Nautical Almanac so that it met all the needs of both navigators and astronomers, since the needs of both groups were the same in many instances.<sup>4</sup> Baily felt that a totally new almanac would pay for itself, if properly constructed. The annual sale of the Nautical Almanac amounted to about seven thousand volumes as compared to a combined sale of about one million volumes for all the

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<sup>1</sup> Ibid., pp. 221-223.

<sup>2</sup> Dreyer, History of the Royal Astronomical Society, p. 58.

<sup>3</sup> Francis Baily, Further Remarks on the Present Defective State of the Nautical Almanac: to Which Is Added an Account of the New Astronomical Ephemeris Published at Berlin (London: Richard Taylor, 1829).

<sup>4</sup> Ibid., pp. 2-3.

other almanacs, so that the customers for a superior almanac already existed.<sup>1</sup>

The concern expressed by the astronomers was finally recognized by the government when, in January of 1829, a Memorandum was presented to the Chancellor of the Exchequer relative to the need to reform the Nautical Almanac. On February 23, 1829, a motion was made in the House of Commons for the production of the papers<sup>2</sup> connected with the Board of Longitude, which had been abolished in 1828, and the Nautical Almanac. On March 17, 1829 the papers were ordered to be printed. These papers included: the Memorandum of January 23, 1829; a copy of the paper read by John Herschel to the Board of Longitude on April 5, 1827; a Report or reply to the Memorandum by Young; and an account of the expenses of the late Board.<sup>3</sup> The Memorandum<sup>4</sup> stated that the Nautical Almanac had fallen into disrepute after Maskelyne's death; that there were fifty-eight errors in the 1818 volume of the Nautical Almanac and

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<sup>1</sup>Ibid., pp. 9-10.

<sup>2</sup>"Copy of the Memorials or Reports Presented to Government Since 1st January 1828, on the Subject of the Nautical Almanack, or the Board of Longitude; also, a Return of the Expenses of the Late Board of Longitude, Classed under the Principal Heads, and Distinguishes Each Year from the Establishment of the Board of Longitude in 1818, Until Its Dissolution," Great Britain, Parliament, British Sessional Papers (House of Commons), XXI (March 17, 1829), 219-232.

<sup>3</sup>Ibid., pp. 219-232.

<sup>4</sup>There is no indication as to the author of the Memorandum.



precisely the same number of errors in the 1830 volume; that it did not contain the lunar distances from the principal planets, nor any occultations; that the tables of the sun used by the computers were known to be inaccurate; that accurate places of all planets, including four small planets, should be given for every day; and that a new Board of Longitude should be formed.<sup>1</sup>

Young's reply to the Memorandum was essentially a restatement of the position he had held regarding the Nautical Almanac from 1818 to 1829. He stubbornly insisted on the primacy of the nautical portion of the Almanac and the secondary importance of the astronomical data.

South decided that, since the Parliamentary papers did not contain any refutation of Young's reply to the Memorandum, the public and the scientific world were entitled to such an account. Therefore, in April of 1829, South published a pamphlet<sup>2</sup> in which he reprinted Young's reply to the Memorandum and commented upon each point made by Young. In one humorous passage South related a story which was based upon a letter he had received from Captain William H. Smyth (1765-1849), an officer in the Royal Navy. The story, with some of South's embellishments, serves to illustrate the low state of

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<sup>1</sup>"Memorandum," in Refutation, by South, pp. v-vi.

<sup>2</sup>South, Refutation, pp. 1-65.

the Nautical Almanac during this period. Captain Smyth, when surveying the shores of the Mediterranean, was obliged to use Continental ephemerides because of the errors and omissions in the Nautical Almanac (this much is stated in Smyth's letter).<sup>1</sup> But wishing to show civility to a Spanish captain, he presented him with his copies of the Nautical Almanac for the current year and for other years. In return, the Spanish captain presented Captain Smyth with copies of his ephemerides. To this South added: "Fortunately for the benefit of hydrography, Captain Smyth with his foreign Ephemerides found his way to England; but, there is an awkward story afloat that the Spanish captain has not since been heard of."<sup>2</sup>

The efforts of the external coterie to reform the Nautical Almanac were finally recognized by the Admiralty which addressed a letter, dated July 28, 1830, to the Astronomical Society asking for suggestions for the improvement of the Almanac. A forty member committee was formed, but it proved too unwieldy. A subcommittee was formed to do the work of the larger committee. The subcommittee was composed of George Biddle Airy, who was later to become the seventh Astronomer Royal, Babbage, Baily,

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<sup>1</sup> Letter, William H. Smyth to James South, April 15, 1829, quoted in Refutation, by South, p. 26.

<sup>2</sup> South, Refutation, p. 27.

Herschel, Pond, South (the chairman), Captain Francis Beaufort (1774-1857), Royal Navy, Lieutenant William Samuel Stratford (1790-1853), Royal Navy, Friedrich Georg Wilhelm Struve (1793-1864) an astronomer from the Russian Academy of Science who was on a visit to England, and Thomas Romney Robinson (1792-1882), an astronomer in charge of the Armagh Observatory in Ireland. The report of the subcommittee was submitted to and accepted by the Admiralty, on November 19, 1830, with full recommendations for the improvement of the Almanac. The recommended changes included substantially all of the suggestions made by Baily, Herschel, and South over the decade from 1820 to 1830 and were included in the 1834 edition of the Almanac.<sup>1</sup> It was recommended that such items as the following be included: the substitution of mean solar time for apparent time--a substitution made possible by the availability of accurate chronometers; a statement that nautical astronomy encompassed more than a means of locating a vessel at sea--it also included the exact determination of the positions of headlands, islands and the general outline of the coast; the computation of the sun's longitude, right ascension and declination; computations of the moon's longitude, latitude, declination, equatorial horizontal parallax and horizontal

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<sup>1</sup>It generally took two to three years to prepare the Almanac for publication. In this case, work on the 1834 edition of the Almanac was begun sometime early in 1831 and was probably printed late in 1833.

semidiameter; the substitution of heliocentric longitude and latitude for geocentric; and a list of moon culminating stars as well as other information concerning the planets, Jupiter's satellites, and comets.<sup>1</sup>

The reform was important to the external coterie in at least two ways. First, the Admiralty asked the Astronomical Society, instead of the Royal Society, for suggestions in improving the Nautical Almanac. The Royal Society had traditionally advised the Admiralty on such matters. Therefore, this act by the Admiralty was indicative of the prestige the Astronomical Society enjoyed after one decade of existence, and the success of the Society could be attributed in part to the efforts the external coterie. Secondly, the Nautical Almanac, was, after approximately ten years of struggle, greatly improved both with respect to its accuracy and with respect to the inclusion of data which was of value to the current needs of navigators, astronomers, and geographers. In short, the Almanac once again fulfilled the goal set by Maskelyne--it was to be "a work which must greatly contribute to the improvement of Astronomy, Geography and Navigation."<sup>2</sup>

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<sup>1</sup>"Report of the Committee of the Astronomical Society of London, Relative to the Improvement of the Nautical Almanac; Adopted by the Council November 19, 1830; Approved by the Right Honourable the Lords Commissioners of the Admiralty, and Ordered by Them to be Carried into Effect," James South, Chairman, Memoirs of the Royal Astronomical Society, IV (1830), 449-470.

<sup>2</sup>Maskelyne quoted in Refutation, by South, pp. 5-6.

## IV. THE DECLINE OF SCIENCE IN ENGLAND--A CONTROVERSY

A national debate arose in 1830 when Babbage published Reflections on the Decline of Science in England: and on Some of Its Causes. Babbage stated that by November 17, 1831, he had collected about thirty reviews on the state of science in England which filled two octavo volumes.<sup>1</sup> One anonymous writer referred to the flood of articles on the decline of science as a "paper war."<sup>2</sup> If Babbage had published his book at a different time, it might not have received so much attention or aroused so much controversy. As noted in the previous chapter, there was much agitation in political circles to extend the voting franchise to a greater number of the populace. There was a great fear on the part of many aristocrats that an extension of the voting franchise, particularly if it meant universal suffrage, might lead to a revolution.<sup>3</sup> The voting franchise was extended by means of the Reform Bill of 1832, which essentially doubled

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<sup>1</sup> Letter, Charles Babbage to Bery Delessert, October 17, 1831, B.M. Add MSS 37186, V. 126.

<sup>2</sup> Extract from the London Literary Gazette, October 2, 1830, p. 643, in A Statement of Circumstances Connected with the Late Election for the Presidency of the Royal Society, by [William Henry Fitton], (London: Richard Taylor, 1831), p. 37. The authorship of this publication was established by Mary Lou Gleason, a graduate student at Harvard University.

<sup>3</sup> Thomas Babington Macaulay, "Parliamentary Reform," The House of Commons, March 2, 1831, in The English Reform Tradition, 1790-1910, ed. by Sydney W. Jackman, p. 55.

the voting population but fell far short of universal suffrage.<sup>1</sup>

Babbage was in effect reflecting the spirit of the times. His book was in reality a party book of the external coterie. He and his friends, who made up the external coterie, claimed, on the basis of their experience with science in England, that science had declined and that changes in the political structure of science were necessary in order to reverse the decline. Babbage urged the extension of the voting franchise within the Royal Society in order to provide for direct control of the affairs of the Society by the fellows.<sup>2</sup> He also urged men of science to intermingle with the craftsman and the tradesman in order to promote the diffusion of scientific ideas among the masses and to obtain ideas from the craftsmen which would in turn help stimulate new inquiries in science.<sup>3</sup> He was reflecting a concern which had been expressed by Lord Henry Brougham (1778-1868).<sup>4</sup> Brougham thought it was necessary to educate the masses so that

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<sup>1</sup> Walter Edwards Houghton, The Victorian Frame of Mind, 1830-1870, (New Haven: Yale University Press, 1957), pp. 2-23.

<sup>2</sup> Babbage, Reflections, pp. 139-166.

<sup>3</sup> Ibid., pp. 6-7. For a more detailed discussion of fusion of the abstract and applied sciences, see Charles Babbage, On the Economy of Machinery and Manufactures (4th ed. enlarged; London: Charles Knight, 1835), pp. 379-392.

<sup>4</sup> Chester William New, The Life of Henry Brougham to 1830 (Oxford: Clarendon Press, 1961), pp. 198-557.

skilled workers would be available to run the industrial machinery of the nation. He also thought it was necessary to provide instruction not only in science but in politics and literature as well so that the working man would be able, if provided with the opportunity to vote, to vote intelligently.<sup>1</sup>

It was in this setting of political turmoil and domestic controversy and change that Babbage published Reflections and in turn stimulated a debate concerning the state of science in England. The debate extended beyond the borders of England because of the pro-Continental and, in particular, the pro-French attitude which Babbage and the other members of the external coterie assumed in their discussion of the relative decline of science in England. By assuming such a stance, the members of the external coterie made the issue of the decline of science a matter of national honor. In particular, when Babbage and David Brewster compared English science unfavorably with French science and French governmental policy toward science, they aroused the ire of those who considered France an archrival and a poor example for England to emulate. In fact, it was Babbage's good friend Michael Faraday (1791-1867) who took him to task for his pro-Continental attitudes with the following polite remarks in the preface of Moll's On the Alleged Decline of Science in England:

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<sup>1</sup>Ibid., pp. 10-20.

Without being considered as expressing an opinion on the subject either one way or the other, I am still desirous of placing my friend's [Gerard Moll] reasons before the public, not merely because no one can judge correctly who has heard but one side of a question, but also as a great literary curiosity; for all must allow that it is an extraordinary circumstance for English Character to be attacked by natives [such as Babbage] and defended by foreigners.<sup>1</sup>

The friend to whom Faraday was referring above was Gerard Moll (1785-1838) of Utrecht. Moll was also appalled by what he described as an observable disposition

amongst some of the most scientific men of England. They appear bent upon undervaluing their own country, and seem to take a secret, and certainly a strange delight in extolling to the skies the scientific excellence of foreigners of every discription.<sup>2</sup>

One of the problems with Babbage's claim that science had "long been neglected and declining in England,"<sup>3</sup> was that he included very little evidence for the decline. In fact, he admitted that the

causes which have produced, and some of the effects which have resulted from, the present state of science in England, are so mixed, that it is difficult to distinguish accurately between them.<sup>4</sup>

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<sup>1</sup> Michael Faraday in the preface to On the Alleged Decline of Science in England, by [Gerard Moll], (London: T. & T. Boosey, 1831), p. i.

<sup>2</sup> [Moll], Alleged Decline, p. 2.

<sup>3</sup> Babbage, Reflections, p. v.

<sup>4</sup> Ibid., p. 2.



He declared that it was his intent "not to attempt any minute description" of the causes of the decline but to present his own reflections on the "concomitant circumstances which have attended the decay."<sup>1</sup> He then proposed to examine some suggestions which had been offered for the advancement of British science. The result of Babbage's declaration of this intent and his adherence to it is a curious mixture of statements as to the relative importance of education, private and governmental patronage, and learned societies in the promotion of science in England and on the Continent. He made only a brief reference in the preface of his book to the state of specific sciences in England relative to those on the Continent, and it was the very brevity and superficiality of those statements which aroused much of the protest from the scientific community. It was the opinion of some of Babbage's fellow scientists that he had offered insufficient evidence to support his argument that science had declined in England. Babbage's decline of science arguments include two examples given by him of specific sciences--chemistry and mathematics--which had declined; the reciprocal influence of science and education upon the progress of scientific discovery; and the degree to which private and governmental patronage promoted or impeded science in England compared to that on the Continent.

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<sup>1</sup>Ibid., p. 2.

Babbage singled out chemistry as a science which had declined in England. He then used the following statement by John Herschel to support his claim. As noted in the previous chapter, Herschel had written that

whole branches of continental discovery are unstudied and indeed almost unknown, even by name. It is invain to conceal the melancholy truth. We are fast dropping behind. In mathematics we have long since drawn the rein, and given over a hapless race. In chemistry the case is not much better. Who can tell us any thing of the Sulfo-salts? Who will explain to us the laws of Isomorphism? Nay who among us has even verified Thenard's experiments on the oxygenated acids, --Oersted's and Berzelius's on the radical of the earths--Balard's and Serrula's on the combinations of Brome, --and a hundred other splendid trains of research in that fascinating science? Nor need we stop here. There are, indeed, few sciences which would not furnish matter for similar remark. The causes are at once obvious and deep seated; but this is not the place to discuss them.<sup>1</sup>

Babbage also included the remarks of the Reverend Francis Lunn, a professor of chemistry at St. John's College, University of Cambridge, in which Lunn had compared the state of chemistry in England with that on the Continent:

In concluding this most circumscribed outline to the History of Chemistry, we may perhaps be allowed to express a faint shade of regret, which, nevertheless, has frequently passed over our minds within the space of the last five or six years. Admiring, as we most sincerely do, the electro-magnetic discoveries of Professor Oersted and his followers, we still, as chemists, fear that our science has suffered some degree of neglect in

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<sup>1</sup> John Herschel, "Sound," Encyclopedia Metropolitana, IV (1845), 810, quoted in Reflections, by Babbage, pp. vi-ix.

consequence of them. At least, we remark that, during this period, good chemical analyses and researches have been rare in England; and yet, it must be confessed, there is ample field for chemical discovery.<sup>1</sup>

Lunn was lamenting the fact that the attention of chemists in England had been diverted away from what he considered more important problems in chemistry. He continued as follows:

How scanty is our knowledge of the suspected flourine!  
Are we sure that we understand the nature of nitrogen?  
And yet these are amongst our elements. Much has been done by Wollaston, Berzelius, Guy-Lussac, Thenard, Thomson, Prout, and others, with regard to the doctrine of definite proportions; but there yet remains the Atomic Theory. Is it a representation of the laws of nature, or is it not?<sup>2</sup>

Lunn concluded that chemistry had declined in England because the chemists had become interested in electro-magnetism. However, an interest in electro-magnetism did not indicate a lack of interest and effort in science in general. It only indicated a shift of interest and effort from one branch of science to another, which had taken place on the Continent as well as in England.

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<sup>1</sup> Francis Lunn, "Chemistry," in the Encyclopedia Metropolitana: or Universal Dictionary of Knowledge; On an Original Plan; Comprising the Twofold Advantage of a Philosophical and an Alphabetical Arrangement, with Appropriate Engravings, IV (1845), 596. The date of Lunn's remarks would have been 1830 or before, since Babbage quoted the above passage in Reflections, p. vi.

<sup>2</sup> Ibid., 596.

The surprising thing about Babbage was that he really did not offer any argument of his own as to the state of specific sciences. He was content to accept the statements by Lunn and the generalization offered by Herschel that the state of chemistry and mathematics were indicative of the general state of science in England. He merely added:

With such authorities, I need not apprehend much doubt as to the fact of the decline of science in England: how far I may have pointed out some of its causes must be left to others to decide.<sup>1</sup>

The apparent willingness of Babbage to accept the opinions of his friends that science had declined lends credence to the idea that his book was a party organ of the external coterie which was intended to express the views of the group in order to draw attention to the state of the Royal Society. Babbage devoted a major portion of his book to the Royal Society and its mismanagement by what he referred to as "the party which governs" the Royal Society meaning the President and the Council.<sup>2</sup>

John Frederick Daniell (1790-1845), professor of chemistry at King's College, London, and F.R.S., was appalled that Babbage had accepted the remarks of Lunn and Herschel as proof that science in England had declined. Such a claim, he noted, had not come from an

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<sup>1</sup> Babbage, Reflections, p. ix.

<sup>2</sup> Ibid., p. xiv.

obscure person. It had been "propagated and reverberated" with an energy which had astounded most men and confounded many at a time when three of England's greatest philosophers had just recently died--Davy, William Wollaston, and Thomas Young.<sup>1</sup> Babbage in Daniell's opinion had not attempted to establish his claim by evidence:

The disease is supposed to be confessed, and the cautery is forthwith applied for its extirpation with no tender hand. With regard to chemistry, indeed, something like definite charges may be collected from this comprehensive indictment; and upon these I shall venture to make a few observations.<sup>2</sup>

David Brewster had reviewed Babbage's Reflections shortly after it was published and referred to Herschel's remarks which had been included in the preface as "specific and incontrovertible proofs" of the fact of the decline of chemical science in England.<sup>3</sup> To Daniell, such statements implied a lack "of acquaintance with the common routine even of our principle schools of chemistry"<sup>4</sup> and were "utterly without palliation in those who adopt them, without

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<sup>1</sup> John Frederick Daniell, An Introductory Lecture, Delivered in King's College, London, October 11, 1831 (London: B. Fellowes, 1831), pp. 5-6.

<sup>2</sup> Ibid., p. 6.

<sup>3</sup> David Brewster, "Observations on the Decline of Science in England," Edinburgh Journal of Science, No. IX, p. 1.

<sup>4</sup> Daniell, An Introductory Lecture, p. 8.

investigation, as the basis of vituperation not only severe but personal."<sup>1</sup> He stated that bromine and its compounds were regularly introduced and illustrated through laboratory experiments at the Royal Institution, London University, and of Oxford University. He also stated that the magnesium, aluminum, and silicon of Oersted and Berzelius had been produced at the Royal Institution

with much less trouble than it cost them to transcribe these questions[he referred to Herschel's questions in the quote above]: and to the merest tyro of both schools which I have named, I refer them for the manner in which the processes are explained.<sup>2</sup>

Daniell went on to explain that the English chemists had explored each of the areas which Herschel had claimed were unstudied. It is apparent from his comments that Daniell thought Babbage, Herschel, and Brewster had not presented valid evidence to support their arguments that chemistry had declined; nor did he think these examples with respect to chemistry could be used to support the claim that science in general had declined. It must be admitted that Daniell presented much more convincing evidence for his point of view than did Babbage, Herschel, and Brewster.

The opinions of Dr. Thomas Thomson (1773-1852), a professor of chemistry at the University of Glasgow, provide an interesting

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<sup>1</sup> Ibid., p. 8.

<sup>2</sup> Ibid., p. 10.

contrast to those of Babbage, Herschel, Brewster, and Daniell.

Thomson's remarks refer to the state of chemistry in Scotland as well as in all of Great Britain. Thomson thought that chemistry had been neglected but for different reasons than those given above. He stated that in chemistry, in 1829, there did not exist

a sufficient number of labourers to carry it to perfection. The field is so vast, that one man is able to do but little. It must be cultivated by a multitude, otherwise it will remain barren and unproductive. Yet, strange as it may appear, the number of working chemists in this country, instead of increasing, has been woefully diminishing. About twenty-five years ago, at least thirty individuals might have been reckoned in Great Britain actively employed in chemical investigations; now, we can scarcely reckon ten. Some cause must exist for this retrogradation, so different from what is exhibited on the Continent, especially in France and Germany.<sup>1</sup>

The reason which Thomson gave for the small number of chemists was the method of teaching chemistry at the universities which consisted only of lectures and illustrative experiments instead of opportunities for "practical instruction in the details of analysis, and all operations of the Laboratory."<sup>2</sup> Such practical instruction was given

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<sup>1</sup>[Thomas Thomson], "History and Present State of Chemical Science," review of Larbok: Kemien (Stockholm, 1817-1823), Af Dr. J. Jac Berzelius, Chemiae Professor vid Carolinska, Medico-Chirurgiska Instuteti Stockholm; and Elements of Practical Chemistry; comprising a Series of Experiments in Every Department of Chemistry, with Directions for Performing Them (Edinburgh, 1829), by David Boswell Reid, in The Edinburgh Review, or Critical Journal, L (October, 1829), 275-276.

<sup>2</sup>Ibid., p. 276.

on the Continent and had contributed to advances in chemical discovery. Thomson also noted, in contrast to Lunn, that the nature of the science of chemistry had been greatly altered by the discovery of atomic theory so that "the minute accuracy now necessary for experimenting is so great, that genius alone, without practical skill, cannot be expected to succeed."<sup>1</sup>

The contention by Thomson that there were not enough persons pursuing chemistry as a branch of science was also maintained by William Henry (1774-1836), M.D. and chemist, but for different reasons. Henry stated in a letter to Babbage that a decline in chemistry had occurred to a considerable extent due to a lack of encouragement and a corresponding lack of exchange of information on chemistry in British periodicals. It was his opinion that the only science which exhibited a healthy aspect and vigorous growth was geology which might actually

have done some harm by leading many from experimental philosophy and from the exact sciences, 'which were' not only more laborious, but less inviting in themselves, and in the circumstances connected with their pursuit.<sup>2</sup>

The claim that chemistry had declined as a science was poorly documented by Babbage and Herschel, as has been discussed, but

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<sup>1</sup>Ibid., p. 276.

<sup>2</sup>Letter, William Henry to Charles Babbage, May 19, 1830, B.M. Add MSS 37185, IV, 179.



Herschel's claim that in mathematics "England has given over a hapless race" was very strange. Both Babbage and Herschel, as members of the Analytical Society, had helped to bring about a reform of mathematics in England. Moll stated that, since no one on the Continent had arisen to replace Lagrange and Laplace, there was no reason

for giving over the race as 'hopeless,' whilst such men as Mr. Ivory, Mr. Herschel, and Mr. Babbage are in full force of their talent. In France, M. Legendre is very old; the venerable Lacroix has done more than any other living man for the diffusion of analytical science: we see no one at present in France but M. Poisson who could enter the lists of the race. I do not perceive, I repeat it, the least necessity for giving it over.<sup>1</sup>

It is puzzling that Babbage and Herschel had not perceived themselves as the persons to place English mathematics on the highest levels; and that they used mathematics as an example of a science that was declining in 1830 almost a decade after the completion of their reform movement. If mathematics had declined, the members of the external coterie had no one to blame but themselves since Herschel, Babbage, Peacock, and Whewell had continued their mathematical inquiries throughout most of the eighteenth and nineteenth centuries. Moll thought the English mathematicians were much more creative than the French:

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<sup>1</sup>[Moll], Alleged Decline, p. 10.

In England no one has rendered himself master of the common rules of arithmetics but he thinks of turning his knowledge to some account; and, aided by that ingenuity of which Englishmen seem to possess a greater share than other nations, his scanty stock of information will often help him to some useful discovery, or some ingenious mechanical contrivance, at which Frenchmen, encumbered with the artillery of his mathematical learning could never arrive.<sup>1</sup>

Thus, from Moll's point of view, the English mathematicians could be justly proud of their own unique abilities to turn their mathematics to useful purposes. He noted that French mathematics was almost entirely theoretical with practical applications "foregone in almost every instance," and the French mathematician only occasionally would descend from the "pure air and bright sky of these higher regions" of mathematics to study applications of mathematics which were considered of "infinitely less value."<sup>2</sup> The consequence of this attitude, Moll wrote,

has been that calculus has been applied to solutions of problems for which the Elements of Euclid would have been quite sufficient. No question of optics, astronomy, or mechanics could be treated without calling in the intervention of the integral calculus; no bridge was built without its assistance; and even sometimes no two thermometers were compared without some pages in analysis.<sup>3</sup>

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<sup>1</sup> Ibid., p. 11.

<sup>2</sup> Ibid., p. 12.

<sup>3</sup> Ibid., p. 12.

Babbage and Herschel were comparing the English contributions to abstract mathematics and not to applied mathematics. However, if Moll were correct such men as Babbage, Herschel and Ivory had only to exert themselves to make English mathematics foremost in Europe.

On the whole, the arguments that chemistry and mathematics had declined and were representative of the general state of science in England were so poorly documented by Babbage as to cast serious doubt upon them. He did better with his arguments concerning the reciprocal relationship of science and education. It was a relatively easy task for him to examine the curricula of the universities and colleges and particularly that of Cambridge with which he was thoroughly familiar. Babbage discussed the reciprocal relationship of science and education from three points of view--the provision of private and governmental patronage to encourage students to pursue science as a profession; the contributions of the colleges and universities to the education of students of science; and the contributions of college and university professors to scientific knowledge. The arguments presented by Babbage with respect to the reciprocal relationship of science and education most nearly support a relative decline of science rather than an absolute decline. The decline was probably relative in the sense that the English colleges and universities had changed their curricula very little compared to those on the Continent; and, the financial support provided by the English

government had not decreased relative to that on the Continent. The question of a relative decline will be developed in more detail below.

It was Babbage's opinion that all of the advantages and opportunities which were present in the Continental nations as encouragement for the individual scientist were in very short supply in England:

The pursuit of science does not, in England, constitute a distinct profession, as it does in many other countries. It is therefore, on that ground alone, deprived of many of the advantages which attach to professions. One of its greatest misfortunes arises from this circumstance; for the subjects on which it is conversant are so difficult, and require such unremitted devotion of time, that few who have not spent years in their study can judge of the relative knowledge of those who pursue them.<sup>1</sup>

Babbage stated that non-scientific careers offered greater monetary rewards and luxury than scientific careers with the result that many promising young men chose other professions such as law which offered greater monetary rewards. Babbage was supported in this opinion by William Henry, except that Henry was not certain how much men such as Newton, Joseph Black, the chemist, James Watt, Dalton, and Davy would have discovered if they had been tempted by too much money. He thought that a moderate amount of patronage should be available to encourage the aspiring young scientist but that it should be extended prudently and at the right time:

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<sup>1</sup> Babbage, Reflections, pp. 10-11.

The boon which is principally to be desired, and may be reasonably claimed, from Government, is that it should confer leisure on men, who, having given demonstration of their ability to extend the boundaries of science, do not enjoy that essential condition. Let such men be spared all low drudgery, and all occupations that have a tendency to extinguish the nobler qualities of the mind, and to bear down its elasticity by never ceasing pressure of petty cases and anxieties.<sup>1</sup>

While Henry was cautious about extending patronage to persons who did not deserve it, he was arguing, as had Babbage and Bromhead, for the creation of a scientific elite to be supported by governmental patronage.

The creation of a scientific elite was in harmony with the idea of a community of scholars who would pursue science for which there were no immediate applications, but in 1830, the political support for such an institution was not in evidence. An anonymous reviewer of Babbage's Reflections was opposed to governmental support of science for which there were no practical and immediate benefits and did not agree with Babbage's plea for support of abstract science. With reference to the support of applied versus pure research, the reviewer asked the following:

Where is the line to be drawn? How shall the government be able to distinguish between the researches that carry in them the seeds of practical convenience, and those which must remain barren to the end of time? Is there not danger, then that a system of such

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<sup>1</sup> Letter, William Henry to Charles Babbage, May 19, 1830, B.M. Add MSS 37185, IV, 179.

indiscriminant encouragement would destroy the very object for which it was instituted? and that, instead of indemnifying exalted genius during its searches after the means of serving, in some way or another, the human community, we should be keeping in our pay, a national establishment of crazy mathematicians. . . .<sup>1</sup>

The reviewer's fear of an "establishment of crazy mathematicians" was probably made in reference to Babbage and his friends and also to Babbage's praise of the French Institute which had provided generous stipends for such mathematicians as Laplace. He was probably trying to discourage the establishment of a similar institution in England. Babbage was very critical of the quality of institutional support of science in England and emphasized the need for an institution in England similar to the French Institute. Such an institution was needed, according to Babbage, because the universities were not able to provide adequate encouragement for abstract science through established professorships, even though he acknowledged that some would argue to the contrary:

It is not however in the power of such institutions to create; they may foster and aid the development of genius; and, when rightly applied, such stations ought to be its fair and honourable rewards. In many instances their emolument is small; and when otherwise, the lectures which are required from the professor are not

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<sup>1</sup>"The Royal Society," review of Reflections on the Decline of Science in England, and on Some of Its Causes, by Charles Babbage, in The Monthly Review, or Literary Journal, Enlarged, N.S., I (January, 1831), 140.

perhaps in all cases the best mode of employing the energies of those who are capable of inventing.<sup>1</sup>

Babbage contended in the passage above that it was not in the power of universities to create since much of a professor's time was spent in instructing students rather than in scientific research. He was fully supported in this viewpoint by Brewster,<sup>2</sup> but he was equally opposed by Daniell who stated that it was his object and the object of the founders of King's College to "steadily advance the students in sound and useful knowledge," and, he added

should my exertions for this end be rewarded with success, I shall not care, though I may be doomed to share, with the professors of our English Universities, the reproach of a Scottish philosopher--that they are disadvantageously distinguished from 'philosophers actively and constantly engaged in original research,' because their names are not found in the lists of medallists and correspondents of foreign academies, in which his own so conspicuously appears. I confess, gentlemen, that I am not of the number of those who so loudly lament that science, in this country, has no peerages--no orders of merit--no ribbons, red or blue--to offer to her votaries.<sup>3</sup>

Daniell was referring to Babbage's lament concerning the lack of recognition and emoluments given to English scientists, such as peerages, as compared to Continental scientists. Moll criticized

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<sup>1</sup> Babbage, Reflections, p. 19.

<sup>2</sup> [David Brewster], "Decline of Science in England," review of Reflections on the Decline of Science in England, and on Some of Its Causes, by Charles Babbage, Quarterly Review, XLIII (October, 1830), 325-327.

<sup>3</sup> Daniell, An Introductory Lecture, p. 29.

Babbage on the same point and stated, "But although Mr. Babbage objects to the term 'wealthy' applied to a nobleman, he is far from disliking wealth in the hands of men of science. "<sup>1</sup>

Babbage criticized the universities from a second point of view with reference to the reciprocal influence of science and education. He argued that the state of knowledge in any country exerted a "directive influence on the general system of instruction adopted in it, " and on the contrary, "it is equally certain that the tastes and pursuits of our manhood will bear on them the traces of the earlier impressions of our education. "<sup>2</sup> He went on to state that it was not unreasonable to suppose that some portion of the neglect of science in England could be attributed to the system of education which allowed a young man to pass from public schools to the universities "ignorant almost at every branch of useful knowledge. "<sup>3</sup> Once at the universities, which had been originally formed for instructing those who were intended for the clerical profession, classical, and mathematical pursuits, the student was not afforded an opportunity to obtain an education which would render him useful to society.

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<sup>1</sup>[Moll], Alleged Decline, p. 27.

<sup>2</sup>Babbage, Reflections, p. 2.

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



Babbage contended that a degree conferred by a university should be a pledge to the public that the holder possessed a certain quantity of knowledge. As an innovation, he suggested that some requirement should be made that the student attend lectures on chemistry, geology, botany, and history in addition to regularly required lectures. To carry out this scheme, Babbage proposed that each candidate for a degree should be examined by those professors whose lectures he had attended, and further that no one should be allowed to take a degree unless he had been placed in the "List of Honours" in at least one of the courses given by the professors.<sup>1</sup>

John Playfair (1748-1819), a geologist, mathematician, and professor at the University of Edinburgh, in 1819, had argued against such a plan because he felt that such expectations interfered with a student's study of mathematics.<sup>2</sup> Babbage, on the other hand, had wide-ranging interests, so that it was not surprising that he should propose such a plan as the following for a university student in which the subjects could be taken with a restriction on the number of examinations required.

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<sup>1</sup>Ibid., p. 5.

<sup>2</sup>[John Playfair], "Woodhouse's Astronomy, review of An Elementary Treatise on Astronomy, by Robert Woodhouse. Vol. I (1812); Vol. II (1818), in The Edinburgh Review, or Critical Journal, XXXI (March, 1819), 392-394.

Group 1: Modern History; Laws of England; Civil Law

Group 2: Political Economy; Applications of Science to Arts  
and Manufactures

Group 3: Chemistry; Mineralogy; Geology

Group 4: Zoology, including Physiology and Comparative  
Anatomy; Botany, including Vegetable Physiology and  
Anatomy

Mathematics was also to be required of all students as had "usually" been required.<sup>1</sup>

A reaction to Babbage's comments on the relationship between education and science came again from Gerard Moll. Moll stated that he did not understand the English system of education, but he did comment upon some aspects of it. Babbage had stated that it was a waste of a scientific man's genius and talents to spend his time instructing students. He had in mind the case of John Dalton (1766-1844) whose genius was wasted in the "drudgery of elementary instruction." He stated that the military renown of England would have been undistinguished if the Duke of Wellington had wasted his mental power by employing his time drilling recruits instead of planning campaigns.<sup>2</sup> To this, Moll replied that he was surprised to learn that a scientist

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<sup>1</sup> Babbage, Reflections, pp. 5-6.

<sup>2</sup> Ibid., pp. 20-21

in an English university could be excused from all instruction. He thought that the cause of science suffered from such a system, since the students were deprived of the knowledge of an association with the outstanding scientists. As an example, Moll pointed out an extremely productive era for French sciences when Laplace and Lagrange were professors at the Ecole Normale, and when the noted geometer, Gaspard Monge (1746-1818) was at the Ecole Polytechnic.<sup>1</sup> France benefited from these men through the students who followed them. Likewise, Germany, according to Moll, could trace some of its "most celebrated astronomers" to the teachings of Frederick Gauss.<sup>2</sup>

The statements made by Babbage with respect to education and science, governmental support of science, and the cultivation of pure science were, according to Augustus Bozzi Granville, M.D. (1783-1872), far too inconclusive to be acceptable as proof for a decline of science:

Now, as to the causes assigned by Mr. Babbage with a view to explain the decline of science in England, I said that they proved too much. Is it not so, when we find him stating that to the present system of education pursued at our public schools and at the universities, the neglect of science in the country is partly to be attributed? If this

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<sup>1</sup>In a letter to Charles Babbage, November 17, 1831, B.M. Add MSS 37186, V, 150, David Brewster wrote that the review by Moll was "utterly contemptible" in equating scientific research with teaching.

<sup>2</sup>[Moll] Alleged Decline, p. 11.

position be true, in as much as the system now pursued at those schools and universities is totibus modis the same which has existed for centuries, it would follow that science, in England, must always have been on the decline, even when Newton filled the chair which our author has the proud distinction of filling at this moment in the University of Cambridge. And if always on the decline for such a cause, I would ask the Lucasian Professor, at what period then did science flourish in this country?<sup>1</sup>

Granville also refuted Babbage's argument that the English government provided little monetary support and encouragement to "the authors of useful discoveries or new and valuable inventions."<sup>2</sup> He stated that the English government had voted funds for projects such as "the improvement of chronometers--the introduction of antiseptic fumigations--the discovery of vaccination--and the encouragement of polar navigation," as well as hundreds of other projects.<sup>3</sup> Granville agreed with Babbage that governmental support of science was not any different in 1830 than in previous years:

I would submit to the author, that as this second cause is said by him to have existed at all periods, the decline of science, which is made to depend upon it, must also have been noticed 'at all periods' in England. Thus, then, according to Mr. Babbage's own shewing, the two series of causes which were at one and the same time to account for and prove the present decline of science in this country, may be considered as proving more than the

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<sup>1</sup>[Augustus Bozzi Granville], Science Without a Head: Or, the Royal Society Dissected (London: T. Ridgway, 1830), pp. 9-10.

<sup>2</sup>Babbage, Reflections, p. 14.

<sup>3</sup>[Granville], Science Without a Head, p. 10.

author desires. I would venture to assert, without much apprehension of being contradicted--and that, too, on the very grounds assumed by Mr. Babbage for quite a different purpose--that pure, difficult, and abstract sciences are, at this time, in precisely the same state in which they have ever been in England, namely, that they are moderately cultivated, abundantly productive, and sparingly endowed with great names and supereminent genius.<sup>1</sup>

Granville presented some very convincing arguments to support his claim that science had not declined in England in an absolute sense, that is, it had remained as it always had been--moderately cultivated. However, in all fairness to Babbage, it is reasonable to suppose that even if the educational system, the level of governmental support, and the cultivation of abstract or pure sciences in England had not changed, science in England could very well have experienced a decline relative to that on the Continent. In fact, Babbage had asserted in 1828 that the Continental nations had already recognized the value of science as a means of supporting military and industrial development. It was also his contention that the nations which valued scientific knowledge would emerge as the most powerful nations both economically and militarily. He was particularly impressed by Prussia and noted that

the severe sufferings of the Prussians previous to the war, by which themselves and Europe were freed, have impressed on them so strongly the lesson that 'knowledge is power,' that its effects are visible in every department

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<sup>1</sup>Ibid., pp. 10-11.

of the government; and there is no country in Europe in which talents and genius so surely open for their possessors the road to wealth and distinction.<sup>1</sup>

It was Babbage's hope that he could arouse public interest in science to a point that the leaders of the educational system, of the Royal Society, and government would be urged to provide the support of science which was necessary to make England first among the European nations in the cultivation of science. It was inconceivable to him that the English should be content with an unchanging educational system and an unchanging level of governmental support when he saw Continental countries such as Prussia increasing their level of support of science. It was in the context of a rising interest in and recognition of the importance of science on the Continent, that Babbage viewed English science as declining--declining relative to the Continental nations and not necessarily relative to its own past support of science.

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<sup>1</sup>Charles Babbage, "Account of the Great Congress of Philosophers at Berlin, on the 18th of September, 1828," Edinburgh Journal of Science, X (April, 1829), 3.

## V. THE FOUNDING OF A BRITISH SCIENTIFIC SOCIETY

The state into which the Nautical Almanac had fallen was viewed by Babbage and his friends as symbolic of the general state and trend of science during the first thirty years of the nineteenth century. At the time Babbage published Reflections, early in 1830, the external coterie had not yet experienced success in reforming the Nautical Almanac, thus the controversy regarding the Almanac was still very heated when Babbage wrote Reflections. Many of the issues he wrote about were related in one way or another either to the Nautical Almanac or to the Board of Longitude which had been abolished in 1828. In both instances the President and the Council of the Royal Society had been involved, and it was the opinion of the external coterie that the Royal Society leadership had not helped to reform the Nautical Almanac or to save the Board of Longitude from extinction. Baily, in particular, was very critical of the Council members. He stated that every time they had been asked to act upon an issue related to either the Board of Longitude or the Nautical Almanac, "some invisible and Boetian influence always paralysed their efforts whenever they met to discuss these matters."<sup>1</sup> Thus, when the Board of Longitude was dissolved in 1828, the external coterie held the President and Council

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<sup>1</sup> Francis Baily as quoted in South, Reply, p. 30. The inhabitants of Boeotia were reputed to be dull and stupid.

of the Royal Society responsible for allowing such an important source of governmental patronage to be eliminated. According to Babbage, the Board was abolished due to misadministration by the government and the poor advice given by those selected by the government on the recommendation of the President of the Royal Society. Brewster maintained that the Board had been abolished because it was actually useless.<sup>1</sup> Davies Gilbert (1767-1839), as President of the Royal Society, did not oppose the bill to abolish the Board. He stated that the Board was not being abolished through any demerits of its own but for reasons of economy.<sup>2</sup> John Croker, who had introduced the bill as Secretary to Admiralty, disagreed with Gilbert. He stated that the Board should be abolished because it was useless. It met only four times a year and was then wholly occupied with the wild ravings of madmen who imagined they had discovered such things as perpetual motion and hoped for a Parliamentary reward for their efforts.<sup>3</sup>

Brewster and Babbage were very critical of the Presidents of the Royal Societies of London, Dublin, and Edinburgh, because they

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<sup>1</sup>[David Brewster], "Decline of Science," The Quarterly Review, XLIII, No. 86 (1830), 321.

<sup>2</sup>Davies Gilbert as quoted in The Times (London), July 5, 1828, in Reply, by South, p. 34.

<sup>3</sup>John Croker as quoted in The Times (London), July 5, 1828, in ibid., p. 34.



had failed to exert their influence to save the Board of Longitude from extinction so that it could be redesigned to serve a useful function. Brewster saw no reason why a combined appeal to the government by Gilbert, Sir Walter Scott (1771-1832), President of the Royal Society of Edinburgh, and Dr. John Brinkley (1763-1835), Bishop of Cloyne and President of the Royal Society of Dublin, could not have preserved the Board.<sup>1</sup> Babbage was even more critical of Davies Gilbert than Brewster had been of all three Presidents of the Royal Societies of London, Edinburgh, and Dublin combined. Babbage criticized Gilbert for manipulating the Council of the Royal Society, as Sir Joseph Banks had done, for paying compliments to the Ministers of government rather than exerting his influence as President, and for assigning a disproportionate number of non-scientific members to the Council. Because of such acts, Babbage considered Gilbert inept both as a President and "as the powerful chief of a united republic, -- that of science."<sup>2</sup> He described Gilbert scathingly.

Why Mr. Davies Gilbert became President of the Royal Society I cannot precisely say. Let him who penned, and those who supported this resolution solve the enigma

'It was Resolved,

'That it is the opinion of the Council that

'Davies

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<sup>1</sup>[Brewster], "Decline of Science," Quarterly Review, XLIII, No. 86 (1830), 330.

<sup>2</sup>Babbage, Reflections, p. 54.

'Gilbert, Esq. is by far the most fit person to be  
'proposed to the Society at the approaching  
'anniversary as President, and that he be  
'recommended accordingly.'

To resolve that he was a fit person might have been flattering: to state that he was the most fit, was a little hard upon the rest of the Society; but to resolve that he was 'by far the most fit,' was only consistent with the strain of compliment in which his supporters indulge and was a eulogy, by no means unique in its kind, I believe, even at that very Council.

That Mr. Gilbert is a most amiable and kind-hearted man will be instantly admitted by all who are, in the least degree, acquainted with him: that he is fit for the chair of the Royal Society, will be allowed by few except those who committed themselves to the above quoted resolution.<sup>1</sup>

On an earlier occasion, Gilbert had incurred the displeasure of the Babbage group in another matter connected with the Board of Longitude. When Gilbert became President of the Royal Society in 1827, he announced that he would accept the office for one year only. He did not want to fill his former office of Treasurer of the Royal Society because he wished to resume his duties in that office once his year as President was completed. Thus, the Treasurer's position remained vacant for one year. As Treasurer, Gilbert had been a member of the Board on Longitude and as President he became an ex officio member of the Board. Therefore, by allowing the office of Treasurer to remain vacant, he also failed to fill his old position

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<sup>1</sup>Ibid., pp. 53-54.

on the Board, which was reserved for the Treasurer. The result was that Gilbert simultaneously filled the positions reserved on the Board for both the President and Treasurer. The Board was dissolved during his second year as President (1828) so that the matter did not become a scandal, but Babbage concluded that while the whole affair was of trifling importance it was indicative of the lack of dignity which characterized the proceedings of the Royal Society.<sup>1</sup>

As noted above, Babbage claimed that the Presidents of the Royal Society had manipulated the proceedings of the Council by assigning to it a disproportionate number of non-scientific members who had no real interest in or knowledge of science. A group of members were determined therefore to reform the Royal Society by changing the qualifications and procedures for membership in the Council as well as for membership in the Society. In order to implement such reforms, a committee was appointed, by resolution of the Council of 1827, prior to the time Davies Gilbert became President of the Royal Society. The Committee was composed of Wollaston, Young, Gilbert, South, Herschel, Babbage, Captain Beaufort, and Captain Henry Kater (1777-1835) Royal Navy and F.R.S.<sup>2</sup> The formation of this group provided an opportunity for the external

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<sup>1</sup> Ibid., p. 56.

<sup>2</sup> Ibid., p. 56.

coterie to achieve one of its goals--the reform of the Royal Society. For example, the committee members had hoped that a selective admission policy for Royal Society membership would favor the scientific elite, and would thus enable the Society to more effectively represent the interests of science. It was also formed in order to make recommendations for changing the laws and proceedings of the Society, particularly with regard to the selection of the Council.<sup>1</sup>

Wollaston suggested that membership in the Royal Society should be limited.<sup>2</sup> A recommendation was made to limit the total membership of the Society to 400 members.<sup>3</sup> Babbage suggested that a dual membership system should be adopted whereby anyone who had contributed at least two papers to the Transactions of the Royal Society would be identified by placing a star by his name. Such a practice, according to Babbage, would make it a matter of ambition to belong to the first class and add to the prestige and reputation of the Royal Society.<sup>4</sup>

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<sup>1</sup>"Report of the Committee Appointed to Consider the Best Means of Limiting the Members Admitted into the Royal Society, as well as to Make Such Suggestions on that Subject as May Seem to Them Conducive to the Welfare of the Society," in Reflections, by Charles Babbage, pp. 160-165.

<sup>2</sup>Babbage, Reflections, p. 152.

<sup>3</sup>Ibid., p. 154

<sup>4</sup>Ibid., pp. 154-156.

The committee was very explicit in its recommendations regarding the selection of the Council and stated:

It requires no argument to demonstrate that the well being of the Society mainly depends on the activity and integrity of its Council; and as their selection is unquestionably of paramount importance, your Committee hope that our excellent President will not consider it any impeachment of his impartiality, or doubt of his zeal, if they venture to suggest, that the usual recommendation to the Society of proper members for the future Council should henceforth be considered as a fit subject for the diligent and anxious deliberation of the expiring Council.<sup>1</sup>

The report of the committee was submitted shortly before the summer adjournment of the Council. Its submission to the Council was recorded June 25, 1827, with the understanding that it would be openly discussed and considered when the Council reconvened in the fall of 1827. However, Sir Humphrey Davy became seriously ill and resigned as President on November 13, 1827. The Council selected Gilbert to serve as the interim President until the election on November 30, 1827, when he was elected as President. Gilbert's views did not coincide with the recommendations of the committee even though he had been a member. As a result, Gilbert, with the help of a nobleman on the Council, got "rid of these improvements completely."<sup>2</sup> Thus, when the remarks made by Babbage with regard

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<sup>1</sup>"Report of the Committee," in Reflections, by Charles Babbage, p. 164. Sir Humphrey Davy was President of the Royal Society at the time the report was prepared.

<sup>2</sup>Babbage, Reflections, p. 149.

to Gilbert's fitness to serve as President of the Royal Society are considered in the context of his inaction in attempting to preserve the Board of Longitude, his manipulation of the Council, and in his successful action to stifle any reforms of the Society, it is no wonder that the external coterie should hold him in such low esteem.

Thus, after 1828 the external coterie became increasingly dissatisfied with Gilbert. Gilbert's declaration that he would accept the Presidency of the Royal Society for one year only gave the external coterie hope that one of their own group might succeed him. However, Gilbert was not to be so easily discouraged. He kept his real intentions with regard to the Presidency to himself. As a result, there was much confusion in November of 1828, the end of his first year in office, as to whether he would run for office again or step aside. Rumors began to circulate about a possible successor to Gilbert. One rumor was circulated that the King's brother, Augustus Frederick, Duke of Sussex (1773-1843),

was disposed to accept the Presidency; a gentleman in the household of His Royal Highness being particularly zealous in making representations to this effect.<sup>1</sup>

This rumor aroused support and provided a general consideration of the candidacy of the Duke of Sussex. The rumor also raised

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<sup>1</sup>[Fitton], A Statement, p. 7.

much opposition. There was much discussion about whether it was desirable for the President to be a man of science or a man such as the Duke of Sussex who had only an interest in science and who was also a nobleman. The reasons given in favor of the Duke's candidacy were that his elevated rank would allow him direct access to the Government and to the King's patronage; his personal appearance, manners, affability and intelligence were highly regarded; and his fluency in French, Italian, German and skill in public speaking were definitely assets. It was also stated that some reorganization of the Society would be carried out by the Duke which would "remodel the Society into distinct committees, as in the French Institute, to which the different matters brought forward will be submitted for consideration and sanction."<sup>1</sup> The reasons given against the Duke's candidacy were that if he were elected his exalted rank would effectively change the constitution of the Society. It would change the Society by

rendering the President a Patron of inferiors, rather than a presiding officer in an assembly of members in other respects his equals, would, to a certain extent, introduce the forms and restraints of a Court--throw doubt upon that purity of motive in the Council without which the proceedings of a deliberative assembly cannot be effectively carried on--and lower the tone of perfect

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<sup>1</sup>"Royal Society," London Literary Gazette, October 16, 1830, p. 674, in A Statement, by [Fitton], pp. 38-40.

independence, which ought in every country to be cherished and regarded as essential in the character of its men of science.<sup>1</sup>

It had been suggested by the Duke of Sussex, that he be placed upon the Council for one year, if his election to the Presidency were thought to be desirable, so that he would have time to become acquainted with the business of the Society and some of its leading members. However for the reasons given in the quotation above, a majority of the members of the Council of 1828, including Gilbert, declined to place the Duke on the Council or to endorse his candidacy for President for the election to be held a year later, on November 30, 1829.<sup>2</sup>

The persons who had supported the candidacy of the Duke of Sussex in 1828 did not give up. On August 14, 1830, an article was published stating that Gilbert was expected to retire from the Presidency of the Royal Society and that the Duke of Sussex was expected to be elected to succeed him.<sup>3</sup> The members of the Council

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<sup>1</sup>[Fitton], A Statement, pp. 8-9. Wollaston was one of the members of the Council who was much opposed to the Duke's candidacy primarily upon the principle that a President of the Royal Society should be chosen as a reward for scientific labors. Sir Humphrey Davy had written a letter to Wollaston expressing opinions to the same effect.

<sup>2</sup>Ibid., p. 9.

<sup>3</sup>"Literary and Learned," London Literary Gazette, August 14, 1830, p. 531, in A Statement, by [Fitton], p. 33.



of 1828 and the Fellows of the Society, who had knowledge of the proposed candidacy of the Duke in 1828 were very surprised and embarrassed. They learned, early in October of 1830, that Gilbert, who had voted against the Duke in 1828, had recently changed his opinion.<sup>1</sup> They had no knowledge of the reasons why Gilbert had changed his mind except "that a correspondence was then and had been for some time in progress,"<sup>2</sup> between Gilbert and the Duke's personal librarian, T.J. Pettigrew. They were very disturbed that Gilbert would compromise the Presidency of the Royal Society by attempting to name his own successor, an action described as

unquestionably contrary to the spirit of the Institution, and equally at variance with the practice both of the Royal Society and of other similar bodies in England; in which, even if there be no positive law to that effect, it is obviously the duty of an officer who wishes to retire, to restore his trust, unconditionally, to the hands of those from whom he had received it, and leave to them the free choice and election of his successor.<sup>3</sup>

Gilbert and his group had renewed their campaign during the summer recess of the Royal Society in 1830. Articles in addition to one on August 14, 1830 began to appear in the periodicals of the day.<sup>4</sup>

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<sup>1</sup>[Fitton], A Statement, p. 12.

<sup>2</sup>Ibid., p. 11.

<sup>3</sup>Ibid., p. 12.

<sup>4</sup>Ibid., pp. 37-47.

The fact that Gilbert had chosen the summer recess for the campaign made it very difficult for the opposition to take any collective action. He addressed a letter to Pettigrew on September 10, 1830, stating in part that

I am induced to address you in consequence of a paragraph which has appeared in most of the newspapers, stating a supposed intention, on my part of retiring from the Chair of the Royal Society; and that my successor would be a very illustrious individual. You will recollect the letter I wrote you two years since, giving reasons why, in my opinion, the extremely elevated situation of the individual in question rendered it not desirable that he should take an active part as an ordinary member in the business of the Society, notwithstanding abilities and acquirements which have eminently qualified him for the discharge of such functions in any other station of life. I must confess, that, as an abstract matter, I think so still; but currents of opinion, and of action founded on them, set in such a direction, that I am practically most desirous of seeing the Society under the protection of one so nearly connected with the throne. And personally to myself, I should justly esteem it the greatest possible addition to the honour I have already received, to be so succeeded.<sup>1</sup>

In the letter above, Gilbert mentioned having written a letter to Pettigrew in 1828, but when the article of August 14, 1830 appeared in the paper he wrote a letter to each member of the Council of the Royal Society stating that the reports of his resignation in favor of any individual because of his "high station, or for any other cause" were

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<sup>1</sup>Letter, Davies Gilbert to T. J. Pettigrew, September 10, 1830, in A Statement, by [Fitton], p. 34.

"utterly without foundation."<sup>1</sup> On October 1, 1830, he wrote another letter to the Council which was still uninformed other than by rumors of any negotiations with the Duke of Sussex through his librarian. Gilbert referred to his previous letter of September 14, 1830, which, he stated, contradicted a "vague report" of his intention to retire from the Presidency of the Royal Society. He also added that, since his letter of September 14, he had received "official information of His Royal Highness the Duke of Sussex, being disposed, in case of vacancy, to offer himself, with the highest sanction, to the Society for their President."<sup>2</sup> Gilbert's last statement of the letter was made as if such information were a complete surprise to him, when he concluded that it was his "bounden duty to declare at once, that I will not interpose myself as an obstacle between the Society and the immediate fulfilment of His Royal Highness's intention."<sup>3</sup> Gilbert's actions and duplicity came as a surprise to the Council. A small number of the Council accidentally learned of the letter of September 10, 1830, but to most its existence was unknown. Following the letter of October 1, 1830, action was taken by a group of thirty-two fellows of the Royal

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<sup>1</sup> Letter from Davies Gilbert to the Members of the Council of the Royal Society, September 14, 1830, in A Statement, by [Fitton], p. 35.

<sup>2</sup> Letter from Davies Gilbert to the Members of the Council of the Royal Society, October 1, 1830, in A Statement, by [Fitton], p. 37.

<sup>3</sup> Ibid., p. 37.

Society to demand that all the correspondence be brought before the Society. This action was taken in the form of a resolution called a "Requisition," and was presented to the Secretary of the Royal Society for consideration by the Council on its first meeting after recess for November 4, 1830.<sup>1</sup> This plan was abandoned in favor of a special meeting of the subscribers to the "Requisition" with the President and the Council. At that special meeting, November 11, 1830, Gilbert formally declared his intention of retiring as President of the Royal Society, but mentioned his desire to remain upon the Council for the ensuing year. He designated Captain Kater, the Treasurer, as the person to preside and then left the meeting. Captain Kater declined to preside in favor of Henry Warburton (1784-1858), a member of the Council.<sup>2</sup> A resolution was moved and seconded by two subscribers to the "Requisition" that in the future the officers and the Council of the Royal Society should be elected from among those members who "were by their acquaintance with the condition and interests of science best qualified to discharge such offices."<sup>3</sup>

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<sup>1</sup>"Requisition," in A Statement, by [Fitton], pp. 16-17.

<sup>2</sup>[Fitton], A Statement, p. 19.

<sup>3</sup>"Resolution," in A Statement, by [Fitton], p. 20.

Based upon the conditions in the foregoing resolution, it was stated that the great majority of the subscribers to the resolution were of the opinion that the Duke of Sussex was not a qualified candidate for President of the Royal Society. Those in favor of the Duke of Sussex stated that under such circumstances he would probably decline the nomination "unless he were elected without opposition."<sup>1</sup>

On November 19, 1830, a letter was circulated by the Council to all the Fellows residing in London and its vicinity in which part of the demands of the Requisitionists were met.<sup>2</sup> One of those demands was for the President and the Council to make out a list of fifty Fellows from which the Council and Officers for the ensuing year were to be chosen. The letter of November 19, 1830 contained such a list with eleven members of the old Council, as required by the charter, and the names of twenty-nine other Fellows. As desired by the Requisitionists, the letter was circulated among the Fellows of the Society. This list did not contain the name of the Duke of Sussex, and the conclusion by the opposition was that he was not eligible as a candidate since he was not named according to the resolution making up part of the letter which stated:

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<sup>1</sup>[Fitton], A Statement, p. 20.

<sup>2</sup>Ibid., p. 22.

Resolved, --That a list of twenty-nine Fellows be selected from a list of Fellows, out of which the Council would recommend the Society to choose the ten new members of the Council.<sup>1</sup>

The implication in the above resolution was supported by Peter Mark Roget (1779-1860), Secretary of the Royal Society, when he opened the letter with the following statement:

I am instructed by the President and the Council of the Royal Society to transmit to you the following Resolutions and Notices which they have agreed upon.<sup>2</sup>

The agreed upon notices and resolutions included: the selection of eleven members of the present Council to remain upon the Council for the ensuing year; a list of twenty-nine Fellows, from the Fellows-at-large, from which the remaining ten members of the Council would be chosen; and a recommendation of persons to fill the positions of Secretary and Treasurer of the Royal Society.<sup>3</sup>

The hopes of the requisitionists were shattered when they discovered that their recommendations and resolutions were to be ignored. Since the statutes of the Society required that the officers should be elected "out of the Council,"<sup>4</sup> the omission of the Duke of

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<sup>1</sup>Letter P[eter] M[ark] Roget, Secretary of the Royal Society to the Fellows resident in London and vicinity, November 19, 1830, in A Statement, by [Fitton], pp. 22-24.

<sup>2</sup>Ibid., in A Statement, by [Fitton], p. 22. The italics are mine.

<sup>3</sup>Ibid., pp. 22-24.

<sup>4</sup>[Fitton], A Statement, p. 24.

Sussex's name in Roget's letter, of November 19, 1830, to the Fellows of the Royal Society, indicated that he would not be eligible as a candidate for President in 1831. But, the requisitionists discovered, much to their dismay, that the conjecture by the Duke's supporters, at the meeting of November 11, 1830, was not true--the Duke was in fact willing to become a candidate in spite of opposition. Although "no distinct and authentic intimation was given of the real intentions of the Royal Duke"<sup>1</sup> until the day of the election on November 30, 1830, the requisitionists reacted by publishing a declaration, on November 25, 1830, to the effect that Herschel, because of his scientific achievements and personal character, was eminently qualified to fill the office of President of the Royal Society. Sixty-three persons signed this declaration with an additional seventeen names appended to it by election day. The subscribers to the declaration still believed that the Duke of Sussex would withdraw from the election when informed of the declaration, but such was not the case.<sup>2</sup>

The supporters of the Duke of Sussex carried on a campaign designed to extol the benefits to be derived from a President with Royal lineage. For example, one newspaper article used the following line of reasoning in comparing Herschel with the Duke of Sussex:

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<sup>1</sup>Ibid., pp. 27-28.

<sup>2</sup>Ibid., p. 27.

Of the other very respectable candidate [Herschel] I know less; as a man of science he stands very and deservedly high, and as a member of society I believe him to be most irreproachable. I only regret, for his own sake, that he should have suffered himself to be set up in opposition to a son of him to whom Sir William Herschel owed the fortune which he enjoyed during so many years of his life, and which was, I understand, transmitted to the very gentleman who is now the rival of the Duke of Sussex.<sup>1</sup>

The implication in the quotation was that Herschel was ungrateful for the fortune given to his father by the father of the Duke of Sussex. Even if this implication had been true, which according to Fitton it was not, since Sir William Herschel had been granted a salary after long years of work and had earned the bulk of his fortune through the sale of his own telescopes, it had nothing to do with either the qualifications of Herschel or the Duke of Sussex for the position of President of the Royal Society.

When the vote was taken, Herschel had one hundred eleven votes, and the Duke of Sussex had one hundred nineteen votes. The implication drawn by the opposition was that the Duke of Sussex won primarily due to his high "station":

If any arguments had been wanting to prove the injury to freedom of election produced by placing personages so exalted in direct competition with individuals of much inferior rank, the events which attended this brief contest would have supplied them: The pressure of station and

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<sup>1</sup> Extract of a letter under the signature of "Scrutator" in The Times (London), November 23, 1830, in A Statement, by [Fitton], pp. 46-47.



official influence employed upon a question on which neither ought to have been heard of, and urged with additional vigour as the opposing force of public opinion increased, having deeply wounded private and honorable feeling, and caused distrust and estrangement to an extent for which no Patronage acquired by the Society can possibly compensate. Names given to Mr. Herschel's supporters were retracted; others, though not withdrawn, were not followed by the votes of the subscribers; in some instances votes were kept back by interposition almost amounting to command; in others, the professional position of parties who had avowed their sentiments was alleged as a reason for their not acting upon them. The evils, in short, of common electioneering were brought in to taint the intercourse of men amongst whom such things never ought to be mentioned; and they have left effects which cannot but be injurious to Science.<sup>1</sup>

According to Fitton, the friends of Herschel did not employ any "indirect expedient" in his behalf. Their goal, he claimed, was the support and assertion of principles involving the personal independence of the Fellows and the general right of free election in the Royal Society.<sup>2</sup> The external coterie had a peculiar dislike for open competition in electioneering among scientific men. However, like their opponents, they held no such dislike for private negotiating in support of a candidate or for writing scurrilous pamphlets or articles. One article, written on October 2, 1830, noted that "the minutes of the Council are still impugned by Mr. Babbage, in a way likely to lead to more paper war; and the question of the Presidency is also a subject

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<sup>1</sup>[Fitton], A Statement, pp. 31-32.

<sup>2</sup>Ibid., p. 32.

of not very pleasant discussion."<sup>1</sup> The reference was to Babbage's original remarks on the minutes of the Council in Reflections<sup>2</sup> and the paper war to the numerous pamphlets and articles written after the publication of Reflections.<sup>3</sup>

There were possible reasons other than the unequal stature of Herschel and the Duke of Sussex which resulted in the victory of the latter. One was Herschel's reluctance to become a candidate. Herschel had expressed this reluctance to Brewster in a letter after the election in such a way as to convince Brewster that Herschel really would not have made a good President.<sup>4</sup> Although there is no evidence to the contrary, if his reluctance were equally apparent before the election some of his potential support might not have been expressed. Another possibility was that Herschel's friends failed to

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<sup>1</sup> Extract from the London Literary Gazette, October 2, 1830, p. 643, in A Statement, by [Fitton], p. 37.

<sup>2</sup> Babbage, Reflections, pp. 62-65.

<sup>3</sup> In a letter to Bery Delessert, dated October 17, 1831, B.M. Add MSS 37186, IV, 126, Babbage stated that he had collected about thirty reviews on the state of science in England which filled two octavo volumes.

<sup>4</sup> In a letter from Sir David Brewster to Charles Babbage, February 7, 1832, B.M. Add MSS 37186, V, 241, Brewster stated that Herschel's "heart moves in a narrow orbit," and added that he had come to the conclusion that Herschel would "never put himself out of his way either to advance the objects of Science or the interests of its activities." These comments were made in connection with a letter Herschel had written to Brewster concerning the British Association.

carry on an effective canvass of support just prior to the election. Evidence that such was the case can be found in a letter to Babbage from George Harvey, a Fellow of the Royal Society, complaining that he was prepared to depart for London when he received a letter from Babbage "saying it was not at all necessary to come up." He added, "From what I hear, however, if every man who signed the requisition had been at his post, Herschel would have been our chief."<sup>1</sup> This statement might be construed as a complacent attitude on the part of Herschel's friends, or they could have been lulled into inaction by the belief that the Duke of Sussex would withdraw if he were forced into direct competition with Herschel.

Once the Duke of Sussex was elected, he attempted to involve the Fellows of the Royal Society into studying and recommending reforms.<sup>2</sup> He invited Baily, Herschel, Babbage and Fitton to become part of a committee for this purpose, but they refused. Babbage sent the first draft of his letter of refusal to Fitton for his comments.<sup>3</sup> Fitton wrote back a list of detailed comments with a general comment that Babbage did not need to go into great detail about his negative

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<sup>1</sup> Letter, George Harvey to Charles Babbage, January 3, 1831, B.M. Add MSS 37185, IV, 429.

<sup>2</sup> Letter, James Hudson to Charles Babbage, March 4, 1831, B.M. Add MSS 37185, IV, 493.

<sup>3</sup> First draft of a letter, Charles Babbage to the Council of the Royal Society, B.M. Add MSS 37185, IV, 501.

feelings toward the Royal Society officers but needed only to politely refuse and let the "weight" of his refusal carry an implied message.<sup>1</sup> Fitton enclosed a copy of his own polite refusal,<sup>2</sup> and Babbage, after two more attempts which echoed his first draft, finally sent a letter which was worded much the same as that of Fitton.<sup>3</sup> Fitton's advice to Babbage to temper his remarks was repeated on many different occasions by his close friends. Such advice usually cautioned Babbage not to compromise his own character by harsh statements but to try to achieve the same objectives by more indirect means.

The success of the Duke of Sussex and his supporters marked a turning point in Babbage's career. Babbage not only refused to join in the reform of the Royal Society, but instead turned his full attention to his calculating engine. When Brewster wrote to him on February 21, 1831, about the possibility of taking part in a general effort to form a new society which would encompass all of Great Britain, Babbage, as implied by Brewster's letters,<sup>4</sup> was too preoccupied to

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<sup>1</sup> Letter, William Henry Fitton to Charles Babbage, March 9, 1831, B.M. Add MSS 37185, IV, 497.

<sup>2</sup> Copy of a letter, William Henry Fitton to the Council of the Royal Society, B.M. Add MSS 37185, IV, 496.

<sup>3</sup> Copy of a letter, Charles Babbage to the Council of the Royal Society, B.M. Add MSS 37185, IV, 505.

<sup>4</sup> In a letter to Babbage, September 16, 1831, B.M. Add MSS 37186, V, 86, Brewster stated that he could not accept the excuse that Babbage could not attend the British Association meeting at York

take an active part in its formation even though he had written to Brewster about such a society in 1828. Brewster was not easily discouraged. He corresponded with influential politicians and scientists in order to carry out the suggestions made by Davy, Herschel, Fitton, Baily, South and Babbage for the promotion of science.<sup>1</sup> Neither Herschel nor Babbage gave him the support he sought. Babbage did give some indication that he was still interested in such a society prior to the Royal Society election of November of 1830. This interest had been shown in a communication to Gilbert, to which Gilbert replied that he differed with Babbage on only one point with reference to the "establishment of a society similar to the Institute of Paris." Gilbert thought that such a society "would greatly advance science in England," but he did not think such a society should be established at the expense of existing societies.<sup>2</sup> In another letter, also written prior to the election of the Duke of Sussex, Babbage had expressed a weariness with the conflict he had aroused with the publication of Reflections and decided that his time and energy would be better spent in completing the calculating engine, a task which caused him much

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because of the calculating engine. He asked Babbage to make any sacrifice to attend the meeting because the success of the meeting depended upon him.

<sup>1</sup> "British Association for the Advancement of Science," North British Review, XIV (November, 1850), 248.

<sup>2</sup> Letter, Davies Gilbert to Charles Babbage, July 7, 1830, B.M. Add MSS 37185, IV, 250.

worry and concern.<sup>1</sup> A friend wrote to Babbage to ask if he thought the projected meeting of the British Association at York was worth attending and would advance the cause of science. He was evidently puzzled that Brewster rather than Babbage was promoting the British Association since he asked, "Why have you not started your plan of a Great European Society?"<sup>2</sup>

Brewster's interest in the establishment of a new society was renewed as a result of reading an account written by James F. W. Johnston (1796-1855), F.R.S. and an agricultural chemist, who had attended the 1830 anniversary meeting of the "German Naturalists" of Hamburg. A suggestion contained in this account strongly impressed Brewster "that the Cause of Science in England will derive just benefit from a meeting of British men of Science at York in July or August next."<sup>3</sup> Brewster evidently thought at this time that the election of the Duke of Sussex had foredoomed the Royal Society in England since he added: "The Royal Society of London seems to be gone--So is that of the Edin. and the R. Irish Academy has long been at an end."<sup>4</sup> The

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<sup>1</sup> Letter, Charles Babbage to Lady Denbigh, November, 1830, B.M. Add MSS 37185, IV, 401.

<sup>2</sup> Letter, Geo[rge] Harvey to Charles Babbage, June 14, 1831, B.M. Add MSS 37185, IV, 551.

<sup>3</sup> Letter, David Brewster to Charles Babbage, February 21, 1831, B.M. Add MSS 37185, IV, 481.

<sup>4</sup> Ibid., f. 481.

time was ripe according to Brewster for a "general effort," meaning that the existing Royal Societies throughout Great Britain were useless for promoting the cause of science. A general effort to establish a new society was, therefore, of great importance. At this particular time, February of 1831, Brewster felt that Herschel should be President of any new society.<sup>1</sup> He later changed his mind when Herschel showed little enthusiasm for such a position.<sup>2</sup>

Brewster also suggested the need for a new "Scientific Journal" and offered to give up his own to help with the new one, but added that he would need the support of men like Babbage and Herschel.<sup>3</sup>

Because he had severely criticized the universities for their lack of accomplishments or contributions to science,<sup>4</sup> Brewster received little encouragement or cooperation from universities, as pointed out by an anonymous reviewer, in the formation of what became the British Association for the Advancement of Science.

Our readers will no doubt be desirous of knowing the composition of the first meeting of the Association. It contained a great number of dissenters, as well as those whom individual zeal brought to York, there were

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<sup>1</sup> Ibid., f. 481.

<sup>2</sup> Letter, David Brewster to Charles Babbage, February 7, 1832, B.M. Add MSS 37186, V, 241.

<sup>3</sup> Letter, David Brewster to Charles Babbage, February 21, 1831, B.M. Add MSS 37185, IV, 481.

<sup>4</sup> [David Brewster], "Decline of Science in England," pp. 325-329.

deputations to the meeting from various philosophical societies, particularly in the north of England. The universities stood aloof--perhaps to watch--perhaps to stem the flowing tide of knowledge. Oxford sent one representative, that ardent and liberal philosopher Dr. Daubeny;<sup>1</sup> but Cambridge like Austria<sup>2</sup> among the German states, sent none of her scientific host. It was understood, however, that she was willing to take an active part, and bring up her contingent to Oxford, provided that the decline of science was not to be the watchword, nor its direct national encouragement among the objects of the Association. By what secret article these concessions were made, or supposed to be made, and by what ingenious modification of the avowed designs of its members these integral parts of the plan were surrendered, or supposed to be surrendered, we never learned, and have no means of ascertaining.<sup>3</sup>

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<sup>1</sup>Dr. Charles G.B. Daubeny (1795-1867) was professor of chemistry and botany at Oxford.

<sup>2</sup>The reference to Austria was in regard to the first meeting of the Deutscher Naturforscher Versammlung, the German Congress of Natural Philosophers held at Leipzig in 1822. This meeting was arranged by Lorenz Oken (1799-1851), founder of the literary and scientific journal Isis, for the promotion of science in the German states. Oken had mixed science and nationalist sentiment in his publication Isis so that some of the leaders of the German states viewed his scientific organization with suspicion. Prince Klemens von Metternich (1773-1859), of Austria, refused to allow passports to be issued to Austrian scientists. In 1832, after the other German states including Prussia had allowed their scientists to attend in 1828, Austria consented to host the meetings in Vienna when the Emperor Ferdinand I overcame Metternich's objection, see "The British Scientific Association," review of the Report of the First and Second Meetings of the British Association for the Advancement of Science, at York in 1831, and at Oxford in 1832; Including Its Proceedings, Recommendations, and Transactions. London: 1833. Report of the Third Meeting of the British Association for the Advancement of Science, Held at Cambridge in 1833. London: 1834, in The Edinburgh Review or Critical Journal, LX (January, 1835), 365-370.

<sup>3</sup>Ibid., p. 374.



One of the Cambridge professors who opposed the formation of the Association was Whewell, who displayed the same conservative attitude as he had in the formation of the Analytical Society. In both instances, Whewell disliked the apparent zeal shown by the most active promoters of the respective societies.<sup>1</sup> In the former instance he was especially critical of Brewster as he stated to James D. Forbes (1809-1868), professor of natural philosophy at the University of Edinburgh:

I am afraid I shall not meet you at York. Even if other circumstances allowed me, I should feel no great wish to rally round Dr. Brewster's standard after he had thought it necessary to promulgate so bad an opinion of us, who happen to be professors in Universities. He seems, with respect to such people, to have the power of imagining the most extraordinary things without a vestige of foundation: and just as he chose to fancy before that we each had a thousand a year<sup>2</sup> (which notion he persists in referring to), he has now chosen to fancy that we are all bonded together to oppose his favourite doctrine of the decline of science; though the only professor who has written at all on the subject is Babbage, the leader of the Declinists. It requires all one's respect for Dr. Brewster's merits to tolerate such bigotry and folly.<sup>3</sup>

Whewell was ready to offer advice and to join in active participation with the Association once he thought it was possible for it to

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<sup>1</sup> George Peacock was a zealous promoter of the Analytical Society and Brewster of the British Association.

<sup>2</sup> A thousand pounds (£1000) annual income.

<sup>3</sup> Letter, William Whewell to James D. Forbes, July 14, 1831, in William Whewell, by Todhunter, II, 121-122.

succeed, an attitude which he had also shown in the formation of the Analytical Society. He wrote to the Rev. W. Vernon Harcourt (1789-1871) of York on two occasions offering detailed suggestions in the first letter<sup>1</sup> on how the Association should be organized, and suggested in the second letter that, if the meetings were to be held in succession at different places, "a large portion of our members would be gratified with your selecting Cambridge."<sup>2</sup> The anonymous reviewer, referred to immediately above, noted that at the second meeting of the Association, at York in 1832, the most prominent feature was

the production of several Reports on the present state of various branches of science, the preparation of which had been suggested by the Rev. Mr. Whewell, now one of the most active members of the Association, and himself the author of the best report which has yet appeared--a report on Mineralogy.<sup>3</sup>

The Cambridge and Oxford professors, and particularly the former, were able to direct and influence the program content of the subsequent meetings. They adopted Whewell's suggestion for sectional reports from the various branches of science. But, the

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<sup>1</sup> Letter, William Whewell to W. Vernon Harcourt, September 1, 1831, ibid., pp. 126-130.

<sup>2</sup> Letter, William Whewell to W. Vernon Harcourt, September 22, 1831, ibid., pp. 130-131.

<sup>3</sup> "The British Scientific Association," in The Edinburgh Review or Critical Journal, LX (January, 1835), 375.

professors were not able to prevent the Association from seeking national encouragement of science. The concept of governmental support of science had been a central issue in the writings of both Babbage and Brewster. Both men maintained that the course of science could not be helped unless it had the support of the nobility<sup>1</sup> and of the political leaders. Brewster had recognized that men such as Lord Brougham who had knowledge of science as well as political influence would be invaluable for the promotion of a national interest in science.<sup>2</sup>

Brewster's role in the founding of the British Association and the reform of science was recognized by an anonymous reviewer who credited Brewster's review of the Reflections with more influence than the book itself in the promotion of a national concern for science. The reasons given for this were due to the new topics Brewster introduced in his review, the recommendations it introduced, and the fact that Brewster published his review in the Quarterly Review rather than the Edinburgh Review. The publication in either review had the advantage of a wider circulation than did Babbage's book. The reviewers preference for the Quarterly Review was given as follows:

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<sup>1</sup> It is rather ironic that Babbage and Brewster were in favor of support from the nobility but had opposed the Duke of Sussex for President of the Royal Society. Their opponents apparently did not use this against them.

<sup>2</sup> Letter, David Brewster to Charles Babbage, July 11, 1830, B.M. Add MSS 37185, IV, 261.

Such an article might have been expected to appear in the Edinburgh Review with the principles of which it might have been supposed to harmonize; but emanating from the Quarterly Review, which was not in practice of pleading for change, it fell with an electric force of its reproof and remonstrance; and we have no doubt that had he continued in power, he would have carried into effect many of its suggestions.<sup>1</sup>

In a subsequent statement the reviewer credited the political climate as an important factor in the success of the British Association:

On the accession of the Whigs to power in 1831, when the reform of our institutions, scientific and political, was the great topic of the day, the attention of Lord Grey's Government was called to the state of English science, and the condition of its cultivators. Lord Brougham, who then adorned the Woolsack, took an active part in promoting the interests of science, and through his instrumentality some of the more important objects of the British Association were secured before it had held its first meeting.<sup>2</sup>

The agitation for reform, which culminated in the Reform Bill of 1832, aroused fears that a revolution on the scale of the French Revolution might occur in England.<sup>3</sup> Brewster's decision to publish

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<sup>1</sup>"British Association for the Advancement of Science," in the North British Review, XIV (November, 1850), 244-245.

<sup>2</sup>Ibid., pp. 245-246.

<sup>3</sup>Sophia DeMorgan, in Memoir of Augustus DeMorgan, p. 42, supported the contention that the reform of science and the fervor for political reform were closely related. She quoted her husband, Augustus, who had stated, "I first began to know the Scientific world in 1828. The forces were then mustering for what may be called the great battle of 1830. The great epidemic which produced the French Revolution, and what is yet (1866) the English Reform Bill, showed its effect on the scientific world."

his views in the Quarterly Review gave assurance to the political leaders that their support of science was not a pledge to support radical political views, but the political reform movement certainly did not harm the cause of reform in science since the scientific community was not required to generate public support for science as an isolated movement. Also, Brewster was wise to encourage the support of men like Lord Henry Brougham (1778-1868), M.P., who were recognized for their interest in science as well as in political causes in general. The Rev. W. Vernon Harcourt (1789-1871), Vice-President of the first meeting of the Association at York in 1831, attempted to soothe the fears of those who felt that national support of science was "un-English." He did not wish to see "the men of science in England become servile pensioners of the Ministry,"<sup>1</sup> but he did think that science was an activity which demanded national support:<sup>2</sup>

I cannot see any reason why, with proper precautions, men of science should not be helped to study for the public good, as well as statesmen to act for it; nor do I see why they should not be as independent with fixed salaries, as statesmen hold themselves to be in places revocable at will.

At the present moment, there is a man of science,<sup>3</sup> and more than one friend, to the direct encouragement of scientific men, at the head of affairs. Our starving

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<sup>1</sup> Ibid., p. 255.

<sup>2</sup> Ibid., p. 258.

<sup>3</sup> Lord Brougham was the person to whom Harcourt referred.

philosophers are indulging no unjustifiable hope that the fortunes of philosophy may be mended under the influence of the present lords of the ascendent. It cannot be wondered that they should be unwilling to have it proclaimed, ex cathedra, from the midst of themselves, that there is something illegitimate in the direct encouragement of science, though they are ready enough to own that there is something in it very un-English.<sup>1</sup>

Thus the opposition of the Cambridge and Oxford professors to national encouragement of science was overruled at the very first meeting, even though Oxford sent only one person and Cambridge sent none. Attempts were made on two different occasions to credit Harcourt as the founder of the Association, but in both instances he repudiated the claim and gave sole credit to Brewster. These attempts were cited by an anonymous reviewer as evidence of the efforts of the Cambridge professors to discredit the efforts of Brewster, possibly to avenge themselves for Brewster's scathing criticism of university professors.<sup>2</sup>

As far as the role of Babbage in the Association is concerned, he was urged to take part in the second meeting at Oxford on June 19, 1832. Babbage not only participated but was named as one of the

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<sup>1</sup>"Address by Rev. W. Vernon Harcourt to the first meeting of the British Association at York in 1831," in Report of the First and Second Meetings of the British Association for the Advancement of Science; at York in 1831 and at Oxford in 1832, I and II (2d ed.; London: John Murray, 1835), 35.

<sup>2</sup>"The British Scientific Association," in The Edinburgh Review or Critical Journal, LX (January, 1835), 390-391.

permanent trustees of the Association. He also urged the formation of a statistical section. This was opposed to some extent by the Rev. Adam Sedgwick (1785-1873) President of the Association, Woodwardian Professor of Geology, University of Cambridge, because the section had proposed to include the "sciences of morals and politics." Sedgwick maintained that as long as statistics were concerned only with "matters of fact, with mere abstractions, and numerical results," the "raw material to political economy and political philosophy," then it was justifiable to include such studies; but he objected when statistics were concerned with higher generalizations of these disciplines which included passion and feelings. A section of statistics was subsequently formed based upon this restriction.<sup>1</sup> The statistical section later became a separate society under the name of the London Statistical Society on March 15, 1834. It was to this society that Babbage devoted his attention for the remainder of his life.<sup>2</sup>

Babbage made a recommendation at the second meeting of the Association, at Oxford in 1832, which reflected his interest in applied

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<sup>1</sup> Report of the Third Meeting of the British Association for the Advancement of Science; Held at Cambridge in 1833, III (London: John Murray, 1834), xxvii-xxx.

<sup>2</sup> William Farr, Address of Dr. Farr, President of the Statistical Society; Session 1871-72 (London: Harrison and Sons, 1872), pp. 411-417.

science. He proposed that, in selecting a city for the annual meeting, consideration should be given to a large manufacturing city where it would be possible to bring "theoretical science in contact with that practical knowledge on which the wealth of the country depends."<sup>1</sup>

Babbage noted that he had gained much valuable information which he had used in his own pursuits from manufacturing districts.<sup>2</sup> His recommendation was adopted and in 1836 the manufacturing city of Bristol was chosen. In his opening address to the Association in 1836 Daubeny supported Babbage in his contention that practical knowledge and theoretical science were complimentary to each other.<sup>3</sup> In accord with the recognition of manufacturing, a section on Mechanical Science was formed of which Babbage became the third President.<sup>4</sup>

Babbage was active in the British Association as a permanent Trustee from 1832 until 1839. The reason he gave for resigning his office was the intrigue within the Association by Roderick I. Murchison

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<sup>1</sup> Charles Babbage, A statement made in a section meeting of the British Association. In: Report of the First and Second Meetings of the British Association for the Advancement of Science; at York in 1831 and at Oxford in 1832, I and II, p. 107.

<sup>2</sup> Ibid., p. 107.

<sup>3</sup> Report of the Sixth Meeting of the British Association for the Advancement of Science; Held at Bristol in August 1836, V (London: John Murray, 1837), xxxiii-xxxvi.

<sup>4</sup> Report of the Eighth Meeting of the British Association for the Advancement of Science; Held at Newcastle in August 1838. VII (London: John Murray, 1839), xiii.



(1792-1871), also permanent Trustee and General Secretary of the Association. Babbage described the intrigue as follows:

1st. That my Co-trustee, the General Secretary, had, about Christmas last [1837] strongly urged me to allow myself to be recommended by the Council, as President for the ensuing year at Birmingham. [To which proposition I had assented.]<sup>1</sup>

2nd. That the General Secretary had, in the month of June last, without the slightest communication with me, HIMSELF proposed to the Council that they should offer the Presidency to Sir John Herschel. He also fully admitted that he had not mentioned his previous communication with me to any member of that Council, which he induced to recommend Sir John Herschel.

3rd. That in the beginning of August [of 1838]<sup>2</sup> the General Secretary wrote to offer me a Vice-Presidency, and enclosed a letter from Mr. W. Vernon Harcourt to himself, approving of the plan. He also informed me that his Co-Secretary, Mr. Peacock, as well as Mr. Corrie (the Representative of Birmingham), approved of it. But the General Secretary fully admitted that, when he procured these opinions, he had not informed either of those three gentlemen of his previous proposal of the Presidency to myself.<sup>3</sup>

When the Council of the Association failed to reprimand Murchison for his intrigue, Babbage resigned in 1839 with the comment that such acts, "if persisted in, must inevitably lead to the ruin of the Association."<sup>4</sup> As the result of the activities of Murchison,

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<sup>1</sup> Brackets and emphasis added by Babbage.

<sup>2</sup> Brackets inserted by me.

<sup>3</sup> Charles Babbage, Letter From Mr. Babbage to the Members of the British Association for the Promotion of Science (London: Richard Clay, 1839), pp. 7-8.

<sup>4</sup> Ibid., p. 14.

Herschel also declined to be a candidate for President of the Association.<sup>1</sup>

Thus Babbage turned away from taking an active part in the Association affairs except in the section on the mechanical arts "which it was anticipated would be amongst the largest yet called forth by the British Association."<sup>2</sup> He later remarked that the original organization of the Association was defective as developed at York and Oxford since there was no provision for the landed proprietor, members of parliament, the manufacturer or the retailer. Babbage was successful in the development of sections such as the statistical section and the mechanical section and in urging that the Association meet in manufacturing towns--all of these reforms helped the Association to develop.<sup>3</sup>

Thus, the movement to reform science in England, which was begun by Babbage and his friends, was successful in arousing a general concern for the state of science among several different publics--the government officials, the general citizenry, and the scientific community. The British Association was the result of that general concern and particularly the unremitting efforts of Brewster.

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<sup>1</sup> Ibid., p. 5.

<sup>2</sup> Ibid., p. 9.

<sup>3</sup> Babbage, Passages, p. 432.

Babbage took an active role in the Association after the first meeting in 1831. His insistence that the Association should involve the landed proprietor, members of parliament, the manufacturer, the retailer and other members of the middle class was a fundamental change and innovation because it extended the franchise and patronage to a greater portion of the citizenry. By contrast, membership in the Royal Society had traditionally included only those persons who were able to secure the endorsement of existing members of the Society and who were able to pay membership dues. As a result the Royal Society was more of a social club than a scientific society. The British Association membership was open to those who were interested in attending the meetings. Augustus Bozzi Granville stated that the British Association

might become at once 'the Royal Society;' if, in addition to its migratory meetings, it were to determine upon holding two meetings in London (a city at present excluded from its visitations).<sup>1</sup>

Granville wrote the above statement in 1836, just five years after the

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<sup>1</sup> Augustus Bozzi Granville, The Royal Society in the XIXth Century: Being a Statistical Summary of Its Labours During the Last Thirty-Five Years, with Many Original Tables and Official Documents (Never before Published), Shewing the Constitution of the Society--the Character of Its Fellows--Its Various Proceedings--and Pecuniary Expenditure for "Improving Natural Knowledge;" and a Plan for Its Reform, to Which Are Added, Alphabetical and Seniority Lists of the Fellows Since the Year 1800 (Arranged Purposely for This Occasion), (London: Mr. Churchill, Bookseller and Publisher, 1836), pp. 210-211.

first meeting of the British Association at York. His statement was indicative of the success of the British Association on the one hand and the dismal state of the Royal Society on the other. Granville, who had supported the candidacy of the Duke of Sussex for President of the Royal Society in 1830, was not at all satisfied with the efforts to reform the Society. He thought that two things had hampered the reform of the Royal Society--the illness of the Duke who was unable to attend the Council meetings<sup>1</sup> and the refusal of Babbage and the other members of the external coterie to participate in the reform of the Society when invited by the Duke.

In forming the committee, which was to discuss the alteration of the then existing laws of the Society, the council met with considerable difficulties. They wished to associate with themselves in this work, an equal number of fellows, selected from the Society, 'as seemed most able, as well as willing, to give their valuable advice and co-operation;' but of the first who were invited, the most likely to be useful, declined the task. First, Baily--then Harrison--next Herschel, and afterwards Babbage, and Robert Brown, and Fitton, and Penn, and Whewell, and so on--one by one--each alleging some excuse, the best men failed them.<sup>2</sup>

Granville felt, and rightly so, that the external coterie lost a "glorious opportunity" for "immortalizing the joint committee of forty-two, who might have placed the Royal Society on a new and

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<sup>1</sup>Ibid., pp. 178-183.

<sup>2</sup>Ibid., pp. 177-178.

sound basis, worthy of the enlightenment of the present age!"<sup>1</sup> The substitutes, he noted, were less squeamish or particular so that the work of revision and reform went on "right merrily--but not right usefully."<sup>2</sup> However, when viewed from another perspective, the external coterie may well have been justified in refusing to participate in any further efforts to reform the Royal Society, if the task of reforming science were more easily accomplished through the formation of new societies where tradition and political influence were not primary considerations. The success of the external coterie in forming the Cambridge Philosophical Society, the Astronomical Society, and the British Association certainly support the latter view. While the external coterie could have participated both in the reform and in the founding of new societies, it is evident that they came to the conclusion, after several abortive attempts to reform the Royal Society, that the reform of the Society and the reform of science were not necessarily interdependent.

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<sup>1</sup>Ibid., p. 178.

<sup>2</sup>Ibid., p. 178.

General Conclusions to Part I -- Individual and Collective Efforts to Reform Science in England

The political climate necessary to bring about reform had developed slowly in England during the first two decades of the nineteenth century because there existed a general resistance to rapid social, economical, political, and institutional change and a general fear that England might experience an upheaval similar to the French Revolution if it did not exercise restraint. This same cautious attitude toward change was exhibited in the scientific community. Robert Woodhouse, for example, was cautious about introducing his ideas concerning the changes needed in the mathematics taught at the University of Cambridge. Babbage and his friends met with resistance when they formed the Analytical Society because they appeared to the college Dons to be too radical in their claim that English mathematics was vastly inferior to Continental mathematics. Even within the Analytical Society, William Whewell warned against excessive changes in the content of mathematics and the manner in which mathematics was taught.

During the third decade of the nineteenth century the demand for political reform gained increasing general support. The demand for reform of scientific institutions within the scientific community also gained support, while the general restraint represented by Sir Joseph Banks as well as his specific opposition in opposing the formation of

new societies, which he maintained would weaken the Royal Society, rapidly diminished after his death in 1820.

Sir Humphrey Davy, who succeeded interim President William Wollaston, was much more sympathetic to reform within the scientific community and to having the Royal Society exercise leadership in the reform movement. He maintained, as did Babbage and John Herschel, that science had declined in England, and it was during his administration that the scientific reform movement gained general support. As has been noted, Babbage and the other members of the external coterie attempted to reform the Royal Society in 1828 but failed. They tried to elect John Herschel as President of the Royal Society in 1830 and thus carry out their reform, but again they failed. Their reform of the Nautical Almanac and Astronomical Ephemeris was successful, although it took all of the third decade of the nineteenth century to accomplish. This reform was probably easier to effect because it was more technical than political and involved fewer opponents. In contrast, the attempted reforms of the Royal Society failed because of the effective opposition of Davies Gilbert, President of the Royal Society, and his Council.

With a political climate favorable to their reform efforts, Babbage and his fellow reformers effected their reforms based upon the following beliefs: that English science had at least declined relative to science on the Continent if not absolutely; that reform

could be accomplished by changing the content of science taught at the colleges and universities and by altering the membership requirements to and revising the policies of scientific institutions such as the Royal Society; that young men could be encouraged to pursue science as a profession if sufficient patronage were provided by colleges, universities, scientific institutions, and the government; that the scientific elite should be well represented in private and governmental positions; and finally that the responsibility for reform lay with their group.

Although Babbage was often the central figure in the reform efforts of the external coterie, its success in reform resulted from the willingness of different members to assume leadership. For example, James South and Francis Baily were leaders in the effort to reform the Nautical Almanac; Henry Fitton, Edward Ffrench Bromhead, Herschel and Babbage were leaders in the attempt to gain control of the Royal Society; George Peacock and William Whewell in the formation of the Cambridge Philosophical Society; and Babbage and Baily in the formation of the Astronomical Society of London. David Brewster assumed the leadership for the external coterie in founding the British Association at a time when the other members were in despair over their failure to elect Herschel as President of the Royal Society. The general desire for political reform, which resulted in the Reform Bill of 1832, provided a suitable climate for



Brewster's efforts because in both instances the participating membership of each group was enlarged. After the first meeting of the British Association in 1831, the other members of the external coterie overcame their preoccupation with their Royal Society defeat and their reluctance to support Brewster and provided him with useful suggestions and ideas which helped make the Association a success. Whewell provided some useful suggestions as to the goals of the Association and Babbage helped to extend its franchise by encouraging and suggesting that manufacturers and artisans take an active part in the proceedings. In conclusion, the external coterie took full advantage of the favorable climate for reform in their attempt to reverse what they considered to be an undesirable trend--a general decline of science in England.

## PART II -- BABBAGE'S CALCULATING ENGINES

If I survive some few years longer, the Analytical Engine will exist, and its works will afterward be spread over the world. If it is the will of that Being, who gave me the endowments which led to that discovery, that I should not complete my work, I bow to that decision with intense gratitude for those gifts: conscious that through life I have never hesitated to make the severest sacrifices of fortune, and even of feelings, in order to accomplish my imagined mission.<sup>1</sup>

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<sup>1</sup> Charles Babbage, Passages from the Life of a Philosopher, pp. 449-450. This was written in 1864 about seven years before his death.

## VI. BABBAGE'S CALCULATING ENGINES

### General Introduction

The following chapters will be devoted to an analysis of the events, difficulties, speculations and theoretical problems which Babbage encountered in his efforts to construct and develop automatic calculating engines. Our consideration of these efforts fall rather naturally into two phases. The first phase is concerned with the invention and development of Difference Engine No. 1, a special purpose machine capable of evaluating a specific function. In order to proceed with the construction of Difference Engine No. 1, Babbage found it necessary to invent new machine tools, new machine techniques and a new means of symbolic representation of the parts of the engine via detailed mechanical drawings. He soon found that the expense of his project was greater than he had anticipated so that it became necessary to seek funds to support it. He was advised by friends to request support from the Royal Society and from the government. He followed this advice and secured a grant of £1500 from the government with what he thought was an official promise to provide additional funds if needed. Babbage continued to negotiate for governmental support for Difference Engine No. 1 from 1823 to 1842 until governmental officials finally decided to abandon the project. The second phase of his efforts to develop automatic

calculating engines began about 1833, nine years before he completed negotiations with the government over the financial arrangements for Difference Engine No. 1. In 1833, Babbage had ceased work upon Difference Engine No. 1 due to a complex set of circumstances, which will be fully explained below, and began to speculate anew upon the problem of automatic, machine calculation. These speculations gave rise to a completely new set of principles by which it was possible to evaluate any function desired. That is, Babbage had invented a completely automatic, universal calculating engine which he called the Analytical Engine. Babbage continued to work on the Analytical Engine from 1833 until his death in 1871, but he did not complete a working model of the engine beyond a few of its component parts and a complete set of mechanical drawings. In 1848, he drew up a complete set of plans for Difference Engine No. 2 which was based upon the technological improvements and theoretical principles developed for the Analytical Engine.

The primary differences between the two phases in the development of the calculating engines were technological, theoretical, and monetary. In the invention of Difference Engine No. 1, Babbage was concerned with a machine which was relatively simple in its theoretical design, that is, it was capable of calculating a specific function by means of the method of differences which will be explained below. However, the technology for the machine tolerances necessary

for Difference Engine No. 1 did not exist in 1823 and had to be developed before construction of the engine could take place. A major consideration in the development of Difference Engine No. 1 was the time Babbage spent in negotiations with the government over the financing of the engine. These negotiations were complex, tedious, and discouraging to Babbage and took a disproportionate share of his time, which in his opinion would have been better spent on Difference Engine No. 1. In contrast, by the time Babbage began work upon the Analytical Engine in 1834, he had developed the machine technology necessary for the fine tolerances needed in the engine. He did continue to develop new tools as he needed them, but the problem of machine technology did not take as much time, effort, and expense with the Analytical Engine as it had with Difference Engine No. 1. The problem of governmental finance of the Analytical Engine did not distract Babbage's attention from his work, particularly after 1842. The development of the Analytical Engine and Difference Engine No. 2 was done solely at Babbage's own expense. The theoretical problems which Babbage encountered in the development of the Analytical Engine were much more complex than with Difference Engine No. 1, primarily because it was designed as a universal computer which could calculate any function while Difference Engine No. 1, as mentioned above, was designed as a special computer capable of calculating only a single function. Babbage therefore spent much more

time on the complexities of the theoretical design of the Analytical Engine than he had on the simpler Difference Engine No. 1.

### Speculative and Theoretical Considerations Relating to the Calculating Engines

With the general introduction to Babbage's calculating engines as a guide to understanding the schema to be followed in Part II, the following discussion will be devoted to a brief analysis of the speculative and theoretical ideas which guided Babbage as he worked on his engines over a period of approximately fifty years. A discussion of these ideas will hopefully provide a background for a more detailed analysis of the development of the calculating engines in the following chapters.

The technical advances required for the construction of Difference Engine No. 1 and the Analytical Engine were of interest to engineers and other persons employed in the machine tool industry because Babbage spared no effort or expense to achieve the necessary tolerances demanded by the gearing mechanisms of his calculating engines. While such advances were of great interest to persons with a technical background, it was the speculative and theoretical, bordering on the futuristic and even mystical, aspects of the calculating engines which kindled the imaginations of the general public, because Babbage's calculating machines were automatons capable of

performing complex calculations with a speed and accuracy which was beyond the abilities of human beings. Babbage related that on many different occasions he was asked questions which revealed both awe and confusion about the powers and functions of his machines. He stated that on two occasions he had been asked,

'Pray, Mr. Babbage, if you put into the machine wrong figures, will the right answers come out?' In one case a member of the Upper, and in the other a member of the Lower House put this question; I am not able to rightly comprehend the kind confusion of ideas that could provoke such a question.<sup>1</sup>

Babbage considered the calculating functions of his machines more than just a reduction of human thought processes to mechanism. He considered his machines to be capable of duplicating the thought processes because the Analytical Engine, in particular, could act with what he called "foresight" by making logical decisions of its own. While Babbage acknowledged that the capability of foresight was in part built into the machine by himself, it was necessary for the Analytical Engine to perform an act of judgement during an analytical inquiry which he could not have foreseen. The basic problem in such an analytical inquiry, he pointed out, was that "when two or more different courses presented themselves," the "proper course to be adopted could not be known in many cases until all the previous

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<sup>1</sup>Babbage, Passages, p. 67.

portion of the calculation had been gone through."<sup>1</sup> Babbage stated that the solution of a numerical equation of any degree by a method of approximation was an example of such an analytical calculation, since the solution could be determined only by testing on each iteration whether the desired approximation had been reached. The claim that a machine could perform an act of judgement was received with scorn by one anonymous reviewer who claimed that, in addition to his extreme egotism and grumbling,

Mr. Babbage, consciously or unconsciously, mixes up mind and matter in a way which is sure to puzzle less philosophic readers. The workings of the intellect and the workings of his machine are always assumed to be conducted on the same principle. Of his engine, we have seen, he speaks habitually as if it were a thinking, reasoning being. It is, to say the least of it, a little startling to hear 'that mechanism had been taught not only to foresee, but also to act upon that foresight.' Note again the marked way in which he always asserts the superiority of mechanism over mind. His automation is better than a living player at all games of skill. He has, indeed, the grace to confess that the first move must be made by human agency, just as (though he denies a superintending Providence) he believes in the First Cause. But that first move is all that is needed.<sup>2</sup>

The speculations which Babbage made concerning his calculating machines may have been puzzling and seemed excessive to the general public, but he viewed his work in science, mathematics, and engineering from a broad perspective. From the very beginning of his

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<sup>1</sup> Ibid., p. 131.

<sup>2</sup> "Mr. Babbage," The Christian Remembrancer, LI (January, 1866), pp. 10-11.



work with calculating engines in the early eighteen twenties, he decided that it was not enough to write an account of his successes in the invention of such things as machinery and mechanical notation. His hope was that by recording his failures as well as his successes, he might contribute to the future formulation of general laws governing the invention of machinery. He noted that it had been a "condition of our race that we must ever wade through error in our advance toward truth," and that in "many cases we exhaust almost every variety of error before we attain the desired goal."<sup>1</sup> Babbage also maintained that the "progress of the mind in pursuit of a mechanical invention"<sup>2</sup> was more important and of greater value than the most ingenious mechanical invention itself. "Every question," he wrote, "which bears upon the mode in which the mind operates when engaged in the process of invention deserves the most profound attention."<sup>3</sup>

One of the ways in which Babbage attempted to follow the progress of his mind, in both invention of machinery and in mathematical analysis, was through the use of notation. He maintained that the improvement of notation in any field of endeavor was particularly

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<sup>1</sup> Charles Babbage, The Ninth Bridgewater Treatise: A Fragment (London: John Murray, 1838), p. 27.

<sup>2</sup> Charles Babbage, "Description of the Difference Engine," November, 1822, Museum of the History of Science, Oxford, MS Buxton 7, ff. 12-15.

<sup>3</sup> Charles Babbage, "Invention Versus Chance," September 1, 1842, Museum, Oxford, MS Buxton 8, f. 38.

important because language was the most powerful tool which man possessed. He had displayed an interest in language early in his career, as discussed above, when, as an undergraduate at the University of Cambridge, he had worked diligently with Herschel, Peacock, and Bromhead on mathematical notation with the intention of introducing the notation of Leibniz into the calculus taught at Cambridge.<sup>1</sup> In a more general sense, Babbage, Herschel, and Bromhead worked on a branch of mathematical analysis, called by Babbage the calculus of functions, in which they created a mathematical notation to aid their mode of reasoning:

Amongst the various causes which combine in enabling us by the use of analytical reasoning to connect through a long succession of intermediate steps the data of a question with its solution, no one exerts a more powerful influence than the brevity and compactness which is so peculiar to the language employed.<sup>2</sup>

Thus, Babbage maintained that the improvements in the symbols used by man had "materially contributed to the progress of science."<sup>3</sup> In the case of his calculating engines, the use of some kind of symbolic representation was an absolute necessity:

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<sup>1</sup> Babbage and Herschel, Memoirs, p. i.

<sup>2</sup> Charles Babbage, "Observations on the Notation Employed in the Calculus of Functions," Cambridge Philosophical Society Transactions, I (1822), 63.

<sup>3</sup> Ibid., p. 23.

By such means I have succeeded in mastering trains of investigation so vast in extent that no length of years ever allotted to one individual could otherwise have enabled me to control. By the aid of Mechanical Notation, the Analytical Engine became a reality: for it became susceptible of demonstration.<sup>1</sup>

Babbage stated that the problem of tracing the workings of his mind while engaged in mathematical analysis was so difficult it was often not possible for the mind "to extend its grasp to another effort whose difficulty is only rivalled by that which it is employed."<sup>2</sup> By comparison he found that the invention of machinery, aided by mechanical notation, required a small exertion which left the "mind at liberty to attend to the progress of its own operations."<sup>3</sup>

Babbage was motivated to follow the progress of his own mind, as noted above, by his hope that it might be possible to discover general laws governing the invention of machinery. This desire was supported by the belief that general, undiscovered laws existed which governed a broad array of phenomena including thought processes and moral and political affairs. Babbage believed that inductive reasoning offered a means of discovering general laws. He had a great admiration for Francis Bacon (1561-1626), Lord Chancellor of

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<sup>1</sup> Babbage, Passages, p. 113.

<sup>2</sup> Charles Babbage, "On the Difference Engine," November, 1822, Museum, Oxford, MS Buxton 7, ff. 12-13.

<sup>3</sup> Ibid., ff. 12-13.

England under King James I.<sup>1</sup> He urged, as had Bacon, that scientific men should take an active interest in the empirical arts, such as manufacturing and technology in general, but it was his interest in inductive methods which linked him closely to Bacon. Harry Wilmot Buxton, a close friend of Babbage, referred to Babbage's mode of reasoning as "purely analogical and founded on the basis of all human reasoning--a gradual series of generalizations of which the last term is merged in the omnipotence of the Godhead."<sup>2</sup> For example, in 1833, at the request of the government, Babbage assembled the completed portions of Difference Engine No. 1, so that he was able to demonstrate its powers. The results provided by the calculating engine greatly enlarged his "views respecting the laws of Nature" and presented "matter for reflection on the subject of inductive reasoning."<sup>3</sup> Thus, from Babbage's point of view, the calculating engine was now more than just a computational device. It was an analytical tool capable of helping man to form and explore vast inductions. Babbage gave a comparison of the powers of two calculating engines to illustrate the power and importance of his invention. He

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<sup>1</sup> Buxton, Charles Babbage, n.d., Museum, Oxford, MSS Buxton 16-17, f. 2.

<sup>2</sup> Letter, Harry Wilmot Buxton to Charles Babbage, n.d., B.M. Add MSS 37200, XIX, 53.

<sup>3</sup> Babbage, Treatise, pp. 33-34.

asked his reader to imagine a machine which had been set to calculate according to a certain law. After a specified number of terms the operator set the machine to calculate according to a new law. Again, the operator set the machine to calculate according to the original law. Such a machine, while useful, depended upon repeated human intervention. By way of contrast, Babbage asked his reader to conceive of a second machine which could be set initially to calculate according to one law for a specified number of terms, but without human intervention it would begin to calculate according to a new law for n terms. Again, without human intervention, the machine would return to the first law and continue without deviation from that law. Babbage rhetorically asked his reader which of the two machines possessed the greater powers and which gave evidence of greater ingenuity on the part of the inventor. The answer, of course, was the latter machine. Both the Difference Engine and the Analytical Engine were capable of performing as did the second machine. The primary difference between the engines was the greater versatility of the Analytical Engine which could calculate all possible functions instead of just one particular type of function.<sup>1</sup>

Babbage was careful to point out that the ability of the Analytical Engine and the Difference Engine to act without human intervention was

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<sup>1</sup>Ibid., pp. 32-41.

not a miracle. He considered a miracle to be a manifestation of a more general law which was merely unknown to man rather than a deviation from a natural law.<sup>1</sup> Thus, the Analytical Engine constituted a mechanical means extending man's ability to explore apparent laws which were generated by a calculating engine as a seeming exception to the law originally given to the machine. Babbage, was also careful to point out that the apparent law which the machine generated by an "almost unlimited" induction

was not the full expression of the law by which the machine acts: and that the excepted case is as absolutely and irresistibly the necessary consequence of its primitive adjustment, as is any individual calculation amongst the countless multitude which it may previously have produced.<sup>2</sup>

Miracles were, therefore, "not the breach of established laws"<sup>3</sup> but they were the very circumstances that indicated the existence of far higher laws, "which at the appointed time produced their pre-intended results."<sup>4</sup> Babbage maintained that his experiments with calculating engines closely paralleled man's attempt to understand the operation of the universe. The universe offered numerous examples of the operation of laws during immense periods such as "the successive

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<sup>1</sup> Ibid., p. 92.

<sup>2</sup> Ibid., p. 97.

<sup>3</sup> Babbage, Passages, p. 391.

<sup>4</sup> Ibid., p. 391.

creations of animal life, as developed by the vast epochs of geological time. "<sup>1</sup> Each discovery made by man resulted from what he called "the loftiest flights of individual genius, or from the accumulated labours of generations of men. "<sup>2</sup> Science was therefore the result of a group mind which was a continued induction "surpassing in its extent the creative powers of any individual, and demanding for its development a length of time to which no single life extends. "<sup>3</sup> When Babbage invented his calculating engines he had not intended to give them the power of making calculations beyond the reach of mathematical analysis or to aid man in making inductions beyond the life span of a single individual. In fact in 1837, he wrote that he could not foresee a probable period when all the powers of his machines would become "practically available to human purposes "<sup>4</sup>

Babbage's ideas on induction and on miracles were received in 1837 with admiration and argument. <sup>5</sup> Since he based many of his ideas on his experiments with Difference Engine No. 1, his comments

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<sup>1</sup> Ibid., p. 389.

<sup>2</sup> Babbage, Treatise, p. 30.

<sup>3</sup> Ibid., p. 30.

<sup>4</sup> Ibid., p. 97.

<sup>5</sup> Letter, Robert Murphy to Charles Babbage, November 1838, B.M. Add MSS 37191, X, 17. Murphy stated that Babbage's Treatise had created a sensation.

once again brought his work with calculating engines to public notice. At the same time he mentioned in a casual way that his new invention, the Analytical Engine, had powers much greater than the Difference Engine, but he offered no detailed explanation of it.<sup>1</sup> It was typical of Babbage to offer the public a brief, but tantalizing, explanation of his calculating engines. However, as noted by Mary Somerville (1780-1872), author of the widely acclaimed Mechanism of the Heavens,<sup>2</sup> Babbage was always willing to devote time to a detailed explanation of any scientific topic including his calculating engines:

He had a transcendent intellect, unconquerable perseverance, and extensive knowledge on many subjects, besides being a first-rate mathematician. I always found him most amiable and patient in explaining the structure and use of the calculating engines.<sup>3</sup>

Babbage's willingness and ability to discuss the speculative and theoretical aspects of his calculating engines made him much more popular with the public than he might have been if he had had only narrow scientific interests. Even though the calculating engines demanded much of his time, it was characteristic of Babbage

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<sup>1</sup> Babbage, Treatise, p. 187.

<sup>2</sup> [John Herschel], "Mrs. Somerville's Mechanism of the Heavens," in the Quarterly Review, XLVII (July, 1823), 547.

<sup>3</sup> Mary Somerville, as quoted in Personal Recollections from the Early Life to Old Age of Mary Somerville: With Selections from Her Correspondence, by Martha Somerville, (Boston: Roberts Brothers, 1874), p. 140.



throughout his life to become interested in many different and varied pursuits which took his attention away from his calculating engines. For example, his publications from 1832-1871 include: an article on the constants of nature and art, that is, a collection of data on animals such as their relative respiratory rates; barometrical observations; geological treatises on the causes of elevations and depressions of the earth's surface; on the physical surface of the moon, and on impressions in sandstone; papers on ciphering and deciphering; on the boric acid works in Tuscany; an article on the planet Neptune; a paper on the principles of taxation; a note on the pink projections of the sun during a solar eclipse; a paper on the laws of mechanical notation; a paper on occulting lights invented by Babbage to be installed in lighthouses; a description of an ophthalmoscope also invented by Babbage; a paper on submarine navigation; a paper on the principles of tools for turning and planing metals; a paper on a method of laying guns in a battery without exposing men to the shot of the enemy; a paper on the action of ocean currents on the formation of the strata of the earth; a paper on the relative frequency of occurrence of the causes of breaking plate glass windows; three full length books: Treatise in 1837, Exposition in 1851, and Passages in 1864; and a "History of the Analytical Engine" which was never

finished in Babbage's lifetime.<sup>1</sup>

Babbage was well aware of his reputation as a man who had knowledge of many things. As his reputation grew, in part due to his calculating engines, so did requests for information upon many diverse topics such as the metre of some ancient Chinese poem or a question as to whether there were any large rivers on the planet Mercury. Babbage stated that "I have so frequently been mortified by having the utterly undeserved reputation of knowing everything that I was led to inquire into the probable grounds of the egregious fallacy."<sup>2</sup> Although he may have been mortified by his reputation, the universality of his own inquiries into scientific as well as other topics, which resulted in many instances in publications, helped to build such a reputation. The completion of the Analytical Engine was very likely hindered by the many interests to which Babbage devoted his time. William Farr (1807-1883), M.D. and statistician, characterized him as a man who "devoted an intellect of rare power to science."<sup>3</sup> He

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<sup>1</sup> Babbage, Passages, pp. 494-496. The work "History of the Analytical Engine," was incorporated in Babbage's Calculating Engines: Being a Collection of Papers Relating to Them, Their History and Construction ed. by Henry Provost Babbage.

<sup>2</sup> Babbage, Passages, pp. 376-377.

<sup>3</sup> William Farr, Address of Dr. Farr, President of the Statistical Society, Session 1871-72, p. 417.

stated that the "constructive power" of Babbage's intellect "was so affluent that its creations interfered with each other's development."<sup>1</sup>

It was the constructive power of Babbage's intellect which gave rise to the invention of Difference Engine No. 1, the Analytical Engine, and Difference Engine No. 2, and to many speculative and theoretical ideas. The following discussion will be concerned with a detailed analysis of the technical details, the theoretical design, and also the speculative questions which arose in the course of Babbage's work with his calculating engines. Babbage gained much satisfaction from the intellectual challenge afforded by scientific problems which tended to compensate for the many difficulties which he encountered in his efforts to construct his calculating engines.

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<sup>1</sup>Ibid., p. 417.

## VII. DIFFERENCE ENGINE No. 1--ORIGIN AND EARLY DEVELOPMENT

The major portion of Babbage's professional career began in 1820 with his efforts to construct a calculating engine which would calculate and print mathematical and astronomical tables automatically and without error. Initially, the invention and development of a calculating engine appeared to Babbage to be a relatively simple task, but he soon found that the invention of complex machinery involved many unforeseen difficulties. He referred to the problems which he encountered as practical and moral difficulties.<sup>1</sup> The practical difficulties were technical problems such as the construction of new machine tools which were necessary to achieve the unprecedented tolerances required by the calculating engine. The moral difficulties included a resistance by the scientific community to the possibility and practicality of machine calculation and the procurement of long term financial support by the government. Fortunately, the theoretical problems and the speculative possibilities which occurred to Babbage as he worked upon his calculating engines provided him with sufficient satisfaction and encouragement to enable him to continue his task in spite of the practical and moral difficulties which arose.

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<sup>1</sup> Charles Babbage, "The Science of Number Reduced to Mechanism," November, 1822, Museum, Oxford, MS Buxton 3, f. 2.

The idea for a calculating engine first occurred to Babbage as he was sitting in the rooms of the Analytical Society at Cambridge with a table of logarithms opened before him. Babbage wrote that another member of the Society came into the room and seeing him half asleep said,

'Well, Babbage, what are you ~~dream~~ dreaming about?' to which I replied, 'I am thinking that all of the Tables (pointing to the logarithms) might be calculated by machinery.'

I am indebted to my friend, the Rev. Dr. Robinson, the Master of the Temple, for this anecdote. The event must have happened either in 1812 or 1813.<sup>1</sup>

The idea of constructing a calculating machine, according to his friend Harry Wilmot Buxton, occurred to Babbage again while he was involved in a comparative study of the notation and methods of the calculus used by Leibniz with those used by Newton. Babbage, as has been shown in Chapter II, had adopted the notation and methods of Leibniz in his mathematical analysis and rejected Newton's use of motion in explaining the concept of a limit. However, he saw the possibility of using the motion of machinery to carry out the operations of arithmetic and analysis, even though he had rejected motion as an unnecessary and foreign concept in his consideration of pure analysis. Motion could be used, Babbage reasoned, as an expression of the "accretion of a quantity" and could be made to represent any arithmetical quantities by means of figure wheels with the proper

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<sup>1</sup> Babbage, Passages, p. 42.

number of teeth. The figure wheels could then be arranged in columns and the results could be transferred to other columns of figure wheels and finally to paper through "racks" and printing mechanisms. A calculating engine composed of columns of wheels could therefore calculate whatever arithmetic results it had been designed to process.<sup>1</sup>

Babbage did not pursue his idea of constructing a calculating engine until about 1820, when he and John Herschel were appointed as a committee of the Astronomical Society to carry out a set of astronomical calculations. The two men agreed upon the formulae to be used and then hired independent computers to carry out the actual numerical calculations. The number of errors which they found when checking the results of the calculations was very great. Babbage was very concerned over the errors and commented that:

After a time many discrepancies occurred, and at one point these discordances were so numerous, that I exclaimed, 'I wish to God these calculations had been executed by steam,' to which Herschel replied, 'It is quite possible.' We continued our tedious comparison without further comment. The labor and uncertainty of reducing formulae to numbers, and then of calculating tables by means of differences, had however been strongly impressed on my mind, and the uncertainty in the result of such computations with the pen, induced me to enquire whether one part at least, namely that depending on the method of differences might not be accomplished by mechanism.

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<sup>1</sup> Harry Wilmot Buxton, Charles Babbage, n.d., an unpublished biography, Museum, Oxford, MSS Buxton, 16-17, ff. 238-240.

The idea continually presented itself during the few succeeding days, and soon after, having a leisure evening, I resolved on devoting it to the preliminary enquiry.<sup>1</sup>

Babbage continued to work out plans and ideas for a calculating machine in the days following his work with Herschel for the Astronomical Society. He went through periods of both doubt and exhilaration to the extent that the "excitement of the enquiry had an unfavorable effect" upon his "bodily health," and it was recommended that he "abstain entirely for a time from all thought of the Calculating Engine."<sup>2</sup> Following the advice of his physician, he took a few days rest by visiting Herschel at Slough. While Herschel was attending to business in London one day, Babbage again returned to the idea of the calculating engine and proceeded to record his ideas through many transitions. When Herschel returned from London, Babbage explained the principles of his machine to Herschel who remarked "that it was an entirely new machine."<sup>3</sup> Herschel's reaction was not surprising Babbage noted, since only to one who had "watched the gradual transitions which its parts underwent when they were submitted to the principles he had laid down"<sup>4</sup> would the machine appear

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<sup>1</sup> Charles Babbage, "Science of Number," November, 1822, Museum, Oxford, MS Buxton 3, f. 1.

<sup>2</sup> Ibid., f. 1.

<sup>3</sup> Ibid., f. 2.

<sup>4</sup> Ibid., f. 2.

to be the product of the principles upon which it was based. Although the calculating machine had by now "approached something like a practical shape," Babbage stated that had he realized "the multitude of difficulties, both practical and moral, which were destined to attend its course," and "had these not opened upon me by degrees, I might perhaps never have ventured on its execution."<sup>1</sup>

When Babbage returned to London from Herschel's residence at Slough, he immediately attempted to build a small model of a difference engine. This model made use of the principle of motion of machinery to perform arithmetical processes through a series of vertical axes with number wheels which rotated horizontally. Each number wheel had ten teeth to represent the digits from zero to nine inclusive. Each axis with its number wheels was connected to a central gearing mechanism so that the number wheels could be rotated by a crank in order to carry out the desired arithmetical processes. Babbage employed different workmen to construct parts of the machine, but he put this model together with his own hands.<sup>2</sup> The framework of the first model lacked rigidity,<sup>3</sup> but Babbage corrected this defect in his second model which gave him "a visible proof

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<sup>1</sup> Ibid., f. 2.

<sup>2</sup> Ibid., f. 3.

<sup>3</sup> Ibid., f. 3.



of the correctness of the principle, by calculating a few simple tables for those who could judge of its value.<sup>1</sup>

Once he had completed these initial attempts to construct a working model of the calculating engine, Babbage set down the principles he would follow if we were to continue his efforts. He decided that if he were to construct a machine to compute tables, it would have to do more than execute mere isolated operations of arithmetic. Such a machine would be of comparatively little value unless it could be set to work very easily and execute its work accurately and with great rapidity. He wanted a general purpose machine which would compute any table desired by one uniform process, including setting the tables in type or supplying a mold from which stereotype plates could be cast. One of the problems which he encountered in designing the calculating machine was that of adding one digit to the other and also carrying, if necessary, the tens digit to the next higher digit. His first idea was to add each digit successively but since much time would be required if the numbers to be added were quite long, he decided to add all the corresponding digits of two numbers simultaneously. Any carriages were to be retained in a "mechanical memorandum" and executed successively.<sup>2</sup>

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<sup>1</sup> Ibid., f. 3.

<sup>2</sup> Babbage, Passages, p. 42.

A period of over a year elapsed between the time Babbage completed his second model early in 1822 and when he began the construction of his fully operational calculating engine, Difference Engine No. 1. This interim period was spent, as will be discussed in detail below, in an attempt to generate support within the scientific community and the government for Difference Engine No. 1 which was based upon the same principle as the first two models-- the Method of Differences. Difference Engine No. 1 was designed to calculate the function  $\Delta^7 U_z = 0$ , whose integral is

$$U_z = a + bx + cx^2 + dx^3 + ex^4 + fx^5 + gx^6.$$

The constants  $a, b, c, d, e, f$ , and  $g$  were represented on each of the seven columns of number wheels, which like the earlier models had ten teeth to represent the digits from zero to nine. The  $z$  in  $U_z$  merely represented the particular term of the function being calculated. If, for example, the constant  $a$  were equal to 234, it would have been stored on the  $\Delta^1$  column of the engine (see Figure 3), with the 4 on the lowest figure wheel  $10^0u$ , the 3 on figure wheel  $10^1u$ , and the 2 on the figure wheel  $10^2u$ , where  $u$  = the units digit,  $10^1u$  the tens digit, and  $10^2u$  the hundreds digit. If a function  $U_z = N^2$ , that is the square of the natural numbers 1, 2, 3, 4, ..., were desired, the number  $N$  would be inscribed on a portion of the engine and  $N^2$  would be inscribed on what was

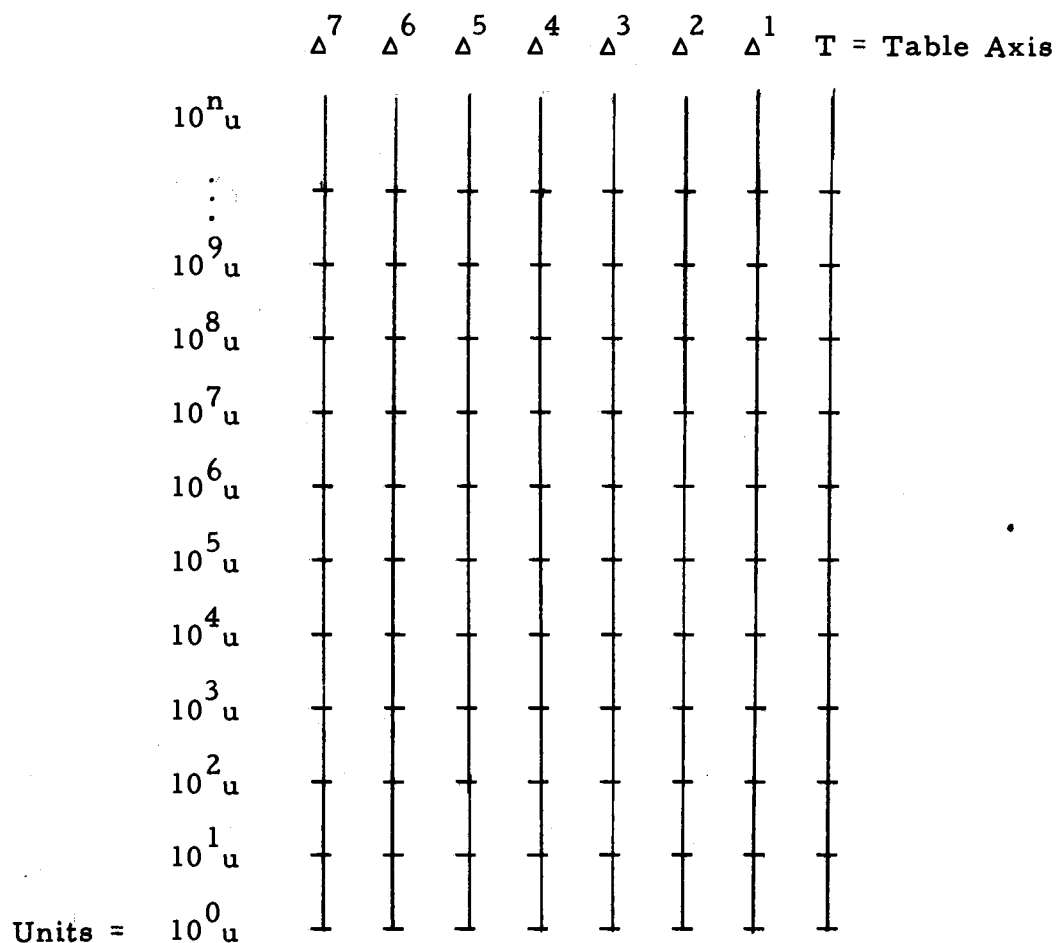


Figure 3. Schematic diagram of Difference Engine No. 1. The vertical bars represent the columns and horizontal bars the number wheels. The engine could calculate the function  $\Delta^7 u_z = 0$  whose integral is  $u_z = a + bx + cx^2 + dx^3 + ex^4 + fx^5 + gx^6$ , where the constants  $a, b, c, d, e, f$  and  $g$  were represented on columns  $\Delta^1, \Delta^2, \Delta^3, \dots, \Delta^h$ , respectively.

referred to as the Table Axis (T). The calculation would then be carried out as illustrated in the following table of square numbers:

Table 1. Table of square numbers.

N	$\Delta^3$	$\Delta^2$	$\Delta^1$	$T = N^2$
1	0	0	0	1
2	0	2	3	4
3	0	2	5	9
4	0	2	7	16
5	0	2	9	25

In the table of square numbers,  $T = N^2$ , was generated by putting the Difference Engine out of gear and manually entering the initial values of the function on the number wheels. Zeroes were placed in the  $\Delta^3$  column and in row one of the  $\Delta^2$ , and  $\Delta^1$  columns. A 1 was placed in row one, column T; a 2 in row two, column  $\Delta^2$ ; and a 3 in row two, column  $\Delta^1$ . Once the initial values were stored in the engine, the table was generated as follows: the 3 in row two, column  $\Delta^1$ , was added to the 1 in row one, column T, to produce the 4 in row two, column T; the 2 in row two, column  $\Delta^2$ , was added to the 3 in row two, column  $\Delta^1$ , to produce the 5 in row three column  $\Delta^1$ ; which, in turn, was added to the 4 in row two, column T, to produce the 9 in row three, column T; and so on until the entire table had been generated.

The following discussion of the Table of Cubes (Table 2) is based upon the actual portion of Difference Engine No. 1, Figure 4, which was assembled in 1833.<sup>1</sup> This portion contained only three columns of number wheels, Figure 5, instead of seven as Babbage had originally intended. However, Difference Engine No. 1 was so well designed that it was possible to add or delete columns of number wheels without adversely affecting its operation. In the portion of Difference Engine No. 1, shown in Figure 4, the natural numbers, 1, 2, 3, . . . , were represented by the three upper-wheels on the left hand axis; the column of third differences by the bottom wheel on the central axis, the second differences by the three lower wheels on the left hand axis; the first differences by the wheels on the central axis; and that of the tabular numbers (in this instance, cubes) by the wheels on the right hand axis.<sup>2</sup>

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<sup>1</sup> B[enjamin] Herschel Babbage, Babbage's Calculating Machine: or Difference Engine: A Description of a Portion of This Machine Put Together in 1833 and Now Exhibited in the Science Collections for Teaching and Research, Victoria and Albert Museum (London: Wyman and Sons, Limited, 1872), pp. 1-2.

<sup>2</sup> Ibid., p. 8.

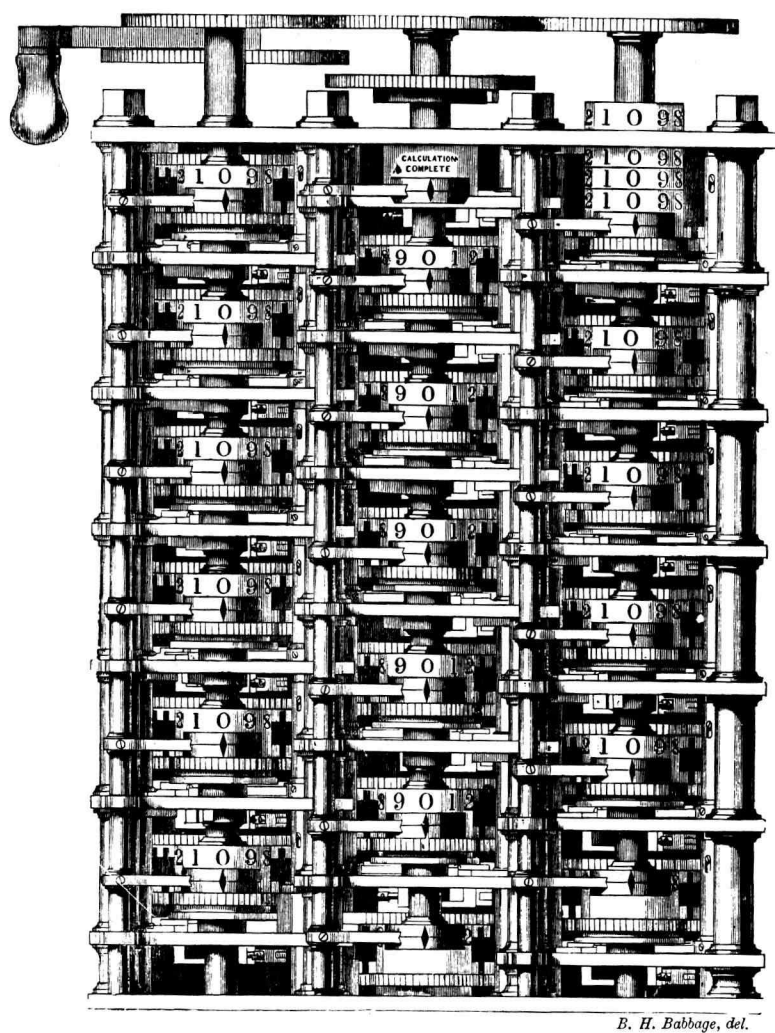
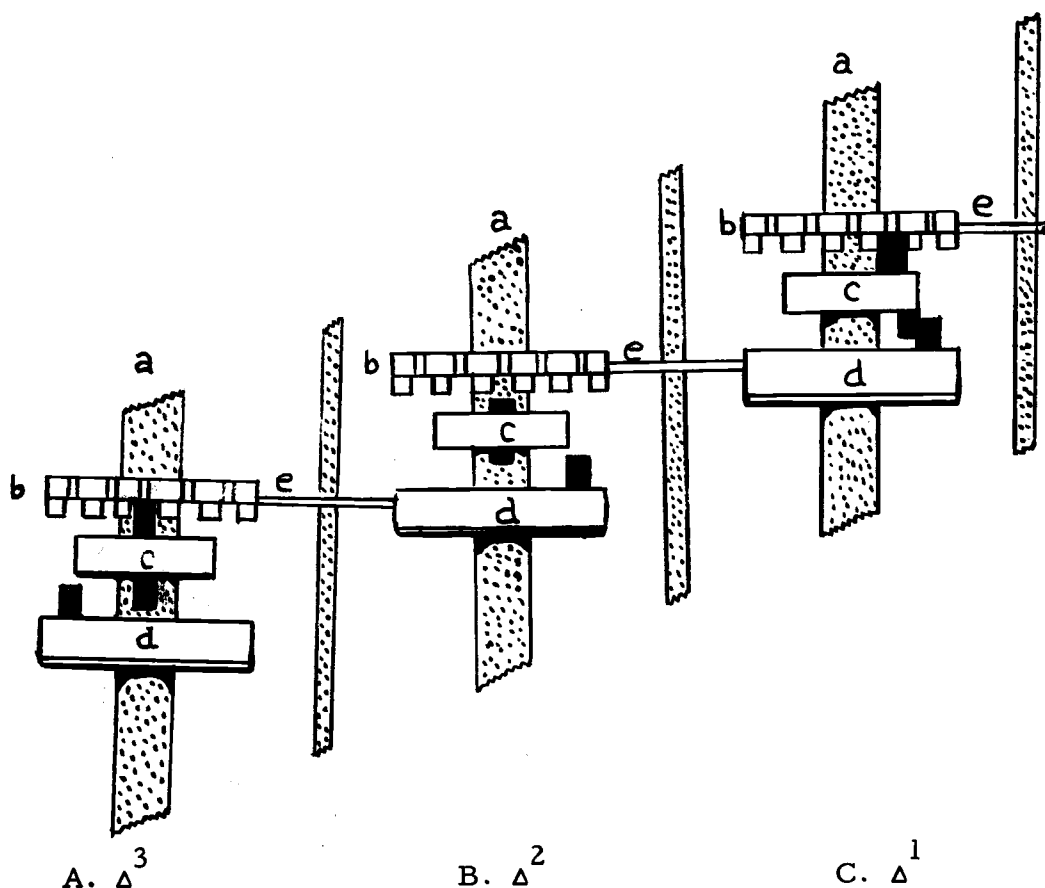


Figure 4. Impression of Difference Engine No. 1. From the frontispiece of Passages from the Life of a Philosopher, by Babbage.



Each axis contained three wheels:

a--axis.

b--adding wheel or number wheel with protruding crown teeth below and spur teeth on the edge of the wheel.

c--bolting wheel with sliding bolt and a wedge as shown on the lower side in Axis C.

d--unbolting wheel with a bolt on the upper side which is shown engaging the wedge on the bolting wheel c in Axis C.

e--claw which engaged the spur teeth on the adding wheel b and transmitted power to another axis.

Figure 5. Diagram of the axis of the Difference Engine. From L. H. Dudley Buxton, "Charles Babbage and His Difference Engines," The Newcomer Society for the Study of the History of Engineering and Technology, Transactions, XIV (1933-1934), 54.

Table 2. Table of cube numbers.

Natural Numbers $u_n$	Orders of Differences			Tabular Number T
	Third Difference $D^3 = \Delta^3$	Second Difference $D^2 = \Delta^2$	First Difference $D^1 = \Delta^1$	
	Bottom Wheel on Central Axis	Lower Wheels on Left Hand Axis	Wheels on Central Axis	
In Upper Left Hand Corner of Machine				Wheels on Right Hand Axis
1	6	6	1	1
2	6	12	7	8
3	6	18	19	27
4	6	24	37	64
5	6	30	61	125
6	6	36	91	216
7	6	42	127	343
8	6	48	169	512
9	6	54	217	729
10	6	60	271	1000
.	.	.	.	.
.	.	.	.	.
.	.	.	.	.

The mechanism of the Difference Engine was designed so that, whatever numbers were placed on the figure wheels of each of the three columns, the following operations took place as long as the handle above the third column was moved.

1st. Whatever number is found upon the column of first differences will be added to the number found upon the Table column [Table 2, column T],

2nd. The same first difference remaining upon its own column, the number found upon the column of second difference will be added to that first difference.

It appears, therefore, that with this small portion of the Engine any Table may be computed by the method



of differences, provided neither the Table itself, nor its first and second differences, exceed five places of figures.<sup>1</sup>

In Table 2, the number 7, in row two, column  $D^1$ , was added to the 1, in row one, column T, to obtain an 8, in row two, column T; the number 6, in row one, column  $D^2$ , to the 1, in row one, column  $D^1$ , to obtain the 7, in row two, column  $D^1$ ; and the number 6, in row one, column  $D^3$ , to the 6, in row one, column  $D^2$ , to obtain the 12, in row two, column  $D^2$ ; and so on until the entire table as shown was generated. The last series of numbers in column  $D^3$  for any given function was either a series of constants as in this example or a series in which the last difference consisted of numbers which remained constant to a given number of decimal places for a long succession of terms. In calculating a table of logarithms for example, the logarithms of numbers were usually calculated to seven places of figures. Even though no constant difference could be found, the Difference Engine could still be used to calculate a table of logarithms. In order to do so, the Difference Engine would have first calculated a table of logarithms to twenty places but would have printed only the first seven figures, "making the printed result true to the last figure by a special contrivance."<sup>2</sup>

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<sup>1</sup>Charles Babbage, Passages, p. 65.

<sup>2</sup>B. Herschel Babbage, Babbage's Calculating Machine, p. 8.

Babbage was particularly concerned that Difference Engine No. 1 should give results that were accurate and free from error through human interference. That he should have been so concerned is clear from an examination of existing mathematical tables in which errors were produced by independent teams of human computers as well as by the press:

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. A friend of mine, whose skill in practical as well as theoretical astronomy is well known, produced me a copy of the tables published by order of the French Board of Longitude, containing those of the Sun by Delambre, and of the Moon by Burg, in which he had corrected above five hundred errors: most of these appear to be errors of the press; and it is somewhat remarkable, that in turning over the leaves in the fourth page I opened we observed a new error before unnoticed. These errors are so much more dangerous, because independent computers using the same tables will agree in the same errors.<sup>1</sup>

Francis Baily shared Babbage's concern that tables should be free of error. Baily, who had urged Young to reform the Nautical Almanac, had laboriously reduced the observations of various

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<sup>1</sup> Charles Babbage, "A letter to Sir Humphrey Davy, Bart., President of the Royal Society, &c. &c., on the Application of Machinery to the Purpose of Calculating and Printing Mathematical Tables, from Charles Babbage, Esq., M. A., F. R. S., Lond. and Edin., Member of the Cambridge Philosophical Society, Secretary of the Astronomical Society of London, and Correspondent of the Philomathic Society of Paris. July 3, 1822," in Engines, ed. by Henry P. Babbage, pp. 212-213.

astronomers as an act of service to the Astronomical Society. He was therefore aware of the problems of producing tables by hand calculation and the need for a machine which could produce a variety of tables quickly, cheaply and accurately:

This invention of Mr. Babbage's is one of the most curious and important in modern times: whether we regard the ingenuity and skill displayed in the arrangement of the parts, or the great utility and importance of the results. Its probable effect on those particular branches of science which it is most adapted to promote, can only be compared with those rapid improvements in the arts which have followed the introduction of the steam-engine; and which are too notorious to be here mentioned.<sup>1</sup>

Baily also pointed out the various problems to which the calculating engine could be applied in mathematics and astronomy and the different types of tables which could be produced:

The great object of all tables is to save time and labour, and to prevent the occurrence of error in various computations. The best proof of their utility and convenience is the immense variety that had been produced since the origin of printing; and the diversity of those which are annually issuing from the press.

The general tables, formed for the purpose of assisting us in our computations, may be divided into two classes: 1°, those consisting of natural numbers; 2°, those consisting of logarithms. Of the former kind are the tables of the products and powers of numbers, of the reciprocals of numbers, of the natural signs, cosines, &c. &c. Of the latter kind are not only the usual

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<sup>1</sup> Francis Baily, "On Mr. Babbage's New Machine for Calculating and Printing Mathematical and Astronomical Tables," in Engines, ed. by Henry P. Babbage, p. 225. This article was originally published in M. Schmacher's Astronmische Nachrichten, No. 46, and later reprinted in the Philosophical Magazine, for May of 1824.

logarithmic tables, whose utility and importance are so well known and duly appreciated, but also various other tables for facilitating the several calculations which are constantly required in mathematical and physical investigations.

. . . . .  
 Besides the general tables above alluded to, there are many others which are applicable to particular subjects only: the most important of which are those connected with ~~astronomy~~ astronomy and navigation. When we contemplate the ease and expedition with which the seaman determines the position of his vessel, and with what confidence he directs it to the most distant quarter of the globe, we are not perhaps, aware of the immense variety of tables which have been formed almost solely for his use, and without the aid of which he dare not venture on the boundless ocean. . . .

In the calculations of astronomical tables the machine will be of very material assistance: not only because an immense variety of subsidiary tables are required to determine the place of the sun, moon, planets, and even of the fixed stars, but likewise on account of the frequent change which it is found necessary to introduce in the elements from which those tables are deduced, and which vary from time to time according to the improvements in physical astronomy and the progress of discovery.<sup>1</sup>

In order for Babbage to construct a calculating engine which would produce tables which were free of errors, he had to develop the machine technology which would enable him to achieve a high degree of precision in the planing and cutting of machine parts. Extremely fine tolerances were needed in order to insure that the engine would perform the arithmetical operations without error. If two gears did not engage properly or if the tolerances were not met, it would not

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<sup>1</sup>Ibid., p. 226.

have been possible to add two digits such as 3 and 4 and obtain a unique answer of 7. Instead, answers such as  $6\frac{1}{2}$  or  $7\frac{1}{2}$  might have been obtained. Babbage therefore made a contribution to the machine tool industry by constructing machine tools such as lathes capable of a higher degree of precision and versatility than existed previously. One particularly important tool was a coordinate machine which was capable of moving copper plate and steel punches in three dimensions -- three rectangular coordinates. He found this machine useful as a general shaping machine in the production of many parts of the Difference Engine.<sup>1</sup>

Babbage also found it necessary to prepare detailed mechanical drawings which were described at the time as "among the best that have ever been constructed."<sup>2</sup> The mechanical drawings, like the invention of new machine tools, were very expensive and added to the total cost of the calculating engine as well as demanding much of Babbage's time. However, Babbage found such drawings absolutely necessary, because, as he described it, "the complicated relations which then arose amongst the various parts of the machinery would

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<sup>1</sup> John Herschel, "Report of the Committee Appointed by the Council of the Royal Society, to Consider the Subject Referred to in Mr. Stewart's Letter, Relative to Mr. Babbage's Calculating Engine, and to Report Thereupon," Journal of the Franklin Institute of the State of Pennsylvania; Devoted to Mechanic Arts, Manufacturers, General Science and Recording of American and Other Patented Inventions, N.S. VII (1831), 211.

<sup>2</sup>Ibid., p. 211.

have baffled the most tenacious memory."<sup>1</sup> He found it necessary to invent his own system of mechanical notation which would describe the particular motion and function of each part of the machine. He again made a contribution to mechanical engineering by setting a new standard for mechanical drawing, since most engineers made only rough sketches of their machines.

The care and effort which Babbage bestowed on Difference Engine No. 1 in order to develop it into a precision machine left little doubt in the minds of those who examined it closely, that it would fill all the expectations of the inventor.<sup>2</sup> Thus, from a technical point of view the calculating engine was a success, but the very reasons for its success in a technical sense caused Babbage to be severely criticized from a political point of view. The development of precision machinery cost far more and took much longer than he had anticipated. Babbage began Difference Engine No. 1 only after he had secured a grant from the government, a circumstance which added political considerations to the invention of the calculating engine. The political problems which Babbage encountered were very complex in nature and will, because of their complexity and importance to the fate of Difference Engine No. 1, be discussed in detail in the following two chapters.

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<sup>1</sup>Babbage, Passages, p. 113.

<sup>2</sup>Herschel, "Report of the Committee," Journal of the Franklin Institute, N.S., VII (1831), 212-213.

# VIII. DIFFERENCE ENGINE NO. 1--THE ISSUE OF GOVERNMENTAL SUPPORT

The theoretical and technical problems which Babbage encountered in the development of Difference Engine No. 1 were simple in comparison to the problems he encountered in securing support for his project. He was aware from the beginning of his work on calculating engines that some of his fellow scientists would view his efforts with scepticism, but, as noted above, he stated that if he had realized the multitude of difficulties he was to encounter he would probably not have continued in his efforts to develop a calculating engine. In order to allay the scepticism of some of his fellow scientists, Babbage carefully explained the current and future needs for mathematical and astronomical tables and the use of machinery for their production in a series of short publications in the early 1820's. In particular, he wrote a letter to Sir Humphrey Davy in July of 1822 in which he explained in detail the rationale for using machinery in calculating and printing mathematical tables. In this letter Babbage made predictions concerning both the cost and future utility of calculating engines by noting the following:

To bring to perfection the various machinery which I have contrived, would require an expense both of time and money, which can be known only to those who have themselves attempted to execute mechanical inventions. Of the greater part of that which has been mentioned, I have at present contented myself with sketches on paper, accompanied by short memorandums, by which I might at any time more fully develop the contrivances; and where

any new principles are introduced, I have had models executed, in order to examine their actions.

I am aware that the statements contained in this Letter may perhaps be viewed as something more than Utopian, and that the philosophers of Laputa<sup>1</sup> may be called up to dispute my claim to originality. Should such be the case, I hope the resemblance will be found to adhere to the nature of the subject rather than to the manner in which it has been treated. Conscious, from my own experience, of the difficulty of convincing those who are but little skilled in mathematical knowledge, of the possibility of making a machine which shall perform calculations, I was naturally anxious, in introducing it to the public, to appeal to the testimony of one so distinguished in the records of British science. Of the extent to which the machinery whose nature I have described may be carried, opinions will necessarily fluctuate, until experiment shall have finally decided their relative value; but of that engine<sup>2</sup> which already exists I think I shall be supported, both by yourself and by several scientific friends who have examined it, in stating that it performs with rapidity and precision all those calculations for which it was designed.

Whether I shall construct a larger engine of this kind, and bring to perfection the others I have described, will in a great measure depend on the nature of the encouragement I may receive.

Induced, by a conviction of the great utility of such engines, to withdraw from some time my attention from a subject on which it has been engaged during several years, and which possesses charms of a higher order, I have now arrived at a point where success is no longer doubtful. It must, however, be attained at a very considerable expense, which would not probably be replaced, by the works it might produce, for a long period of time, and which is an undertaking I should feel unwilling to commence, as altogether foreign to my habits and pursuits.<sup>3</sup>

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<sup>1</sup> Laputans were inhabitants of a flying island in Johnathan Swift's Gulliver's Travels. They were characterized by a neglect of useful occupations and a devotion to visionary projects.

<sup>2</sup> Babbage was speaking of the model discussed in Chapter VII which he built during the years from 1820 to 1822.

<sup>3</sup> Babbage, "Letter to Sir Humphrey Davy," in Engines, ed. by Henry P. Babbage, pp. 213-215.



While Babbage stated that he was unwilling to commence work on such a vast, costly and formidable project as the development of a calculating engine, he was definitely convinced that such a machine should be developed. It was quite unlikely that he would have willingly relinquished the opportunity to produce a calculating engine to some other willing and able scientist even though mechanical engineering was foreign to his habits and pursuits. On the other hand, he was, by his own admission, reluctant to give up his purely mathematical pursuits.

One of Babbage's friends, Olinthus Gregory (1774-1841), a mathematician, recognized that if Babbage were to construct a calculating machine, he would need the political and scientific support of his friends in order to secure financial aid from the government. He wrote that he hoped Babbage could get Davies Gilbert and other friends to use their influence "to obtain an adequate grant from the Government to complete and render extensively effectual the whole of your curious inventions."<sup>1</sup> Bromhead, Babbage's friend from his undergraduate days, cautioned him not to undertake the "trouble and expense in forming one of the larger Engines"<sup>2</sup> by himself. Bromhead

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<sup>1</sup> Letter, Olinthus Gregory to Charles Babbage, July 16, 1822, B.M. Add MSS 37182, I, 425.

<sup>2</sup> Letter, Edward Ffrench Bromhead to Charles Babbage, August 20, 1822, B.M. Add MSS 37182, I, 431.

thought that it would be unnecessary for Babbage to actually produce a machine to gain fame. He stated that a

Memoir in the Phil. Trans<sup>1</sup> containing the particulars of your Method will give all the fame you ever can procure by it. Napier and Newton did not increase their glory by making Rods and Reflectors.<sup>2</sup>

Bromhead gave some practical advice which supported that given by Gregory:

I have always objected to your undertaking your machine yourself, it would have been just and would have had a better effect, that you should have thrown out the Principle, and a Committee of scientific men taken it up. You should have two courses in view, on the Parliamentary Inquiry, 1st a remunerable Grant, such as was granted to Dr. Fenner and many others, 2<sup>ly</sup> an annual Grant of 5 or 10 thousand a year to the Board of Longitude, for scientific purposes. Either of these would be carried, but you will find it hard to persuade Parliament to address the Crown for the manufacture of a Machine. The addresses for printing the National Records are almost the only cases in point. I shall mention the Business among my friends who may be in Parliament.<sup>3</sup>

If Babbage had followed the advice given above by Bromhead, the whole course of events surrounding his work with the calculating

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<sup>1</sup> Philosophical Transactions of the Royal Society of London.

<sup>2</sup> Letter, Edward Ffrench Bromhead to Charles Babbage, August 20, 1822, B.M. Add MSS 37182, I, 431.

<sup>3</sup> Letter, Edward F. Bromhead to Charles Babbage, n.d., B.M. Add MSS 37182, I, 433. A date had been entered on this letter as [August 10, 1822], but from the context it would appear to be later than the letter noted above from Edward Ffrench Bromhead to Babbage, August 20, 1822, ibid., f. 431.

engines as well as his other scientific activities might have been altered. A "remunerable Grant," would have insured adequate compensation for the calculating machine while an "annual Grant" to the Board of Longitude, for Babbage's use, would have provided him with sufficient funds not just for a single invention but also for the broad spectrum of scientific pursuits which he subsequently undertook. A committee of scientific men might have placed the matter on a purely scientific basis rather than on a personal basis between Babbage and the government. Also, such arrangements might have promoted governmental support of science as a profession by setting a precedent for the support of scientific investigations by an individual scientist. Babbage was hopeful, but also doubtful, that he would receive governmental support, as shown in a letter of August, 1822:

I hope then [October of 1822] to receive sufficient encouragement from Government to complete the machinery I have contrived. If, as is not impossible from the economical task of the ministry, I should not meet with support, I scarcely think I shall be induced to sacrifice fortune as well as time to the construction of machinery however curious. Unfortunately none of the members of our government have the least scientific taste or knowledge.<sup>1</sup>

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<sup>1</sup> Letter, Charles Babbage to Jean Baptiste Biot (1774-1862), August, 1822, B.M. Add MSS 37182, I, 441. Babbage included copies of the letter to Sir Humphrey Davy for Laplace, Delambre, Poisson, Pronward, Arago, Humboldt and Legendre.

The events which followed fully supported Babbage's pessimistic view of governmental officials, but he might have saved himself from much grief if he had set aside all other matters until he had gained the political support needed to finance his calculating engine.

Davies Gilbert was asked, as Vice President of the Royal Society and as a member of Parliament, to bring the matter to the attention of the government. Gilbert did bring the problem of financing the calculating engine to the attention of Sir Robert Peel (1788-1850), the First Lord of the Treasury, who in turn asked the advice of John Wilson Croker, the Secretary to the Admiralty and a severe critic of the state of the Nautical Almanac in 1818. Peel's letter to Croker, on March 8, 1823 was written in a light vein as follows:

You recollect that a very worthy seafaring man declared that he had been intimate in his youth with Gulliver, and that he resided (I believe) in the neighborhood of Blackwell. Davies Gilbert has produced another man who seems to be able to vouch at least for Laputa. Gilbert proposes that I should refer the enclosed to the Council of the Royal Society, with the view of their making such a report as shall induce the House of Commons to construct at the public charge a scientific automaton, which, if it can calculate what Mr. Babbage says it can, may be employed in the destruction of Hume.<sup>1</sup>

Peel concluded his letter by asking Croker to provide him with information on the calculating engine before he brought the matter to

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<sup>1</sup> Louis J. Jennings, ed , The Croker Papers: The Correspondence and Diaries of the Late Right Honourable John Wilson Croker, LL.D., F.R.S., Secretary to the Admiralty from 1809 to 1830, Vol. I (London: John Murray, 1884), pp. 262-263.

the attention of the House of Commons. From the comments made by Croker in his reply to Peel, some of Bromhead's suggestions for financial support were probably considered and some followed.

Croker's reply to Peel mentioned Gilbert's proposition of having a new machine constructed. It is particularly ironic that Croker, who had expressed indignation about the large number of errors in the mathematical and astronomical tables in the Nautical Almanac in 1818, doubted the utility of the machine and thought that the model Babbage had constructed was sufficient for the purpose for which it was intended. Croker admitted that he had not seen the model. However, he suggested that a sum of money given to Babbage to "reward his ingenuity, encourage his zeal and repay his expense would lead eventually to the perfection of his machine."<sup>1</sup> He mentioned that a proposal to the Board of Longitude to give Babbage £500 for a sum designated for inventions was not approved because the Board doubted that the machine "was likely to be practically useful to a degree to justify a grant of this nature."<sup>2</sup> Croker suggested referring the matter to a committee of the Royal Society,<sup>3</sup> to which he belonged as a non-scientific member, and relying upon the judgment of the

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<sup>1</sup> Letter, J[ohn] W[ilson] C[roker] to Sir Robert Peel, March 21, 1823, ibid., pp. 263-264.

<sup>2</sup> Ibid., p. 264.

<sup>3</sup> Ibid., p. 264.

committee<sup>1</sup> in making a decision as to whether to recommend a grant from Parliament.

On April 1, 1823, the Lords of the Treasury at Peel's request referred Babbage's letter to Sir Humphrey Davy to the Royal Society requesting "The opinion of the Royal Society on the merits and utility of this invention."<sup>2</sup> The Royal Society reported back to the Treasury on May 1, 1823, that:

'Mr. Babbage has displayed great talent and ingenuity in the construction of his Machine for Computation, which the Committee think fully adequate to the attainment of the objects proposed by the inventor; and they consider Mr. Babbage as highly deserving of public encouragement in the prosecution of his arduous undertaking.'<sup>3</sup>

This letter was followed by activity within the House of Commons concerning the calculating engine. Gregory was asked to

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<sup>1</sup>This committee was composed of "Sir H[umphrey] Davy, Mr. Brande, Mr. Combe, Mr. Francis Baily, Sir Mark Isambard Brunel, General Colby, Mr. Davies Gilbert, Sir John Herschel, Captain Henry Kater, Mr. Pond (Astronomer Royal), Dr. Wollaston, and Dr. Young," Charles Richard Weld, Eleventh Chapter of the History of the Royal Society with Additions (London: Richard Clay, Printer, 1849), pp. 371-372. Weld stated, "I am informed on good authority, that Dr. Thomas 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 such a machine as was proposed, in the funds, and apply the dividends to pay for calculations."

<sup>2</sup>Nicas Harris Nicolas, "Statement Relative to the Difference Engine," in Passages, by Charles Babbage, p. 69.

<sup>3</sup>Parliamentary Paper, No. 370, printed May 22, 1823, quoted in Nicolas, "Statement Relative to the Difference Engine," in Passages by Charles Babbage, p. 69.

give evidence before a committee in the House of Commons,<sup>1</sup> and a suggestion was made to call in some mathematicians from Scotland to consider the matter.<sup>2</sup> Francis Baily suggested to Babbage that he assemble all the information he could find on all former machines which had been constructed for calculating. He thought that such a suggestion would probably come from the "country gentlemen on the committee" and that the information would be valuable "to show how far they went and why they failed."<sup>3</sup> Baily added,

A celebrated mathematician, who has seen your machine, says it would take as much time to make calculations with the pen!!! -- You see how difficult it is to lead the public.<sup>4</sup>

Gilbert also confirmed the general unwillingness by the administration to support inventions but thought that he could persuade them to change their minds.<sup>5</sup> He was evidently slow to do so because Bromhead inquired of Babbage as to why Gilbert had not introduced

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<sup>1</sup> Letter, Olinthus Gregory to Charles Babbage, May 13, 1823, B.M. Add MSS 37183, II, 26.

<sup>2</sup> Letter, Davies Gilbert to Charles Babbage, May 17, 1823, B.M. Add MSS 37183, II, 28. In a letter to Babbage, June 24, 1826, David Brewster, B.M. Add MSS 37183, II, 49, wrote that he had been contacted to give testimony in the House of Commons on the utility of the calculating engine. Thus the suggestion became a reality.

<sup>3</sup> Letter, Francis Bailey to Charles Babbage, May 26, 1823, B.M. Add MSS 37183, II, 30.

<sup>4</sup> Ibid., f. 30.

<sup>5</sup> Letter, Davies Gilbert to Charles Babbage, May 28, 1823, B.M. Add MSS 37183, II, 32.

a motion in Babbage's "affair" as he had promised. He wanted Babbage to let him know of Gilbert's progress or lack of progress in the matter.<sup>1</sup>

Besides this support given him by the Royal Society and his friends, Babbage received the official sanction of the Astronomical Society of London. Having had success with group efforts in forming the Analytical Society and the Astronomical Society, Babbage probably hoped for similar support and success in the development of calculating engines. The support which the Astronomical Society gave was in the form of a recognition of the importance of the calculating engine in the production of accurate mathematical and astronomical tables which could be revised quickly and cheaply. On July 13, 1823 a Gold Medal was presented to Babbage by Henry Thomas Colebrooke, President of the Astronomical Society of London. Colebrooke briefly traced the history of previous attempts to develop mechanical calculators but noted that Babbage's "invention is in scope, as in execution, unlike anything before accomplished"<sup>2</sup> to assist computation. He also recognized the importance of the

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<sup>1</sup>Letter, Edward Ffrench Bromhead to Charles Babbage, June 9, 1823, B. M. Add MSS 37183, II, 37.

<sup>2</sup>Henry Thomas Colebrooke, "Address of Henry Thomas Colebrooke, Esq., F.R.S., President of the Astronomical Society of London: On Presenting the Gold Medal to Charles Babbage, Esq., F.R.S.: Awarded 13th July, 1833," in Engines, ed. by Henry P. Babbage, p. 233.



Difference Engine as an aid to astronomical research as well as for the calculation of mathematical and astronomical tables:

In no department of science or of the arts does this discovery promise to be so eminently useful as in that of astronomy and its kindred sciences, with the various arts dependent on them. In none are computations more operose than these which astronomy in particular requires: in none are preparatory facilities more needful: in none is error more detrimental. The practical astronomer is interrupted in his pursuit, is diverted from his task of observation, by the irksome labour of computation, or his diligence in observing becomes ineffectual for want of yet greater industry of calculation. Let the aid, which tables previously computed afford, be furnished to the utmost extent which mechanism has made attainable through Mr. Babbage's invention, the most irksome portion of the astronomer's task is alleviated, and a fresh impulse is given to astronomical research.<sup>1</sup>

Astronomers were in a particularly advantageous position to appreciate the burden of tedious and complex calculations, but Colebrooke went beyond the field of astronomy in recognizing, as did Babbage, that calculating engines were important to science as a whole.

It may not therefore be deemed to sanguine an anticipation, when I express the hope, that an instrument, which in its simpler form attains to the extraction of the roots of numbers and approximates to the roots of equations, may in a more advanced state of improvement rise to the approximate solution of algebraic equations of elevated degrees. I refer to solutions of such equations proposed by La Grange, and more recently by other analysts, which involve operations too tedious and intricate for use, and which must remain without efficacy, unless some mode be devised of abridging the labour or

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<sup>1</sup>Ibid., p. 224.

facilitating the means of its performance. In any case this engine tends to lighten the excessive and accumulating burden of arithmetical application of mathematical formulae, and to relieve the progress of science from what is justly termed by the author of this invetion, the overwhelming incumbrance of numerical detail.

For this singular and pregant discovery, I have the authority of the Astronomical Society of London to present to Mr. Babbage its Gold Medal, which accordingly I now do, as a token of the high estimation in which it holds his invention of an engine for calculating mathematical and astronomical tables.<sup>1</sup>

The statement, by Babbage, to which Colebrooke referred with regard to the "overwhelming incumbrance of numerical detail" which would eventually impede the progress of science was essentially the same statement Babbage made to David Brewster in November of 1822. Babbage stated that if the government did not provide any encouragement to produce the calculating engine the progress of science would be impeded to a point where some equivalent method would be necessary to relieve the burden of numerical detail.<sup>2</sup>

Unfortunately for Babbage only in the field of astronomy was there a recognition that the burden of numerical detail was already present. However, in all fairness to the committee of the Royal Society, it should be noted that its report, on May 1, 1823, to the Lords of the Treasury, gave him hope that he would be able to obtain the money he

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<sup>1</sup> Colebrooke, "Address of Henry Thomas Colebrooke," in Engines, by Henry P. Babbage, p. 224.

<sup>2</sup> Letter, Charles Babbage to David Brewster, November, 1822, B.M. Add MSS 37182, I, 458.

needed to finance the construction of Difference Engine No. 1. As noted above, the committee praised Babbage for his display of great talent and ingenuity and stated that he was "highly deserving of public encouragement in the prosecution of his arduous undertaking."<sup>1</sup> Thus encouraged, Babbage arranged for an interview in July of 1823 with the Chancellor of the Exchequer, Frederick John Robinson (1782-1859).<sup>2</sup> The sequence of events, to which this meeting gave rise, determined the direction of Babbage's lifetime efforts and became the source of many of his frustrations. The meeting was held "to ascertain if it was the wish of the Government,"<sup>3</sup> that Babbage should construct a large engine, based upon the two prototypes he had constructed, which would print the results it calculated.

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<sup>1</sup>Quoted in Nicolas, "Statement Relative to the Difference Engine," in Passages, by Babbage, p. 69.

<sup>2</sup>As indicated by a letter to Babbage from William Brougham, n.d., B.M. Add MSS 37183, II, 99, Brougham urged Babbage to get an appointment with Robinson. As noted, this letter was not dated, but from the context of the letter it was probably written prior to Babbage's meeting in July of 1823 with Robinson. Brougham wrote, in reference to financial support of the calculating engine, that "Robinson had got it into his head that 800 or £1000 will be the outside of what you will require and that he talked of advancing about £700 to you . . . . I think the sooner you call on Robinson, the better. There will be no impropriety in using my brother's name [Lord Henry Brougham] to Robinson as he understands that the matter is to have my brother's support when it is mentioned in the H[ouse] of Com[mons]."

<sup>3</sup>Letter, Charles Babbage to the Duke of Wellington, Prime Minister, and Lord Goderich (Frederick John Robinson), Chancellor of the Exchequer, December 16, 1828, B.M. Add MSS 37184, III, 163-165.

The manner in which the meeting was conducted was indicative of the lack of experience of both Babbage and Robinson, at this time, in matters concerning governmental aid to science. Robinson stated that in general the government was unwilling to make grants for inventions, since if the inventions turned out to be successful then the monetary rewards would more than pay the inventor for his efforts. He considered the calculating engine an exception in that it was apparent that the engine could not be constructed for a profit; "and that, as the tables it was intended to produce were peculiarly valuable for nautical purposes, it was deemed a fit object of encouragement by the Government. "<sup>1</sup>

Unfortunately for Babbage, no minutes were made of his meeting with Robinson. The only official record indicating that a meeting had taken place was a letter from the Lords of the Treasury to the Royal Society written shortly afterward. It was stated in the letter that the Lords of the Treasury "had directed the issue of £1,500 to Mr. Babbage, to enable him to bring his invention to perfection, in the manner recommended. "<sup>2</sup> The words, "in the manner recommended, " proved to be the only reference to any plan, terms, or conditions for financing the construction of Difference Engine No. 1.

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<sup>1</sup> Ibid., ff. 163-165.

<sup>2</sup> Weld, Eleventh Chapter, p. 373.

The failure of either Babbage or Robinson to take notes at their meeting resulted in problems for Babbage. His recollection of the meeting was that Robinson had recommended a grant from two possible sources--either through the recommendation of a committee of the House of Commons or by taking a sum from the Civil Contingencies. The latter source of funds was selected because it was the most convenient since Parliament was almost ready to adjourn. He recalled that Robinson had made some observations about the mode of accounting for the money received and for its expenditure but that no definite system was suggested except that Babbage should use his own judgement in the matter.<sup>1</sup>

Babbage had estimated in 1823 that the calculating engine would cost between £3000 and £5000 when completed.<sup>2</sup> Even though the cost of his machine soon exceeded this estimate, Babbage was not unduly concerned about the indefinite arrangements with the government. He noted that several of his friends had urged him to apply to the government for more money during the five year period from 1823-1828 but at those times he had

replied that resting confidently on the ultimate repayment he wished not to make such application at a period of public difficulty and that he thought it better to wait until

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<sup>1</sup> Nicolas, "Statement Relative to the Difference Engine," in Passages, by Babbage, pp. 70-71.

<sup>2</sup> Ibid., p. 71.

the completion of the machine. But he did at those times communicate with the R<sup>t</sup> L. G. [Robinson] through his private Sec. thinking it his duty as he had received a grant of public money to show to the Government that he really was occupied in carrying on the construction of the calculating machine.<sup>1</sup>

Babbage continued to work on the engine without interruption until October of 1827. At that time, as noted above, he became ill due to his grief over the loss of four members of his family and was encouraged by his medical friends to travel abroad for six to twelve months.

Upon his departure for the Continent, Babbage left sufficient drawings to enable his workmen to continue work on Difference Engine No. 1. By October of 1827 he had incurred expenses which amounted to £3,475 and subsequently authorized his banker to advance another £1,000 during his absence. He evidently became concerned about the mounting cost of the machine because he asked his brother-in-law, Wolryche Whitmore, to meet with Robinson for the purpose of obtaining more money from the government. It is puzzling that Babbage asked Whitmore to request money from the government rather than waiting until he returned from his travels on the Continent and attending to the matter himself.<sup>2</sup> He evidently did not adequately inform

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<sup>1</sup>Letter, Charles Babbage to the Duke of Wellington and Lord Goderich, December 16, 1828, B.M. 37184, III, 163-165.

<sup>2</sup>Nicolas, "Statement Relative to the Difference Engine," in Passages, by Babbage, p. 72.

Whitmore of the details of his meeting with Robinson in July of 1823, because Whitmore stated in a letter to Babbage, dated February 29, 1828, that

it was under disadvantageous circumstances that I entered upon the subject with him [Robinson] as I was ignorant of the precise nature of the promise made to you at the time and all I could do was to take it for granted & argue accordingly. I did not find him very cordial about it. He did not like to admit that there was any understanding at the time the £1500 was advanced that more would be given by government.<sup>1</sup>

When Babbage returned to England toward the end of 1828, he met with Robinson who admitted that the arrangements made in 1823 were not very definite. Babbage wanted to make certain that he had a firm agreement with the government before he continued with the construction of the calculating engine. He sent a statement to the Prime Minister, Arthur Wellesley, the first Duke of Wellington (1769-1852), with a copy to Robinson in which he explained the details of his meetings with Robinson. He referred Wellington to Robinson for further details and stated that he hoped his recollections of the meetings were in accord with those of Robinson.<sup>2</sup>

The Duke of Wellington, in answer to Babbage's statement,

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<sup>1</sup> Letter, Wolryche W. Whitmore to Charles Babbage, February 29, 1828, B.M. Add MSS 37184, III, 114.

<sup>2</sup> Letter, Charles Babbage to the Duke of Wellington and Lord Goderich (Robinson), December 16, 1828, B.M. Add MSS 37184, III, 163-165.

requested that the Royal Society inquire

whether the progress of the Machine confirms them in their former opinion, that it will ultimately prove adequate to the important object it was intended to attain.<sup>1</sup>

A committee was appointed by the Royal Society to consider the Duke of Wellington's request. The committee's report of February, 1829 strongly endorsed Babbage as follows:

In the actual execution of the work they find that Mr. Babbage has made a progress, which considering the very great difficulties to be overcome in an undertaking so novel, they regard as fully equalling any expectations that could reasonably have been formed; and that although several years have now elapsed since the first commencement, yet that when the necessity of constructing plans, sections, elevations, and working drawings of every part; that of constructing, and in many cases inventing, tools and machinery of great expense and complexity (and in many instances of ingenious contrivances, and likely to prove useful for other purpose hereafter), for forming with the requisite precision parts of the apparatus dissimilar to any used in ordinary mechanical works; that of making many previous trials to ascertain the validity of proposed movements; and that of altering, improving, and simplifying those already contrived and reduced to drawings; your Committee are so far from being surprised at the time it has occupied to bring it to its present state, that they feel more disposed to wonder it has been possible to accomplish so much.

The drawings form a large and most essential part of the work; they are executed with extra-ordinary care and precision, and may be regarded as among the best that have ever been constructed. On these all the contrivance has been bestowed, and all the alternations made, so that scarcely any work excepting drawing has been thrown away. When it is mentioned that upwards of 400 square feet of surface are covered with drawings, many of them of the

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<sup>1</sup> Wellington as quoted in Passages by Babbage, p. 73.



most elaborate description, it will easily be understood that a very great expense of time, thought, and capital must have been incurred in producing them, but without which your committee consider that success would have been impossible. <sup>1</sup>

The committee stated that about three-fifths of the work had been completed and that this might form a "probable conjecture" on which to base the final cost. It was added that this conjecture "would require to be received with very great latitude." <sup>2</sup> Finally, the committee commented that it would have no hesitation in giving it as their opinion, that "in the present state of Mr. Babbage's engine, they do regard it as likely to fulfill the expectations entertained of it by its inventor." <sup>3</sup>

As a result of the committee's report, a Treasury Minute, April 28, 1829, directed a further payment to Babbage of "£ 1500 to enable him to complete the Machine by which such important benefit to Science might be expected." <sup>4</sup> Although Babbage had now received £ 3000 from the government and had spent approximately £ 4000 of his own money, he had not really solved the problem of how to finance the construction of the engine. His friends were not satisfied with

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<sup>1</sup> John Herschel, "Report of the Committee," Journal of the Franklin Institute, N.S. VII (1831), 211.

<sup>2</sup> Ibid., p. 213.

<sup>3</sup> Ibid., p. 213.

<sup>4</sup> "Extract from Treasury Minutes dated 28th April 1829," B.M. Add MSS 37184, III, 285.

such an arrangement with the government and accordingly held a meeting on May 12, 1829 at which some resolutions were made, including the following statement:

That Mr. Babbage was originally induced to take up the work on its present extensive scale, by an understanding on his part that it was the wish of Government that he should do so, and by advancing £1500 at the outset with a full impression on his mind that such further advances would be made as the progress of the work should require and as should secure him from ultimate loss.<sup>1</sup>

The resolution also included an estimate of the total cost of the machine when completed -- £11,000. It was noted in the resolution that if adequate governmental financing could not be secured then Babbage should no longer be called on to go on with an undertaking which may prove the destruction of his health and the dread injury if not ruin of his fortune."<sup>2</sup> In accordance with the last statement of the resolution, Herschel and Whitmore requested an interview with the Duke of Wellington which was granted. They were successful in their interview in presenting Babbage's side of the issue in favor of governmental support for the calculating engine. Sometime afterward they were informed by Henry Goulburn (1784-1856), the new Chancellor of the Exchequer, that Wellington wanted to see the portion

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<sup>1</sup> Rough draft of a resolution written by Babbage's friends, the Duke of Somerset, Lord Ashley, Sir John Franklin, W. W. Whitmore, Dr. William Henry Fitton, Francis Baily, and John Herschel, May 12, 1829, B. M. Add MSS 37184, III, 301.

<sup>2</sup> Ibid., f. 301.

of the Engine which had been completed. Since Babbage had not yet assembled Difference Engine No. 1, Wellington and Goulburn, in November of 1829, saw the model of the engine which Babbage had assembled in 1823 and the drawings and parts of the new engine.<sup>1</sup> It was subsequently recommended that the Treasury make further payment of £3000 to assist in the completion of the engine. Babbage received a letter to this effect on November 20, 1829 with the suggestion that the calculating and printing mechanism of the engine be constructed separately so that if the printing mechanism failed, the calculating portion of the engine could still be used.<sup>2</sup>

Babbage inferred from the letter that the government was now committed to supporting the cost of his engine as he had originally understood in 1823, but he wanted to "endeavor to remove all those doubts"<sup>3</sup> which might arise in those persons not knowledgeable with

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<sup>1</sup>Letter, Edward Adolphus Seymour, 11th Duke of Somerset to Charles Babbage, June 8, 1829, B. M. Add MSS 37184, III, 337; letter Edward Drummond to Charles Babbage, November 5, 1829, *ibid.*, f. 400; letter, Charles Babbage to Edward Drummond, November 13, 1829, *ibid.*, f. 405; letter Edward Drummond to Charles Babbage, November 16, 1829, *ibid.*, f. 412; letter Charles Babbage to Edward Walpole, November 17, 1829, *ibid.*, f. 415; and letter, Edward Walpole to Charles Babbage, November 18, 1829, *ibid.*, f. 417. All of the above correspondence is concerned with arrangements for the Duke of Wellington's visit to see Babbage's calculating engine.

<sup>2</sup>Letter, Henry Goulburn to Charles Babbage, November 20, 1829, B. M. Add MSS 37184, III, 421.

<sup>3</sup>Letter, Charles Babbage to Henry Goulburn, November 24, 1829, B. M. Add MSS 37184, III, 426.

regard to the calculating engine. However, from the pattern which had developed in his negotiations with the government it seems likely that governmental officials were committed to a point by point review of the progress which Babbage had made in the construction of the engine rather than a long-term grant of money without a periodic review. But, Babbage was anxious to obtain a long-term grant in order to avoid the delays which accompanied each request for money. Accordingly, he wrote to Goulburn that before he received the £ 3000, he wished to propose some general arrangements between the government and himself in another communication.<sup>1</sup> Babbage wrote to his friend, Anthony Ashley Cooper, Seventh Earl of Shatesbury (1801-1885), and asked him to meet with Goulburn on his behalf.<sup>2</sup> In his letter to Lord Ashley, Babbage revealed the inner turmoil which he had experienced due to the constant delays and doubts concerning the government's intentions regarding the machine. Babbage maintained that the conflict and uncertainty which he had experienced had combined to injure his health and to divert his thoughts from his invention.

You are aware in common with many of my best friends in what state of anxiety and irritation I have existed for the last ten months. None of you can have regretted it more than I have, yet this very irritation was heightened by the feeling that I was wearing away existence not in the

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<sup>1</sup>Ibid., f. 426.

<sup>2</sup>Letter, Charles Babbage to Lord Ashley, November 25, 1829, B.M. Add MSS 37184, III, 430-432.

fever of excitement which discovery always produces, which if it shorten life condenses in those moments the pleasure of years, but that I was investing it on what may be called moral difficulties of the machine--difficulties which perhaps the very constitution of my mind rendered me as incompetent to contend with as it seems to have rendered it fatally susceptible of them as a source of disquiet.<sup>1</sup>

In order to prevent a recurrence of his misunderstandings with government officials over money, Babbage wrote to Lord Ashley that it would be wise to suggest to Goulburn the following proposals:

1<sup>st</sup> That the machine should be considered as property of government.

2<sup>nd</sup> That Mr. Rennie and Mr Donkin<sup>2</sup> or any other fit persons should be appointed by Government to examine from time to time that the charges made by Mr. Clement [Babbage's engineer] are consistent with the work he has executed and that those charges should be defrayed by Government.

3<sup>rd</sup> That under these circumstances I should entirely direct Mr. Clement as I have hitherto done.<sup>3</sup>

Goulburn's reply to Babbage's proposal was entirely unsatisfactory to him. It contained two major points which represented not only the views of Goulburn and the Duke of Wellington but the basic

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<sup>1</sup> Ibid., f. 432.

<sup>2</sup> George Rennie (1791-1866), engineer, who with his brother John Rennie (1794-1874), was Knighted in 1831 for completion of the London Bridge; and Bryan Donkin, also an engineer.

<sup>3</sup> Letter, Charles Babbage to Lord Ashley, November 25, 1829, B.M. Add MSS 37184, III, 432. For an edited version, see Nicolas, "Statement Relative to the Difference Engine," in Passages by Charles Babbage, p. 76.

attitude of the English Government towards patronage of science. The first point was that

The view of the Government was to assist an able and ingenious man of science whose zeal had induced him to exceed the limits of prudence in the construction of a work which would if successful redound to his honour and be of great public advantage.<sup>1</sup>

Babbage was particularly irritated by Goulburn's description of him as a man who had exceeded the limits of prudence due to his zeal. He wrote to Lord Ashley on December 15, 1829 that Goulburn appeared to think that he had begun Difference Engine No. 1 and then applied to Lord Goderich (Robinson) for assistance. Babbage added:

The fact is that the small machine the Duke of Wellington and himself saw, was finished and seen by hundreds of persons before my interview with Lord Goderich. And it was in consequence of verbal communications between several of my friends and various members of the government and subsequent to the printing by order of the House of Commons of several papers relating to the machine that I waited on Lord Goderich to know the wishes of Government.

This fact appears also by my printed letter addressed to the late Sir Humphrey Davy in the concluding paragraphs of which my opinion of the making of a machine as a matter of speculation is spoken of as 'an undertaking I should feel unwilling to commence as altogether foreign to my habits and pursuits.'

I have no hesitation in declaring that having finished the first small machine which clearly established the invention, I had no idea whatever of undertaking that which is now so far advanced at my own risk and with my own

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<sup>1</sup> Letter, Henry Goulburn to Lord Ashley, December 13, 1829, B.M. Add MSS 37184, III, 451.

means and had I not seen Lord Goderich the present machine never would have been commenced.<sup>1</sup>

Thus Babbage was very emphatic in stating the circumstances under which he had commenced Difference Engine No. 1 in his private remarks to Lord Ashley. However, in a letter to Lord Ashley, on December 16, 1829, which Babbage wished to have communicated to Goulburn, he stated the same points but with more tact.<sup>2</sup>

In Goulburn's letter to Lord Ashley which elicited the above response from Babbage, he raised a second point which explained why he, as the Chancellor of the Exchequer, was unwilling to enter into a firm, long-range agreement to finance not only the calculating engine but similar projects. Goulburn wrote:

It was no part of our intention to divest Mr. Babbage of the machine or by transferring the property in it to Government to place him in the situation of a Government Agent acting under the instructions of the Treasury. To adopt this course would, as it appears to me, be neither to Mr. Babbage's advantage nor to that of Government. It would be in many respects particularly objectionable as applicable to other cases in which Government have contributed towards without undertaking to conduct scientific improvements.<sup>3</sup>

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<sup>1</sup> Letter, Charles Babbage to Lord Ashley, December 15, 1829, B.M. Add MSS 37184, III, 455.

<sup>2</sup> Letter, Charles Babbage to Lord Ashley, December 16, 1829, B.M. Add MSS 37184, III, 459.

<sup>3</sup> Letter, Henry Goulburn to Lord Ashley, December 13, 1829, B.M. Add MSS 37184, III, 451.

Goulburn did not want Babbage to consider himself a governmental agent because this would have in effect set a precedent of direct governmental support of science, which was, as shown by the reaction to Babbage's Reflections, a very controversial issue. Also, Goulburn did not want the government involved in conducting "scientific improvements" because this too would have set a precedent of national commitment to and direction of science such as had occurred in France under Napoleon.

Babbage was not satisfied with these arrangements, and as noted above, he wrote again to Lord Ashley, on December 16, 1829, to ask his assistance in another attempt to gain a firm commitment from the government before he resumed work upon Difference Engine No. 1. He enclosed a list of questions which he wished to have answered.<sup>1</sup> Lord Ashley carried out Babbage's request and obtained the following commitment:

1st. Although the Government would not pledge themselves to complete the Machine, they were willing to declare it their property.

2nd. That professional Engineers should be appointed to examine the bills.

3rd. That the Government were willing to advance 3,000 more than the sum (£ 6,000) already granted.

4th. That, when the Machine was completed, the Government would be willing to attend to any claim of

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<sup>1</sup> Letter, Charles Babbage to Lord Ashley, December 16, 1829, B.M. Add MSS 37184, III, 459.



Mr. Babbage to remuneration, either by bringing it before the Treasury, or the House of Commons.<sup>1</sup>

Babbage had at last obtained a firm commitment from the government, but other difficulties now interfered with his efforts to complete the Difference Engine. Babbage had had difficulty with his engineer, Joseph Clement from the beginning of the construction of Difference Engine No. 1 in 1823. Clement's health was poor which worried Babbage because he thought Clement might have to withdraw from his work upon the engine.<sup>2</sup> Clement was also a very irritable and stubborn man, which made him a difficult person for Babbage to work with and depend upon especially in a project such as the construction of a calculating engine. But, the complex nature of Difference Engine No. 1--new standards of tolerances, new machine tools for cutting and planing, and precise mechanical drawings were needed--made it necessary for Babbage to employ the best engineer he could find and Clement had a reputation as an excellent engineer. As work on the calculating engine progressed it became increasingly difficult for Babbage to replace Clement with another engineer.

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<sup>1</sup> Letter, Lord Ashley to Charles Babbage, February 24, 1830, B.M. Add MSS 37185, IV, 69. In a letter, James Stewart to Charles Babbage, December 24, 1830, *ibid.*, f. 394, Stewart asked Babbage to "distinctly state" that the calculating engine was the property of the government.

<sup>2</sup> Letter, Charles Babbage to George Rennie, April 11, 1829, B.M. Add MSS 37184, III, 252.

Clement knew the details of the work as well as Babbage and had constructed the lathes and other machine tools needed for the engine. In spite of Clement's personality Babbage respected him for his skill as an engineer and was highly satisfied with the quality of his work. He thought Clement should be amply rewarded for his work and had therefore been very liberal in paying him without demanding a close accounting of his bills. However, after the report of the committee of the Royal Society in February of 1829, Babbage changed his attitude toward Clement. Babbage evidently assumed, from the Duke of Wellington's acceptance of the report and his approval of an additional grant of money for construction of the calculating engine, that the government now claimed ownership of the engine. Babbage wrote that

in conversation with Mr. Clement, I have hitherto always treated the work as my own private property for two reasons: first I thought his charges might be more moderate than if they were paid by government and secondly I did not feel myself quite authorized to state the contrary. Since I have been acquainted with the result of my letter to the Duke of Wellington and the report of the Comm. of the R.S. I can scarcely continue this to be the case.<sup>1</sup>

It is puzzling that Babbage should make such an assumption because it was not until December of 1829 that he received notice from governmental officials that the engine was the property of the government. There is no mention of such an intention in the Royal Society report, but Babbage decided, that since Clement knew the

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<sup>1</sup>Ibid., f. 252.

machine was being financed by the government, to ask two engineers, George Rennie (1791-1866) and Bryan Donkin, to examine Clement's accounts.<sup>1</sup> Clement did not cooperate with the engineers so that Babbage was forced to consult Richo Penn, an attorney. Penn, in a letter dated May 5, 1829, wrote to Babbage that he should submit the matter of Clement's bills to an arbitrator and added:

It appears to me at first sight that (as Mr. Clement has chosen by his offensive Conduct to forfeit all claim to that liberality on your part which disposed you at first not to question the amount of his charges, but merely to require the particulars of them) your best course would now be to force the matter to a more hostile Arbitration. I cannot but think that if you should do so, Mr. Clement's low cunning will be defeated because I conceive that under such an Arbitration he would not be allowed to charge both for his own time and also a Tradesman's profit on the articles [machine tools and parts of the engine]; and I am sure that an investigation made in this spirit would greatly reduce the amount of his charges, and shew that you purchased & paid for every thing which is now done. A conclusive Settlement made now seems to be absolutely necessary for your future Comfort in the business and I would advise you to speak to Mr. Warburton on the subject.<sup>2</sup>

Letters were exchanged over a period of about three years to secure an arbitrator who was acceptable to both men,<sup>3</sup> but for some reason the arbitration was never carried out. One possible reason was that, regardless of the outcome, the loss of Clement's services

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<sup>1</sup> Ibid., f. 252.

<sup>2</sup> Letter, Richo Penn to Charles Babbage, May 5, 1829, B.M. Add MSS 37184, III, 281.

<sup>3</sup> Letter, Richo Penn to Charles Babbage, February 18, 1830, B.M. Add MSS 37185, IV, 59.

would have caused more difficulties because of the advanced state of the calculating engine than the settlement of the account.<sup>1</sup> Even though the dispute over the bills was not handled through arbitration, this did not mean that Babbage and Clement had settled their differences. According to Babbage, Clement would promise to submit an itemized bill to the engineers but would fail to do so by pretending that he had misunderstood the arrangements. Babbage, in turn, irritated Clement by delaying his payment to him. Babbage claimed that he could not pay Clement on time because the government was slow in paying him for the work done on the engine. Clement had constantly demanded that Babbage pay him regularly. He did not accept Babbage's excuse that the lack of an efficient and prompt system of payment of bills by the government caused Babbage personal hardship. Clement maintained that when Babbage was late in his payment he, Clement, was forced to pay his workmen from his own personal account.<sup>2</sup>

The differences between Babbage and Clement continued to worsen even though Babbage tried to be accommodating to Clement. When Babbage decided that he needed a new fireproof workshop in

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<sup>1</sup> Letter, Charles Babbage to Lord Ashley, February 8, 1830, B.M. Add MSS 37185, IV, 42.

<sup>2</sup> Letter, Joseph Clement to Charles Babbage, November 18, 1829, B.M. Add MSS 37184, III, 419.

1830<sup>1</sup> and when it was recommended that the workshop be located adjacent to his residence,<sup>2</sup> he unwittingly created the conditions for a major confrontation with Clement in 1833 when the workshop was finally completed. Clement objected when Babbage asked him to either travel four miles from Clement's own workshop or take up residence in an apartment above the new workshop. Clement maintained that he would not only lose time in travel but that the income he received from work on the engine was only a small part of his business. This latter statement was particularly ironic since Clement's business had increased because of his association with Babbage and because of the new machine tools he had acquired over the years from 1823-1833 which had been paid for by Babbage and the government. The following letter from Clement to Babbage concerned money which Babbage owed Clement and resulted from the major confrontation between the two men:

"31 St. George's Road,

"Southwark

"26 March 1833

"Sir.

"After what passed between you and I on Wednesday last respecting settling my account you then stated that you could not pay me as you had not received the money from Government. I said I had nothing to do with

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<sup>1</sup> Letter, Charles Babbage to Henry Goulburn, Chancellor of the Exchequer, July 13, 1830, B.M. Add MSS 37185, IV, 263.

<sup>2</sup> "Report of a Committee Appointed by the Council of the Royal Society," March 26, 1831, B.M. Add MSS 37185, IV, 513.

Government respecting the calculating machine, that you were the only person I had made my accounts out to and the only person who paid my accounts, that you were responsible to me for all expense incurred on the machine up to the present time, and that it would be impossible for me to proceed much further without money. Your answer was that for the future you would never advance or pay me a single shilling on your own account respecting the machine.

Now after that declaration I do not think that I should be justified in proceeding any further with the calculating machine until someone is made responsible to me for the work that may be done hereafter. I therefore gave notice the following morning to all the men employed on the calculating machine that I should not be able to employ them after this week as there was a misunderstanding between you and me. Now if it be a misunderstanding, I hope for the sake of my men and the machine that you will be pleased to take the earliest opportunity of arranging things in a more satisfactory manner.

"I remain

"Dear Sir,

"Your obedient servant

"(signed) Joseph Clement"

"P. S. I should have written to you sooner had my health permitted. "

To C. Babbage Esq.<sup>1</sup>

After receiving this letter Babbage did not try to compromise with Clement which was particularly unfortunate since no further work was done on the Difference Engine. Both men consulted the Treasury. Clement explained in great detail that Babbage, on the advice of the engineers who had examined his accounts, had declined to pay him

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<sup>1</sup>Letter, Joseph Clement to Charles Babbage, March 26, 1833, B.M. Add MSS 37187, VI, 453.

until all the drawings, patterns, and parts of the engine were removed to Babbage's fireproof workshops. Clement informed the Treasury that he was willing to continue to work on the machine at his own shop with the drafting to be carried out in Babbage's workshop. He also explained in detail the conditions under which he expected to be paid if he were to continue to work for Babbage.<sup>1</sup> The Treasury, surprisingly, accepted Clement's conditions for payment and paid the money, £1782.11, he claimed Babbage owed him.<sup>2</sup> This was the first time the government had paid money to Clement directly instead of paying the money to Babbage. Babbage was informed of Clement's letter and directed to take the necessary steps for removing the drawings and completed parts of the engine to his own workshop.<sup>3</sup>

When Babbage began the transfer of the engine and drawings to his own workshop, a dispute arose over who owned the machine tools which had been devised for the construction of the engine. Under English law Clement had the right of ownership to all specialized tools made in his shop even though they were designed by Babbage.

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<sup>1</sup> Letter, Joseph Clement to the Right Honorable the Lords Commissioners of His Majesty's Treasury, July 22, 1833, B.M. Add MSS 37188, VII, 16.

<sup>2</sup> Copy, Treasury Minutes, to Charles Babbage, August 13, 1833, B.M. Add MSS 37188, VII, 28. A copy was also sent to Clement.

<sup>3</sup> Letter, James Stewart to Charles Babbage, August 17, 1833, B.M. Add MSS 37188, VII, 29.

Babbage might have been able to establish a claim for the tools if he had followed the advice of Richo Penn in May of 1829, as noted above, and entered into arbitration with Clement. Penn had stated that he did not think that Clement would be allowed "to charge both for his own time and also a Tradesman's profit on the articles," that is, on the machine tools and parts of the engine.<sup>1</sup> However, Clement claimed the tools had been made at his own expense, and, without any proof to the contrary, Babbage had no claim upon them.<sup>2</sup>

It was unfortunate that Babbage did not compromise with Clement at this time because he not only lost his services and knowledge of the engine but also the tools and lathes necessary to produce the parts to the engine. Babbage may have acted upon the advice given to him in 1833 by his friend Mark Isambard Brunel (1769-1859), an engineer, knighted in 1851 and famous for the construction of the Thames Tunnel (1825-1843), to employ another workman, a friend of his, who had "Skill, longevity," and who was "a good Draftsman, Experienced in conducting the execution of machinery of the most perfect and most elegant description, with as good an assortment of

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<sup>1</sup> Letter, Richo Penn to Charles Babbage, May 5, 1829, B.M. Add MSS 37184, III, 281.

<sup>2</sup> Letter, Bryan Donkin to Charles Babbage, April 22, 1829, B.M. Add MSS 37184, III, 266.



tools as Clement has."<sup>1</sup>

Work on Difference Engine No. 1 ceased after July of 1833 following Babbage's confrontation with Clement and was never resumed. Babbage had planned to continue the construction of the engine without Clement's help but new circumstances arose which interfered with his plans. For some reason which is unclear, there was a delay of about a year in removing all the parts of the engine and the drawings from Clement's shop to Babbage's shop. As noted above, the Treasury had directed Babbage to remove all drawings and parts of the machine from Clement's shop to his own in a communication dated August 17, 1833.<sup>2</sup> The delay which occurred following this directive may have resulted from a need to prepare rooms within the new workshop for the drawings and the engine.

It is particularly unfortunate that Babbage did not complete Difference Engine No. 1 at his own expense because by so doing he would have almost certainly left no doubt that the engine was a success, and he would have satisfied the public that the money for the construction of the engine had not been misused. He had received the support of the majority of the scientific community in 1823 when he had taken

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<sup>1</sup>Letter, Mark Isambard Brunel to Charles Babbage, August 8, 1833, B.M. Add MSS 37188, VII, 24. The emphasis was added by Brunel.

<sup>2</sup>Nicolas, "Statement Relative to the Difference Engine," in Passages, by Babbage, p. 82.

the time to explain the purpose and utility of the engine. With the engine nearly completed he would have had no trouble in reviving that support if he had decided to continue his work regardless of the personal sacrifice. Babbage evidently thought at the time that he had done as much as he could do on his own and that the government had an obligation to him to support the engine until he had completed it regardless of the difficulties. In looking back upon the circumstances which caused him to cease work on the engine, Babbage was of the opinion, in 1864, that he had done all that should have been expected of him:

Enough has already been said about that unfortunate discovery [Difference Engine No. 1] in the previous part of this volume. The first and great cause of its discontinuance was the inordinately extravagant demands of the person whom I had employed to construct it for the Government. Even this might, perhaps, by great exertions and sacrifices, have been surmounted. There is, however, a limit beyond which human endurance cannot go.<sup>1</sup>

It is to Babbage's credit that he did continue to negotiate with the government for support of the engine until 1842, when the government abandoned it. However, as will be discussed in detail, there was much misunderstanding as to the circumstances which caused the government to withdraw its support. Neither Babbage nor the governmental officials were able to avoid criticism for abandoning a machine which had been so widely acclaimed by the Astronomical Society in 1823 as an aid to astronomical research and to science as a whole.

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<sup>1</sup> Babbage, Passages, p. 449.

## IX. REACTIONS TO THE CIRCUMSTANCES SURROUNDING THE DEVELOPMENT OF DIFFERENCE ENGINE NO. 1

The many delays and difficulties which Babbage encountered in his work with Difference Engine No. 1 were not very well understood by either the general public or by the scientific community including some members of the Royal Society. Babbage did not take the time to redress this situation by explaining the circumstances surrounding the construction of the engine and his negotiations with the government. Part of the difficulty lay in the complexity of the project itself. Babbage had acted as his own production engineer, researcher, machine shop manager, and negotiator for governmental support of his engine. There were simply too many details for any one individual to attend to and still remain creative and productive. It is therefore to Babbage's credit that he managed as well as he did, particularly with the technical details of the engine. Edward Ffrench Bromhead was particularly farsighted when he advised Babbage, as noted above, that he should ask a committee of scientific men to act as advisors to secure governmental support and to maintain liason with the government rather than to attempt to negotiate as an individual. Babbage did follow this advice in the early stages of the project, but in the latter stages, 1829-1833, he called upon a group of scientific men only when a crisis arose. After 1833, he negotiated with the government almost exclusively as an individual, and these negotiations, as

will be discussed, left much to be desired.

The public did not understand why Babbage had not completed Difference Engine No. 1 after ten years of labor and considerable public expense. Babbage added to this confusion by his invention of a new calculating engine, the Analytical Engine, during the time he was deprived of his drawings of Difference Engine No. 1 following his break with Clement. Babbage stated that the Analytical Engine was based upon an entirely new principle which was general in nature and therefore made possible the most complicated arithmetical calculations. When his drawings were returned to him sometime in 1834, Babbage re-examined them and noted that the new principle appeared to be limited only to the extent of his ability to construct the mechanical means to execute it. In addition, the new calculating engine, as envisioned by Babbage, would be able to execute calculations more rapidly than the Difference Engine.<sup>1</sup>

Babbage doubted whether he should withhold from the government his view that the Analytical Engine was of greater utility and of a simpler mode of construction than the old engine. He therefore asked Lord Melbourne, the Prime Minister, to come to his shop and inspect both the Difference Engine and his first drawings of the new engine. Babbage wrote to Melbourne that it would be much more

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<sup>1</sup>Babbage, Passages, p. 83.

effective to discuss the circumstances of his calculating engines in person than in a letter.<sup>1</sup> Melbourne agreed on September 26, 1834<sup>2</sup> to visit Babbage's workshop, but the visit was postponed with Melbourne going out of office before it could be rescheduled.<sup>3</sup>

Just prior to Babbage's letter to Melbourne, an article was published concerning his work on the calculating engine which was generally very favorable but which raised questions as to why Babbage had not communicated more effectively with the public. The article was written by Dionysis Lardner (1793-1850), a mathematician and a man noted for his ability as a popularizer of difficult and abstract scientific subjects. Lardner actually did much to help the public understand the difficulties which Babbage had faced as an inventor because he chose to focus attention upon the importance of the Difference Engine to science, its utility as a calculating device, and the peripheral benefits to be derived from the research of a man such as Babbage who understood both abstract mathematical theory and the practical workings of a mechanic's shop. Of Babbage's qualities

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<sup>1</sup> Letter, Charles Babbage to James Frederick Lamb, third Viscount Melbourne, September 25, 1834, B.M. Add MSS 37188, VII, 480.

<sup>2</sup> Letter, Lord Melbourne to Charles Babbage, September 26, 1834, B.M. Add MSS 37188, VII, 485.

<sup>3</sup> Letter, Lord Melbourne to Charles Babbage, October 9, 1834, B.M. Add MSS 37188, VII, 489.

as a scientist and his position in society, Lardner wrote:

There is no position in society more enviable than that of the few who unite a moderate independence with high intellectual qualities. Liberated from the necessity of seeking their support by a profession, they are unfettered by its restraints, and are enabled to direct the powers of their minds, and to concentrate their intellectual energies on those objects exclusively to which they feel that their powers may be applied with the greatest advantage to the community, and with the most lasting reputation to themselves. On the other hand, their middle station and limited income rescue them from those allurements to frivolity and dissipation, to which rank and wealth ever expose their possessors. Placed in favorable circumstances, Mr. Babbage selected science as the field of his ambition; and his mathematical researches have conferred on him a high reputation, wherever the exact sciences are studied and appreciated. The suffrages of the mathematical world have been ratified in his own country, where he has been elected to the Lucasian Professorship in his own University--a chair, which, though of inconsiderable emolument, is one on which Newton has conferred everlasting celebrity. But it has been the fortune of this mathematician to surround himself with fame of another and more popular kind, and which rarely falls to the lot of those who devote their lives to the cultivation of the abstract sciences. This distinction he owes to the announcement, some years since, of his celebrated project of a Calculating Engine. A proposition to reduce arithmetic to the dominion of mechanism, --to substitute an automaton for a compositor, --to throw the powers of thought into wheelwork could not fail to awaken the attention of the world.<sup>1</sup>

Lardner understood very well the difficult task that Babbage had chosen for himself. Rumors concerning the delays which had plagued the construction of the Difference Engine from its inception had

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<sup>1</sup> Dionysius Lardner, "Babbage's Calculating Engine," Edinburgh Review, LIX (July, 1834), pp. 263-264.

circulated which impugned Babbage's motives and character. Lardner saw his task as that of removing and correcting erroneous impressions, and at the same time converting the vague sense of wonder of the public towards a machine which seemed incomprehensible, "into a more rational and edifying sentiment."<sup>1</sup> To this end, Lardner argued that many people regarded the calculating engine more as a philosophical curiosity than as an instrument for more practical uses. Such people, Lardner noted, were ignorant of the extensive utility of the numerical tables which the calculating engine was able to produce. Lardner's most perceptive insight into the problems of an inventor are shown in the following passage.

There are also some persons who, not considering the time requisite to bring any invention of this magnitude to perfection in all its details, incline to consider the delays which have taken place in its progress as presumptions against its practicability. These persons should, however, before they arrive at such a conclusion, reflect upon the time which was necessary to bring to perfection engines infinitely inferior in complexity and mechanical difficulty. Let them remember that--not to mention the invention of that machine--the improvements alone introduced into the steam-engine by the celebrated Watt, occupied a period of not less than twenty years of the life of that distinguished person, and involved an expenditure of capital amounting to £50,000. The calculating machinery is a contrivance new even in its details. Its inventor did not take it up already imperfectly formed, after having received the contributions of human ingenuity exercised upon it for a century or more. It has not, like almost all other great mechanical inventions, been gradually advanced to its

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<sup>1</sup> Ibid., p. 266.

present state through a series of failures, through difficulties encountered and overcome by a succession of projectors. It is not an object on which the light of various minds has thus been shed. It is, on the contrary, the production of solitary and individual thought, --begun, advanced through each successive stage of improvement, and brought to perfection by one mind. Yet this creation of genius, from its first rude conception to its present state, has cost little more than half the time, and not one-third of the expense, consumed in bringing the steam-engine (previously far advanced in the course of improvement) to the state of comparative perfection in which it was left by Watt. Short as the period of time has been which the inventor has devoted to this enterprise, it has nevertheless, been demonstrated, to the satisfaction of many scientific men of first eminence, that the design in all its details, reduced as it is, to a system of mechanical drawings, is complete; and requires only to be constructed in conformity with those plans, to realize all that its inventor has promised.<sup>1</sup>

Lardner devoted much of his article to an explanation of the various uses of astronomical, navigational and mathematical tables. The errors in these tables often contained compound errors--the extensive errata sheets were found to contain many errors. A ship's captain depending on the existing navigational tables was never very secure as to the location and safety of his ship and crew.<sup>2</sup> The time required to produce accurate tables made their cost extremely high and in many instances discouraged both their use and production. Lardner ably justified the expenditure of governmental funds on Babbage's calculating engines by making use of these arguments.<sup>3</sup>

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<sup>1</sup> Ibid., pp. 265-266.

<sup>2</sup> Ibid., pp. 267-284.

<sup>3</sup> Ibid., pp. 320-323.



While Lardner was very favorable in his discussion of the technical details of Difference Engine No. 1, he was very critical of what he considered a failure on Babbage's part to press for governmental cooperation and support. He was evidently unaware of Babbage's efforts in this regard:

Mr. Babbage is known as a man of unwearied activity, and aspiring ambition. Why, then, it may be asked, is it that he, seeing his present reputation and future fame depending in so great a degree on the successful completion of this undertaking, has nevertheless allowed it to stand still for so long a period without distinctly pointing out to the Government the course which they should adopt to remove the causes of delay? Had he done this (which we consider to be equally due to the nation and to himself), he would have thrown upon the Government and its agents the whole responsibility for the delay and consequent loss; but we believe he has not done so. On the contrary, it is said that he has of late almost withdrawn from all interference on the subject, either with the Government or the engineer. Does not Mr. Babbage perceive the inference which the world will draw from this course of conduct?<sup>1</sup>

Lardner concluded that if Babbage did not try to obtain governmental cooperation and support then the government itself should appoint a committee to look into the matter and recommend a course of action.<sup>2</sup>

Lardner's article, which was published in July of 1834, evidently persuaded Babbage to do something in his own behalf,

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<sup>1</sup> Ibid., pp. 326-327.

<sup>2</sup> Ibid., p. 327.

because in September of 1834 he began to write a history of the invention of the calculating engine.<sup>1</sup> He decided not to describe the Difference Engine in detail because it would occupy too much space and because detailed drawings of the machine already existed. He wrote that the object of the paper was to

explain as far as I am able that train of thought which led me to the invention and to the successive modifications of the system of machinery I have contrived and I shall add such observations on what might be called the moral difficulties of the undertaking as I will conceive be of use to those who may hereafter be placed in similar circumstances.<sup>2</sup>

It is unfortunate that Babbage did not complete his paper and publish it at this time because the public would have been provided with a firsthand account and possibly an answer to some of the questions which were raised by Lardner. Also, as will be discussed, Babbage might have avoided the charge that he had personally failed to inform the public about the status of the engine.

Babbage attempted once again to negotiate with the government on December 17, 1834<sup>3</sup> by asking the Duke of Wellington, the new Prime Minister, for an interview concerning the engine. In reply,

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<sup>1</sup> Charles Babbage, "History of the Invention of the Calculating Engine," September 6, 1834, Museum, Oxford, MS Buxton 8, ff. 1-2.

<sup>2</sup> Ibid., f. 2.

<sup>3</sup> Letter, Charles Babbage to the Duke of Wellington, December 17, 1834, B.M. Add MSS 37188, VII, 520.

Wellington asked for a written statement before he granted the interview<sup>1</sup> which statement Babbage sent pointing out the following plans relative to Difference Engine No. 1:

There appears to me to be but four ways in which the question relative to this engine can be disposed of.

1st. The government may desire me to continue its construction in hands of the same person who has hitherto been employed in making it. . . .

2ndly. The government may enquire whether it is not possible to find some other person than the one to whom I have alluded [Clement], competent to complete the work. . . .

3rdly. The government may find some person to supply my place, who may from my designs and drawings, undertake to finish the Engine. . . .

4thly. The last and apparently the only remaining is to give up the undertaking entirely. . . .<sup>2</sup>

Babbage also informed Wellington of the circumstances which led to the invention of the Analytical Engine with its far more extensive powers. In describing the Analytical Engine he noted that it was

a totally new engine possessing much more extensive powers, and capable of calculations of a nature far more complicated [than the Difference Engine]. It is singular that amongst the numerous contrivances, which it comprises, no one has yet been adopted which has been employed in the former one. Much as it exceeds that Engine in power, it is not intended to supersede it, on the contrary it will greatly extend its power and add to its utility.<sup>3</sup>

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<sup>1</sup> Letter, Duke of Wellington to Charles Babbage, December 18, 1834, B.M. Add MSS 37188, VII, 522.

<sup>2</sup> Letter, "Statement addressed to the Duke of Wellington Respecting the Calculating Engine," from Charles Babbage, December 23, 1834, B.M. Add MSS 37188, VII, 525.

<sup>3</sup> Ibid., f. 525.

Babbage made the comment that it might be possible that a foreign government would be willing to finance the construction of the Analytical Engine, but what he really wanted was a decision concerning what should be done about Difference Engine No. 1.<sup>1</sup> No decision was reached concerning the engine since the Duke of Wellington and his administration were replaced near the end of 1835 after a tenure of about a year. Babbage received a communication from Spring Rice, the new Chancellor of the Exchequer, who expressed a desire to come to a definite decision concerning the subject of the calculating engine.<sup>2</sup> Rice had concluded that Babbage wished the government to "defray the expense of this new machine."<sup>3</sup> While Babbage had not asked for the government to finance the Analytical Engine, his reference to the possibility of a foreign government providing support for its construction in his letter to Wellington, which Rice had read, probably convinced Rice that Babbage really wanted the English government to finance the new engine. Babbage tried in vain to explain that the statement to Wellington contained no such intent. On February 1,

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<sup>1</sup> Ibid., f. 525.

<sup>2</sup> Letter, Spring Rice to Charles Babbage, January 14, 1836, B.M. Add MSS 37189, VIII, 273.

<sup>3</sup> Ibid., f. 273.

1836, he sent a reply<sup>1</sup> to Rice which in a sense determined the fate of Difference Engine No. 1 as well as that of the Analytical Engine. Babbage meant his reply as an explanation of the differences between the two engines, but his wording in the letter confused Rice even more than had the letter to Wellington. The Analytical Engine, Babbage explained,

is not only capable of accomplishing all those other complicated calculations which I had intended, but it also performs all calculations which were peculiar to the old Engine, both in less time, and to greater extent: in fact, it completely supersedes the old Engine.<sup>2</sup>

Babbage also noted that due to the mechanical simplicity to which the elements of the Analytical Engine had been reduced, it would probably cost more to finish Difference Engine No. 1 on its original plan than to construct a new one with the simplified elements he had devised for the Analytical Engine.

Babbage's statement that the Analytical Engine "completely supersedes the old Engine" apparently confused Rice because it contradicted the letter Babbage had written to the Duke of Wellington in December of 1834 in which he stated that the new engine merely added to the utility of Difference No. 1. The references to the cost and

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<sup>1</sup>Letter, Charles Babbage to Spring Rice, February 1, 1836, B.M. Add MSS 37189, VIII, 291.

<sup>2</sup>Charles Babbage, "Report to the Rt. Honorable the Chancellor of the Exchequer [Spring Rice] on the Calculating Engine," February 1, 1836, B.M. Add MSS 37189, VIII, 292.

obsolescence of the old engine probably confirmed the conclusion by Rice that Babbage did indeed want the government to finance a new calculating machine even though Babbage was emphatic that this was not his intent as he stated in his letter:

The fact of a new machine superseding an old one in a very few years, is one of constant occurrence in our manufactories: and instances might be pointed out in which the advance of invention has been so rapid, and the demand for machinery so great, that half finished machines have been thrown aside as useless before their completion.

It is now nearly fourteen years since I undertook for the government to superintend the making of the Difference Engine. During nearly four years its construction has been absolutely stopped, and, instead of being employed in overcoming the physical impediments, I have been harassed by what may be called the moral difficulties of the question. It is painful to reflect that, in the time so employed, the first Engine might, under more favorable circumstances, have been completed.

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 construct the other, but I make it from the conviction that the information it contains ought to be communicated to those who must decide the question relative to the Calculating Engine.<sup>1</sup>

No more was heard from the government until about the middle of 1836.<sup>2</sup> Babbage contended that without a decision he was prevented

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<sup>1</sup> Ibid., f. 292.

<sup>2</sup> Nicolas, "Statement Relative to the Difference Engine," in Passages, by Babbage, p. 90. Rice had made reference to obtaining an opinion from the Royal Society but Babbage had suggested a committee of engineers instead and had pointed out that the Royal Society had already commented affirmatively on its utility. Because of the advanced state of the engine, Babbage thought that a small committee

from making any personal plans without possible disruption. He asked for help from his friends and continued his own efforts as well.

Babbage's brother-in-law, Wolryche Whitmore, repeatedly urged Rice in both letter and personal appeal to end the injustice to Babbage by coming to a decision. Two more years passed with no change in the issue. Babbage, determined to resolve the matter, wrote to Lord Melbourne, the First Lord of the Treasury, on July 26, 1838 recalling his previous efforts in asking for a final decision.<sup>1</sup> On August 16, 1838, Rice, in reference to Babbage's note to Lord Melbourne, explained that he now understood that he had misunderstood Babbage's intent concerning the construction of the Analytical Engine in lieu of finishing the old machine. Rice enquired whether Babbage wished to complete the old machine or begin a new one and asked for a cost estimate for either course of action.<sup>2</sup> Babbage replied on October 21, 1838 that

the question I wish to have settled is whether the government required me to superintend the completion of the Calculating Engine, which had been suspended

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of engineers would be better qualified; see the letter, Charles Babbage to Spring Rice, May 22, 1835, B.M. Add MSS 37189, VIII, 101.

<sup>1</sup> Letter, Charles Babbage to Viscount Melbourne, July 26, 1838, B.M. Add MSS 37190, IX, 496.

<sup>2</sup> Letter, Spring Rice to Charles Babbage, August 16, 1838, B.M. Add MSS 37190, IX, 518.

during the last five years, according to the original plan and principles; or whether they intended to discontinue it altogether?<sup>1</sup>

In fairness to Rice, it was no wonder he was confused as to Babbage's true intentions. In this case Babbage did not answer the question of which machine he wished the government to finance and he did not offer a cost estimate as Rice had requested. He stated that he was very reluctant to give such an estimate as a "non-professional man," having "myself experienced the inconvenience arising from such estimates."<sup>2</sup> After approximately sixteen years of experience in mechanical engineering there was no reason why Babbage could not have offered some reasonable estimate of the cost of either completing Difference Engine No. 1 or constructing the Analytical Engine. In a sense, Rice offered Babbage the option of abandoning the old engine and continuing the new one with governmental support if he would provide the government with a cost estimate and decide which engine he would like to work upon. Babbage probably did not want to take the responsibility for such a decision because he had contended that the government had been largely responsible for the delays and problems he had experienced in the construction of

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<sup>1</sup> Letter, Charles Babbage to Spring Rice, October 21, 1838, B.M. Add MSS 37191, X, 14.

<sup>2</sup> Ibid., f. 14.



Difference Engine No. 1.<sup>1</sup> However, he was not very wise in refusing to assume any responsibility for a decision and in failing to offer a cost estimate. Rice could hardly have been expected to give an opinion upon the advisability of proceeding with the construction of the Difference Engine without a direct positive statement either by Babbage or by some engineers appointed by Babbage to provide a cost estimate. Even a relatively high estimate would have been better than none.

The matter rested with the above communication until November of 1841, when Babbage returned from travels on the Continent to find that Sir Robert Peel had become the First Lord of the Treasury. Babbage renewed his effort to secure a decision from the government. He forwarded a letter to Peel in which he observed the following:

When I undertook to give the invention of the Calculating Engine to the Government, and to superintend its construction, there must have been an implied understanding that I should carry it on to its termination. I entered upon that understanding, believing that two or at the utmost that three years would complete it. The better part of my life has now been spent on that machine, and no progress whatever having been made since 1834, that understanding may possibly be considered by the Government as still subsisting: I am therefore, naturally very anxious that this state of uncertainty should be put an end to as soon as possible.<sup>2</sup>

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<sup>1</sup> Letter, "Statement addressed to the Duke of Wellington Respecting the Calculating Engine," from Charles Babbage, December 23, 1834, B.M. Add MSS 37188, VII, 525.

<sup>2</sup> Nicolas, "Statement Relative to the Difference Engine," in Passages, by Babbage, p. 93.

After many delays Babbage finally received a decision from the Government. Goulburn, the new Chancellor of the Exchequer, wrote a letter to Babbage, in November of 1842,<sup>1</sup> in which he stated that he and Sir Robert Peel both regretted the necessity of abandoning the completion of the calculating engine since the expense necessary for rendering it satisfactory or generally useful was far in excess of any justified expense the government could incur. Goulburn did offer Babbage the completed portion of Difference Engine No. 1, but he declined the offer.<sup>2</sup>

Babbage was not ready to accept the decision of Peel and Goulburn even though he had stated to Rice in 1838 and again to Peel in 1842 that he was anxious for the matter to be resolved. He was probably disgusted because of Goulburn's rather negative reference to the expense necessary for rendering the machine satisfactory or generally useful. Babbage had consistently maintained that the engine far exceeded his original expectations and that the portion assembled in 1833 fully supported this conclusion.<sup>3</sup> He accordingly obtained an interview with Peel on November 11, 1842. The interview was not

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<sup>1</sup> Letter, Henry Goulburn to Charles Babbage, November, 1842, Museum, Oxford, MSS Buxton 16-17, ff. 614-616.

<sup>2</sup> Ibid., f. 615.

<sup>3</sup> Letter, "Statement addressed to the Duke of Wellington Respecting the Calculating Engine," from Charles Babbage, December 23, 1834, B.M. Add MSS 37188, VII, 525.

satisfactory as far as Babbage was concerned. He maintained that he had been kept in a state of uncertainty only to receive a notice of the termination of governmental support of the engine without an adequate explanation of why it had been abandoned, and he added:

Now after nine years I have just received notice of the intention of the Government altogether to abandon the engine. During that time I have myself expended a large sum of money, and the public have constantly accused me of having myself received that money which Government has paid to the workmen. On the grounds of the great pecuniary and personal sacrifices which I have made, and on the expectations I might reasonably have entertained upon completion of such a machine, I thought that the services I had rendered ought not to be utterly unrequited and unrewarded.<sup>1</sup>

Babbage also claimed that compared to other men of science he had been neglected, but Peel maintained that the men of science who held governmental positions had received them as rewards for professional services. Babbage replied that he knew of several such men who had never done anything to distinguish themselves professionally, but such arguments did not convince Peel, who, according to Babbage,

seemed excessively angry and annoyed during the whole interview, but more particularly when I knocked over, with some vivacity, his argument about professional service. He then proceeded to attempt humbug, saying the Institutions of the country &c. admitted of certain places being given to certain professions for services not exactly professional and so on.

I listened to all his statements looking him steadfastly in the face. When he got aground I still retained my

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<sup>1</sup> Notes, Charles Babbage's interview with Sir Robert Peel, November 11, 1842, Museum, Oxford, MSS Buxton 16-17, 620.

view of him as if expecting at least some argument would be produced. This position of course was far from agreeable, and certainly not very dignified for a Prime Minister.

Finding Sir R. Peel unwilling to admit that I had any claim, I merely remarked that I considered myself as having been treated with great injustice, but that as he seemed to be of a different opinion, I could not help myself, on which I got up and wished him good morning.<sup>1</sup>

Even before the Difference Engine was abandoned by the government, Babbage had been criticized for not completing the engine and for not communicating the reasons to the public. His only written acknowledgement to the public of the moral difficulties which he had experienced in the construction of the engine was a brief reference in his book, The Ninth Bridgewater Treatise: A Fragment, which he first published in 1837: He wrote as follows:

About the year 1821, I undertook to superintend, for the Government, the construction of an engine for calculating and printing mathematical and astronomical tables. Early in the year 1833, a small portion of the machine was put together, and was found to perform its work with all the precision which had been anticipated. At that period circumstances, which I could not control, caused what I then considered a temporary suspension of its progress; and the Government, on whose decision the continuance or discontinuance of the work depended, have not yet communicated to me their wishes on the question.<sup>2</sup>

The above passage was followed by a brief comparison of Difference Engine No. 1 and the Analytical Engine. Babbage then stated that he would have willingly left the subject of his calculating

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<sup>1</sup> Ibid., f. 615.

<sup>2</sup> Babbage, Treatise, pp. 187-188.

engines,

but the public having erroneously imagined, that the sums of money paid to the workmen for the construction of the engine, were the remuneration of my own services, for inventing and directing its progress; and a committee of the House of Commons having incidentally led the public to believe that a sum of money was voted to me for that purpose, --I think it right to give to that report the most direct and unequivocal contradiction.<sup>1</sup>

In reference to the committee in the House of Commons it should be noted that on May 15, 1835, about two years before Babbage published his Treatise, the Chancellor of the Exchequer had explained to the House of Commons that the entire sum of money voted by the House had been used to pay the workmen employed by Babbage in the construction of the engine and that not one shilling had ever gone into his own pocket.<sup>2</sup> For some reason Babbage did not mention this endorsement of his honesty and integrity.

Babbage's brief mention of the difficulties he had experienced with Difference Engine No. 1 certainly was not satisfactory as an explanation of why no further work had been done on it for three or four years. He devoted a large portion of his Treatise to a comparison of the powers of the Difference Engine and the Analytical Engine. This was just the opposite of what he had intended in 1834, that is, to

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<sup>1</sup> Ibid., pp. 187-188.

<sup>2</sup> Nicolas, "Statement Relative to the Difference Engine," in Passages by Babbage, p. 87.

give an explanation of the moral difficulties he had encountered in the construction of the first engine. His description of the Analytical Engine was a signal to the public that he had the time and money to work on a new engine. The logical question for the public to ask was why he did not have the time and money to work on the old engine in spite of the lack of a decision by the government.

One source of the rumors which had circulated about Babbage and the Difference Engine was George Biddle Airy (1801-1892), who became the Astronomer Royal in 1836. Airy's opinion of the calculating engine was particularly important because he was a governmental advisor on scientific matters. Babbage was informed of Airy's opinion in a letter from a friend, Thomas Romney Robinson (1792-1882), on December 20, 1835. Robinson wrote:

The opinion which I (and [Captain] Beaufort simultaneously) formed respecting Airy's being privy to some plan of attacking you, arose rather from the manner and look than any thing 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 a 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 spent 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 of this, but, wherever it comes from let me extreat you not to despise the attack as unimportant because it is contemptible. It is not, I assume, mere Tory feeling that makes me believe the present Government incapable of looking beyond the shifts of the present minute, and there is not one among them who has elevation of mind enough to

appreciate you or your invention. You must therefore make preparations for defense and don't let yourself be trifled with.<sup>1</sup>

Robinson also advised Babbage to furnish some of his friends with clear statements of the original arrangements between himself and the government, of his entanglements with Clement, and finally with a statement of what was needed to complete the Difference Engine. For some reason Babbage did not take Robinson's advice seriously until after Sir Robert Peel had denied his final request for governmental funds in 1842. Babbage then requested that Sir Nicolas Harris Nicolas (1799-1848), draw up a statement based upon his letters with governmental officials and other persons who had been concerned with Difference Engine No. 1. Nicolas' "Statement" was dated August of 1843, but it is not clear just what Babbage did with it at that time. He included it as a chapter in Passages in 1864 and also listed it in his bibliography as one of his printed papers. Babbage probably circulated it privately among his friends and supporters. The first published account of the circumstances of the engine was written by Charles Richard Weld (1813-1869), in 1849 with footnotes inserted in it by Babbage.<sup>2</sup> This account was in close agreement with the one

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<sup>1</sup> Letter, Thomas Romney Robinson to Charles Babbage, December 20, 1835, B.M. Add MSS 37189, VIII, 220.

<sup>2</sup> C[harles] R[ichard] Weld, The Eleventh Chapter of the History of the Royal Society (London: Richard Clay, Printer, 1849).

by Nicolas. It had two reviews appended to it--one by Augustus De Morgan (1806-1871),<sup>1</sup> a mathematician, and the other by an anonymous reviewer.<sup>2</sup> As will be discussed in detail, the accounts by Nicolas and Weld as well as the two reviews were generally favorable to Babbage, but they could not take the place of a detailed, personal statement by Babbage himself.

It is difficult to understand why Babbage waited until 1851 to publish an account of his own--The Exposition of 1851: or, Views of the Industry, the Science, and the Government of England.<sup>3</sup> In this book Babbage not only repeated some of the same sentiments concerning governmental support of science as he had in Reflections in 1830, but he also devoted a chapter to "Intrigues of Science," in which he referred to Weld's account of the Difference Engine and stated that he hoped it would make any further explanation on his part unnecessary. He devoted the remainder of the chapter to a discussion of why he thought his relationship with the government had been so difficult from about 1831-1843. He named Airy and his friend Richard Sheepshanks (1794-1855) as co-conspirators in an attempt to discredit

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<sup>1</sup> Augustus De Morgan, "Review of the Eleventh Chapter of Mr. Weld's History of the Royal Society," Athenaeum (London), Saturday, October 14, 1848.

<sup>2</sup> "Mr. Babbage's Calculating Machine," Athenaeum (London), Saturday, December 16, 1848.

<sup>3</sup> (London: John Murray, 1851).



him and his calculating engines.<sup>1</sup> To fully understand Babbage's charges against Airy and Sheepshanks and the relationship of those charges to the circumstances surrounding the development of the Difference Engine, it will be useful to give the following account of the conflict between Airy and Babbage and Sheepshanks and Babbage.

Babbage's first conflict with Airy began in 1826 when the two opposed each other for the Lucasian Professorship of Mathematics at Cambridge.<sup>2</sup> Babbage lost to Airy but was elected in 1828 when Airy took another position.<sup>3</sup> In 1832 Babbage supported James South who was engaged in arbitration over a telescope mounting. Sheepshanks represented the defendant Edward Troughton (1753-1835), who was a well known manufacturer of optical instruments, by acting both as an attorney and witness. He was assisted in the dispute by Airy. South was eventually forced to pay for the mounting after he refused to allow it to be repaired. Babbage gained nothing for his trouble except to become an enemy of Sheepshanks and of Airy. During the trial Sheepshanks threatened to discredit Babbage for his support of South.<sup>4</sup>

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<sup>1</sup> Ibid., pp. 149-172.

<sup>2</sup> Letter, William Whewell to the Rev. R. Jones, December 10, 1826, in William Whewell, by Todhunter, II, 79-80.

<sup>3</sup> Letter, William Whewell to John Herschel, March 6, 1828, ibid., pp. 90-91.

<sup>4</sup> Babbage, Exposition of 1851, pp. 156-164.

Babbage claimed in 1851 that Sheepshanks had said to him in March of 1831:

'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.' He told me at the same time he intended to put Captain Baufort down.<sup>1</sup>

Babbage learned in 1835 through Robinson,<sup>2</sup> as noted above, that Airy had made disparaging remarks about him in connection with the calculating engines. As a result of this information Babbage had suspected in 1842 that Airy had advised Sir Robert Peel against further support of his calculating machines but he had no definite proof.<sup>3</sup> However, by 1851 he claimed to possess firm evidence that Airy and Sheepshanks had participated in an intrigue against him in order to discredit him, but he did not state what it was.<sup>4</sup> In 1854 Sheepshanks replied to Babbage's charges by stating that it was very probable that he had threatened to discredit those who supported South in March of 1831. As far as his opinion of the Difference Engine was concerned, he stated that:

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<sup>1</sup> Ibid., p. 157.

<sup>2</sup> Letter, Thomas Romney Robinson to Charles Babbage, December 20, 1835, B.M. Add MSS 37189, VIII, 220.

<sup>3</sup> Babbage's minutes of an interview with Sir Robert Peel, November, 1842, Museum, Oxford MSS Buxton 16-17, ff. 616-642.

<sup>4</sup> Babbage, Exposition of 1851, pp. 166-167.

What I have always felt since the acknowledged failure of that unlucky undertaking, and what I have said doubtless to Mr. Airy and other people, is, that Mr. Babbage must be considered as under a cloud until he himself gives a full account of the reason of the failure. I have said that Mr. Babbage owed this explanation, not merely to his own character, but to the country whose money he wasted, and his friends who pressed his schemes on the Government.<sup>1</sup>

Sheepshanks evidently did not acknowledge Weld's account of the Difference Engine as a creditable substitute for a personal account by Babbage. Weld had stated that he had examined a statement written up by Babbage (this was probably the one Nicolas had written in 1843) as well as his letters and other documents in the archives of the Royal Society.<sup>2</sup>

Airy remained silent on the whole issue and did not confirm the statements made by Sheepshanks or deny the charges of an intrigue made by Babbage. In fact, it was not until long after Babbage's death, that Airy admitted in 1896, that he had advised Goulburn, the Chancellor of the Exchequer to scuttle the Difference Engine in 1842:

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 fully into the matter, and giving my opinion that it was worthless.<sup>3</sup>

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<sup>1</sup> Richard Sheepshanks, A Letter to the Board of Visitors of the Greenwich Royal Observatory: in Reply to the Calumnies of Mr. Babbage at Their Meeting in June 1853 and in His Book Entitled "The Exposition of 1851" (London: G. Barclay, 1854), pp. 63-64.

<sup>2</sup> Weld, Eleventh Chapter, p. 8.

<sup>3</sup> George Biddle Airy, Autobiography of Sir George Biddle Airy: R.C.B., M.A., L.L.D., D.C.L., F.R.S., F.R.A.S., Honorary Fellow

Although Babbage had not direct knowledge of the conversation between Airy and Goulburn his suspicions concerning Airy were fairly accurate. He felt that Airy had acquired excessive power as Astronomer Royal by accepting additional duties on governmental commissions and by acting as an advisor to the government on important scientific matters which included calculating machines.<sup>1</sup>

Babbage also attacked Sheepshanks on another issue. He accused Sheepshanks of perjury and of forgoing Troughton's name on an astronomical instrument in order to avoid paying an importation duty. Sheepshanks had purchased the instrument in France. Sheepshanks admitted to the general substance of the charges but offered a slightly different version.<sup>2</sup> The whole affair was debated in a Royal Society meeting in 1851 in which Airy defended Sheepshanks and attempted to stifle any discussion of the issue. As a result, both Airy and Sheepshanks were severely criticized by an anonymous reviewer of Babbage's Exposition for conspiring on various occasions to discredit not only Babbage but other persons who opposed them:

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Trinity College, Cambridge, Astronomer Royal from 1836 to 1881, ed. by Wilfrid Airy (Cambridge: University Press, 1896), p. 152.

<sup>1</sup> Babbage, Exposition of 1851, pp. 165-172.

<sup>2</sup> Royal Society: the Morality of Its Members (London: William Tyler, 1854), pp. 4-6.

The Astronomer Royal has availed himself of the position that he holds in the public service to give his reverend and pious friend's [Sheepshanks] malice practical operation through the Government influence; and, as a quid pro quo for this congenial patronage and assistance, the other has on all occasions acted as the Astronomer Royal's tool and defender.<sup>1</sup>

The anonymous reviewer called upon both Airy and Sheepshanks to provide written answers to the charges brought by Babbage and by himself.<sup>2</sup> In 1854 Sheepshanks did provide such an answer in a ninety-two page letter which did little to vindicate either Airy or himself.<sup>3</sup>

The entire debate did give substance to Babbage's charge that Airy and Sheepshanks had conspired against him and by so doing had seriously damaged his scientific career. The advantage to Babbage was dubious since he was open to such criticisms as having a "morbid impatience" with those who dissented from his ideas and opinions. However, he was aware that even though his own scientific pursuits "were of such a character that they interfered with those of none of my colleagues in the paths of science," yet he may have "trusted too much to this circumstance as exempting me from rivalry and

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<sup>1</sup> Ibid., p. 12.

<sup>2</sup> Ibid., pp. 14-15.

<sup>3</sup> Sheepshanks, Letter, pp. 1-92.

jealousy."<sup>1</sup> To this he added:

As a reformer both in science and in politics, I knew that I should excite enmity in the minds of some honest men, and also in those of many other persons who dreaded inquiry into jobs not yet exposed. When I published the *Decline of Science*, in 1830, I certainly was not aware how many would include themselves in the latter class: but had I foreseen it, I should not have altered my course.<sup>2</sup>

In spite of these comments, Babbage was certainly not satisfied with the reputation he had acquired throughout his career. His conduct in all the matters mentioned above made him appear as a person who was dissatisfied with anyone who opposed him and particularly with governmental officials.

In his review of Weld's Eleventh Chapter, Augustus De Morgan took a very caustic view of Babbage's treatment by the government, but, at the same time, he did not let Babbage off lightly. He stated that the public had a right to an explanation from the Government and a further explanation from Babbage. As noted above, Babbage did give a partial explanation of his own in the Exposition of 1851 and in Passages in 1864, but this was after DeMorgan's scornful remark in 1848 concerning Sir Robert Peel's conduct of the matter:

Sir R. Peel turned it off with a joke in the House of Commons. He recommended that the machine should be set to calculate the time at which it would be of use. He

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<sup>1</sup> Babbage, Exposition of 1851, p. 154.

<sup>2</sup> Ibid., p. 155.

ought rather to have advised that it should be set to compute the number of unanswered applications which might remain unanswered before a Minister, if the subject were not one which might affect his parliamentary power. If it had done this, it would have shown that its usefulness had commenced.<sup>1</sup>

De Morgan maintained in general that Babbage's conduct in his negotiations with the government appeared to have been correct, but he did offer a possible explanation of why the calculating engine was abandoned:

It is possible that Sir R. Peel and Mr. Goulburn allowed Mr. Babbage's well-known wish to abandon the first plan [machine] in favour of a new one to influence their decision. It may be that they were startled at finding the £17,000 expended upon one project was only the precursor of another. If so, we think they put themselves in the wrong by not fastening on Mr. Babbage the alternative of either proceeding with the existing construction, or taking the entire responsibility of refusal upon himself. As the matter now stands, and unless Mr. Babbage can be refuted, the answer to the question why he did not proceed is, that during the eight years in which he had to bear the blame of the delay he could not procure even the attention of the Government, much less any decision on the course to be taken.<sup>2</sup>

De Morgan was evidently not aware of all the correspondence Babbage had carried on from 1833-1842 in an attempt to procure a decision from governmental officials. De Morgan's figure of £17,000 as the cost of Difference Engine No. 1 was misleading because only

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<sup>1</sup> Augustus De Morgan, "Review of the Eleventh Chapter," pp. 5-6.

<sup>2</sup> Ibid., pp. 5-6.

£ 11,000 was actually expended upon the engine with another £ 6,000 expended upon a fireproof workshop constructed adjacent to Babbage's residence. Also De Morgan failed to mention that part of the total expenditure was for the construction of machine tools, which, like the workshop, could have been sold to recover part of the money which had been expended. When De Morgan claimed that it was Babbage's well-known wish to abandon Difference Engine No. 1 and to start on the Analytical Engine, he placed Babbage in an awkward position because he offered no evidence. If Babbage had such a desire, it may have been known to his friends and associates, but there is no evidence in his correspondence with governmental officials, except possibly the correspondence with Rice in which he first stated that the Analytical Engine "completely supersedes the old Engine,"<sup>1</sup> and thus confused Rice as to his true intentions. Babbage refuted De Morgan's claim as follows:

It is scarcely possible that this supposed wish could have influenced Sir Robert Peel, because he had before him a written disavowal of it from Mr. Babbage himself.<sup>2</sup>

De Morgan had pointed out however that

scientific rumour states that Mr. Babbage compelled the Government to give him up by demanding permission

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<sup>1</sup> Letter, Charles Babbage to Spring Rice, February 1, 1836, B.M. Add MSS 37189, VIII, 291.

<sup>2</sup> Babbage in a footnote to De Morgan, "Review of the Eleventh Chapter of the Royal Society," p. 6.



to abandon the Difference Engine and substitute the Analytical Engine. To this, in the formal point of view, Mr. Babbage has fully answered, by showing that the Government never communicated to him that it was their pleasure he should proceed on the plan originally. The question now remains--Did Mr. Babbage, or did he not, in the several unanswered applications which he made to the Ministry, press the claims of the new machine and the abandonment of the old?<sup>1</sup>

De Morgan answered his own question by stating that Babbage had indeed asked Rice in October of 1838 whether it was the wish of the government to complete the Difference Engine "according to the original plan and principles; or whether they intended to discontinue it altogether?"<sup>2</sup> Although De Morgan concluded that by this question Babbage had placed the responsibility for an unequivocal reply upon the government, he was evidently unaware of Rice's request that Babbage decide which plan he wished to follow.

Babbage may have been encouraged by De Morgan and the Countess of Lovelace to reconsider the completion of the Difference Engine as a means of satisfying his critics, even though the government had withdrawn its support. After he had completed the essential design of the Analytical Engine in 1848,<sup>3</sup> Babbage made a complete set of plans for a Difference Engine No. 2 which was based

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<sup>1</sup> Ibid., p. 7.

<sup>2</sup> Ibid., p. 7.

<sup>3</sup> Buxton, Charles Babbage, Museum, Oxford, MS Buxton 16-17, f. 642.

upon all the improvements and simplifications developed in the Analytical Engine. Difference Engine No. 2 had seven orders of difference as did Difference Engine No. 1, but its column of number wheels were arranged in order around a central cylinder.<sup>1</sup>

In 1852 Babbage was asked by William Parsons Rosse, third Earl of Rosse (1800-1867), an astronomer, if he were willing to give his drawings of Difference Engine No. 2 to the government with the provision that the engine would be constructed. Babbage reluctantly agreed and with the help of a friend Sir Benjamin Hawes (1797-1862), Undersecretary of War, drafted a letter making such an offer. The suggestion was also made to ask the government to enlist the aid of the Institution of Civil Engineers to make an estimate of the cost of the machine.<sup>2</sup> This step would have eliminated the difficulties which Babbage had experienced with trying to act as his own production engineer in estimating costs for Difference Engine No. 1. Unfortunately, the suggestion was not followed. Instead, when the proposal was submitted to Lord Derby, Stanley Edward George Geoffery Smith, Fourteenth Earl of Derby (1799-1869), the Prime Minister, he in turn referred it to Benjamin Disraeli, First Earl of Beaconsfield,

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<sup>1</sup>Ibid., f. 1514.

<sup>2</sup>Ibid., f. 644.

(1804-1881), the Chancellor of the Exchequer.<sup>1</sup> Disraeli reported back to Lord Derby

That Mr. Babbage's projects appear to be so indefinitely expensive, the ultimate success so problematical, and the expenditure certainly so large and so utterly incapable of being calculated, that the Government would not be justified in taking upon itself any further liability.<sup>2</sup>

The excuse given by Disraeli with regard to cost did not make much sense. First, the long and costly development of the Analytical Engine and Difference Engine No. 2 had already been financed entirely by Babbage. All the mechanical drawings for the machines existed in a final state ready for production. The only steps necessary to determine the cost of final production was to ask for an estimate by engineers as suggested by the Earl of Rosse. Second, the state of machine technology in 1852 had considerably advanced beyond what it had been in the development and production period of Difference Engine No. 1 due in great part to Babbage's own efforts. Therefore, Disraeli had only to consult a group of engineers to support his conclusions about the expense and utility. This he did not do.<sup>3</sup> It is very likely that he consulted Airy instead, as Goulburn had done in 1842. Third, the production of Difference Engine No. 2 would have primarily

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<sup>1</sup> Ibid., f. 646.

<sup>2</sup> Ibid., f. 688.

<sup>3</sup> Ibid., f. 734.

consisted in the duplication of existing parts which Babbage had constructed for use in the Analytical Engine. The costly experimentation necessary to develop a final prototype of any given part had already been completed.<sup>1</sup>

Babbage, never at a loss for words when he thought someone had undervalued his calculating engines, stated that the "much abused Difference Engine" was "like its prouder relative the Analytical, a being of sensibility, of impulse and of power,"<sup>2</sup> which could certainly calculate the millions the ex-Chancellor of the Exchequer had squandered. He continued:

Yet should any unexpected course of events ever raise the ex-Chancellor of the Exchequer to his former dignity, I am sure he will be its friend as soon as he is convinced that it can be made useful to him.

It may possibly enable him to un-muddle even his own financial accounts, and to--

But I have no wish to crucify him, I will leave his name in obscurity.

The Herostratus of Science, if he escape oblivion, will be linked with the destroyer of the Ephesian Temple.<sup>3</sup>

For a second time Babbage had failed to convince at least one of his publics--governmental officials--that his invention was worthy of

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<sup>1</sup> Ibid., f. 738.

<sup>2</sup> Babbage, Passages, p. 110.

<sup>3</sup> Ibid., p. 111. Herostratus, an Ephesian, set fire to the temple of Artemis at Ephesus on the same night that Alexander the Great was born, 356 B. C. He was put to torture and confessed that he had fired the temple to immortalize himself. The Ephesians passed a decree condemning his name to oblivion.

consideration and financial support. He may have helped in advance to scuttle the attempt by the Earl of Rosse to obtain financing for Difference Engine No. 2 by his ill chosen remarks about Airy in his Exposition in 1851. Airy, as has been discussed with reference to Difference Engine No. 1, was respected as a governmental advisor on scientific matters and had advised Sir Robert Peel against further governmental financing of the calculating engines in 1842. As noted above, it is therefore very likely that he was once again consulted in 1852--this time by Disraeli.

It is to Babbage's credit that he attempted to obtain governmental financing for Difference Engine No. 2 because by so doing he demonstrated that he was willing to complete at least one of his calculating machines. He also placed the responsibility for its construction upon the government because he had agreed not only to turn over all the drawings which he had paid for but also to allow a group of engineers to superintend its construction. Thus, if the engine had not been completed, the responsibility would have rested with someone other than Babbage. Babbage's generosity in the matter of Difference Engine No. 2 should have satisfied his critics.

In considering Babbage's negotiations with governmental officials, it is apparent that he was not able to present his point of view to the government in a consistent and straightforward manner. The interests of all concerned would almost certainly have been much

better served if Babbage had followed Bromhead's advice and negotiated with the government through a committee made up of scientists, engineers, and governmental officials. The last committee to act upon the matter of the Difference Engine was the one appointed by the Royal Society in 1829 with John Herschel, Chairman. This committee was able to secure a commitment of governmental support for the engine. Unfortunately, Herschel, who Babbage had always depended upon, spent from November of 1833 to March of 1838 at the Cape of Good Hope observing and mapping the stars of the southern hemisphere and was therefore unable to lend his active support. Also, after 1830 Babbage and his circle of friends were not on good terms with the leaders in the Royal Society, particularly following Babbage's publication of Reflections on the Decline of Science. The external coterie declined to serve on a committee to reform the Royal Society which did little to help Babbage with his problems. Babbage might have been able to negotiate successfully with the government if he had taken advantage of the Lardner's favorable comments and acted promptly to answer the questions which Lardner raised. A standing committee would again have been a help in providing the public with an objective report which would have answered Lardner's questions, supported his favorable comments, and acted as a guide to Babbage particularly when he vacillated in his negotiations with governmental officials. Such a committee could also have

exerted its influence upon those governmental officials who chose to ignore Babbage's request for a firm commitment or for an unequivocal reply to his request for a decision on the Difference Engine.

It was Babbage's opinion that he had been ill served by his own countrymen, that his reputation had been impugned unfairly by his critics, and that of all the men who had worked upon the Difference Engine he had benefited the least. In particular, he claimed that two of the most able men he had employed--Joseph Clement and Joseph Whitworth--had derived much more benefit from their work on the engine than he:

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.'

When I first employed Clement he possessed one lathe (a very good one) and his workshop was in a small kitchen. When I ceased to employ him [about 1833], he valued his tools at several thousand pounds, and had converted a large chapel into a workshop. Whitworth has made a fortune of which he spends with great liberality.<sup>1</sup>

The peripheral benefits which Lardner had mentioned were primarily those related to scientific matters, but Babbage certainly had a fair claim to some public endorsement of his efforts not only for his work on calculating engines but also for his contributions to the machine tool industry. It was ironical that no governmental or private body existed at the time to provide encouragement to persons like Babbage and to assess the value of contributions to science and technology in terms of the peripheral as well as the direct benefits.

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<sup>1</sup>Memorandum, Charles Babbage on Joseph Clement, November 9, 1869, B. M. Add MSS 37189, VIII, 499.

## X. THE ANALYTICAL ENGINE--A TECHNICAL AND THEORETICAL DESCRIPTION

When Babbage announced the invention of Difference Engine No. 1 in the early eighteen twenties, he won the acclaim of the Astronomical Society. In contrast, the initial ideas for the Analytical Engine occurred to Babbage during a period of personal adversity about 1833-1834 when he was involved in a dispute with Clement. The adversity was advantageous as far as the development of the Analytical Engine was concerned because he had a long time to reflect upon the entire problem of machine calculation. During this time he was relatively unencumbered by the details of the theory and mechanical construction of the Difference Engine. Also, since he did not have access to the engine and its drawings for about a year, he was not influenced by its design to the extent that he might have been if the machine and drawings had been accessible.<sup>1</sup> He began to reconsider the problem of machine calculation from a much broader point of view. Difference Engine No. 1, for example, had been designed as a special purpose machine which was capable of calculating and printing mathematical, nautical and astronomical tables by means of a single function. While both engines employed columns of wheels and gearing mechanisms for performing arithmetical operations, such as in

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<sup>1</sup>Babbage, Treatise, pp. 187-187.



Figures 6 and 7, the design for the Analytical Engine was far more extensive and complex. As shown in Figure 8, the components of the Analytical Engine included a counting apparatus for counting repetitive cycles of operations; an anticipating carriage for performing all carriages simultaneously; and numerous barrels, gears, and racks which were assembled around a large central barrel. In addition, peripheral apparatus such as a curve drawing device could be attached to the engine. However, the most important feature of the Analytical Engine was that it was designed as a universal calculating machine and was therefore not limited to the calculation of any single function.

Babbage had designed the engine so that it would be capable of

developing and tabulating any function whatever. In fact the engine may be described as being the material expression of any indefinite function of any degree of generality and complexity, such as for instance,

$$F(x, y, z \log x, \sin y, x^p, \& c.),$$

which is, it will be observed a function of all other possible functions of any number of quantities.<sup>1</sup>

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<sup>1</sup> Ada Augusta, Countess of Lovelace, trans. and ed., "Notes by the Translator," in Sketch of the Analytical Engine: Invented by Charles Babbage, Esq., by L[uis] F[rederico] Menabrea (London: Richard and John E. Taylor, 1843), p. 691, with Notes by the translator. Menabrea's original article, "Sketch of the Analytical Engine Invented by Charles Babbage," was published in the Biblioteaue Universelle de Geneve, III, No. 82 (October, 1842). The roles which the Countess of Lovelace, Menabrea, and Babbage's youngest son, Henry Babbage, played in the development of the calculating engine will be discussed in the following chapter which concerns the speculative and philosophical questions relating to machine calculation. Their writings relate to the technical and theoretical features of the Analytical Engine and will therefore be used in this chapter.

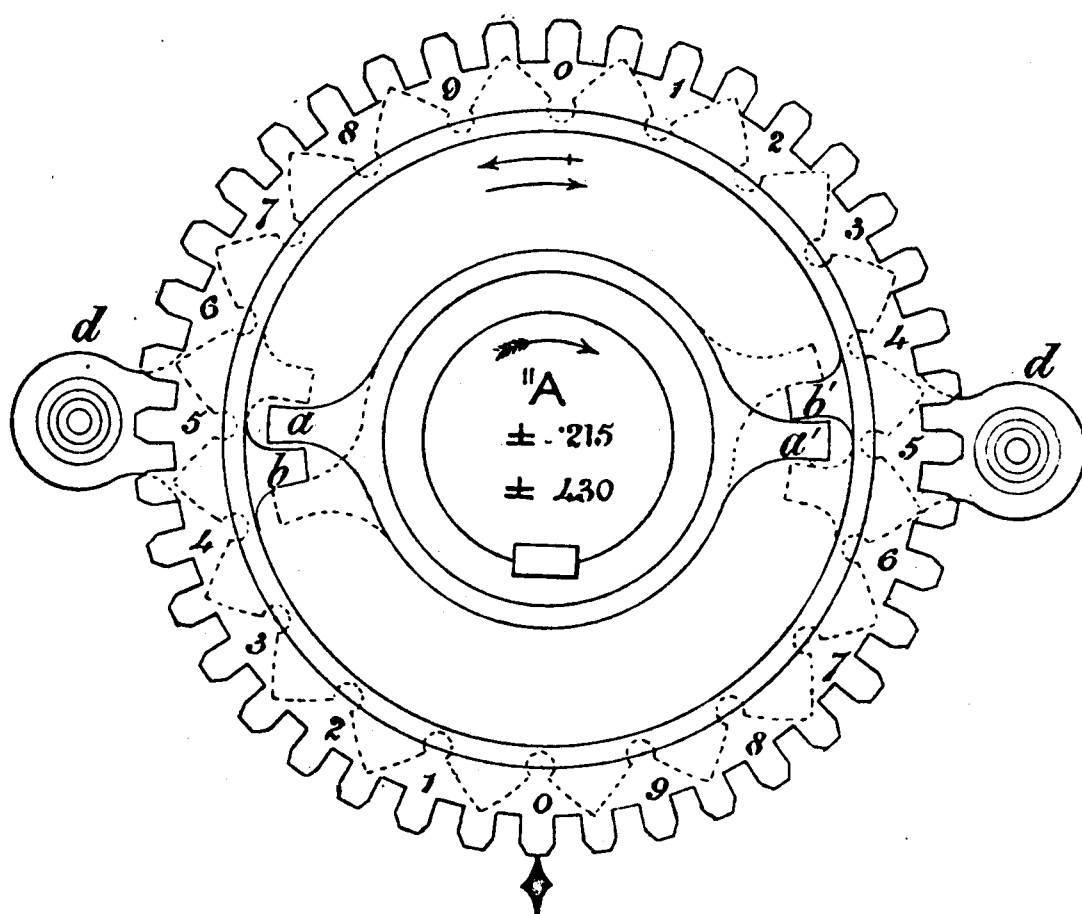


Figure 6. Figure wheels. From Engines, ed. by Henry P. Babbage. Plan of the figure wheels for one method of adding numbers.

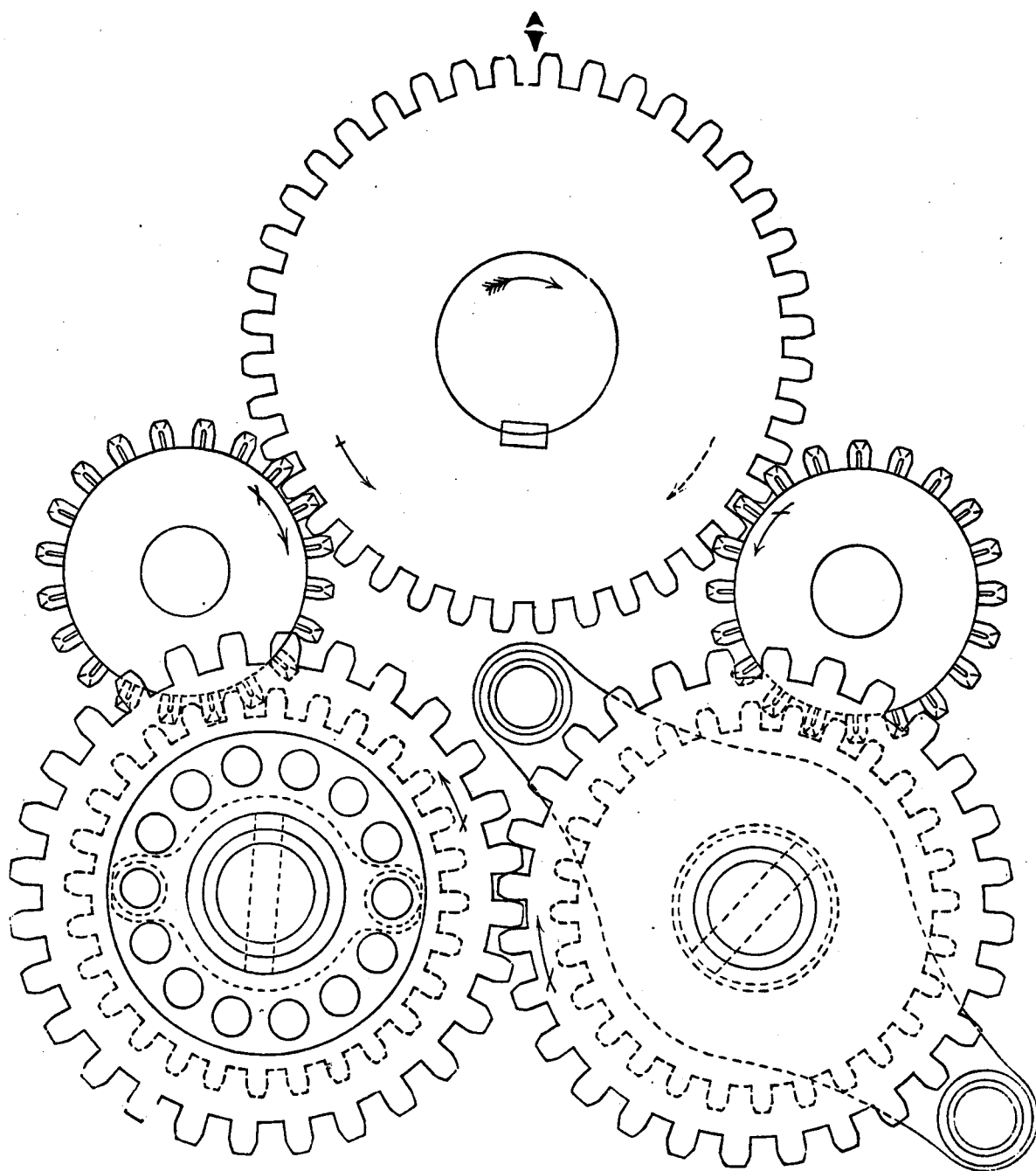


Figure 7. Stepping wheels. From *Engines*, ed. by Henry P. Babbage. Plan of adding wheels and of long and short pinions, by means of which stepping is accomplished. N. B. -- This process performs the operation of multiplying or dividing a number by any power of ten.

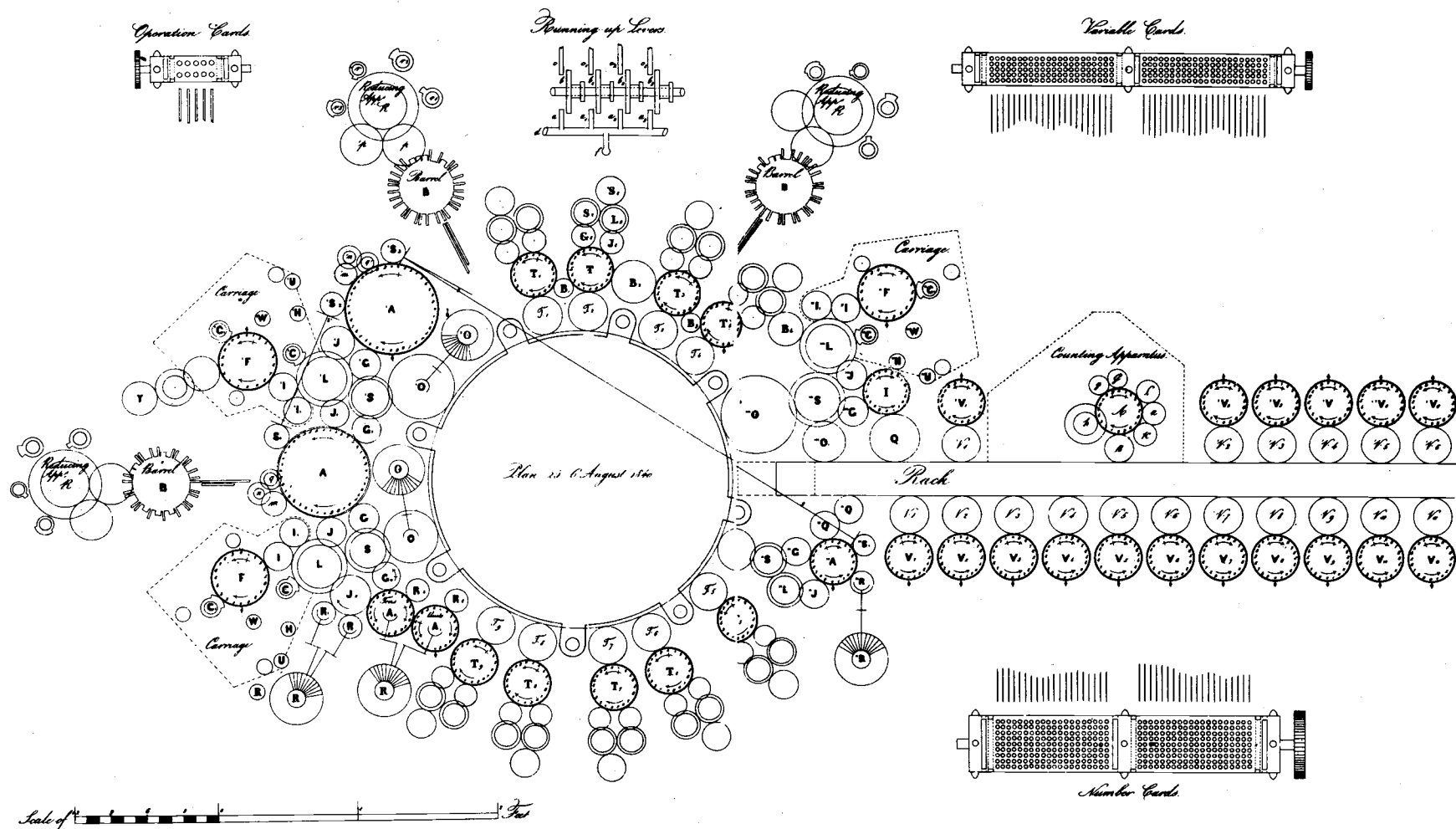


Figure 8. General plan of the Analytical Engine. From Engines, ed. by Henry P. Babbage. Plan 25, dated August 6, 1840.

Thus, the step from a special purpose machine capable of calculating only one kind of function to one capable of "developing and tabulating any function whatever" was particularly ingenious. Until 1834, Babbage had not seriously considered such an idea except in a very fleeting manner. When the idea reached a certain critical stage within his mind, it changed from what Babbage described as a "shadowy vision"<sup>1</sup> or those vague and inexact ideas which usually precede the more formal stage of a theory or invention<sup>2</sup> to the stage whereby the ideas become "susceptible of demonstration."<sup>3</sup> The time Babbage had spent in speculation and work on Difference Engine No. 1, as well as related activities such as his visitation to factories and workshops scattered throughout Europe, helped him to transform his "shadowy vision" into detailed drawings and metal work. In 1832, for example, Babbage published a book in which he described the ideas and information he had gained as the result of his travels.<sup>4</sup> He was particularly imbued with the idea of division of labor as it applied

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<sup>1</sup> Babbage, Passages, p. 112.

<sup>2</sup> Charles Babbage, "Of the Invention of Machinery," about August, 1841, Museum, Oxford, MS Buxton 8, f. 58.

<sup>3</sup> Babbage, Passages, p. 113.

<sup>4</sup> Charles Babbage, On the Economy of Machinery and Manufactures (4th ed. enlarged; London: Charles Knight, 1835). The first edition was published in 1832.

both to the economy of manufacturing<sup>1</sup> and to the economy of mental labor.<sup>2</sup> Since the invention of the Analytical Engine made it necessary to invent some new machine tools and techniques of manufacturing as well as to replace those tools which Clement retained,<sup>3</sup> Babbage was interested in developing or adopting manufacturing practices and techniques which would conserve his own financial means and yet satisfy his desire for excellence in machine work.<sup>4</sup> The Analytical Engine was to Babbage more than just a reduction of mental process to machinery. He considered it an extension of mental processes in that it was capable of performing mental tasks which were formerly performed only by humans. Therefore, Babbage was also interested in the idea of division of labor as it related to the economy of mental labor. He had already adopted a means of conserving his own mental labor as early as 1827 with his invention of mechanical notation.<sup>5</sup> But in 1834 he was ready to incorporate the idea of economy of mental labor into the theoretical design of the Analytical Engine.

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<sup>1</sup> Ibid., pp. 169-190.

<sup>2</sup> Ibid., pp. 191-202.

<sup>3</sup> Clement retained most of the machine tools Babbage had invented for Difference Engine No. 1 as was his right under English law.

<sup>4</sup> Ibid., pp. 119-122.

<sup>5</sup> Babbage, Passages, p. 113.

The idea for the division of mental labor as applied to the Analytical Engine came from a procedure for calculating mathematical tables developed by Gaspard Clair François, baron de Prony (1755-1839), director of the Ecole des Ponts et Chaussees in France.<sup>1</sup> Prony had been commissioned by the French government, shortly after the French Revolution, to produce a series of mathematical tables to facilitate the application of the recently adopted metric system. Prony did not possess any means of carrying out his calculations mechanically, but he made use of the idea of division of labor in dividing the work to be done by his staff into three sections. The duty of the first section was to choose the best analytical function which could be readily adapted to simple numerical calculation. The duty of the second section, consisting of persons highly skilled in mathematics, was to substitute numbers into the formulae given to them by the first section. The second section then gave the formulae and data to the third section, which did the actual computing. Finally, the second section verified the results of the third section. The members of the third section had little knowledge of mathematics beyond the simple arithmetical operations of addition and subtraction, but they were usually more accurate in computation than those who had extensive knowledge of mathematics.<sup>2</sup> In Babbage's Difference Engine

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<sup>1</sup> Babbage, Economy, pp. 192-194.

<sup>2</sup> Ibid., pp. 194-195.

No. 1, the operations performed by the third section of Prony's group of computers were done mechanically as discussed by Babbage in the following:

The exertions of the first class are not likely to require, upon another occasion, so much skill and labour as they did upon the first attempt to introduce such a method; but when the completion of a calculating--engine shall have produced a substitute for the whole third section of computers, the attention of analysts will naturally be directed to simplifying its application, by a new discussion of the methods of converting analytical formulae into numbers.<sup>1</sup>

The development of a universal calculating machine capable of converting analytical formulae into numbers required completely new methods compared to those employed in Difference Engine No. 1. In the Difference Engine the data for a calculation was supplied by manually turning the number wheels of the machine to the desired setting. For each new calculation the process had to be repeated. Also, as has been shown, the machine was specifically built to calculate only one kind of function. Babbage achieved the flexibility of a universal calculating machine, that is, his Analytical Engine, by adopting another idea he had discovered while studying mechanical devices throughout Europe. One of the devices which caught his attention were the pattern cards used in the weaving industry. The pattern cards were the invention of Joseph Marie Jacquard (1752-1834),

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<sup>1</sup>Ibid., p. 195.



who used the cards to automatically weave the complex patterns in the fabrics which he manufactured.<sup>1</sup> As described by Babbage, the pattern cards introduced a great versatility into the automation of the weaving industry.

It is a known fact that the Jacquard loom is capable of weaving any design which the imagination of man may conceive. It is also the constant practice for skilled artists to be employed by manufacturers in designing patterns. These patterns are then sent to a peculiar artist, who, by means of a certain machine, punches holes in a set of pasteboard cards in such a manner that when those cards are placed in a Jacquard loom it will then weave upon its produce the exact pattern designed by the artist.<sup>2</sup>

Using the Jacquard loom a manufacturer was able to use either threads of all the same color or one color for the warp or longitudinal thread and another for the weft or transverse thread. In either case, the form of the pattern designed by the artist and placed on a pattern card was exactly the same but with different colors.<sup>3</sup> Prior to the invention of the pattern cards the process of weaving a pattern was lengthy and difficult. A workman was required to regulate the movements of the threads according to the design he was supposed to copy.<sup>4</sup> With pattern cards this whole operation was not only automatic, but it

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<sup>1</sup>Babbage, Passages, p. 116.

<sup>2</sup>Ibid., pp. 116-117.

<sup>3</sup>Menabrea, "Sketch," p. 677.

<sup>4</sup>Ibid., p. 677.

was possible for every thread to be of a different shade of color so that one particular pattern would yield many different varieties of color combinations.<sup>1</sup>

Babbage noted that the analogy of the Analytical Engine to the Jacquard process was nearly perfect:

The Analytical Engine consists of two parts:--  
[see Figure 9]

1st. The store in which all the variables to be operated upon, as well as all those quantities which have arisen from the result of other operations, are placed.

2nd. The mill into which all the variables to be operated upon are always brought.

Every formula which the Analytical Engine can be required to compute consists of certain algebraical operations to be performed upon given letters, and of certain other modifications depending upon the numerical value assigned to those letters.<sup>2</sup>

In mentioning the analogy between the Jacquard loom and the Analytical Engine, Babbage was referring in particular to the cards which governed the weaving patterns in the loom and the cards which governed the calculation of algebraic or analytical formulas such as integration formulas. Just as pattern cards governed the operation of the Jacquard loom, Babbage reasoned that punched cards could govern the operations of the Analytical Engine and carry out a variety of functions during a calculation.<sup>3</sup> For example, whenever any

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<sup>1</sup> Babbage, Passages, p. 117.

<sup>2</sup> Ibid., p. 117.

<sup>3</sup> Menabrea, Sketch, pp. 705-706.

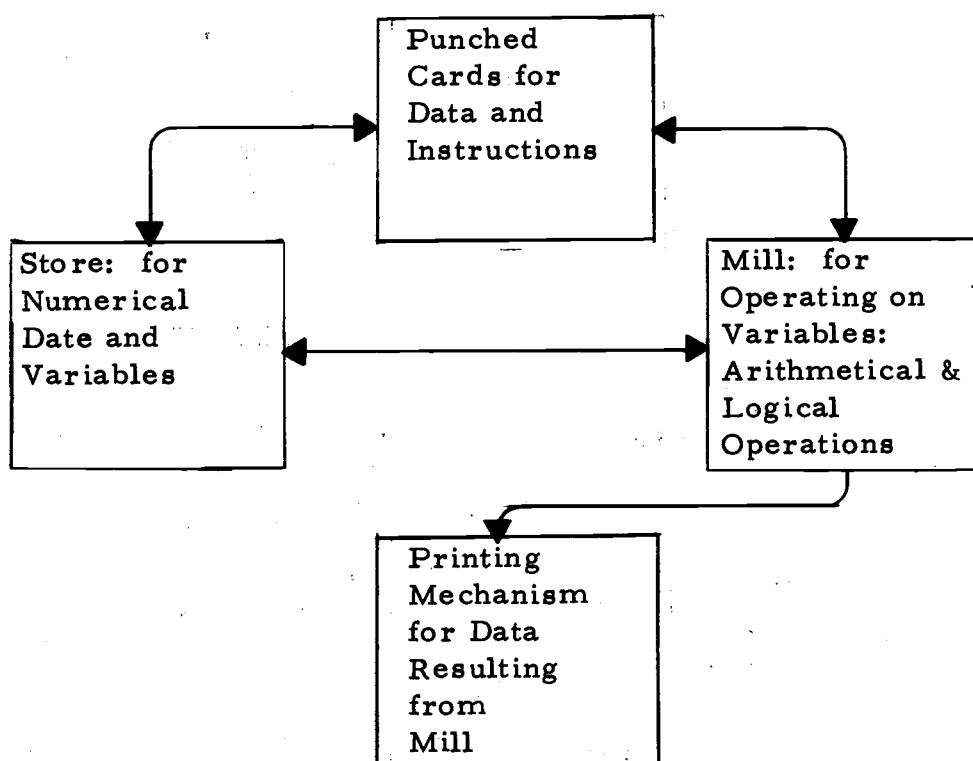


Figure 9. Organizational diagram of the Analytical Engine.

formula was required for the engine it was transferred to a set of cards called operation cards. The cards were then strung on wires in the sequence demanded by the operation. The function of the operation cards was to put the engine into a mechanical state or condition to perform whatever arithmetical operations were desired, in the order desired. Another set of cards, called number cards, communicated the "given numbers or constants" to the engine. A third set of cards, called directive cards, had three basic functions: to direct the given numbers or constants recorded on the number cards to a particular column of the engine; to direct any intermediate numbers arising in the course of the calculation from one column of the engine to another; and to carry out certain control operations within the engine as necessary.<sup>1</sup>

Using such cards, it was possible to reduce any mathematical formula into distinct, sequential, arithmetical operations (see Table 3 and Figures 10 and 11 for details). A formula such as  $(ab + c)d$  made use of both directive and operation cards. Four directive cards for the variables  $a$ ,  $b$ ,  $c$ , and  $d$ , could be strung together and placed by hand on a roller. Each number was then assigned to a

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<sup>1</sup> Henry P. Babbage, "The Analytical Engine," Proceedings of the British Association for the Advancement of Science, September 12, 1888; in Engines ed. by Henry P. Babbage, pp. 331-332.

Table 3. Program of the formula  $(ab + c)d$ .

Directive Card	Operation Card	Intermediate and Final Results and Comments
1st	---	Places a on column 1 of Store
2nd	---	" b " 2 "
3rd	---	" c " 3 "
4th	---	" d " 4 "
5th	---	Brings a from Store to Mill
6th	---	" b " " "
---	1	Multiplies $a \cdot b = p$
7th	---	Takes p to column 5 of Store where it is kept for use and record
8th	---	Brings p into Mill
9th	---	" c " " "
---	2	Adds $p + c = q$
10th	---	Takes q to column 6 of Store
11th	---	Brings d into Mill
12th	---	" q " " "
---	3	Multiplies $d \cdot q = p_2$
13th	---	Takes $p_2$ to column 7 of Store
14th	---	Takes $p_2$ to printing or stereo-moulding apparatus

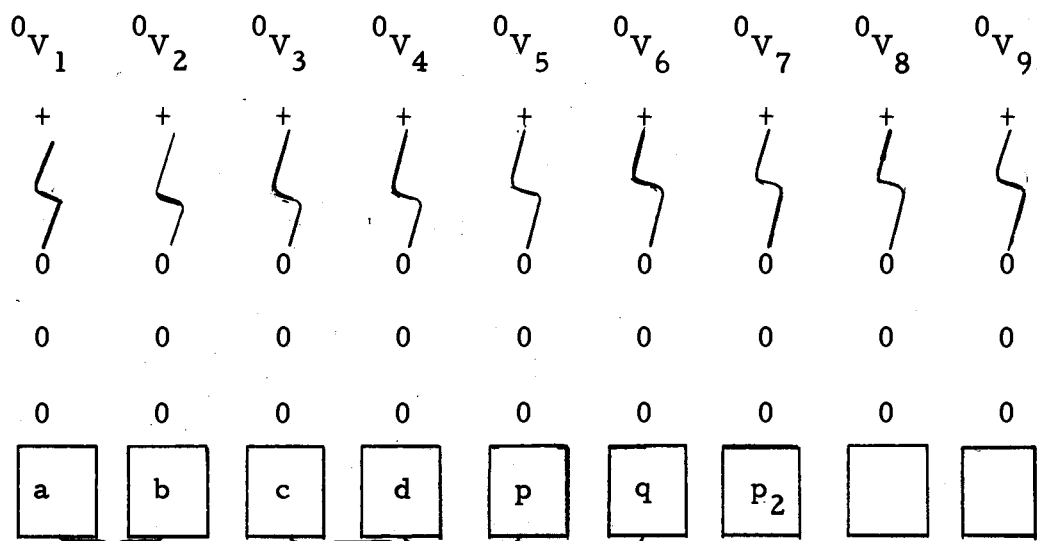


Figure 10. Symbolic diagram of the Analytical Engine: Showing formula  $(ab + c)d$ .

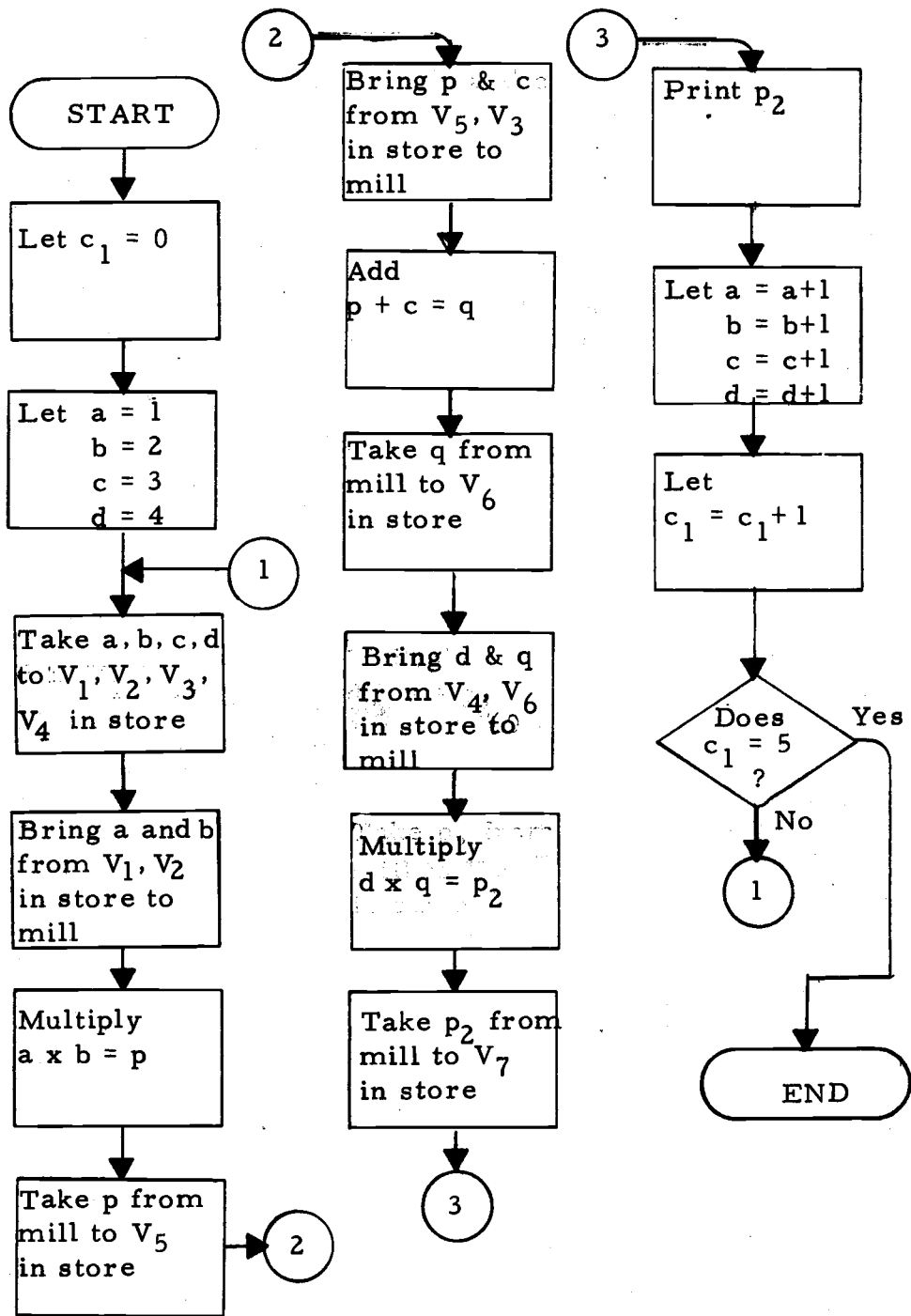


Figure 11. Logical diagram for Table 3.

particular column of the engine called the "store," where every quantity was first received and kept ready for use as needed.<sup>1</sup> In Table 3, the expression  $(ab + c)d$  is illustrated as a set of sequential instructions in the manner in which they were received by the engine. This set of instructions contains both arithmetical and control operations. Instructions for carrying out control operations, such as transferring constants from the store to the mill and vice versa, were, of course, punched on directive cards.<sup>2</sup>

The number cards supplied the data for the calculations in Table 3. For the particular set of data on the number cards, the Analytical Engine became a special purpose machine as was the Difference Engine. The directive cards and operation cards could be used repeatedly, thus giving the Analytical Engine the advantage of great generality compared to the Difference Engine. A new set of number cards containing new data was necessary for each iteration if new results were desired.

In addition to the number cards, the formula  $(ab + c)d$  required three operation cards and fourteen directive cards. Each set of cards was strung together and placed on a roller or prism of its own.<sup>3</sup>

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<sup>1</sup> Ibid., p. 332.

<sup>2</sup> Ibid., p. 332.

<sup>3</sup> Ibid., pp. 332-333.

The prism was suspended so that it could be moved back and forth.

Each backward motion caused the prism to move one face and brought the next card into play as described in the following:

A method was devised of what was technically designated backing the cards in certain groups according to certain laws. The object of this extension is to secure the possibility of bringing any particular card or set of cards into use any number of times successively in the solution of one problem. Whether this power shall be taken advantage of or not, in each particular instance, will depend on the nature of the operations which the problem under consideration may require.<sup>1</sup>

The method of backing and the process of branching in modern computers are analogous. The method of backing accommodated the redundancy found in mathematical calculations. For example, if it were desired to calculate several values of an algebraic expression such as  $(ab + c)d$ , a counter would be devised to instruct the Analytical Engine to repeat the calculation for different values of one or more of the variables in the expression. The engine would then be directed to branch or loop back and calculate the expression a specified number of times before proceeding to the next instruction.<sup>2</sup> The method of backing gave the Analytical Engine great versatility since logical decisions could be introduced into the program or set of instructions. It also provided a means of utilizing another idea which

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<sup>1</sup> Ada Augusta, Countess of Lovelace, "Notes," in Sketch, by Menabrea, p. 706.

<sup>2</sup> Ibid., p. 706.



gave the Analytical Engine great versatility--the repeated use of sets of instructions for carrying out redundant processes such as adding or multiplying two numbers or extracting roots (such sets of instructions are now called subroutines). As Babbage predicted, the creation of a library of such sets of instructions and mathematical tables provided means of economizing both mental and physical labor since the library could be used repeatedly.<sup>1</sup>

The function of the directive cards was particularly important in the Analytical Engine. Directive cards were used to convey instructions to a mechanical device called the "Chain," (see Figure 12). When an operation such as multiplication was to be carried out, a directive card caused the "Chain" to transmit power to the needed portions of the machine. The "Chain" in turn controlled another portion of the engine called the "Anticipating Carriage." The anticipating carriage controlled all the possible carries for a given operation. Babbage had designed the anticipating carriage so that whenever a number wheel rotated past the nine digit to the zero digit, the carry was held in memory until all intermediary operations were completed.<sup>2</sup> Once the intermediary operations were completed all the carries were performed simultaneously, which greatly reduced the

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<sup>1</sup> Babbage, Passages, pp. 118-119.

<sup>2</sup> Henry P. Babbage, "The Analytical Engine," in Engines, ed. by Henry P. Babbage, pp. 332-333.

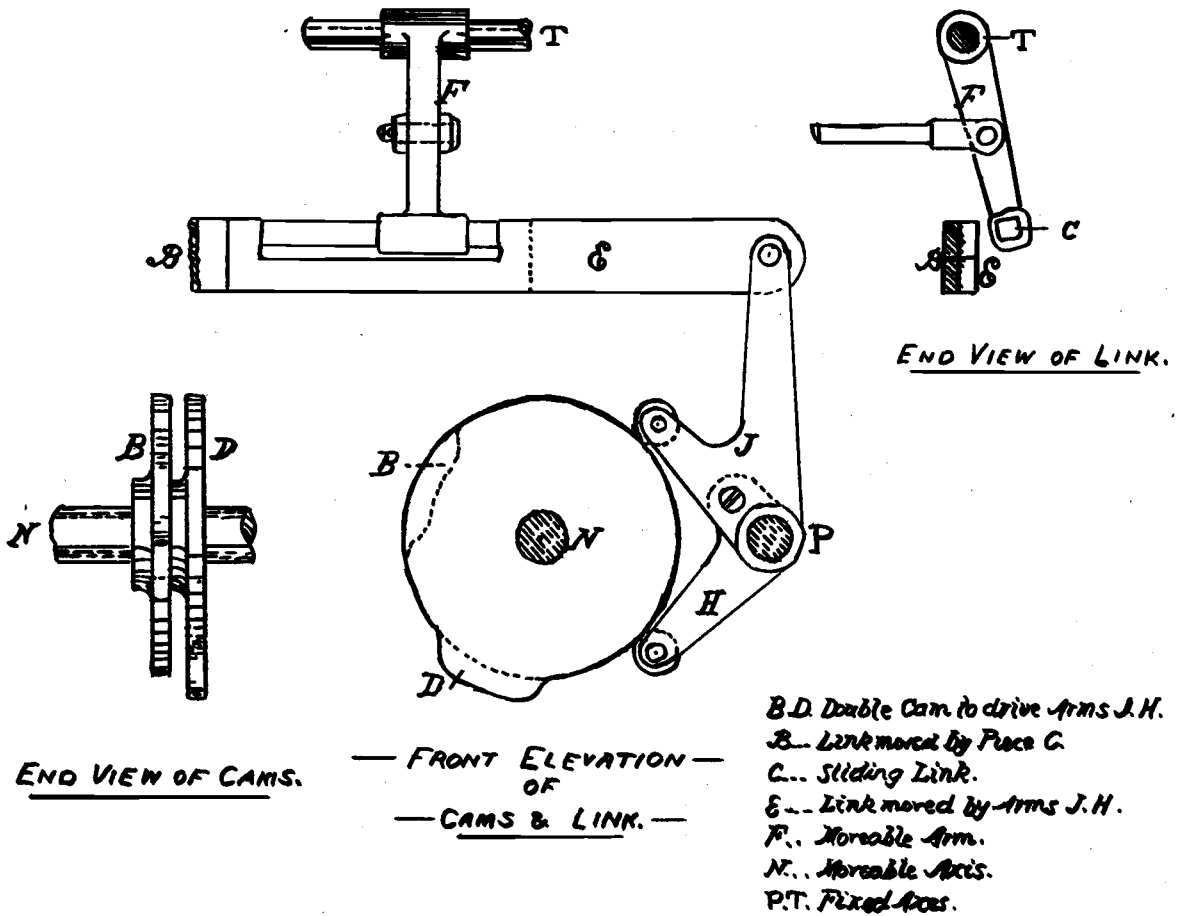


Figure 12. Chain mechanism--a device for coupling and decoupling the drive from any desired shaft as the program commanded. From "The Analytical Engine," by Henry P. Babbage, Proceedings of the British Association, Bath, September 12, 1888, in Engines, ed. by Henry P. Babbage, 338.

time of calculation. Babbage stated that no other portion of the machine took him so much time and effort to develop as the anticipating carriage.<sup>1</sup>

Babbage had designed a universal, automatic calculating machine, that is, the machine had to be able to calculate any function automatically or without human interference. To effect the automatic operations of the machine Babbage coded the instructions needed to calculate a given function on cards made of cardboard. Each card had room for nine digits in a column. A digit was represented by a set of holes punched in the card. In Figure 13, for example, a zero was represented by blanks, a three by three holes, and so on. The card representing the number 2303, in Figure 13, would then be fed to an appropriate column of the engine by wires and the 3 in the units position would be placed on the units number wheel as in Figure 14, the 0 on the tens wheel, the next 3 on the hundreds wheel, and the 2 on the thousands wheel. It should be noted, that each number wheel on a column was numbered from 0 to 9, equally spaced around the wheel. The topmost wheel of any column was the sign wheel on which + and - signs were alternately inscribed. The Analytical Engine was designed for as many as a thousand columns, with each column

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<sup>1</sup>Babbage, Passages, pp. 114-116.

Number				Table						
2	3	0	3	3	6	2	2	9	3	9
●	●	○	●	●	●	●	●	●	●	●
●	●	○	●	●	●	●	●	●	●	●
○	●	○	●	●	●	○	○	●	●	●
○	○	○	○	○	●	○	○	●	○	●
○	○	○	○	○	●	○	○	●	○	●
○	○	○	○	○	●	○	○	●	○	●
○	○	○	○	○	○	○	○	●	○	●
○	○	○	○	○	○	○	○	●	○	●
○	○	○	○	○	○	○	○	●	○	●

Figure 13. Operation or number card.

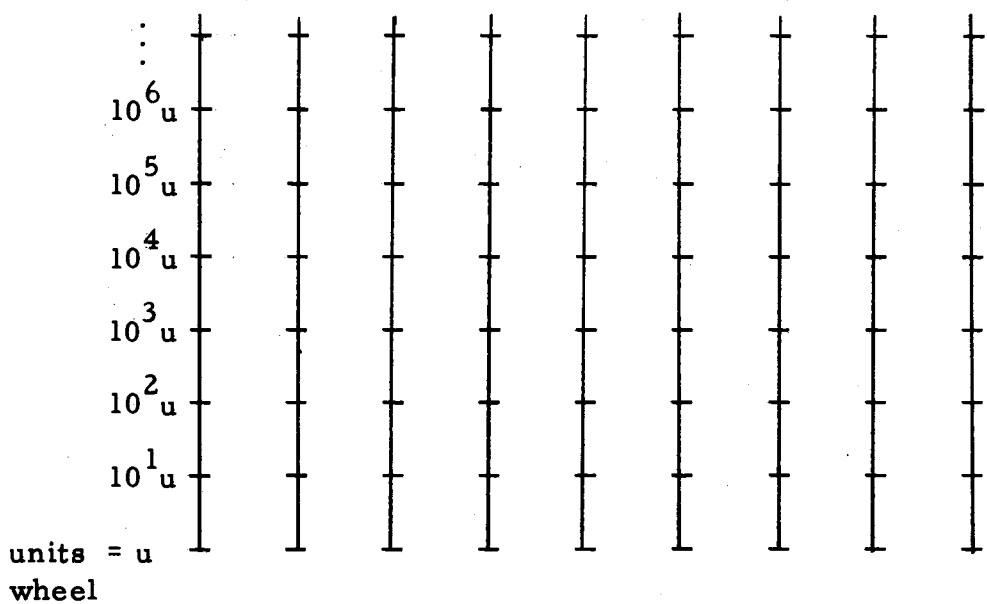


Figure 14. Structural diagram of the Analytical Engine. Each column and each horizontal bar represents a number wheel. The top wheels were sign wheels with alternating + and - signs.

containing as many as fifty number wheels equally spaced on the column and able to revolve horizontally around it.<sup>1</sup>

In Figure 15, the symbolic diagram shows how Babbage would have reduced an expression written in literal numbers to a form which the engine could interpret. For example, it might be desired to calculate the algebraic term  $\frac{ax^n}{bpy}$  using the Analytical Engine.<sup>2</sup> In order for the engine to evaluate this term, it was necessary to assign numerical values to each literal number such as  $a = 5$ ,  $n = 7$ , and  $x = 98$ . In order to locate the particular portion of the engine where a constant or variable was stored, Babbage also employed a symbol  $V$  which indicated a column on the engine.<sup>3</sup> The symbol  $V$  also had upper and lower indices such as  $^0V_1$  or  $^1V_1$ ;  $^0V_2$  or  $^1V_2$  and so on. The upper index denoted whether the numerical value zero or a value other than zero was inscribed on the column, while the lower index gave the column number. In Figure 15, the first column on the left  $^1V_1$  has an upper index of 1 indicating that a numerical value had been inscribed on the column. If in the course of the calculation the numerical value changed, the upper index also changed, such as to  $^2V_1$  where the 2 indicated that the column had a second

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<sup>1</sup>Ibid., p. 125.

<sup>2</sup>Ada Augusta, Countess of Lovelace, Notes, in Sketch by Menabrea, p. 705.

<sup>3</sup>Menabrea, Sketch, p. 676.

number value inscribed on it as the result of performing another operation or of storing the results of an intermediate operation. If a zero were introduced in the course of a calculation in column one the symbol would change from  ${}^2V_1$  to  ${}^0V_1$ .<sup>1</sup>

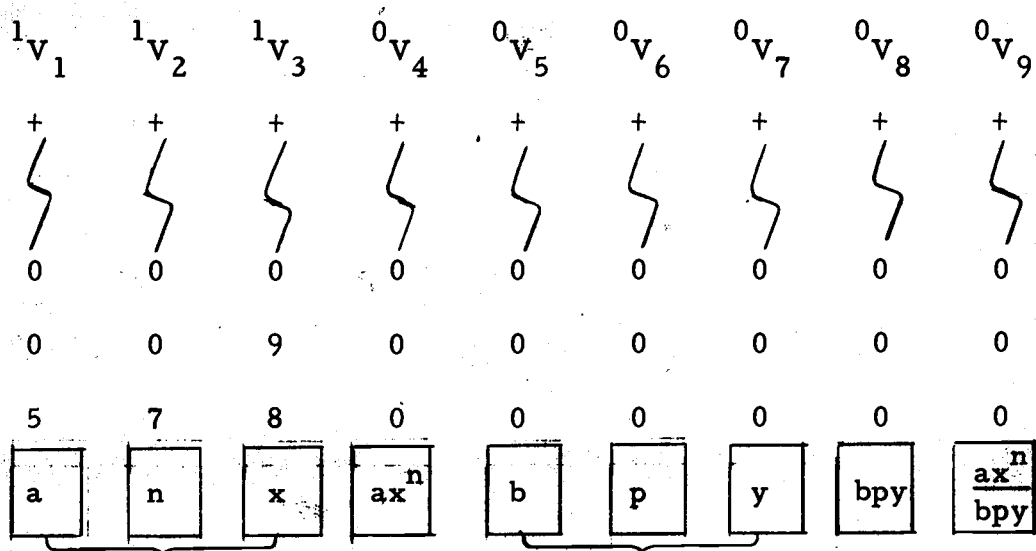


Figure 15. Symbolic diagram of the Analytical Engine. If  $a = 5$ ,  $n = 7$ , and  $x = 98$  the columns  $V_1$ ,  $V_2$ , and  $V_3$  would be represented as shown respectively.

In calculating the term  $\frac{ax^n}{bpy}$ , the multiplication indicated for  $ax^n$  would be performed by transferring the values for  $a$ ,  $x$ , and  $n$  to the mill (see Figure 9) where the operation of multiplication would take place. The product, in this case, would then be transferred back to the store to column  ${}^0V_4$  which would then be

<sup>1</sup> Ada Augusta, Countess of Lovelace, Notes, in Sketch, by Menabrea, p. 708.

represented as  ${}^1V_4$  since the numerical value of this operation would be inscribed on column 4. The literal term for column  ${}^1V_4$  of the engine,  $ax^n$ , and its numerical value  $5(98)^7$  represented an intermediate result. In order to complete the calculation, it would be necessary to assign numerical values to the literal numbers  $b$ ,  $p$ , and  $y$  and to inscribe those values on columns  $V_5$ ,  $V_6$ , and  $V_7$  respectively (see Figure 15). Again, the values of  $b$ ,  $p$ , and  $y$  would be transferred to the mill where the indicated multiplication of the algebraic term  $bpy$  would be performed as a binary operation--  $b$  and  $p$  would be transferred to the mill and multiplied with the result transferred to some column in the store;  $bp$  would then be returned to the mill and multiplied by  $y$  with the result transferred to column  ${}^0V_8$  in the store. To complete the calculation, the values stored in columns  ${}^1V_4$  and  ${}^1V_8$  would be transferred from the store to the mill and the indicated operation, division, would be performed with the result  ${}^1V_4 \div {}^1V_8$  transferred to column  ${}^0V_9$  in the store and printed out on a card or a stereo mold (a metal plate). The process of transferring a numerical value from the store to the mill for calculation, and then returning each value to its former location in the store plus returning the result of the calculation to some column in the store was necessary, even though it might appear redundant,

because each numerical value might be used more than once in the course of a complex calculation.<sup>1</sup>

The Analytical Engine was so complex that the number of parts necessary for its completion was more than 20,000. Each part of the engine was represented by a mechanical drawing of the highest quality. These covered about four or five hundred large folio sheets of paper.<sup>2</sup> Since Babbage was a perfectionist, the drawings were constantly in a state of revision as a new idea or a better way of accomplishing an old idea occurred to him. He recorded ideas as they occurred to him in "scribbling books," totaling sixteen volumes and approximately 6000 pages.<sup>3</sup>

As noted above, Babbage had envisioned that the Analytical Engine would consist of one thousand columns with each column containing number wheels--it would handle a fifty digit number. He realized, of course, that while a machine of this size was finite, its utility and basic functions were not necessarily limited. He therefore made use of "two great principles" in the design of the engine which "were embodied to an unlimited extent." The first principle involved "the entire control over arithmetical operations, however large, and

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<sup>1</sup> Ibid., pp. 705-706.

<sup>2</sup> Henry P. Babbage, Engines, p. 4.

<sup>3</sup> Ibid., p. 294.



whatever might be the number of their digits." The second principle involved "the entire control over the combinations of algebraic symbols, however lengthened those processes may be required."<sup>1</sup> Babbage then set down eight conditions which referred to these two principles. Those relating to arithmetic were as follows:

- (a). The number of digits in each constant inserted in the Engine must be without limit.
- (b). The number of constants to be inserted in the Engine must also be without limit.
- (c). The number of operations necessary for arithmetic is only four, but these four operations may be repeated an unlimited number of times.
- (d). These operations may occur in any order, or follow an unlimited number of laws.<sup>2</sup>

The following conditions related to the algebraic portion of the Analytical Engine:

- (e). The number of literal constants must be unlimited.
- (f). The number of variables must be without limit.
- (g). The combinations of the algebraic signs must be unlimited.
- (h). The number of functions to be employed must be without limit.<sup>3</sup>

Thus, Babbage recognized that, while it was impossible for a finite machine to include infinity, it was "possible to construct finite machinery, and to use it through unlimited time. As he put it, "It is this substitution of infinity of time for the infinity of space which I

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<sup>1</sup>Ibid., p. 123.

<sup>2</sup>Ibid., p. 124.

<sup>3</sup>Ibid., p. 124.

have made use of to limit the size of the engine and yet retain its unlimited power."<sup>1</sup>

The discussion above has been concerned with the technical and theoretical description of the Analytical Engine. However, the complexity and universality of the machine gave rise to many theoretical and speculative possibilities which have not been discussed. Babbage and his associates were well aware that the Analytical Engine provided a means of solving problems involving complex calculations which had not been attempted because of tedious and time consuming manual calculations. They were also aware that, as a universal, automatic computer came into general use by the scientific community, many questions would arise as to its use and many new techniques would be developed to supplement the ones they had envisioned. The development of a library of programs for solving standard problems such as compound interest would add greatly to its utility. The much broader speculative, and philosophical questions raised by the possibility of such phenomena as artificial intelligence will be discussed in detail in the subsequent chapter.

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<sup>1</sup>Ibid., p. 124.

## XI. THE ANALYTICAL ENGINE--AN AUTOMATON WITH EXECUTIVE POWERS

During the years that Babbage was working on Difference Engine No. 1, he worked in a sense in a state of isolation. He did consult with Herschel and other friends as well as Clement, but he had no one who fully shared his interests in the computing machine or his vision of its possibilities. With the Analytical Engine he was fortunate in having a least four persons who understood its utility and complexity and who shared his vision of its power and universal application. These four persons were Ada Augusta, Countess of Lovelace (1815-1852), the daughter of the poet, Lord Byron; General Luis Frederico Menabrea (1809-1896), who at the time Babbage first met him in 1840 was a junior officer in the Italian Army; Major General Henry Prevost Babbage (1824-1915), Babbage's youngest son and an engineer in the British Army in India; and Harry Wilmot Buxton, his good friend and biographer.

Of the four persons, it was the Countess of Lovelace who was a constant source of inspiration to Babbage, particularly during the eighteen thirties and throughout the eighteen fourties. The Countess first became acquainted with Babbage about 1830 and visited his shop to view the Difference Engine. According to Sophia De Morgan, wife of Augustus De Morgan, while other visitors merely looked at Babbage's calculating machine with awe and wonder, the Countess,

"young as she was, understood its workings and saw the great beauty of the invention."<sup>1</sup> The Countess showed an interest in mathematics and was encouraged and helped in her study of the subject by Mary Somerville (1780-1872),<sup>2</sup> author of the widely acclaimed Mechanism of the Heavens,<sup>3</sup> Babbage, and Augustus De Morgan,<sup>4</sup> who referred to the Countess as a mathematical genius.<sup>5</sup> The interest in the calculating engines shown by the Countess was especially welcome to Babbage when some of his fellow scientists such as Airy were critical of his efforts.

The general features of the Analytical Engine were worked out in a relatively short period of time but not as rapidly as Babbage had predicted or hoped. He was very optimistic when he stated late in 1834 "that the greatest difficulties of the invention have already been

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<sup>1</sup> Sophia De Morgan, Memoir of Augustus De Morgan, p. 89.

<sup>2</sup> Ibid., p. 89.

<sup>3</sup> [John Herschel], "Mrs. Somerville's Mechanism of the Heavens," review of Mechanism of the Heavens (by Mary Somerville (London: 1832), in the Quarterly Review, XLVII (July, 1832), 547..

<sup>4</sup> Sophia De Morgan, Memoir of Augustus De Morgan, p. 89.

<sup>5</sup> Ethel Colburn Mayne, The Life and Letters of Anne Isabella, Lady Noel Byron: From Unpublished Papers in the Possession of the Late Ralph, Earl of Lovelace; with an Introduction and Epilogue by Mary Countess of Lovelace (New York: Charles Scribner's Sons, 1929), p. 477.

surmounted, and the plans will be finished in a few months."<sup>1</sup> By 1843, although much had been accomplished as shown by the following account, Babbage had not yet begun construction of the engine:

The present state of the Analytical Engine is as follows:--

All the great principles on which the discovery rests have been explained, and drawings of mechanical structures have been made, by which each may be carried into operation.

Simpler mechanisms, as well as more extensive principles than were required for the Difference Engine, have been discovered for all the elementary portions of the Analytical Engine, and numerous drawings of these successive simplifications exist.

Mechanical Notations have been made both of the actions of detached parts and of the general action of the whole, which cover about four or five hundred large folio sheets of paper.

The original rough sketches are contained in about five volumes.

There are upwards of one hundred large drawings.

No part of the construction of the Analytical Engine has yet been commenced. A long series of experiments have, however, been made upon the art of shaping metals; and the tools to be employed for that purpose have been discussed, and many drawings of them prepared. The great object of these inquiries and experiments is, on the one hand, by simplifying as much as possible the construction, and on the other, by contriving new and cheaper

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<sup>1</sup>This statement was based upon a letter from Babbage to Lambert Adolphe Jacques Quetelet (1796-1874), a Belgian statistician and astronomer, which Quetelet quoted in a report to the General Meeting of the Royal Academy of Sciences at Brussels, held on the 7th and 8th of May, 1835, in Engines, ed. by Henry P. Babbage, p. 5. From the context of the entire statement, it appears that Babbage wrote the letter to Quetelet late in 1834.

means of execution, at length to reduce the expense within those limits which a private individual may command.<sup>1</sup>

Five more years passed before Babbage completed his development of the Analytical Engine and "mastered the invention."<sup>2</sup> This long period of development was not without satisfaction, even though Babbage had not been able to complete the plans for the Analytical Engine as soon as he had so confidently predicted in 1834. He did receive recognition for his efforts, when he was invited in 1839 to attend a meeting of Italian scientists to be held in 1840 in Turin. He was delighted to be honored by such an invitation because he looked forward to being able to converse with those whom he called the "elite of the science of Italy."<sup>3</sup> At this meeting Babbage received the first recognition by an organized group of scientists that the Analytical Engine was an important contribution to science. Jean Plana (1781-1864), a mathematician, was very eager for a personal conversation with Babbage about his Analytical Engine. Plana had made inquiries of his countrymen concerning the engine, but he could collect little information. However, it was Plana's

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<sup>1</sup> Charles Babbage, "Statement of the Circumstances Attending the Invention and Construction of Mr. Babbage's Calculating Engines," in Engines, ed. by Henry P. Babbage, pp. 3-4. This article was first published in the Philosophical Magazine, September, 1843, p. 235.

<sup>2</sup> Babbage, Passages, p. 97.

<sup>3</sup> Ibid., p. 293.

understanding that

Hitherto the legislative department of our analysis has been all powerful--the executive all feeble.

Your engine seems to give us the same control over the executive which we have hitherto only possessed over the legislative department.<sup>1</sup>

Plana's remark about the legislative and executive departments of analysis referred to the contributions made by mathematicians in devising theoretical mathematical systems which were so complex that applications were extremely difficult to attempt because of the tedious computational problems inherent in the executive department of analysis. An expanded explanation of this particular power of the Analytical Engine will be given below in a slightly different context.

When Babbage arrived at the meeting in Turin in 1840, he met, in addition to Plana, Menabrea, Ottaviano Frabrizio Mossotti (1791-1863), an astronomer and mathematician, James MacCullagh (1809-1847), a mathematician and Professor of Natural Philosophy at Trinity College in Dublin, Ireland, Emile Plantamour (1815-1882), a physicist and geographer, and "others of the most eminent geometers and engineers of Italy."<sup>2</sup> In order to give a clear explanation to the Italian scientists, Babbage selected models, drawings, and notations on the principles and operation of the Analytical Engine. Plana had

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<sup>1</sup> Ibid., p. 129.

<sup>2</sup> Ibid., p. 130.

planned to take notes on the lectures, but his own duties in relation to the meeting were so numerous that he decided to ask Menabrea to assume the task. Menabrea, who had already established a reputation as analyst, proved so able in the task that he produced a paper<sup>1</sup> for publication which Babbage described as "that lucid and admirable description" of the elementary principles of the Analytical Engine.<sup>2</sup> It is particularly noteworthy that Menabrea was able to write an article which was both lucid enough for the general public to understand and technical and theoretical enough for those who desired a thorough understanding of the Analytical Engine. It is a credit to Menabrea's ability that he was able to write such an exemplary article after a very brief exposure to the theory and design of the Analytical Engine.

Some time after Menabrea's article on the Analytical Engine was published, the Countess of Lovelace informed Babbage that she had translated the article from French into English. Babbage asked why she had not written an original paper of the Analytical Engine instead. The Countess stated that the idea had not occurred to her. Babbage then suggested that she should add some notes to Menabrea's memoir. The Countess did as suggested and published Menabrea's

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<sup>1</sup>Menabrea, Sketch.

<sup>2</sup>Babbage, Passages, pp. 135-136.



memoir with explanatory notes which enlarged and improved it. The Countess' notes showed her own mastery of the theory of the Analytical Engine and her originality as a mathematician.<sup>1</sup> Babbage greatly appreciated the solid efforts to popularize his engine by Menabrea and the Countess of Lovelace, since he had not written on account himself:

The notes of the Countess of Lovelace extend to about three times the length of the original memoir. Their author has entered fully into almost all the very difficult and abstract questions connected with the subject.

These two memoirs taken together furnish, to those capable of understanding the reasoning, a complete demonstration--That the whole of the developments and operations of analysis are now capable of being executed by machinery.<sup>2</sup>

Babbage did not attempt to enlarge upon the memoirs of Menabrea and the Countess of Lovelace. He was so fully engaged in the development of the Analytical Engine--the mechanism, the mechanical drawings and its theoretical bases--that he felt that he did not have time to devote to a comprehensive written explanation of it.<sup>3</sup>

Both Menabrea and the Countess of Lovelace recognized the important contributions Babbage had made to science through the theoretical development of techniques for preparing and coding

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<sup>1</sup>Ibid., pp. 136-137.

<sup>2</sup>Ibid., p. 137.

<sup>3</sup>Charles Babbage, "History of the Invention of the Calculating Engine," September 6, 1834, Museum, Oxford, MS Buxton 8, ff. 1-2.

information for processing by the Analytical Engine. Unlike the Difference Engine, the Analytical Engine by its very generality demanded that the techniques necessary for handling the numerical information delivered to the engine should also possess great generality. In the Difference Engine, as noted above, the necessary data was manually supplied to the engine for each particular solution desired. In the Analytical Engine, a whole array of data was prepared to be used by a general set of instructions for the solution of a problem. Once this set of instructions was prepared for the solution of a problem, a repetitive cycle of operations was possible, with the result being that a series of solutions was generated rather than a single solution as in the Difference Engine.

Menabrea pointed out that the labors which belong to the various branches of mathematical sciences can be divided into two distinct sections: (1) the mechanical, which is subjected to precise and invariable laws and which was capable of being expressed by means of the operations of matter; and (2) that demanding the intervention of reasoning which belonged to the domain of understanding. The mechanical branch of the mathematical sciences was to be executed by means of machinery, while reserving for the "pure intellect" the section of the mathematical sciences depending on the "reasoning faculties."<sup>1</sup>

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<sup>1</sup>Menabrea, Sketch, pp. 669-670.

Menabrea's opinion of special purpose machines, such as Pascal's calculator, was that, while displaying the "powerful intellect" of their inventors, the machines were of little utility since their powers extended no further than the execution of the first four operations of arithmetic.<sup>1</sup> The Countess of Lovelace pointed out in her translation that in one sense even the Analytical Engine only performed the four operations of addition, subtraction, multiplication, and division. She added that Menabrea entered into a more complete explanation of the differences between special purpose and universal calculating machines, but, in order to remove any confusion on the point, she explained

that in the one case the execution of these four operations is the fundamental starting point, and the object proposed for attainment by the machine is the subsequent combination of these in every possible variety; whereas in the other case the execution of some one of these four operations, selected at pleasure, is the ultimatum, the sole and utmost result that can be proposed for attainment by the machine referred to, and which result it cannot any further combine or work upon. The one [the Analytical Engine]<sup>2</sup> begins where the other ends. Should this distinction not now appear perfectly clear, it will become so by perusing the rest of the Memoir, and the Notes that are appended to it.<sup>3</sup>

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<sup>1</sup> Ibid., p. 670.

<sup>2</sup> The insertion is mine.

<sup>3</sup> Ada Augusta, Countess of Lovelace in a footnote in Sketch, by Menabrea, p. 670.

As the Countess of Lovelace had noted, Menabrea was fully aware of the uniqueness and the power of the Analytical Engine, when he pointed out other important differences in special purpose machines. That is, the Analytical Engine was unique because it was universal and automatic. Special purpose machines had an important drawback, Menabrea noted, in that they required the continual intervention of humans to regulate their movements, which was a constant source of errors. The use of special purpose machines had not become general for large numerical calculations because they did not resolve the double problem of "correctness in the results, united with economy of time."<sup>1</sup> Giving full credit to Babbage for having first recognized a solution for this double problem, Menabrea pointed out that,

Struck with similar reflections, Mr. Babbage has devoted some years to the realization of a gigantic idea. He proposed to himself nothing less than the construction of a machine capable of executing not merely arithmetical calculations, but even all those of analysis, if their laws are known. The imagination is at first astounded at the idea of such an undertaking; but the more calm reflection we bestow on it, the less impossible does success appear, and it is felt that it may depend on the discovery of some principle so general, that, if applied to machinery, the latter may be capable of mechanically translating the operations which may be indicated to it by algebraical notation.<sup>2</sup>

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<sup>1</sup> Menabrea, Sketch, p. 670.

<sup>2</sup> Ibid., p. 670.

Menabrea had assumed that Babbage's ideas for the Analytical Engine had originated from the Difference Engine,<sup>1</sup> but the Countess of Lovelace disagreed with him. She noted that Menabrea's idea was "an exceedingly natural and plausible supposition, until reflection reminds us that no necessary sequence and connection need exist between two such inventions, and that they may be wholly independent." She added that Menabrea shared this idea "with persons who have not the profound and accurate insight into the nature of either engine," but only because his opportunities to study the intricacies between the two engines were not "adequate to afford him information on a point like this."<sup>2</sup>

After Babbage had invented the first prototype of the Difference Engine No. 1 in 1822, he raised the idea that his machine could in a sense approximate human thought,<sup>3</sup> but he was ambiguous when he explained that the machine could only perform those very elementary and redundant mathematical operations for which it was designed and was "not intended to answer special questions."<sup>4</sup> However, the idea persisted, and with the invention of the Analytical Engine the

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<sup>1</sup> Ibid., p. 671.

<sup>2</sup> Ada Augusta, Countess of Lovelace in a footnote in Sketch, by Menabrea, p. 671.

<sup>3</sup> Babbage, Passages, p. 67.

<sup>4</sup> Ibid., p. 49.

possibility of machine intelligence, although a controversial topic, was seriously considered. Menabrea alluded to the idea when discussing the double problem of calculation--correctness and economy of time.<sup>1</sup> For any particular problem to be solved, "primitive numerical data" had to be supplied to the Analytical Engine. Once this data had been supplied there was no longer any need for human intervention, since all successive operations for the problem were dependent upon the primitive numerical data and the set of instructions for solving the problem.

Therefore, since, from the moment that the nature of the calculation to be executed or of the problem to be resolved have been indicated to it, the machine is, by its own intrinsic power, of itself [able] to go through all the intermediate operations which lead to the proposed result, it must exclude all methods of trial and guess-work, and can only admit the direct process of calculation.

It is necessarily thus; for the machine is not a thinking being, but simply an automaton which acts according to the laws imposed upon it.<sup>2</sup>

The Countess of Lovelace took exception, in a footnote, to Menabrea's statement that the Analytical Engine "must exclude all methods of trial and guess-work and can only admit to the direct process of calculation."<sup>3</sup> She pointed out that

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<sup>1</sup> Menabrea, Sketch, p. 675.

<sup>2</sup> Ibid., p. 675.

<sup>3</sup> Ibid., p. 675.

This must not be understood in too unqualified a manner. The engine is capable, under certain circumstances, of feeling about to discover which of two or more possible contingencies has occurred, and of then shaping its future course accordingly.<sup>1</sup>

The process to which the Countess was referring were those logical decisions which arose in the course of a calculation. For example, in a calculation of a function it might be necessary to test on every iteration whether a variable  $N = -1000$ . If it did not, the engine would be instructed to return to a given point in the set of instructions and calculate the function again with a new set of data. If  $N = -1000$ , then the next sequential instruction might direct the machine to print the final result of the calculation and stop. With such logical decisions an intrinsic part of numerical calculation, the engine would, as the Countess suggested, "feel about to discover which of two possible contingencies" had occurred, and in so doing it would exhibit an intelligence of its own. In another portion of Sketch, Menabrea actually confirmed what the Countess had stated.

Considered under the most general point of view, the essential object of the machine being to calculate, according to the laws dictated to it, the values of numerical coefficients which it is then to distribute appropriately on the columns which represent the variables, it follows that the interpretation of formulae and of results is beyond its province, unless indeed this very interpretation be itself susceptible of expression by means of the symbols which

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<sup>1</sup> Ada Augusta, Countess of Lovelace, "Note," in Sketch, by Menabrea, p. 675.

the machine employs. Thus, although it is not itself the being that reflects, it may yet be considered as the being which executes the conceptions of intelligence.<sup>1</sup>

The Countess again appended a detailed note to Menabrea's Sketch in which she discussed the last sentence of the above passage. Her comments will again be discussed in detail, because both she and Menabrea fully understood the importance of the Analytical Engine. That is, they understood that, unlike other machines previously invented by man, the Analytical Engine was a machine which in a sense constituted a duplication of the human thought processes through its ability to choose between two or more alternatives and was therefore as unique as it was powerful. Yet, even with such a realization, the Countess was conservative and cautious in her discussion.

It is desirable to guard against the possibility of exaggerated ideas that might arise as to the powers of the Analytical Engine. In considering any new subject, there is frequently a tendency, first, to overrate what we find to be already interesting or remarkable; and, secondly, by a sort of natural reaction, to undervalue the true state of the case, when we do discover that our notions have surpassed those that were really tenable.<sup>2</sup>

The Countess went on to explain that the Analytical Engine could not originate anything but could only "do whatever we know how

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<sup>1</sup> Menabrea, Sketch, p. 689.

<sup>2</sup> Ada Augusta, Countess of Lovelace, "Note G," in Sketch, by Menabrea, p. 722.



to order it to perform. " It could follow analysis via a set of instructions and in so doing might aid men in discovering the relations and nature of many subjects in analysis, which was, she stated, "a decidedly indirect, and a somewhat speculative consequence of such an invention."<sup>1</sup> The Countess was by no means dogmatic in her statements because she was aware that an invention which extended man's intellectual powers also extended man's ability to ask speculative questions.

There are in all extensions of human power, or additions to human knowledge, various collateral influences, besides the main and primary object attained.

To return to the executive faculties of this engine: the question must arise in every mind, are they really even able to follow analysis in its whole extent? No reply, entirely satisfactory to all minds, can be given to this query, excepting the actual existence of the engine and the actual experience of its practical results.<sup>2</sup>

Babbage's speculations concerning the idea of machine intelligence were related to games of skill. He stated that after the translation of Menabrea's Sketch by the Countess of Lovelace, he once again began "to meditate upon the intellectual means by which I had reached to such advanced and unexpected results."<sup>3</sup> He decided that the power of the principles he had discovered in the invention of

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<sup>1</sup> Ibid., p. 722.

<sup>2</sup> Ibid., pp. 722-723.

<sup>3</sup> Babbage, Passages, p. 465.

the Analytical Engine could be tested by assuming some question of an entirely new kind. He decided to develop a machine with the ability to play a game of purely intellectual skill such as chess. He then asked friends and strangers if they thought it required human reason to play games of skill and received affirmative replies. Some stated that if human reason were not needed for a game of skill, then an automaton could play such games. Some of his mathematical friends thought it might be possible to construct machinery for playing a game except that even a simple game involved too many possible combinations for such an idea to be practical. However, Babbage decided that it was possible. He reasoned that if any position of the pieces of chess, for example, were assumed then, if the automaton made the first move correctly, it would be able to win the game as long as the pieces were placed so that a win was possible. He stated that the question of an automaton was now reduced to making the best move under any possible positions of men, which would in turn force it to consider the following questions:<sup>1</sup>

1. Is the position of the men, as placed before him on the board, a possible position? that is, one which is consistent with the rules of the game?
2. If so, has Automaton himself already lost the game?
3. If not, then has Automaton won the game?
4. If not, can he win it at the next move? If so, make that move.

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<sup>1</sup>Ibid. , pp. 465-466.

5. If not, could his adversary, if he had the move, win the game.
6. If so, Automaton must prevent him if possible.
7. If his adversary cannot win the game at the next move, Automaton must examine whether he can make such a move that, if he were allowed to have two moves in succession, he could at the second move have two different ways of winning the game; and each of these cases failing, Automaton must look forward to three or more successive moves.<sup>1</sup>

Babbage maintained that since the Analytical Engine already possessed mechanical means equivalent to memory and possessed other means equivalent to foresight, then the engine could act upon that foresight. Thus, by allowing one hundred moves on the longest game at chess, he found that the Analytical Engine enormously surpassed the capacity to handle the combinations required even by the game of chess.<sup>2</sup>

The invention of such a universal machine as the Analytical Engine did not elicit the approbation from the general public or the scientific community that might be expected.<sup>3</sup> There was much confusion as to whether the Analytical Engine was an improved version of Difference Engine No. 1 or a distinct invention. Some regretted that

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<sup>1</sup> Ibid. , pp. 466-467.

<sup>2</sup> Ibid. , p. 467.

<sup>3</sup> "Addition to the Memoir of M. Menabrea on the Analytical Engine," Scientific Memoirs, Vol. III, Part 12, p. 666, appended to Sketch, by Menabrea, p. 1. This "Addition" is paged separately and covers four pages.

the Analytical Engine had been invented because they felt Babbage had given up a successful invention without substituting a superior invention in its place.<sup>1</sup> Some who were inclined to strictly utilitarian views felt that the Analytical Engine was designed for exploring questions of abstract and speculative science rather than those questions involving common human needs and interests. The Countess of Lovelace stated that even if the utilitarian value of the two calculating engines were compared, she did not doubt that very valuable and practical results would be realized by the extended powers of the Analytical Engine. She thought that the increasing requirements of science would bring forth many practical results which were not presently foreseen. She did concur with those who felt that "the completion of the Difference Engine would be far preferable to the non-completion of any calculating machine,"<sup>2</sup> but she also hoped that the difficulties which had arisen regarding the Difference Engine would not prevent the completion of the Analytical Engine, and that

for the honour of our country's reputation in the future pages of history, these causes will not lead to the completion of the undertaking by some other nation or government. This could not but be a matter of just regret; and equally so, whether the obstacles may have

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<sup>1</sup> Ada Augusta, Countess of Lovelace, "Note A," in Sketch by Menabrea, p. 698.

<sup>2</sup> Ibid., pp. 699-700.

originated in private interests and feelings, in considerations of a more public description or in causes combining the nature of both such solutions.<sup>1</sup>

The very thing that the Countess of Lovelace had feared occurred--the financing of a foreign invention of a difference engine. The failure of the British government to finance the construction of Difference Engine No. 2 in 1852 allowed Sweden to claim the credit for the first fully operable difference engine. In 1834, George Scheutz (1785-1873), a printer and editor of a technological journal in Stockholm read an article by Dionysis Lardner on Babbage's calculating engines.<sup>2</sup> Scheutz became fascinated with the idea of building a calculating machine of his own. With the aid of his son Edward Scheutz (1821-1881), a graduate of the Royal Technological Institute, he was able to complete the engine in 1852 after eighteen years of struggle. In fact, the struggles of Scheutz and his son closely paralleled those of Babbage and consisted of many disappointments including difficulties in securing adequate governmental financing.<sup>3</sup>

In 1854, both Scheutz and his son visited England and France<sup>4</sup> where they exhibited their machine. Through William Gravatt,

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<sup>1</sup> Ibid., p. 700.

<sup>2</sup> Buxton, Charles Babbage, MS Buxton 16-17, f. 1602.

<sup>3</sup> Ibid., ff. 1602-1618.

<sup>4</sup> Ibid., f. 1622.

F.R.S., the engine was brought to the attention of the Royal Society. It was placed on exhibit in the Society's Somerset House where Gravatt explained its operation to interested persons. The engine was then exhibited in the Great Exhibition of 1854 in Paris where a jury awarded a Gold Medal to the inventors. The machine was returned to England where it was purchased by Professor Benjamin Apthorp Gould (1824-1896), astronomer for the Dudley Observatory in Albany, New York.<sup>1</sup> An exact copy of the machine was also purchased by the British Government where it was used by the Registrar General's department at Somerset House.

Babbage had known of the struggles which the inventors had experienced and generously offered them both advice and assistance. He took advantage of every opportunity to praise them for their excellent workmanship.<sup>2</sup> After the Copley Medal was awarded to Scheutz and his son by the Royal Society on November 30, 1855, Babbage delivered an address to the Society in which he gave an account of the circumstances of the Swedish difference engine.<sup>3</sup> He explained that he hoped to allay any public misapprehension respecting

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<sup>1</sup> Ibid., f. 1622.

<sup>2</sup> Ibid., ff. 1622-1624.

<sup>3</sup> Charles Babbage, Observations Addressed to the President and Fellows of the Royal Society after the Delivery of Medals (London: John Murray, 1856).

the originality of the invention which, although it calculated via the principle of differences, was in general mechanically different from his own. Babbage noted with reference to Lardner's account of his own difference engine that,

Unfortunately for himself, Mr. Scheutz was fascinated by the subject, and impelled by an irresistible desire to construct an engine for the same purposes. He has always avowed, in the most open and honourable manner, the origin of his idea. But his finished work contains undoubted proof of great originality, and shows that little beyond the principal could have been borrowed from my previous work.<sup>1</sup>

Thus, Babbage gave the praise and credit to the Swedish inventors which he had been denied by his own countrymen. The practicality of his idea for producing a fully operative difference engine was demonstrated by someone other than himself. However, Babbage did not give up in his effort to complete the Analytical Engine but continued, after he had completed the major design of the engine in 1848, to improve and simplify many of its parts. Babbage worked diligently for a period of time prior to the Great Exhibition of 1862 to assemble a working model of the Analytical Engine which he hoped to enter as an exhibit. He encountered so many difficulties that he was not able to accomplish this task.<sup>2</sup> He took out time from his labors on the engine to write Passages from the Life of A Philosopher which

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<sup>1</sup> Ibid., p. 5.

<sup>2</sup> Buxton, Charles Babbage, MS Buxton 16-17, f. 2032.

he published in 1864. He then resumed work on the Analytical Engine and continued to employ a draftsman until shortly before his death in 1871.<sup>1</sup> His intense desire to attain perfection in the engine was probably the primary reason he was unable to complete it before his death. Buxton's commentary upon this point is particularly revealing. Writing of the years of Babbage's life after 1865 with reference to the engine, he noted:

Model after model was constructed and completed, and so often as this happened, models were laid aside, plans redrawn, and the whole process so often repeated that he began to doubt whether he should ever live to complete the undertaking. Plan after plan was tried, and disappointment succeeded to disappointment, or perhaps some improvement suggested itself which rendered what had preceded no longer applicable to the object in view. So day succeeded day, and month succeeded month, and though his labours were on rare occasions relieved by his visits to friends and other relaxation, still his almost constant fatigue and persistent industry began manifestly to impair his health.<sup>2</sup>

Buxton praised Babbage for his unceasing devotion to his invention even as his infirmities grew more pronounced. Babbage went on working until shortly before his death when his memory and physical strength failed him. Buxton, who as a personal friend mourned his loss, wrote that nature had intended Babbage to be a benefactor of his race but that "adverse circumstances intercepted

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<sup>1</sup> Henry Babbage, ed., Engines, p. 292.

<sup>2</sup> Buxton, Charles Babbage, MS Buxton 16-17, f. 2046. No date is given as to when Buxton wrote this account.



and partially destroyed the efficiency of his mission, and thus deprived mankind of benefits, which, under more auspicious circumstances would have been conferred upon them."<sup>1</sup>

Babbage's death did not terminate all efforts to complete his calculating engines. Fortunately, his youngest son Henry P. Babbage took an interest in his father's work. He taught himself his father's mechanical notation while on a three year furlough (1853-1856) from the British Army in India. He drew up detailed mechanical drawings of both Difference Engine No. 2 and the Scheutz difference engine.<sup>2</sup> When he again returned to England from India in 1872, he decided to attempt to complete both the Difference Engine and the Analytical Engine from the drawings of the machines which had been willed to him after his father's death. His first task was to begin work upon the arithmetical-logical portion of the Analytical Engine called the mill, which he hoped would be able to print a table of  $\pi$  to 25 places of figures for 100 multiples. This work was interrupted when he returned to India in January of 1873.<sup>3</sup> However, when he retired from the army in 1874, he resumed work upon the engine once more. He

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<sup>1</sup> Ibid., f. 2052.

<sup>2</sup> H[enry] P. Babbage, Memoirs, pp. 84-85.

<sup>3</sup> Ibid., p. 183.

was given some support from Professor William K. Clifford (1845-1879), of the Royal Institution. Clifford persuaded the British Association to appoint a committee to report on the feasibility of providing both technical and financial support for the Analytical Engine. As a result, a committee was appointed in 1878 by the British Association to consider whether it would be advisable to attempt to construct the Analytical Engine from the drawings left by Charles Babbage.<sup>1</sup> As with the Difference Engine the probable cost was a major consideration. The committee concluded that it was not possible to form any exact estimate as to cost except that

it would surprise us very much if it were found possible to obtain tenders for less than £10,000, while it would pretty certainly cost a considerable sum to put the design in a fit state for obtaining tenders. On the other hand, it would not surprise us if the cost were to reach three or four times the amount suggested.<sup>2</sup>

The committee made another comment in their report which provided an insight into why it was so difficult to determine a cost estimate for the construction of the Analytical Engine. It was noted

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<sup>1</sup> Ibid., p. 184.

<sup>2</sup> "Report of the Committee, Consisting of Professor Cayley, Dr. Farr, Mr. J.W.L. Glaisher, Dr. Pole, Professor Fuller, Professor A.B.W. Kennedy, Professor Clifford, and Mr. C.W. Merrifield, Appointed to consider the Advisability and to Estimate the Expense of Constructing Mr. Babbage's Analytical Machine, and of Printing Tables by Its Means. Drawn up by Mr. Merrifield " extracted from the Proceedings of the British Association, 1878, in Engines, ed. by Henry P. Babbage, p. 328.

that Babbage had always followed the rule of designing mechanism in utter disregard of any questions of complexity. While it was admitted that such a rule was wise whether applied to analytical, mechanical, or administrative problems, in the case of a mechanical invention "it leaves in doubt, until the design finally leaves the inventor's hands in a finished state, whether it really represents what is meant to be rendered in metal, or whether it is simply a provisional solution, to be afterward simplified."<sup>1</sup> This statement directly questioned whether Babbage's mechanical drawings for the Analytical Engine were working drawings which could be directly "rendered in metal." The committee concluded that the drawings were not in such a state.<sup>2</sup> In a commentary upon the committee's report which was written ten years later in 1888, Henry Babbage refuted this conclusion and stated that the drawing did represent what was at the time intended to be put into metal. This did not mean, however, that no modifications would be made once the actual construction began since very few machines were invented without modification of its original design. He added that in a second Analytical Engine it would be important to make improvements in the original design, "but no one ever stops a useful

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<sup>1</sup> Ibid., , p. 327.

<sup>2</sup> Ibid., , p. 330.

invention for fear of improvements."<sup>1</sup> Henry Babbage's comment is somewhat ironical because his father in effect gave up the construction of Difference Engine No. 1 because the Analytical Engine was of greater utility.

The committee made a suggestion which would certainly not have pleased Babbage. It was suggested that because of the probable cost a modified version of the Analytical Engine should probably be considered. That is, only the mill should be constructed and used for the operations of addition and subtraction and possibly multiplication, "with or without store-columns."<sup>2</sup> Such a modification would have reduced the Analytical Engine to little more than a simple, special purpose calculating machine with little more generality than a difference engine. If Babbage had wished to construct such a modified machine he certainly would not have needed to spend thirty-seven years of his life on such a project. The committee appeared to have some reservations and doubts as to the need for a fully operative Analytical Engine:

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<sup>1</sup>H[enry] P[rovoost] Babbage, "The Analytical Engine," in Engines, ed. by Henry P. Babbage, p. 337.

<sup>2</sup>"Report of the Committee," in Engines, ed. by Henry P. Babbage, p. 329.

If intelligently directed and saved from wasteful use, such a machine might mark an era in the history of computation, as decided as the introduction of logarithms in the seventeenth century did in trigonometrical and astronomical arithmetic. Care might be required to guard against misuse, especially against the imposition of Sisyphean tasks upon it by influential sciolists.<sup>1</sup> This, however, is no more than has happened in the history of logarithms. Much work has been done with them which could more easily have been done without them, and the old reproach is probably true, that more work has been spent upon making tables than has been saved by their use. Yet, on the whole, there can be no reasonable doubt that the first calculation of logarithmic tables was an expenditure of capital which has repaid itself over and over again. So probably would the analytical engine, whatever its cost, if we could be assured of its success.<sup>2</sup>

The committee apparently did not share with the Countess of Lovelace, Plana, and Menabrea the idea the Analytical Engine was an automaton capable of exercising executive control over mathematical analysis; and, in particular, did not discuss the possibility that such an engine could "feel about to discover which of two possible contingencies"<sup>3</sup> had occurred and thus exhibit an intelligence of its own. No mention was made by the committee of such possibilities which, from the context of its report, was more concerned with the cost and structural

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<sup>1</sup> Sisyphus, in Greek mythology, was the legendary King of Corinth who was doomed forever in Hades to roll a heavy stone up a steep hill only to have it roll down again as it neared the top. A sciolist is one who puts on a superficial show of learning.

<sup>2</sup> "Report of the Committee," in Engines, ed. by Henry P. Babbage, p. 329.

<sup>3</sup> Ada Augusta, Countess of Lovelace, "Note," in Sketch, by Menabrea, p. 675.

soundness of the engine than with its theoretical possibilities.

Henry Babbage was more optimistic than the committee and decided to continue his father's work despite the committee's statement that "we cannot advise the British Association to take any steps, either by way of recommendation or otherwise, to procure the construction of Mr. Babbage's Analytical Engine."<sup>1</sup> It should be noted that Henry Babbage probably had a better understanding of mechanical drawings, and particularly those of the Analytical Engine, than anyone on the committee since he had mastered the mechanical notation in 1855.<sup>2</sup> He demonstrated such a mastery by producing the "mill" or arithmetical-logical unit of the engine, beginning in 1888 and completing it by 1896, in the workshop of W.R. Munro of Tottenham. From 1910 to 1911 he again worked on the engine and added an anticipating carriage and printing mechanism. The machine was now able to perform addition and subtraction to 29 places of figures.<sup>3</sup> He then printed off twenty-two multiples of  $\pi$  to twenty-eight places, which he later discovered contained several mistakes due to a weak

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<sup>1</sup>"Report of the Committee," in Engines, ed. by Henry P. Babbage, p. 330.

<sup>2</sup>Henry P. Babbage, "Mechanical Notation, Exemplified on the Swedish Calculating Machine, by Major Henry P. Babbage," extracted from Proceedings of British Association, Glasgow, September, 1855, in Engines, ed. by Henry P. Babbage, pp. 246-247.

<sup>3</sup>H. P. Babbage, Memoirs, p. 227.

spring. Several exhibitions of the mill were held, but since "there was nobody to explain it on these occasions," no great interest was taken in it.<sup>1</sup> After 1911, the mill was returned to the South Kensington Museum where Henry Babbage had placed it in 1896.<sup>2</sup> The remarks he had made in 1888, with reference to the British Association report of 1878 on the Analytical Engine were particularly prophetic in view of the lack of interest shown in the mill once it had been successfully demonstrated:

I see no hope of any Analytical Engine, however useful it might be, bringing any profit to its constructor, . . . Those who wish for such an engine would, I think, give it a helping hand if they could show what pecuniary benefit it would bring. The History of Babbage's Calculating Machines is sufficient to damp the ardour of a dozen enthusiasts.<sup>3</sup>

However, Charles Babbage's faith in the importance of the Analytical Engine had been unwavering. He had written in 1864 that the great principles on which the engine had been invented had been examined, admitted, recorded and demonstrated. To this he added,

If, unwarned by my example, any man shall undertake and shall succeed in really constructing an engine embodying in itself the whole of the executive department of a mathematical analysis upon different principles or by simpler

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<sup>1</sup>Ibid., p. 228.

<sup>2</sup>Ibid., p. 227.

<sup>3</sup>Henry P. Babbage, "Analytical Engine," in Engines, ed. by Henry P. Babbage, p. 337.

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>1</sup>

The above passage was read by Howard Aiken about 1940, after he had been working for three years on a universal automatic computer called the Automatic Sequence Controlled Calculator or Mark I.

Aiken, now a professor of information technology at the University of Miami, in Florida, and President of Howard Aiken Industries,

Incorporated, was working in 1937 on a PhD. at Harvard University.

Since the theoretical aspects of his thesis involved the solution of non-linear, ordinary differential equations which could be solved only

by means of numerical approximation, he needed some automatic

means of computing the equations. He stated that "if Babbage had

lived seventy-five years later, I would have been out of a job,"<sup>2</sup> and

when he first came across the above passage he felt Babbage was

addressing him personally from the past.<sup>3</sup> Thus, Babbage at last had

someone who fully understood and appreciated his efforts and who had

the financial resources, provided by the International Business

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<sup>1</sup> Charles Babbage, Passages, p. 450.

<sup>2</sup> Howard Aiken as quoted in Jeremy Bernstein, The Analytical Engine: Computers--Past, Present, and Future (New York: Random House, 1963), p. 52.

<sup>3</sup> Ibid., p. 52.



Machine Corporation (IBM), to sustain his efforts.<sup>1</sup>

Babbage might have actually completed the Analytical Engine during his lifetime if he had had the help of Henry Babbage over a longer period of time than the three years from 1853-1856. Henry Babbage demonstrated, by his completion of the mill of the Analytical Engine, that he had the ability to accomplish the practical engineering problems which his father lacked because of his tendency to introduce changes even when his draftsman had completed the drawings necessary for a part to be produced in metal. Henry Babbage had actually considered resigning from the army during his three year furlough, but he decided against it for financial reasons.<sup>2</sup> If he had taken such a step and had worked with his father, the outcome of his father's efforts to complete the Analytical Engine might have been much different. As it happened, approximately seventy years passed before Howard Aiken, confronted with a need for a calculating machine and supported by corporate funds, re-invented an automaton with executive powers and thus attained the goal sought by Babbage, the Countess of Lovelace, Menabrea and Henry Babbage.

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<sup>1</sup> A. W. Burks, H. H. Goldstine, and John von Neumann, "Preliminary Discussion of the Logical Design of an Electronic Computing Instrument," in John von Neumann Collected Works, ed. by A. H. Taub (5 vols.; New York: Macmillan, 1963), V. 50-51.

<sup>2</sup> Henry P. Babbage, Memoirs, p. 94.

## EPILOGUE

The reform movements, the mathematical inquiries, the invention of calculating engines, and the other varied scientific investigations in which Babbage was engaged for nearly sixty years were well known to the public. Throughout his career Babbage's advice was sought on many scientific questions by friends and associates both at home and abroad, but he maintained that he had not received recognition from his own countrymen. He referred to the acclaim of the Italian scientists as an example of the kind of recognition he would have liked to receive in his own country. However, it is only fair to Babbage to note that he wanted recognition not for himself alone but for scientists in general. In fact, his contributions to the reform movements within the scientific community derived partly from his belief that scientific knowledge was needed by those in high governmental and political offices in order to make decisions in an industrialized nation which depended upon science and technology for its sustenance and welfare.

As has been noted, Babbage and Bromhead enunciated the general views of the external coterie, which views were strangely liberal and elitist, concerning the role a scientist should play in governmental and political affairs. Bromhead and Babbage thought that a scientist should take an active part in government and in

politics and that the opportunities for such service should be expanded, particularly in an industrial nation where technical and scientific knowledge were needed to make intelligent and informed decisions. While Babbage and Bromhead and the other members of the external coterie encouraged the expansion of the voting franchise for scientists, artisans, and industrialists, they also proposed that a recognized elite be established from this group through an Order of Bath, which would grant peerage for life only and which would allow its recipients to serve in the House of Lords. In this manner, they were proposing to substitute knowledge and achievement for heredity as a basis for choosing the members of the House of Lords. Babbage, using a mechanical analogy, maintained that the House of Lords was the fly-wheel of a Parliamentary system of government in that it averaged out the weaknesses and excesses which were to be expected in the House of Commons, wherein the constituency was drawn from a larger socio-economic spectrum. Thus, the Order of Bath would provide for an elite class of citizens, who would always constitute an important minority in an industrialized nation, and whose views would be represented through the House of Lords.

The idea for such an elite was not accepted because it was counter to the popular trend of enlarging the voting franchise with a corresponding decrease in an emphasis upon class distinction. The idea was equally counter to the interests of the existing aristocracy

whose hereditary peerage had afforded them immense power and prestige. The newly rich industrialists and artisans were not accepted as social or political equals with the hereditary peers so that regardless of their knowledge, achievement, and wealth, they were not acceptable replacements, at the time, for the hereditary peerage. The scientists were in a somewhat different class than the artisans and industrialists in that they had not directly acquired wealth and power as the result of their scientific knowledge and did not as a group constitute a threat to the aristocracy. In fact, one of the complaints which Babbage and the external coterie voiced was that governmental and political leaders were generally indifferent to the scientist except when there was a specific problem to be solved which required scientific knowledge.

Babbage often entertained ideas which were either too futuristic or which included a peculiar point of view which made them novel and not generally acceptable. As noted, his idea for a life peerage was novel but not acceptable at the time. His work with calculating engines were futuristic and novel to the general public and even to many of his fellow scientists. Except in the field of astronomy, there was no apparent great demand for a universal, automatic calculating machine such as the Analytical Engine. If there had been, Babbage would have very likely received the support of the government, but, as Lardner pointed out, the public generally viewed the calculating

engines as novel and curious objects with very little utility. Babbage was not helped, of course, on an official level by Airy, who was both an Astronomer Royal and a governmental advisor. If Airy saw no utility in Babbage's calculating engines as an astronomer then it was quite unlikely that Babbage could have convinced the governmental officials of their utility. Babbage's ideas as well as those of other scientists did not receive much support unless there was immediate and practical benefit. Babbage's career was contemporary with the introduction of the utilitarian philosophy of Jeremy Bentham (1748-1832), but the inclination toward the practical rather than the theoretical was already present in English science and in political thought, so that his novel and theoretical ideas very likely suffered in such a climate.

Babbage believed that there existed general, undiscovered laws which governed not only physical and biological phenomena but also moral and political laws. His commentary on general laws in the Ninth Bridgewater Treatise, arising out of his experiments with Difference Engine No. 1, were received with admiration and elicited much discussion and argument. His discussion of general laws included the idea that science was a product of a group mind which extended beyond the life span of a single individual. That is, Babbage held the idea that science was progressive and that the apparent exceptions to scientific laws, which were often referred to as

miracles, were only manifestations of a higher but unknown general law which would some day be discovered.

Babbage's view of science and of the universe was, not surprisingly, very mechanistic. For example, in his ideas concerning general laws, he saw the universe as one vast machine. The supposed differences in physical, biological, political, economic, and moral laws were more a matter of the knowledge and orientation of the investigator than substantive in nature. However, he departed somewhat from a mechanistic point of view when he described the powers of his calculating automaton, the Analytical Engine, and employed such terms as "feels about" for the most likely answer and the engine can "foresee" when a carriage was to take place in an addition. When Babbage, Menabrea, and the Countess of Lovelace discussed the theoretical powers of the Analytical Engine, they introduced a new dimension into the general conception of the function of machines. The Analytical Engine could now duplicate, in a limited sense, the thought processes which had formerly been exclusively within the power of man. The engine could exhibit the ability to reason in playing a game such as chess, if it were assumed that it could improve upon its skill in playing the game, which was not at all unlikely if it could feel about for the best move and then store that information for later use. The next logical step for the machine would be for it to develop a consciousness of its own as suggested in 1872 by the writer,

Samuel Butler (1835-1902), in his satire Erewhon, in reference to the growing sophistication of machines in an industrialized nation.

In considering Babbage's complaint that his contributions to science had not been duly recognized by his own countrymen, it should be noted that he was probably as well known to the public as were any of his contemporary scientists with the possible exception of John Herschel. His books were widely read and reviewed, his calculating engines highly publicized, his views on the need to reform science and for greater governmental support of science were debated in political and private circles, and people constantly sought his advice on a wide variety of scientific as well as other subjects because they respected his knowledge and insight. Why then did Babbage claim that he had not received recognition for his achievements? At least a partial answer to this question is that he, above all else, valued official recognition by scientific societies and by governmental officials. He often commended Continental nations because they honored their scientists with such rewards as governmental posts, gold medals, and membership in an exclusive national academy which usually included an annual stipend. Babbage was often chided in reviews of his works for what was called his excessive vanity and obsession with superficial honors and recognition. It was only near the end of his life when he began to realize that, while he might not receive the kind and degree of recognition he had sought in his own

lifetime, his work with the calculating engines would be recognized in the future as an outstanding contribution to science.

Another possible reason for Babbage's dissatisfaction with the recognition he had received was the severe criticism he had endured because he had not completed any of his calculating engines. While he often stated that he had not completed the engines because of circumstances over which he had no control, it is very likely that he blamed himself for his inability to complete the engines regardless of personal sacrifice. He knew very well that the public might get excited over a novel invention such as a calculating engine, but he also acknowledged that the real test of the utility and value of a mechanical invention was a complete and fully operable machine. While Difference Engine No. 1 had fulfilled all his expectations, the fact that he did not complete all seven orders of differences and the printing mechanism left him open to criticism from the public.

On the whole, Babbage's scientific career was very productive and exemplary. In the early part of his career he had expended much time and energy in the scientific reforms, and yet contributed his own original ideas to mathematical analysis, probability theory, and to the development of life assurance societies. In the latter part of his career his primary contribution was in the development of calculating engines, but he also devoted much time to a wide range of scientific investigations which were reported in published articles. Unlike many



of his colleagues he remained scientifically active and productive until shortly before his death. Although he spent the last few years in relative obscurity, he was remembered upon his death as a man with a penetrating and affluent intellect who had looked upon the whole of science as his domain. The originality of his scientific investigations, the determination with which he pursued them, and the depth and breadth of his scientific efforts ultimately ensured him the recognition which he had so fervently desired. In particular, the re-invention of an automatic calculating machine by Howard Aiken in 1937 and his subsequent discovery of Babbage's work about 1940 brought about a renewed interest in Babbage's contributions to science. For example, Babbage had predicted that the "overwhelming encumbrance" of numerical calculation would impede the progress of science. In Aiken's case, he needed a universal calculating machine to solve the differential equations he was using in his research. The demand for computers was greatly increased by the special problems created by military needs in World War II, particularly in the sighting devices used in planes and in the artillery. The directors of the Manhattan project, which was formed for the development of an atomic bomb, also recognized that computers were essential to the success of the project and made funds available for research and development. Thus, the demand for calculating machines was present in the nineteen forties. If such a demand had been present during Babbage's

lifetime he would have probably received the financial support and the encouragement to complete his calculating engines.

Ironically, these same conditions which would have helped Babbage in his work with calculating engines would very likely have brought him recognition for his ideas concerning governmental support of science. Industrialized nations with increasingly complex technology have relied upon the talents and abilities of artisans, industrialists, and scientists, and Babbage would have very likely received the recognition he desired in a setting such as was afforded Aiken. On the other hand, some of the personality traits which Babbage exhibited would have probably been disadvantageous if he had been a member of a research team and had been required to write proposals for research money. Babbage was very independent, often impatient with his associates and particularly with governmental officials, and at times unwilling to compromise his views even when it was to his advantage. He could afford such personality traits in nineteenth century England and still make significant contributions to science because he was moderately wealthy and could pursue his scientific interests without undue concern, although Babbage was very concerned that he might ruin his fortune because of the expenses incurred in constructing the calculating machines. His affluent intellect was an asset at the time because it was acceptable and even fashionable to pursue whatever scientific problem caught his fancy, or,

as John Herschel put it, to casually examine any shiny pebble on the seashore of science. Babbage would have almost certainly chafed at any restriction placed upon his research. The English government was really quite lenient in allowing him to expend about £17,000 of public money with only the requirement of an itemized bill. Babbage was also given much freedom as the Lucasian Professor of Mathematics at the University of Cambridge. His only duty was to help administer the annual examinations in mathematics.

In a sense, Babbage lived in a time which was well suited to his broad scientific pursuits because of his own personal wealth and because of the few restrictions placed upon his time and efforts. His disappointment that he was not recognized for his contributions in science was due in part to his impatient nature and his own vanity. Any one of his contributions to science would have been sufficient to satisfy some men. His contributions to mathematics and mathematical reform in the early part of his career brought him recognition in Great Britain and on the Continent and led to his being awarded the Lucasian Professorship of Mathematics. His efforts in scientific reform and the founding of new scientific societies brought him to the attention of the public. His work with calculating engines, which he invented and developed without aid of the work of other scientists and inventors, was original and important enough to ensure him the recognition he had sought. His varied interests and researches

marked him as a man with an ability to apply his intellect usefully to almost any field of science he chose. He pursued his goal of developing a calculating engine over a period of about fifty years with a determination, in spite of many difficulties, which is reminiscent of Kepler who similarly experienced great difficulties in his work with planetary orbits. In short, Babbage did receive the recognition he had sought but not necessarily in the manner he desired or at the time he desired.

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