NOTES AND RECORDS

Notes Rec. (2022) **76**, 117–153
doi:10.1098/rsnr.2020.0027
Published online 30 September 2020

JOULE'S 1840 MANUSCRIPT ON THE PRODUCTION OF HEAT BY VOLTAIC ELECTRICITY

by

ROBERTO DE ANDRADE MARTINS*

Universidade Federal de São Paulo, Rua São Nicolau 210, Diadema, SP 09913-030, Brazil

In 1840, James Prescott Joule submitted to the Royal Society a paper describing experimental research on the heat produced by electric currents in metallic conductors, and inferring that the effect was proportional to the resistance of the conductors and to the square of the intensity of the current. Only an abstract of this paper was published in the *Proceedings of the Royal Society*, although a full paper with a similar title was printed in the *Philosophical Magazine* in 1841. Several authors have assumed that the content of the 1841 publication was the same as the rejected 1840 paper; however, the unpublished manuscript has been found within the archives of the Royal Society and is published here for the first time, along with a detailed analysis and comparison with the 1841 paper. The unpublished version is much shorter, and is different in certain respects from the published article. A detailed comparison throws light on several shortcomings of the unpublished version. The present work also studies the assessment of Joule's paper by the Royal Society, and elucidates the roles of Peter Roget and Samuel Christie in this connection.

Keywords: James Prescott Joule; history of physics; nineteenth-century science; electric heating; Royal Society; scientific manuscripts

INTRODUCTION

Before he passed away, James Prescott Joule (1818–1889) had become one of the most famous British physicists of the nineteenth century. His involvement in the establishment of the law of conservation of energy as well as his other contributions are well known. The experimental skill he exhibited in his delicate assessment of the mechanical equivalent of heat was praised by his contemporaries and by many historians of science. However, he spent all his life in a provincial town and he never studied at any university. How did he become a capable man of science in the mid-nineteenth century, in Manchester?

*roberto.andrade.martins@gmail.com

Joule's formative period is very hard to investigate, for lack of documentation. Donald Cardwell, who studied his life for decades,[1] was unable to elucidate how James Joule became a great experimentalist. Details about his education are scanty; and the early scientific influences he received have never been identified. This paper will not provide an answer to all the issues concerning Joule's initial scientific development. However, by analysing the context of one of his first investigations, some light will be thrown upon the making of that man of science.

The earliest papers published by James Prescott Joule, between 1838 and 1840, were concerned with electromagnetic devices such as motors and electromagnets.[2] In this initial part of his career, Joule can be regarded as an electrician, a title applied to those who deal with electrical devices.[3] All of his early papers were published in the *Annals of Electricity, Magnetism, and Chemistry; and Guardian of Experimental Science*, edited by William Sturgeon (1783–1850), who mostly published works of a technical nature.[4]

In 1840 Joule produced his first purely scientific (or 'philosophical') research, on the heat produced by electric currents in metals. He was apparently aware that this was a different kind of work, since he did not send it for publication in the *Annals of Electricity*, submitting it instead to the Royal Society. In this work, Joule described several experiments on the heat produced by electric currents in metallic conductors, and concluded that this effect was proportional to the resistance of the conductors and to the square of the intensity of the current. The paper was not accepted for publication, and only a short abstract was published in the *Proceedings of the Royal Society*.[5] A few months later, Joule sent a paper with similar content to the *Philosophical Magazine*, where it was accepted and published.[6] A previous paper has presented a detailed analysis of the printed version, and compares it to former and later research by other authors, taking into account only published sources.[7]

It has been generally supposed that the *Philosophical Magazine* article had the same content as the manuscript submitted to the Royal Society; however, this was not the case. The original document was recently found within the archives of the institution, and is published and commented on here for the first time. It is different from the work published in 1841, and a comparison throws new light on Joule's early research on the production of heat by electricity. This specific case study will also provide a better assessment of the development of Joule's experimental expertise.

1 Donald Stephen Lowell Cardwell, *James Joule: a biography* (Manchester University Press, Manchester, 1989). Cardwell's book is the main biographical source concerning Joule. Its first edition was published in 1978, but the 1989 edition was used in this paper.

2 James Prescott Joule, *The scientific papers of James Prescott Joule*, vol. 1 (Taylor & Francis, London, 1884), pp. 1–42.

3 Iwan Rhys Morus, 'Currents from the underworld: electricity and the technology of display in early Victorian England', *Isis* **84**, 50–69 (1993), at p. 52.

4 Iwan Rhys Morus, 'Different experimental lives: Michael Faraday and William Sturgeon', *Hist. Sci.* **30**(1), 1–28 (1992).

5 James Prescott Joule, 'On the production of heat by voltaic electricity', *Proc. R. Soc. Lond.* **4**(46), 280–281 (1841). Sometimes the date of this abstract is given as 1843, because the volume containing it reads, on its title page: *Abstracts of the papers printed in the Philosophical Transactions of the Royal Society of London*, vol. 4, 1843. However, the *Proceedings* were usually published in monthly instalments. Issue 46, which contained Joule's abstract, comprised a description of the meetings of the Royal Society held from December 1840 to February 1841, and was published in March 1841.

6 James Prescott Joule, 'On the heat evolved by metallic conductors of electricity, and in the cells of a battery using electrolysis', *Lond. Edinb. Dubl. Phil. Mag. J. Sci.* **19**(124), 260–277 (1841). The paper was reprinted and translated: *Ann. Electr. Magn. Chem. Guard. Exp. Sci.* **8**, 287–301 (1842); 'Sur la chaleur développée dans les conducteurs métalliques et dans les auges d'une pile sous l'influence de l'éléctricité', *Archives de l'Électricité. Supplément a la Bibliothèque Universelle de Genève* **2**, 54–79 (1842).

7 Roberto de Andrade Martins and Ana Paula Bispo da Silva, 'Joule's experiments on the heat evolved by metallic conductors of electricity', *Found. Sci.* (20 June 2020), https://doi.org/10.1007/s10699-020-09681-1.

Joule's education

James Prescott Joule was born on 24 December 1818, at Salford.[8] His father, Benjamin, was the wealthy owner of a brewery established by Walter Joule (James's grandfather) in the previous century.[9] James was Benjamin Joule's second child. He had two brothers and two sisters. From boyhood James Joule suffered from a deformity of the spine that required treatment, but this did not prevent him riding horses, climbing hills and rowing with his brother.[10] Overall, he seems to have led the life of a healthy youth.

James Joule and his older brother, Benjamin, never went to school. They were educated at their father's house, Broom Hill.[11] They were first tutored at home by their mother's half-sister, Frances Prescott, next by a resident master called S. T. Porter, and afterwards by Frederick Tappenden, until December 1834.[12] This was the end of their tutelage. The subjects they studied during that period, the textbooks they had read and the depth of their study are unknown.

After that, James Joule, now 16 years old, began to work at the brewery every day, from nine in the morning to six in the evening.[13] As his father's health declined, the management of the business gradually fell into the hands of the two older sons. Benjamin was not much interested in that activity and he later became a musician. Their younger brother, John, seemingly led a life of leisure.[14] So responsibility for the brewery fell mainly to James, until it was sold, 20 years later.

James Joule's early involvement with the brewery influenced his later scientific work in several ways. He learned to use instruments such as balances, thermometers and hydrometers, which were widely used in the brewing activities. He became acquainted with machines driven by steam power;[15] and he was in contact with engineers and mechanics

8 For general biographical information about James Joule, see Cardwell, *op. cit.* (note 1). The borough of Salford was attached to Manchester. Although separated by the river Irwell, they were connected by six bridges. Around 1840, the two towns together extended from east to west about two miles, and somewhat less from north to south. In 1831 the population of the whole parish was 271,000 inhabitants, and that of Salford was 41,000. *The parliamentary gazetteer of England and Wales*, 4 vols (A. Fullarton, London, 1847–1848), vol. 3, pp. 353–355; Society for the Diffusion of Useful Knowledge, *Penny cyclopaedia*, 27 vols (Charles Knight, London, 1833–1846), vol. 20, p. 350.

9 The Joule brewery, on New Bailey Street, was quite large, and its machines were moved by steam power. Heinz Otto Sibum stated that it produced from 10,000 to 20,000 barrels a year: Heinz Otto Sibum, 'Les gestes de la mesure: Joule, les pratiques de la brasserie et la science', *Ann. Hist. Sci. Soc.* **53**(4–5), 745–774 (1998), at p. 763. Donald Cardwell remarked that its annual production should have been well over 20,000 barrels a year: Donald Stephen Lowell Cardwell, 'The origins and consequences of certain of J. P. Joule's scientific ideas', in *Springs of scientific creativity: essays on founders of modern science* (ed. Rutherford Aris, H. Ted Davis and Roger H. Stuewer), pp. 44–70 (University of Minnesota Press, Minneapolis, 1983), at p. 65, n. 4. Actually, by the 1840s the brewery had about 13,000 barrels in use, according to Alan Gall, 'James Joule—brewer and man of science', *Brewery Hist.* **115**, 2–6 (2004). Assuming that the barrels were returned and filled again in one month, the production of the brewery could have reached 150,000 barrels a year. The family also owned a number of pubs.

10 Osborne Reynolds, *Memoir of James Prescott Joule* (Manchester Literary and Philosophical Society, Manchester, 1892), pp. 25–32. His brother Benjamin kept a personal diary from early life, and that was a valuable source of information about James Joule's youth used by Reynolds in his *Memoir*.

11 James Joule's home education was certainly not caused by his health problem, as stated by some authors. Wealthy families preferred to keep their children apart from the more humble ones.

12 Cardwell, *op. cit.* (note 1), pp. 13–14.

13 *Ibid.*, p. 29.

14 *Ibid.*, p. 47.

15 In 1837, the total steam power installed in Salford amounted to 1,596 horse power, most of it employed in spinning and weaving, bleaching and dyeing fabrics. The breweries of the town made use of 56 horse power. It is impossible to assess how much was used in the Joule brewery. Society for the Diffusion of Useful Knowledge, *op. cit.* (note 8), vol. 20, p. 351.

from whom he could derive scientific and technical knowledge he used later in his studies on electromagnetic engines.[16]

James's father conjectured that some knowledge of chemistry would be useful for his sons. In 1835, after the end of their tutelage, James and Benjamin started receiving two one-hour weekly lessons from the famous chemist John Dalton.[17] Private teaching had been Dalton's only paid occupation since 1800, when he resigned from the Manchester Academy, a short-lived dissenting college. He was allowed to use a room at the Manchester Literary and Philosophical ('Lit. & Phil.') Society to conduct his research and to teach mathematics, calligraphy, grammar, physics and chemistry.[18] The two brothers started learning mathematics—not chemistry, as their father expected. It is unlikely that they had received any deep training in mathematics, chemistry or natural philosophy (physics) from their tutors—otherwise, Dalton might have been willing to teach them chemistry from the beginning.[19]

According to James Thomson Bottomley, after teaching arithmetic, algebra and geometry to Benjamin and James, Dalton introduced them to natural philosophy using Tiberius Cavallo's textbook.[20] Afterwards, he started lessons in chemistry using his own *New system of chemical philosophy*. It is quite possible that Dalton made some demonstrations during his lessons, since they took place in his laboratory. The chemistry sessions extended for just a few months. On 18 April 1837 Dalton suffered a stroke and was speechless and paralysed in his right side for several days. He had a partial recovery, but his memory of words remained permanently impaired. In the following years he suffered two further strokes and he was unable to continue his private lessons due to the decline of his physical and mental powers; he died in 1844.[21]

It seems that, under Dalton, James Joule first became acquainted with scientific apparatus and experiments, and was stimulated to begin his own trials.[22] Much speculation has been published about Dalton's impression upon the teenage Joule, but it is impossible to ascertain the extent of this hypothetical scientific influence.

16 Donald Stephen Lowell Cardwell, 'Science and technology: the work of James Prescott Joule', *Technol. Cult.* **17**(4), 674–687 (1976).

17 Cardwell, *op. cit.* (note 1), p. 14. Sibum mistakenly stated that the two brothers were tutored by Dalton at their home: Sibum, *op. cit.* (note 9), p. 763; Heinz Otto Sibum, 'Reworking the mechanical value of heat: instruments of precision and gestures of accuracy in early Victorian England', *Stud. Hist. Phil. Sci. A* **26**(1), 73–106 (1995), at p. 87; Heinz Otto Sibum, 'An old hand in a new system', in *The invisible industrialist: science, technology and medicine in modern history* (ed. Jean-Paul Gaudillière and Ilana Löwy), pp. 23–57 (Palgrave Macmillan, London, 1998), at p. 32.

18 Robert Hugh Kargon, *Science in Victorian Manchester: enterprise and expertise* (Routledge, New York, 2017), p. 12.

19 The Special Collections of the John Rylands Library, Manchester University, contain a mathematical textbook that belonged to James Prescott Joule: Francis Walkingame, *The tutor's assistant: being a compendium of practical arithmetic for the use of schools or private students*, 11th edn (Henry Mozley & Sons, Derby, 1832). It is unknown whether James Joule studied this book under Dalton or when tutored by Frederick Tappenden.

20 James Thomson Bottomley, 'James Prescot Joule', *Nature* **26**, 617–620 (1882), at p. 617. Bottomley's short biographical note is the only source of information concerning the study of Cavallo's work by the Joule brothers. Bottomley was a nephew of William Thomson and worked as his assistant for years; he probably obtained the information from Thomson. Tiberius Cavallo (1749–1809) wrote a textbook that was widely used: Tiberius Cavallo, *Elements of natural and experimental philosophy*, 4 vols (T. Cadell & W. Davie, London, 1803). The work contained an exposition of what we would nowadays call physics (mechanics, heat, optics, electricity, magnetism); it also included general chemistry, astronomy and other topics. It was outdated in 1836, since it did not contain the important developments of optics and electricity that had occurred in the first decades of the nineteenth century, such as the establishing of the wave theory of light and the discovery of electromagnetism. The Special Collections of the John Rylands Library, Manchester University, do not contain this treatise.

21 William Charles Henry, *Memoirs of the life and scientific researches of John Dalton* (Cavendish Society, London, 1854), pp. 199–200; cf. Frank Greenaway, *John Dalton and the atom* (Heinemann, London, 1966), pp. 196–197; Kargon, *op. cit.* (note 18), pp. 24, 50.

22 Bottomley, *op. cit.* (note 20), p. 617.

Since Dalton was unable to resume his teaching activities, in 1839 the Joule brothers started taking private lessons in chemistry from John Davies (*ca* 1790–1850).[23] James Joule was even allowed to attend Davies' lectures for medical students. Davies was the vice-president of the Manchester Mechanics' Institution, where he also delivered lectures—a place that was frequented by Joule.[24] Besides his role as a teacher, John Davies established a strong link with Joule and he may have given him some assistance during his early electrochemical research.

Scientific institutions in Manchester

James Joule's interest in science may have begun with his contact with John Dalton, in 1835, and he began his first research a few years later. What kind of stimulus and opportunities for scientific education did he have at that time, besides the private lessons he received? It is conceivable that an engineer or other technician who worked for his father, at the brewery, could have informally imparted to him some scientific knowledge and/or supplied the stimulus for reading scientific books—but we have no direct evidence that this occurred. It is likely that he received some influence from some of the scientific and cultural institutions of his town.[25]

Manchester (including Salford and surroundings) was the most important industrial city in the world in the 1830s. Its population had increased from 111,000 in 1801 to 271,000 in 1831 and to about 300,000 in 1839.[26] In Britain, only London had a larger population at that time. About three-quarters of the Manchester inhabitants worked in its industries. Profit, not science, was the main concern in the town. It had no university or college,[27] but there were some relevant science institutions in Manchester, and there was a widespread interest in the use of scientific knowledge to improve the techniques that produced the local prosperity.[28] There was also a general curiosity about scientific novelties, as in most other places.

The oldest learned institution in Manchester was the Literary and Philosophical Society, founded in 1781, before the industrial boom.[29] Four of its founding members were Fellows of the Royal Society of London.[30] Its members belonged to the cultural élite of the town, but many of them never published a single scientific paper; the society was mainly a league of gentlemen with cultural aspirations.[31] The Lit. & Phil. was a restricted association, with no educational activities or open lectures. Its activities were the meetings

23 Little is known about John Davies. His main activity was chemistry, but he did not produce any books or relevant papers on the subject. He was elected a member of the Manchester Literary and Philosophical Society in 1816 and hence we may infer that he was born before 1800. His portrait by Samuel William Reynolds, engraved and published in 1833, suggests that he was about 40 years old at that time. He was a lecturer and private teacher and played an important role in some of Manchester's learned societies. Kargon, *op. cit.* (note 18), pp. 23–24.

24 Cardwell, *op. cit.* (note 1), p. 28; Mabel Phytian Tylecote, *The mechanics' institutes of Lancashire and Yorkshire before 1851* (Manchester University Press, 1957), p. 138.

25 Kargon, *op. cit.* (note 18), provides an adequate overview of the scientific state of affairs in Victorian Manchester.

26 *The parliamentary gazetteer of England and Wales*, *op. cit.* (note 8), vol. 3, pp. 353–354.

27 The Manchester Royal Infirmary, founded in 1752, became a medical school in the late eighteenth century, but it could not grant degrees to its students. Thomas Neville Bonner, *Becoming a physician: medical education in Britain, France, Germany, and the United States, 1750–1945* (Johns Hopkins University Press, Baltimore, 2000), p. 50.

28 Kargon, *op. cit.* (note 18), p. 3.

29 *Ibid.*, p. 4.

30 *Ibid.*, p. 7.

31 *Ibid.*, p. 28.

and the irregular printing of memoirs and proceedings. In the 1830s it was internationally known and its library received the publications of many foreign academies. Although John Dalton's private lessons were dispensed at the premises of the Lit. & Phil., it is unlikely that the teenagers James and Benjamin Joule could have had access to the meetings or to the library. The Lit. & Phil. was the only institution in Manchester that offered discussion and publication of research. James did not submit any of his first papers to the society, and this suggests that initially he had no direct connection with that association, although he was later to become one of its most active members.[32]

By contrast with the Lit. & Phil., the Royal Manchester Institute, founded in 1823, and the Manchester Athenæum for the Advancement and Diffusion of Knowledge, founded in 1835, were cultural associations for the middle classes.[33] They offered libraries, lectures and discussion meetings of a cultural and scientific flavour. It is quite possible that their special social status could have interested Benjamin Joule and his family, but there is no direct evidence that James Joule frequented these institutions.

A more directly relevant organization was the Manchester Mechanics' Institution, established in 1824, which aimed to instruct the working classes in scientific subjects and other useful knowledge.[34] There were regular evening classes on several subjects, including mathematics, technical drawing, chemistry and natural philosophy, but it is unlikely that the Joule brothers would have frequented them, given their higher social status and the elitist educational approach of their father. However, the Mechanics' Institution also offered frequent lectures on scientific subjects that could have been attended by James Joule; and it had a large library, which was open both to its members and to those who were willing to pay a small charge.[35] Some of the activities of the Mechanics' Institution, such as its annual exhibitions, attracted an enormous number of observers.[36] It was probably at this institution that Joule first attended lectures on electricity delivered by William Sturgeon, as will be shown below.[37]

There were other learned institutions in Manchester, devoted either to specific scientific subjects (geology, statistics) or to the arts; but none of them seems to have played any role in James Joule's scientific development. There were many public libraries available at that time, both in Manchester and in Salford, containing scientific and technical works that could have been used by Joule;[38] but there is no information about his habit of consulting any of those collections.

32 Joule was elected a member of the Lit. & Phil. in 1842; he became its librarian in 1844, honorary secretary in 1846, a vice-president in 1851, and president for the first time in 1860. Richard Tetley Glazebrook, 'Joule, James Prescott', in *Dictionary of national biography (DNB)*, 63 vols (ed. Leslie Stephen and Sidney Lee), vol. 30, pp. 208–214 (Smith, Elder & Co., London, 1885–1900), at p. 209.

33 Howard M. Wach, 'Culture and the middle classes: popular knowledge in industrial Manchester', *J. Brit. Stud.* **27**(4), 375–404 (1988), at pp. 376–378.

34 Tylecote, *op. cit.* (note 24), presents a detailed description of the Manchester Mechanics' Institution.

35 In 1839 the Manchester Mechanics' Institution offered 79 lectures on different subjects (most of them related to the sciences), and its library had 5,500 books and 50 periodicals. Kargon, *op. cit.* (note 18), p. 24; Tylecote, *op. cit.* (note 24), pp. 149–156.

36 The first popular exhibition, from December 1837 to February 1838, was attended by 50,000 people. Tylecote, *op. cit.* (note 24), p. 178.

37 According to Mabel Tylecote, new types of electrical apparatus were popular exhibits at the Mechanics' Institution; and William Sturgeon was one of the lecturers at that institution. Tylecote, *op. cit.* (note 24), pp. 151–152, 182.

38 William Edward Armytage Axon, *Handbook of the public libraries of Manchester and Salford* (Abel Heywood & Son, Manchester, 1877).

Electromagnetic devices and William Sturgeon

In late 1837 or early 1838 James Joule obtained his father's permission to use a room at Broom Hill where he started a laboratory. There he began electrical experiments and the construction of batteries and electromagnetic devices. It seems that the motivation of his undertaking was practical: improving electromagnetic engines so that they could replace steam machines.

Steam engines were quite common and essential in Manchester at that time. Besides their most important applications—cotton spinning and weaving—they were used in breweries, as already mentioned. On the other hand, electromagnetic devices were just toys—or, to use a more respectable title, they were 'philosophical instruments'. However, there was an optimistic view that they would soon attain practical utility.[39]

Shortly after the discovery of electromagnetism in 1820 by Hans Christian Ørsted (1777–1851), several researchers created small devices that produced the circular motion of material pieces using magnets and electric currents.[40] The invention of soft-iron-core electromagnets, the development of better voltaic batteries and the creation of new types of devices produced a short-lived excitement in the 1830s and early 1840s, with the prospect of building powerful and economic appliances that could supersede steam engines.[41] In Britain, one of the chief champions of this movement was William Sturgeon (1783–1850).

William Sturgeon was born to a poor family.[42] His father was a shoemaker who did not care about his responsibilities and spent his time fishing and rearing fighting cocks. As a boy, William became the apprentice of another shoemaker, but afterwards he attempted to make a living as a professional soldier. In 1802 he enlisted in the Westmoreland militia, and two years later as a private in the Royal Artillery.

In leisure time, during his military career, he studied mathematics and several languages and decided to acquire the rudiments of a scientific education after witnessing a powerful thunderstorm. He left the army in 1820 and settled as a shoemaker in Woolwich, but he retained his interest in science—and, especially, in electricity. He had mechanical talent and with the use of an old lathe he began to produce some instruments. He supplemented his income by lecturing to schools and teaching officers' families. In 1823 he wrote his first scientific paper, which was published in the *Philosophical Magazine*. His devotion to science attracted the attention of several men of science, such as James Marsh, Olinthus

39 Ben Marsden and Crosbie Smith, *Engineering empires: a cultural history of technology in nineteenth-century Britain* (Palgrave Macmillan, Basingstoke, 2005), pp. 65–82.

40 A detailed historical account of the early development of electromagnetic motors is presented by Brian Gee, 'Electromagnetic devices: pre-technology and development immediately following Faraday's discovery of electromagnetic rotations', *Hist. Technol.* **13**, 41–72 (1991). See also the historical account presented by one of the participants of this development: William Sturgeon, 'Historical sketch of the rise and progress of electromagnetic engines for propelling machinery', *Ann. Electr. Magn. Chem. Guard. Exp. Sci.* **3**(17), 429–437 (1839).

41 Practical electromagnetic engines only began to be used in the 1880s, after the development of central power stations. Brian Bowers, 'Electricity', in *An encyclopedia of the history of technology* (ed. Ian McNeil), pp. 350–387 (Routledge, London, 1990), at pp. 381–384.

42 Biographical information about Sturgeon can be found in: James Prescott Joule, 'A short account of the life and writings of the late Mr. William Sturgeon', *Mems Lit. Phil. Soc. Manch.* (ser. 7) **14**, 53–83 (1857); William Gee, 'Sturgeon, William', in *DNB*, *op. cit*. (note 32), vol. 55, pp. 131–135; Silvanus Phillips Thompson, *The electromagnet and electromagnetic mechanism* (E. & F. N. Spon, London, 1892), Appendix A, pp. 412–418; Iwan Rhys Morus, 'Different experimental lives: Michael Faraday and William Sturgeon', *Hist. Sci.* **30**(1), 1–28 (1992).

Gilbert Gregory, Samuel Hunter Christie and Peter Barlow.[43] Through their influence, in 1824 he became lecturer in science and philosophy at the East India Company's Royal Military College of Addiscombe. From that time, he made a living by lecturing, and continued to dedicate himself to the study of electricity and the development of several inventions, including the soft-iron electromagnet, the use of amalgamated zinc plates in batteries and the creation of improved electromagnetic devices. In 1832 he built and made public demonstrations of an electromagnetic rotary engine that could move several miniature machines. In the following decades he devoted himself to research, invention and scientific lectures. The *Catalogue of scientific papers* of the Royal Society lists 69 published papers authored by him.

Sturgeon was one of the key participants of the short-lived Adelaide Gallery of Practical Science in London, and of the London Electrical Society. In 1836 he founded a journal called *The Annals of Electricity, Magnetism, and Chemistry: And Guardian of Experimental Science* that went through ten volumes.[44] Without any permanent job, he kept travelling around with a horse cart, carrying electrical devices and presenting lectures.[45]

In 1838 Sturgeon published a missive he had received from a young man from Manchester called John Collis Nesbit (1818–1862).[46] From the beginning of that letter, which was dated 10 February 1838, we may infer that a few weeks earlier Sturgeon had presented a series of lectures at the Manchester Mechanics' Institution:

> Dear Sir,
>
> It is with sincere pleasure that I take up my pen to address a few lines to you, and must apologize for not doing so before, but I have been so very much engaged since you were in Manchester, that I have not had time.
>
> Your lectures in our Institution have induced many of our members to engage in the study of Electro-magnetism; and they have stirred up a spirit of enquiry and a desire for philosophical knowledge, which cannot fail to be attended with beneficial effects.[47]

The same volume of the *Annals of Electricity* also reproduced a letter written by James Prescott Joule to the Editor, dated 8 January 1838, describing his project of a new electromagnetic device.[48] It is very likely that Joule had likewise attended Sturgeon's lectures at the Manchester Institution.

This letter by Joule was his first published paper. It did not contain the description of a real device, but the project of an electromagnetic device he intended to build. The description is quite detailed, and it was accompanied by careful drawings (figure 1).

43 Iwan Rhys Morus, *Frankenstein's children: electricity, exhibition, and experiment in early nineteenth-century London* (Princeton University Press, Princeton, 1998), pp. 45–46.

44 The beginning of the *Annals of Electricity* in October 1836 corresponded with the rejection of a paper submitted by Sturgeon to the *Philosophical Transactions of the Royal Society*. Gee, *op. cit.* (note 41), p. 134; Joule, *op. cit.* (note 42), p. 65. The paper was published in Sturgeon's own journal, the next year.

45 Tylecote, *op. cit.* (note 24), p. 152.

46 John Collis Nesbit, 'On electro-magnetic coil machines', *Ann. Electr. Magn. Chem. Guard. Exp. Sci.* **2**, 203–205 (1838). John Nesbit later became a famous agricultural chemist. For biographical information about Nesbit, see Richard Bissell Prosser, 'Nesbit, John Collis', in *DNB*, *op. cit.* (note 32), vol. 40, pp. 224–225.

47 Nesbit, *op. cit.* (note 46), p. 203.

48 James Prescott Joule, 'Description of an electro-magnetic engine', *Ann. Electr. Magn. Chem. Guard. Exp. Sci.* **2**, 122–123 (1838). Both the text and images of the version of this paper published in Joule's *Scientific papers* (edited by himself) are quite different from the original: Joule, *op. cit.* (note 2), pp. 1–3. Hitherto, historians of science have tacitly assumed that the collected papers could be safely used as a reliable reproduction of the original articles, but much care should be taken when using this revised edition.

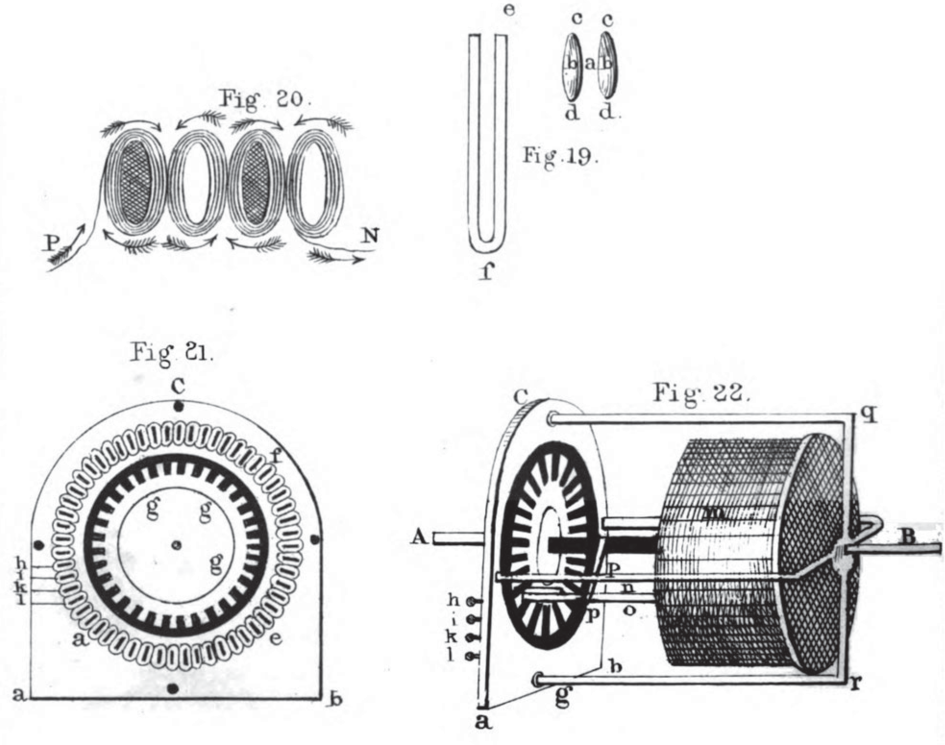

Figure 1. The drawings of Joule's 1838 plan for an electromagnetic engine. *Annals of Electricity* **2** (1838), plate III.

James Joule was 19 years old when he wrote up this project. The sketches accompanying the description were very well made, and it is doubtful that Joule would have had the necessary expertise to produce them. It is quite likely that he had the help of an engineer or, at least, of a technician ('mechanic') with adequate technical drawing training.[49] Very nice technical drawings accompany some of his other early papers, though Joule never acknowledged the collaboration of anyone in those articles.[50]

Besides the illustrations, the execution of the device would have required considerable technical skill, especially because of its small dimensions. It would contain 40 magnets, but 'the whole size of the machine will not be more than a six inch cube'.[51] Engineers or

49 From its foundation, the Manchester Mechanics' Institution offered regular courses on mechanical and architectural drawing: Tylecote, *op. cit.* (note 24), at p. 156. The classes were held three days a week, from half-past-seven to half-past-nine in the evening. Around 1842, seventy students attended this course. The same kind of classes were also offered at the Salford Mechanics' Institution. Benjamin Love, *The hand-book of Manchester: containing statistical and general information on the trade, social condition, and institutions, of the metropolis of manufactures* (Love & Barton, Manchester, 1842), at pp. 182–183. Hence, in both Salford and Manchester, Joule could easily find trained people who could prepare the drawings he published.

50 A few of Joule's later collaborators have been clearly identified—for example, the famous instrument maker John Benjamin Dancer (1812–1887), who moved to Manchester in 1841 and was soon to become a close associate of James Joule. See Osborne Reynolds, 'John Benjamin Dancer', *Mems Proc. Manch. Lit. Phil. Soc.* (ser. 4) **1**, 149–153 (1888), at p. 151. There were, however, some invisible assistants such as the person (or persons) who must have helped Joule in the performance of his paddle-wheel experiments. Sibum, *op. cit.* (note 9), at p. 767.

51 Joule, *op. cit.* (note 48), p. 123. The *Scientific papers* version omitted the size of the device and its parts.

mechanics working with large-sized steam engines would be unable to build such a delicate mechanism. A clock maker or scientific instrument maker might be required to manufacture the device.

Although the planned machine was quite small, Joule intended to apply its arrangement to practical uses: 'to the axle *A*, *B*, wheels or paddles may be affixed so as to answer either locomotive or sailing purposes'.[52]

In the following years, Joule published several other papers in the *Annals of Electricity*. He soon established a personal relationship with Sturgeon, possibly through John Davies, who became James Joule's chemistry teacher, who was at that time the vice-president of the Mechanics' Institution where Sturgeon had delivered a series of lectures and one of the organizers of the Victory Gallery that was soon to employ him.[53] In March 1840 Sturgeon had definitely left London and he was residing in Manchester, as the superintendent of a new institution, the Royal Victoria Gallery for the Encouragement of Practical Science.[54] However, it is possible that since 1838 he had made frequent visits to Manchester or had even moved there, because Joule himself identified that year as the date of Sturgeon's relocation.[55]

Hence, in 1840 or before that time, an informal scientific circle was formed in Manchester, first comprising James Joule, John Davies and William Sturgeon, and afterwards including John Leigh (1812–1888) and Edward William Binney (1812–1881).[56] Davies and Sturgeon were much older than the others; James Joule was the youngest of the group and he would certainly benefit from his association with those more experienced men of science. Sturgeon's scientific influence upon Joule was undoubtedly highly effective; and their personal relationship was also robust and led him to write a detailed account of William Sturgeon's life and work, after his passing.[57] This was the only biographical notice ever written by Joule.

Heat produced by voltaic electricity

Joule began his research on the heat produced by electric currents in metallic wires in 1840. This marked the very beginning of his wide-ranging studies of the transformation, equivalence and conservation of 'forces'. However, before 1843 he published no general statements about this subject, and only presented the results of specific studies, with little theoretical discussion.

The University of Manchester Library has preserved a collection of manuscripts by Joule. The original set once included a notebook with 260 pages, which contained, among other

52 Joule, *op. cit.* (note 48), p. 123. In 1837 Sturgeon had already suggested the use of electromagnetic engines to propel a boat and a locomotive carriage: Marsden and Smith, *op. cit.* (note 39), p. 76. The version of Joule's letter published in the *Scientific papers* contains, instead: 'The axle carrying the movable circle is supported by the bearing S, and may be used to turn any kind of machinery'. Joule, *op. cit.* (note 2), p. 3.

53 Cardwell, *op. cit.* (note 1), p. 28.

54 Kargon, *op. cit.* (note 18), pp. 37–39.

55 Joule, *op. cit.* (note 42), p. 77. See also Cardwell, *op. cit.* (note 16), p. 678.

56 Kargon, *op. cit.* (note 18), pp. 39–40; see pp. 60–66 for information about Binney, and pp. 66–74 about Leigh. See also Robert Hunt, 'Binney, Edward William', in *DNB*, *op. cit*. (note 32), vol. 5, pp. 56–57.

57 Joule, *op. cit.* (note 42).

things, a description of Joule's experimental work from 1839 to 1843.[58] Unfortunately, this notebook was lost during World War II,[59] but before its disappearance, it was studied by William Gee, who presented the only extant account of its content:[60]

> The first record relating to electric heating is dated 'later end of August 1840'. By October 3 in the same year Joule had completed a series of tests which enabled him to state that the heat produced by an electric current in a wire is directly proportional to its resistance, the square of the current strength, and the time of its flow. He next proceeded to examine if these laws applied to electrolytes, and met many serious difficulties which delayed the publication of his results for many months. Finally he was able to state with certainty that electrolytes also obeyed the heating laws. Many unpublished experiments relating to this section of his researches are to be found in the first Laboratory Book.

Shortly after finishing the first series of his experiments, Joule wrote a paper describing the results. The collection of Joule's manuscripts that are now kept in the University of Manchester Library formerly contained a volume of 138 pages entitled 'Papers in rough'.[61] This volume is also missing, and it is unknown whether it contained the draft of the 1840 paper. Recently, however, Joule's original paper was retrieved from the archives of the Royal Society.[62]

Joule wrote a report on his research with the title 'On the production of heat by voltaic electricity', and submitted it to the Royal Society. The paper was dated 13 October 1840, and was received by the Society on 16 October. On 17 December 1840, it was communicated to the Fellows by Peter Mark Roget (1779–1869), the First Secretary of the Royal Society.[63] An abstract was published towards the end of February or beginning of March 1841, in the issue of the *Proceedings of the Royal Society* which described the meetings between December 1840 and February 1841.[64] It was reproduced in the 6 March issue of the weekly journal *The Athenæum*; and in the April 1841 issue of the *Philosophical Magazine*.[65] The abstract announced:

58 Harry Lowery, 'The Joule collection in the College of Technology, Manchester. Part II. B. Manuscripts', *J. Sci. Instrum.* **8**(1), 1–7 (1931), at p. 3.

59 'James Prescott Joule Manuscripts, 1839–1887', University of Manchester Library, GB 133 JPJ, Archives Hub website, https://archiveshub.jisc.ac.uk/data/gb133-jpj (accessed 14 February 2020). John Forrester claimed that Eric Mendoza had used the notebook in his study of Joule, but the manuscript cited by Mendoza was a different one, containing a lecture delivered by Joule in 1847 (University of Manchester Library, James Prescott Joule Manuscripts, GB 133 JPJ/2). See John Forrester, 'Chemistry and the conservation of energy: the work of James Prescott Joule', *Stud. Hist. Phil. Sci.* **6**(4), 273–313 (1975), p. 313; Eric Mendoza, 'The surprising history of the kinetic theory of gases', *Mems Proc. Manch. Lit. Phil. Soc.* **105**, 15–28 (1962–1963), pp. 23–24.

60 William Winson Haldane Gee, 'Joule's laws of electric heating', *Mems Proc. Manch. Lit. Phil. Soc.* **69**, xiii–xv (1925), p. xiv. This article contains only the summary of a paper read by Haldane Gee on 16 December 1924. The archive of the Lit. & Phil. does not contain the full version of the paper; if a longer version was archived, it was probably lost in the fire that destroyed its building in late December 1940, due to German bombing. See Donald Sheenan, 'The Manchester Literary and Philosophical Society', *Isis* **33**(4), 519–523 (1941), p. 519; Peter Thorsheim, *Waste into weapons: recycling in Britain during the Second World War* (Cambridge University Press, New York, 2015), p. 183. See also Guy R. Hodgson, *War torn: Manchester, its newspapers and the Luftwaffe's Christmas blitz of 1940* (University of Chester Press, Chester, 2015).

61 Lowery, *op. cit.* (note 58), p. 1.

62 James Prescott Joule, 'On the production of heat by voltaic electricity', 13 October 1840. Manuscript AP/23/34, Royal Society Library, London. I am very grateful to Dr Ellen Embleton, Picture Curator of the Royal Society, who found the manuscript, which had been wrongly catalogued.

63 From this point onwards, this is referred to as 'Joule's 1840 paper', or 'Joule's 1840 manuscript'.

64 Joule, *op. cit.* (note 5).

65 *Athenaeum* **697**, 192 (6 March 1841); James Prescott Joule, 'On the production of heat by voltaic electricity', *Lond. Edinb. Dubl. Phil. Mag. J. Sci.* **18**(117), 308–309 (April 1841).

> The inquiries of the author are directed to the investigation of the cause of the different degrees of facility with which various kinds of metal, of different sizes, are heated by the passage of voltaic electricity. The apparatus he employed for this purpose consisted of a coil of the wire, which was to be subjected to trial, placed in a jar of water, of which the change of temperature was measured by a very sensible thermometer immersed in it; and of a galvanometer, to indicate the quantity of electricity sent through the wire, which was estimated by the quantity of water decomposed by that electricity. The conclusion he draws from the results of his experiments is, that the calorific effects of equal quantities of transmitted electricity are proportional to the resistances opposed to its passage, whatever may be the length, thickness, shape, or kind of metal which closes the circuit: and also that, *cæteris paribus*, these effects are in the duplicate ratio of the quantities of transmitted electricity; and consequently also in the duplicate ratio of the velocity of transmission. He also infers from his researches that the heat produced by the combustion of zinc in oxygen is likewise the consequence of resistance to electric conduction.

However, the full paper was not accepted for publication in the *Philosophical Transactions of the Royal Society*. A few months later, Joule published an article in the *Philosophical Magazine* with a similar title: 'On the heat evolved by metallic conductors of electricity, and in the cells of a battery during electrolysis'.[66]

Osborne Reynolds (1842–1912), Joule's friend and biographer, assumed that the content of the rejected article was equivalent to the paper published in the *Philosophical Magazine*, as did other authors.[67] This was a natural conjecture, since at the beginning of the 1841 paper, we read:

> There are few facts in science more interesting than those which establish a connexion [*sic*] between heat and electricity. Their value, indeed, cannot be estimated rightly, until we obtain a complete knowledge of the grand agents upon which they shed so much light. I have hoped, therefore, that the results of my careful investigations on the heat produced by voltaic action, are of sufficient interest to justify me in laying them before the Royal Society.[68]

The mention of the Royal Society suggests that this article contained the manuscript that was rejected in 1840; however, this was not the case, as remarked for the first time by Sir Arthur Schuster (1851–1934).[69] The author did not state that he consulted the unpublished manuscript, but his comparison between the paper submitted to the Royal Society and Joule's 1841 paper leaves no doubt on this point.

> The Royal Society paper deals with solid conductors, covering only the ground which in the *Philosophical Magazine* appears as 'Chapter I', and is there followed by a second chapter, twice as long, dealing with electrolysis and adding considerably to the range and importance of the results. Even in the first part the two papers are not identical, only a few short paragraphs being unaltered—though it must be admitted that the alterations are not material.[70]

66 Joule, *op. cit.* (note 6). From this point onwards, this is referred to as 'Joule's 1841 paper'.

67 Reynolds, *op. cit.* (note 10), p. 46; Forrester, *op. cit.* (note 59), pp. 285–286, cites parts of Joule's 1841 paper as if they belonged to the 1840 manuscript.

68 Joule, *op. cit.* (note 6), p. 260.

69 Sir Arthur Schuster, *Biographical fragments* (Macmillan, London, 1932), pp. 201–206.

70 *Ibid.*, p. 203.

Schuster was the Secretary of the Royal Society between 1912 and 1919,[71] and it is likely that he used this prerogative to gain access to the archives. If this was the case, it is conceivable that he did not want to describe how he could be acquainted with the unpublished manuscript.

The next section of this paper contains a complete transcription of the 1840 manuscript.[72] Its title is 'On the production of heat by voltaic electricity', exactly as shown in the abstract published in the *Proceedings*. The manuscript contains one unnumbered cover leaf, along with seven numbered leaves, with the text written on only one side of each sheet. It contains 16 numbered paragraphs, while the 1841 *Philosophical Magazine* paper has 75. The only subject of the 1840 manuscript is the production of heat by electrical currents in metallic conductors, whereas the *Philosophical Magazine* version is divided into two chapters: the first, containing 19 paragraphs, deals with heat in metallic conductors, while the second, with 56 paragraphs, describes the heat produced in electrolysis. The unpublished paper corresponds roughly to the first chapter of the 1841 publication, as correctly pointed out by Schuster.[73]

It has been suggested that the numbering of the paragraphs by Joule was inspired by a similar scheme used by Michael Faraday in his *Experimental researches in electricity*;[74] however, several other authors, both before and after Faraday, also used numbered paragraphs. One example is Eugène Péclet's *Traitê élêmentaire de physique*, a book owned by Joule.[75]

Transcription of Joule's 1840 manuscript

[Unnumbered folio, *recto*]

N°. 196.
On the Production of Heat by Voltaic Electricity.
By James Prescott Joule, Esq.
Communicated by D Roget: Sec. R. S. &c
Received Oct. 16: 1840
address "New Bailey Street, Salford, Manchester"

[Unnumbered folio, *verso*]

Joule on heat from electricity
read Dec. 17: 1840

[f. 1r]

1. Having lately investigated with great care a class of the calorific effects of electricity, and in so doing having ascertained some interesting and, I believe, novel facts belonging to the

71 George Clarke Simpson, 'Sir Arthur Schuster. 1851–1934', *Obit. Notices Fell. R. Soc.* **1**(4), 408–423 (1935), at pp. 419–420.

72 Joule, *op. cit.* (note 62). The publication of this transcription and its figures was authorized by the Royal Society.

73 Schuster, *op. cit*. (note 69), p. 203.

74 See, for instance, James Gerald Crowther, *British scientists of the nineteenth century* (Kegan Paul, Trench, Trübner & Co., London, 1935), p. 167; Crosbie Smith, *The science of energy: a cultural history of energy physics in Victorian Britain* (Athlone Press, London, 1998), p. 58.

75 Jean-Claude-Eugène Péclet, *Traité élémentaire de physique*, 3 vols (Société Typographique Belge, Brussels, 1838). Joule's copy of this edition is extant in the Joule Book Collection (J200055), University of Manchester Library: https://www.librarysearch.manchester.ac.uk/permalink/44MAN_INST/1lr7mpn/alma9921128854401631 (accessed 25 February 2020).

production of heat in the connecting wire of the voltaic battery, I have now the honor to submit the results of my experiments to the consideration of the Royal Society.

2. My first endeavour was to discover the cause of the different degrees of facility with which various sizes and kinds of metal are heated by the passage of an equal quantity of electricity.—It will be proper to describe the method and the instruments used, before proceeding to the relation of experiments.

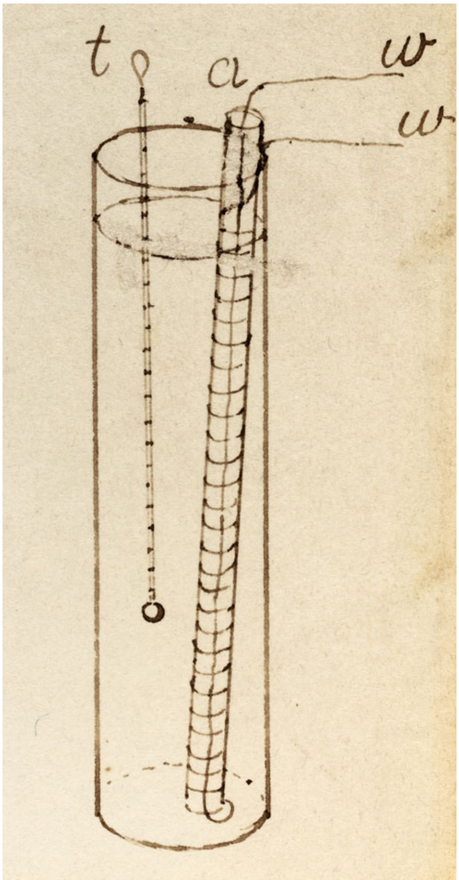

Figure 2. Joule's drawing of the device used to measure the heat disengaged by electricity, f. 1r.

3. The wire, the heating power of which I was anxious to try, was wound regularly and closely round a thin glass tube, and returned by the inside of it. The two extremities of the coil thus formed were then drawn in contrary directions so as to separate the convolutions from each other, and if this could not be well done, a piece of cotton thread was interposed. This apparatus, a, Fig., was placed in a glass jar containing a given quantity of water, and, on passing a current through it by the wires w, w, the rise of the temperature of the water

was indicated by the thermometer, t, graduated on its glass stem,[76] [f. 2r] and by means of which may be estimated the tenth part of ~~Fahrenheit's~~ [deleted] degrees on Fah[ts] scale.

4. The battery with which I worked consisted of two of Wollaston's arrangement, each of which consisted of 10 four inch pairs of sheet iron and amalgamated zinc, charged with a dilute solution of sulphuric acid; by its means I found that a powerful current could be readily commanded, and the only precaution necessary[77] to ensure constancy, is to supply more acid as the old is exhausted.
5. A galvanometer was also provided which consisted of a thick copper wire bent into the form of a rectangle measuring one foot by 6 inches, and going only once round a needle of nearly 4 inches long. It was sufficiently delicate to indicate a deflection of half a degree, and was of course placed at a sufficient distance from the iron battery.
6. I have expressed my quantities of electricity on the basis of Faraday's great discovery of definite electrolyzation.[78] I have ventured to call that quantity of current electricity which is able in one hour to decompose a chemical equivalent expressed in grains, a degree. By a series of [f. 3r] experiments, the deviation of the needle of my galvanometer was found to be 33½° of the graduated card, when a current was passing of sufficient quantity to decompose 9 grains of water per hour; that deviation indicated therefore, one degree on the scale that I propose to adopt. The slight aberrations of the real quantities of electricity from the ratio of the tangents of deviation, are corrected by a rigid experimental process.
7. Exp 1. I took two copper wires, each 2 yards long; one of them 1/28 of an inch thick, the other, 1/50 of an inch; and arranged them into coils in the manner that I have described (3): each of these was immersed in a glass jar containing 9 oz. avoirdupois of rain water. A current of the mean quantity 1°.1 (6) was then passed consecutively through both apparatus, [*sic*] and at the close of one hour I observed that the water in which the thin wire was immersed had gained 3°.4. of heat, whilst the thick wire had produced only 1°.3.
8. Now by direct experiment I found that 3 feet of the thin wire could conduct* [79] exactly as well as 8 feet of the thick, and hence it is plain that the resistances were in [f. 4r] the ratio of 3.4 to 1.27, which is very close to the ratio of heating power produced by similar specimens (7).[80]
9. Exp. 2. I now substituted a piece of iron wire, 1/27 of an inch thick, and 2 yards long, for the thick copper wire used in Exp. 1, and placed each apparatus in half a pound of rain

76 On this page of the manuscript, we find several marks in pencil, which were most likely not made by Joule. Circles were drawn around 'a, Fig.', 'w, w', and 't', probably by someone who was reading the manuscript and trying to understand the description of the figure. There is also a pencilled circle around 'the', in the sentence containing 'Indicated by the thermometer', with 'a' written above 'the'. This correction seems to have been made by someone who was editing the manuscript.

77 'Necessary' was written as an interpolation, above the line but in the same hand.

78 The word 'electrolyzation' was introduced by Faraday in 1834. Michael Faraday, 'Experimental researches in electricity: seventh series', *Phil. Trans. R. Soc. Lond.* **124**(1), 77–122 (1834), p. 80. Faraday usually consulted William Whewell when choosing new scientific terms, and did so in the case of 'electrolyzation'. Sidney Ross, 'Faraday consults the scholars: the origins of the terms of electrochemistry', *Notes Rec. R. Soc. Lond.* **16**(2), 187–220 (1961), p. 199. See also Ralph E. Oesper and Max Speter, 'The Faraday–Whewell correspondence concerning electro-chemical terms', *Sci. Monthly* **45**(6), 535–546 (1937); Allie Wilson Richeson, 'On Faraday's terminology in electrolysis', *Isis* **36**(3/4), 160–162 (1946).

79 * [Joule's footnote:] 'The relative conducting powers of my specimens of metal of equal length and weight were Mercury, 1; Iron, 11.6; Copper, 36.6.'

80 There is a calculation at the foot of f. 3r, in a different hand. This computation contains the following steps: 3°4×3=10°2; and 10°2 / 8=1°275.

water. It was found that after a current of 1°.25 had passed through both during one hour, the rise of temperature caused by the iron was 6°, while that produced by the copper was 5°.5: in this case the resistances of the iron and copper were found to be[81] in the ratio of 6, to 5.51.

10. Exp. 3. A coil of copper wire was then compared with one of mercury; which was accomplished by enclosing the later in a bent glass tube. In this way I had immersed, each in half a pound of rain water, 11¼ feet of copper wire of .02 of an inch in thickness, and 22¾ inches of mercury of .065 of an inch in diameter. At the close of one hour, during which the mean quantity of current electricity was 0°.72, the former had caused a rise of temperature of 4°.4, the latter of 2°.9, whilst their total resistances were found by a careful experiment to be in the ratio of 4.4 to 3.
11. Other trials were made with precisely the same satisfactory results, but I think that those which I have given are sufficient to prove that the heating power of voltaic [f. 5r] electricity (at least with metals) is the consequence of resistance to conduction, and that when the same quantity of electricity is passed, the quantity of heat produced is always proportional to the resistance encountered, whatever may be the length, thickness, shape, or kind of metal which closes the circuit.—It is well known that heat is more readily produced in bad, than in good conductors, but the exact law has not, I think, been hitherto proved.
12. On considering the above ~~theory~~ conclusion,[82] I thought that if it were correct, the effect produced by the increase of passing electricity would be as the square of that element; for it is obvious that in that case the resistance would arise from a double source, namely, the increase of quantity, and the increase of velocity. We shall see that this view is actually borne out by experiment.
13. I took the coil of copper wire described under Exp. 3, and by a series of experiments, I have determined the number of degrees by which half a pound of rain water was heated in a given time, by different quantities of electricity passing through the wire. My results are arranged in the following table, which I think sufficiently explains itself. [f. 6r]

Mean deviations of the Galvanometer (5).	Quantities of electricity expressed by degrees (6).	Heat produced in ½ an hour by the above °s.	Ratio of the Squares of the degrees in column 2.	Heat produced in 1 hour by °s in column 2.	Ratio of the squares of the °s in column 2.
16°	0°.43			1°.2	.995
31½°	0°.92	3°	2.9	4°.7	4.55
55°	2°.35	19°.4	18.8		
57⅔°	2°.61	23°	23.2		
58½°	2°.73	25°	25.4	39°.6	40.

Figure 3. Joule's table showing the results of his fourth experiment, f. 6r.

81 'To be' is interpolated and written above the line, in the same hand.
82 'Theory' was crossed out, and 'conclusion' was written above it in pencil, in a different hand.

14. Previous to each of these experiments, the necessary precaution was taken of bringing the water, and the air of the room, to the same temperature. The propriety of this care is sufficiently obvious from the circumstance that the heat produced during the last half hour of an experiment, is not nearly so great as that produced in the first half hour, which shows the great cooling power of the atmosphere. It is probably the difficulty of keeping the air of the room in the same state of heat and quiet during the experiments, that produces slight differences on their comparison.—That no appreciable current of electricity was conducted by the water was ascertained by experiment, nor could any evidence of such a current be discovered either by oxidation, or the evolution of hydrogen.
15. We find then, that the heats generated in the same wire are as the squares of the electrical velocities, and as this [f. 7r] effect has been shown to be the necessary consequence of resistance ~~to conduction~~ (12), it is evident that in every case, whatever may be the length, thickness, shape, or kind of metal, and whatever the quantity or source of electricity, the heat produced is proportional to, and the product of, the resistance to conduction.
16. I may just observe in conclusion, that De la Rive has used a very ingenious application of the metallic thermometer of Breguet in some of his researches, and that others have been led into error by supposing that the heats are simply proportional to the currents. If such instruments are employed, (though they are certainly far inferior in delicacy to the common galvanometer,); it is obvious that the square roots of their indications are alone proportional to the electricities which they are intended to measure.

Broom Hill, near Manchester,
October 13th. 1840.
[f. 7v]

M. Joule on heat from Electricity
Archives
11 March 1841
S.H.C.

Joule's 1840 and 1841 papers: a comparison

A comparison between the unpublished manuscript and the first chapter of the printed version allows us to glimpse the process used by the young Joule to improve his research report. Joule was 21 when he wrote the original version of the paper, and this was his first attempt to produce something we would call a 'scientific' article, unlike the 'technological' ones he had already published. He was learning how to do science, and unsurprisingly made several mistakes.[83] The aim of pointing out the shortcomings of his work is not to criticize Joule, but to understand how difficult it is to produce any new amount of experimental work. Of course, this case of microscopic history allows us to perceive that scientists are not angels or superheroes, in contrast to the hagiographical accounts that are fortunately becoming rare in the academic literature. Donald Cardwell, who wrote an extensive biography of Joule, was influenced by the old approach and went as far as to assert:

83 A detailed analysis of Joule's 1841 paper and a comparison with similar investigations by contemporaneous researchers allows us to draw this conclusion while avoiding an anachronistic assessment. See Martins and Silva, *op. cit.* (note 7).

> Another conspicuous feature of his scientific personality was the immense skill and precision that marked his experiments from the very beginning. How did he acquire these gifts, characteristic of the thorough professional? Unfortunately we know little of his early tutors; they are shadowy figures. And Dalton was never renowned for experimental accuracy. For the rest we may suppose that experience at the brewery would have familiarized him with temperature measurements and the working of the steam engine. But in the final analysis I must conclude that these particular gifts were bestowed by Providence and not by his fellow men.[84]

Neither Joule nor any other person was born an accomplished scientist endowed with immense skill and precision. In the same way as any human being, he had to learn from other people and from his own trials, successes and failures. Let us see what happened in this particular case.

Some obvious differences between the 1840 manuscript and the first part of the 1841 paper can be noticed by comparing their division into paragraphs: §1–§2 of the manuscript roughly correspond to §1–§2 of the 1841 paper, but §3 of the manuscript, which contains a description of the apparatus Joule used, was expanded and corresponds to §6–§7 of the 1841 published paper. The paragraph of the manuscript describing the battery he used (§4) was not retained in the printed version. The account of the galvanometer (§5) was expanded (§3–§4 of the 1841 paper), and a sketch of the instrument was added. The sixth paragraph of the manuscript roughly corresponds to §5 of the 1841 paper. The precaution of using water at the same initial temperature as the air of the room (§14 of the manuscript) was transposed to §8 of the 1841 paper. The descriptions of the experiments and their conclusion (§7–§16 of the manuscript) have close counterparts in the printed paper (§9–§18), although the latter contains an additional paragraph (§19) comparing voltaic and friction electricity, a subject that was not dealt with in the 1840 manuscript.

The use of footnotes is also quite different in the two versions. The 1840 manuscript contains a single footnote in §8, in which Joule reported the values he obtained for the conducting power of mercury, iron and copper. This footnote was not retained in the printed paper. However, the first part of the 1841 paper contains several bibliographic footnotes. The first provides information about Joule's former papers dealing with his galvanometer, while the others supply references to works published by Auguste de la Rive, Jean Charles Peltier and Sir William Snow Harris, and cite the names of Charles Brooke and John Cuthbertson. Note that the *Philosophical Magazine* article was Joule's first publication containing bibliographic references. The Royal Society manuscript contains no bibliographic references whatsoever, although it does cite the names of Faraday, de la Rive and Louis-François-Clément Breguet (the last of whom does not appear in the printed version).[85]

The changes made to Joule's paper over a few months can be ascribed to two possible causes: Joule himself may have become aware of a need to change and improve the manuscript, and/or someone criticized the first version and suggested changes. Both of these were very likely to have occurred. During the period 1840–1841, Joule interacted

84 Cardwell, *op. cit.* (note 9), p. 64.

85 Both the motivation for studying the heat generated by electric currents and the bibliographic references published by Joule may have originated in Faraday's discussion on this subject, in Series XIII of his *Experimental researches in electricity*, originally published in 1838: Michael Faraday, *Experimental researches in electricity*, vol. 1 (Bernard Quaritch, London, 1839), §§ 1625–1626, pp. 517–518.

with several engineers and scholars in Manchester, and especially those related to the Lit. & Phil., and may have discussed his research with some of them. It is also possible that he received some guidance from Roget, the First Secretary of the Royal Society. However, any attempt at reconstructing these events is conjectural, given the lack of documents concerning these hypothetical exchanges.

It is quite understandable that, in the 1841 paper, Joule would add some relevant information that was lacking in the 1840 manuscript, but why would he omit from the printed paper any information that had appeared in the original manuscript? It is necessary to understand the additions, omissions and changes he made in order to get a clearer picture of his attempt to improve the paper. It is unfeasible to discuss every modification, and only a few of the most relevant will be analysed in the following.

Aim and scope of the papers

The 1840 manuscript accepted as well known the fact that electric currents produce heat in metallic conductors, and tried to find out how this heat depended on the properties of the wires and other factors. It seems that Joule was unaware of previous research on the subject, since he stated that he had 'ascertained some interesting and, I believe, novel facts on the production of heat in the connecting wire of the voltaic battery' (1840, §1).[86] After finding out that heat generation is proportional to the resistance of the wire, he commented: 'It is well known that heat is more readily produced in bad, than in good conductors, but the exact law has not, I think, been hitherto proved' (1840, §11).

The 1841 paper, on the other hand, acknowledges some of the research that had been done previously (1841, §2):

> It is well known that the facility with which a metallic wire is heated by the voltaic current is in inverse proportion to its conducting power, and it is generally believed that this proportion is exact; nevertheless I wished to ascertain the fact for my own satisfaction, and especially as it was of the utmost importance to know whether resistance to conduction is the sole cause of the heating effects. The detail, therefore, of some experiments confirmatory of the law, in addition to those already recorded in the pages of science, will not, I hope, be deemed superfluous.

In the published version, after confirming that the heat produced is proportional to the electrical resistance of the wire, Joule commented in a footnote: 'Mr Harris, and others, have proved this law very satisfactorily, using common electricity' (1841, footnote to §13), and after showing that this effect is proportional to the square of the electric current, he remarked that de la Rive and Peltier had already studied the effect, and that '[t]he experiments of Brooke, Cuthbertson and others, prove that the quantity of wire melted by common electricity is as the square of that battery's charge' (1841, footnotes to §17, §19). Hence, a few months after he wrote the 1840 manuscript, Joule knew that his results were not altogether original, and the central justification for his research therefore changed.

86 To avoid the need for a large number of footnotes, the paragraphs of these two papers will be identified simply as (1840, §*x*) and (1841, §*y*).

Instruments

The main instruments used by Joule were a galvanometer (to measure electric currents), a voltaic battery (to produce the current), a thermometer (to measure temperature changes) and a glass vessel containing water and the conductor, which was heated by the electric current. He also measured the lengths of the wires, the amount of water in the vessel and so on, although the more common instruments used to do this were not described in his papers.

The Greater Manchester Museum of Science and Industry keeps an instrument that may be Joule's original 1840 calorimeter.[87] A photograph of the glass cylinder, accompanied by the thin glass tube with coiled wire around it, is very similar to Joule's drawing. Even the number of revolutions of the metallic wire (26 or 27 turns) is, curiously enough, pretty similar—a circumstance that could not be anticipated, because a simple sketch such as the one presented by Joule was not required to reproduce the exact number of turns.

The unpublished manuscript contains a succinct description of the galvanometer (1840, §5), while the published paper has a more detailed account of the instrument (1841, §3–§4), a change that may have been prompted by someone who read and criticized the 1840 manuscript. The published account included a sketch of the instrument and some additional details, such as the method used to correct the measurements, which Joule had discussed in previous papers. It is curious, however, that he omitted the following sentence: 'It was sufficiently delicate to indicate a deflection of half a degree, and was of course placed at a sufficient distance from the iron battery' (1840, §5). A small magnetic compass would scarcely be able to provide such precise measurements, and the description of the battery was omitted altogether in the published version.

For his experiments, Joule needed a voltaic battery that could provide a strong and constant electric current. He used a battery of the Wollaston type (1840, §4), which did supply a powerful current. However, this was not as stable as other available types such as, for example, those of Daniell and Grove.[88] This was certainly a deficiency of Joule's experimental set-up, because the relation between heat production and electric current could only be ascertained if the current had a well-defined and constant intensity throughout each experiment. In the unpublished version, he remarked that 'the only precaution necessary to ensure constancy, is to supply more acid as the old is exhausted', but this crude method would not ensure a constant current over the long periods of the experiments (one hour). The 1841 paper makes no mention of the battery used, thereby concealing an experimental flaw. He did not repeat the experiments with another battery, since the experimental data are identical in the 1840 manuscript and in the published version.

The single sentence describing the thermometer in the first version (1840, §3) was expanded to a full paragraph (1841, §6):

87 Stella V. F. Butler, 'The universal agent of power. James Prescott Joule, electricity, and the equivalent of heat', in *Electrochemistry, past and present* (ed. John Thomas Stock and Mary Virginia Orna), pp. 50–62 (American Chemical Society, Washington, DC, 1989), pp. 53–54. The device was not described in Harry Lowery, 'The Joule collection in the College of Technology, Manchester. Part I. A. Instruments', *J. Sci. Instrum.* **7**(12), 369–378 (1930).

88 Everyone who dealt with galvanism and electrical instruments at the time was conscious of this problem. Joule was certainly aware of this, since several papers in the *Annals of Electricity*, where he published his early papers, commented on this issue and proposed new types of battery that produced steady currents. See, for instance, Edward S. Clarke, 'On an improved form of the voltaic sustaining battery, and on the size proper to be given to the zinc element of sustaining batteries in general', *Ann. Electr. Magn. Chem. Guard. Exp. Sci.* **3**, 85–92 (1838).

> 6 . The thermometer which I used had its scale graduated on the glass stem. The divisions were wide, and accurate. In taking temperatures with it, I stir the liquid gently with a feather; and then, suspending the thermometer by the top of its stem, so as to cause it to assume a vertical position, I bring my eye to a level with the top of the mercury. In this way a little practice has enabled me to estimate temperatures to the tenth part of Fahrenheit's degree with certainty.

These details were probably added as a response to criticism of his measurements of temperature.

Experimental procedure

The description of the calorimeter is very similar in the two versions. However, after describing the arrangement of the wire, Joule added two sentences to the published paper (1841, §7): 'When the voltaic electricity is transmitted through the wire, no appreciable quantity passes from it to take the shorter course through the water. No trace of such a current could be detected, either by the evolution of hydrogen, or the oxidation of metal.' The experiment was devised to measure the heat produced by the electric current in the wire, and it was therefore necessary that all of the current should pass through the metallic wire. If part of the current was conveyed by the water, no conclusions could be drawn. A similar sentence appears in the 1840 manuscript (1841, §14), but this seems to apply only to the last series of experiments. It is plausible that Joule changed this remark as a response to criticism.

Of course, pure water has a very low electrical conductivity. In the unpublished paper, Joule stated that he had used *rain water* in his experiments (1840, §7, §9, §10). However, in the 1841 paper, he did not state that he had used rain water in those experiments, although the descriptions are almost identical (1841, §9, §11, §12). Why did he omit this detail?

Rain water can be fairly pure, if it is collected in a way that avoids contamination and if the atmosphere is clean. By 1840, Manchester was a large industrial city with 250,000 inhabitants, and the coal smoke was giving rise to groups campaigning for its reduction.[89] The coal, which was burned to power the machines, contained sulphur and, as a result, the atmosphere around the city contained large amounts of carbon dioxide and sulphur oxides, producing sulphuric acid in the rain water. For this reason, the electrical conductivity of Manchester rain water may have been very high. John Dalton had detected the presence of sulphuric acid in the rain water of Manchester two decades earlier.[90] Some years after this, Robert Angus Smith studied the phenomenon, and later coined the expression 'acid rain' to describe it.[91]

89 Harold L. Platt, 'The invisible evil', in *Smoke and mirrors: the politics and culture of air pollution* (ed. Erna Melanie DuPuis), pp. 27–76 (New York University Press, 2004), p. 30.

90 In 1824, Dalton published observations of the chemical composition of rain water collected in Manchester that exhibited the presence of sulphuric acid: John Dalton, 'Appendix to the essay on salt rain, with additional observations on the succeeding storms of wind and rain', *Mems Lit. Phil. Soc. Manch.* **4**, 363–372 (1824), pp. 367 and 369–370. He was aware that coal burning was the main cause of the chemicals found in rain: 'in ordinary circumstances the rain is destitute of salt, except we may find an infinitely small quantity from the effluvia of burning fuel, which in a large town may possibly become sensible in the rain falling through the atmosphere, and carrying along with it particles of muriate or sulphate of ammonia': *ibid.*, p. 368.

91 'All the rain was found to contain sulphuric acid in proportion as it approached the town, and with the increase of acid the increase also of organic matter.' Robert Angus Smith, 'On the air and rain of Manchester', *Mems Lit. Phil. Soc. Manch.* (ser. 2), **10**, 207–218 (1852), p. 212. In some places, he obtained more than one grain of sulphuric acid per gallon of rain water (*ibid.*, p. 217). In

At the outset, Joule was probably unaware of the unsuitability of the rain water collected in Manchester when he carried out his experiments, and hence mentioned in the 1840 manuscript that he had used it, as if this would guarantee that the water had low conductivity. It is conceivable that he was later informed about the inadequacy of rain water, and thus omitted from the 1841 paper the description of the kind of water he had used.

Another relevant experimental procedure involved starting the experiments when the temperatures of the water and of the room atmosphere were the same (1840, §14). In the unpublished manuscript, this precaution was described only after the last series of experiments, motivated by the observation that the rise of temperature in the water in the second half hour of the experiments was smaller than in the first half hour. This was only observed in the last series of experiments, because in experiments 1 to 3 he measured only the total rise in temperature after one hour. Since his experiments attempted to measure the heating effects of electric currents, this significant source of heat loss should be a concern. He admitted that the cooling effect was substantial, but did not attempt to compute it (to correct the raw data) or to reduce it (by a change in the experimental set-up).

In the published version, the description of this precaution concerning the initial water temperature was moved to another position in the paper, prior to the description of Experiment 1 (1841, §8), thus implying that all experiments followed the same procedure. In addition to the change in position, the justification given for this precaution is completely different:

> 8. Previous to each of the experiments, the necessary precaution was taken of bringing the water in the glass jar, and the air of the room to the same temperature. When this is accurately done, the results of the experiments bear the same proportions to one another as if no extraneous cooling agents, such as radiation, were present; for their effects in a given time are proportional to the difference of the temperatures of the cooling and cooled bodies; and hence, although towards the conclusion of some experiments this cooling effect is very considerable, the absolute quantities alone of heat are affected, not the proportions that are generated in the same time.

This argument, introduced in the 1841 paper, is physically correct.[92] No insulation or correction was necessary in the specific case of his experiments, which only needed to ascertain the *ratios* of the heat generated in different wires. However, this reasoning is not easily appreciated, and some authors of the period criticized Joule because they did not understand it. A theoretical justification of the procedure was beyond Joule's ability, since he had had a fairly limited mathematical training. It is conceivable, of course, that Joule could have arrived at the above conclusion by an intuitive semi-quantitative reasoning, in the same way as Faraday had done. It seems more likely, however, that someone else with greater mathematical and physical knowledge advised him on the suitability of this procedure.

his book published 20 years later, he stated 'When the air has so much acid that two to three grains are found in a gallon of the rain-water, or forty parts in a million, there is no hope for vegetation in a climate such as we have in the northern parts of the country.' Robert Angus Smith, *Air and rain: the beginnings of a chemical climatology* (Longmans, Green, & Co., London, 1872), p. 246. Concerning his work, see Eville Gorham, 'Robert Angus Smith, FRS, and chemical climatology', *Notes Rec. R. Soc. Lond.* **36**(2), 267–272 (1982).

92 See Martins and Silva, *op. cit.* (note 7), sections 4.2 and 7.

Experimental data

The measurements published by Joule in 1841 are the same as those found in the 1840 manuscript. However, he omitted from the printed version some of the data that can be found in the unpublished manuscript. One of these exclusions was the entire content of a footnote in which Joule stated: 'The relative conducting powers of my specimens of metal of equal length and weight were Mercury, 1; Iron, 11.6; Copper, 36.6' (1840, footnote to §8). Rather than 'weight', Joule probably intended to refer to the volume or cross-section; this mistake is understandable since, when comparing the resistance of metallic wires, some authors stated that conductivity was proportional to mass or weight, although this only applied when the same metal was used to produce wires of different diameters.[93]

At first sight, the information provided in the footnote was relevant and should have been retained in the 1841 paper. The omission suggests that this was another vulnerable point that was concealed. Let us try to understand this.

The electrical resistance R of a wire is inversely proportional to its conductivity C. It was also well known at the time that the electrical resistance R is directly proportional to the length L of the wire, and inversely proportional to its section (or to the square of its diameter D).[94]

$$\frac{R_1}{R_2} = \frac{C_2}{C_1} \cdot \frac{L_1}{L_2} \cdot \left(\frac{D_2}{D_1}\right)^2.$$

Given the relative electric conductivities of several metals, it is possible to compute the relative electrical resistances of wires of those metals with known lengths and diameters, and vice versa. Hence, we can use the conductivities given by Joule to compute the resistances of the wires he used.

In experiment 2 (1840, §9; cf. 1841, §11), Joule used a copper wire and an iron wire, both of which were two yards long. The diameter of the copper wire was 1/50 of an inch, and that of the iron wire was 1/27 of an inch. Their relative electrical resistances should therefore be:

$$\frac{R_i}{R_c} = \frac{36.6}{11.6} \cdot \left(\frac{1/50}{1/27}\right)^2 = 0.920.$$

Joule stated that 'the resistances of the iron and copper [wires] was [*sic*] found to be in the ratio of 6 to 5.51' (1840, §9; 1841, §11). Notice that 5.51/6=0.918, very close to the ratio obtained above. However, according to our computation, the resistance of the iron wire should be *lower* than that of the copper wire—the opposite of Joule's statement. The data

93 For instance: 'Davy found that when different portions of the same wire, plunged in a non-conducting fluid, were connected with different parts of the same battery equally charged, their conducting powers appeared in the inverse ratio of their lengths and that the conducting power of a wire for electricity was nearly as the mass, that is, when the wire was of a given length'. Samuel Hunter Christie, 'The Bakerian Lecture: experimental determination of the laws of magneto-electric induction in different masses of the same metal, and of its intensity in different metals', *Phil. Trans. R. Soc. Lond.* **123**, 95–142 (1833), p. 130.

94 In 1840, this was a well-known property. The most careful confirmation of the law was published by Lenz in 1835, and an English translation appeared two years later: Heinrich Friedrich Emil Lenz, 'On the laws of the conducting power of wires of different lengths and diameters for electricity', in *Scientific memoirs, selected from the transactions of foreign academies of science and learned societies, and from foreign journals* (ed. Richard Taylor), vol. 1, pp. 311–324 (Richard & John E. Taylor, London, 1837). Even if Joule had not known it before, he was sure to learn it from accounts in Sturgeon's journal, where he published his early papers. See, for instance, Carl August von Steinheil, 'Upon telegraphic communication, especially by means of galvanism', *Ann. Electr. Magn. Chem. Guard. Exp. Sci.* **3**(18), 439–452, 509–520 (1839), p. 509.

Table 1. Analysis of Joule's data from his fourth experiment.

Column 1 Electric current (i) in Joule's units	Column 2 Temperature increase (ΔT) in one hour (°F)	Column 3 Square of the electric current (i^2) in column 1	Column 4 Column 2 divided by column 3 ($\Delta T/i^2$)
0.43 Q	1.2	0.185	6.49
0.72 Q*	4.4	0.518	8.49
0.92 Q	4.7	0.846	5.55
2.73 Q	39.6	7.45	5.31

*This row contains information from his third experiment.

provided in the footnote therefore contradict the data described by Joule in his second experiment.

Observe that only by accepting Joule's values does the experiment seem to provide confirmation that the heat produced by an electric current is proportional to the resistance of the wire: 'the rise of temperature caused by the iron was 6°, while that produced by the copper was 5°.5: in this case the resistances of the iron and copper [wires] were found to be in the ratio of 6, to 5.51' (1840, §9; cf. 1841, §11). In the published version of the paper, Joule omitted this footnote, concealing this inconsistency.

Another omission can be seen in Joule's description of his third experiment (1840, §10; 1841, §12). For the first and second experiments, both the unpublished manuscript and the printed version state the value of the electric current, in his units, as 1.1 Q and 1.25 Q, respectively, while for the third experiment the manuscript states that the current was 0.72 Q and the printed version does not contain this information.

Now, in the three first experiments, Joule was comparing the heat generated in two different metallic conductors connected in series, and therefore traversed by the same current. The intensity of the current was irrelevant, and he did not need to mention it. However, he did provide this information, and retained it for experiments 1 and 2 while omitting it from the 1841 paper in the case of the third experiment. Why did he do this?

It is necessary to analyse the next set of experiments to find the answer. This unnumbered set, referred to here as the fourth experiment, measured the rise in temperature produced by different electric currents in a given copper wire (1840, §13; 1841, §15). Joule's aim was to check whether the production of heat was proportional to the square of the electric current, and the measurement of their intensities was therefore crucial. In the fourth experiment, Joule employed the same copper wire that had been used in experiment 3, and the amount of water was also the same. He could have made use of the result obtained in experiment 3, that is, an increase in the temperature of the water of 4.4°F after one hour, with a current of 0.72 Q. However, this information is missing from the table in which Joule presents the results of his fourth experiment; it contains only the changes in temperature after one hour for currents of 0.43, 0.92 and 2.73.

To enable comparison, columns 1 and 2 of table 1 reproduce Joule's relevant measurements from the fourth experiment, and the result of the third experiment is added. The rise in temperature ΔT was expected to be proportional to the square of the current intensity I, and if this were the case, then $\Delta T/i^2$ would be constant. The fourth column of table 1 clearly shows that this was not the case. However, if the rise in temperature corresponding to $i = 0.43$ Q is taken as 1 (instead of 1.2), the corresponding value in column 4 changes to 5.40, giving good agreement with the results for $i = 0.92$ and $i = 2.73$, although no trick can be used to fit the results of the third experiment to the others. To obtain confirmation of the proportionality between heat and the square of the current, it is necessary to get rid of the inconvenient measurement from the third experiment. It may have been for this reason that Joule omitted from the 1841 paper the information on the electric current used in experiment 3.

Reasons for discarding Joule's 1840 paper

In 1840 Joule was an unknown and inexperienced young man, trying to report his first scientific findings. Had he died soon afterwards, who would have worried about the rejection of his 1840 paper by the Royal Society? However, ten years later, he was a well-known scientist and had been elected a member of the Royal Society, among other tokens of recognition. In view of his subsequently successful career, several authors have discussed the motivation behind the negative response to his 1840 paper.

In the opinion of Osborne Reynolds, this rejection was unjust:

> This paper, from whatever point it is now viewed, shows throughout the hand of a master; but the matter it contained was so far in advance of the knowledge of the time, that the Royal Society declined to publish it in full in the *Philosophical Transactions*—a circumstance which not only takes a principal place in Joule's career, but has also secured a place in the history of the Royal Society.[95]

Reynolds' claim does not hold. To begin with, he never saw the 1840 manuscript and assumed that its content was the same as that of the 1841 paper. However, even if the rejected paper had been the one published in the *Philosophical Magazine*, it could not be alleged that it 'shows throughout the hand of a master', since it had many shortcomings and was criticized and superseded by other works that were published shortly afterwards. One also cannot maintain that 'the matter it contained was … far in advance of the knowledge of the time', because its subject, technique and most results were not new.

Leon Rosenfeld assumed that Joule had from the very start accepted the idea of the equivalence and transformation of all forms of energy, and that this concept had guided his research. He even ascribed to this theoretical background the rejection of Joule's 1840 paper by the Royal Society:

> The attention that he paid to the relations of equivalence between heat and other physical phenomena was an entirely novel outlook at the time. … Joule's way of looking at it was so strange, so novel, that it was not immediately understood, even by the most prominent scientists of the time. This is convincingly demonstrated by the fact that the first paper submitted by Joule to the Royal Society was not judged of sufficient interest to be published in full.[96]

However, Joule did not refer to *equivalence* between electricity and heat, either in the 1840 manuscript or in the 1841 paper. When interpreting Joule's early writings, it is necessary to keep in mind the difference between the existence of *proportionality* between two magnitudes, and the possibility of *conversion* of one into the other. For instance, the

95 Reynolds, *op. cit.* (note 10), p. 46.

96 Leon Rosenfeld, 'Joule's scientific outlook', *Bull. Brit. Soc. Hist. Sci.* **1**(7), 169–176 (1952), p. 170.

pressure of a gas is proportional to its temperature, but pressure cannot be converted into temperature, or vice versa. In 1840 (and even before then), Joule searched for quantitative relations between physical magnitudes, but this does not imply that he was looking for transformation or equivalence between different 'forces'.

Rosenfeld also approvingly reproduced another explanation for the rejection of Joule's paper:

> It is said that Joule, when asked many years later what he thought of this rejection of his first paper by the Royal Society, answered: "I was not surprised. I could imagine those gentlemen in London sitting round a table and saying to each other, 'what good could come out of a town where they dine in the middle of the day?'" This remark shows, at any rate, that Joule was keenly aware of the real motive underlying this lack of understanding between the gentlemen in London and the lonely young man in Manchester.[97]

Rosenfeld did not provide a reference for this anecdote, but the source is known to be Arthur Schuster's *Biographical fragments*. Schuster was acquainted with Joule, and wrote: 'I once asked Joule what he felt like when he heard that one of his papers was rejected by the Royal Society', and described the response reproduced by Rosenfeld.[98]

There is no documentary evidence, however, that the rejection was motivated by prejudice by the members of the Royal Society against every piece of research coming from Manchester. Let us remember that the famous Manchester chemist John Dalton (1766–1844) had been elected Fellow of the Royal Society on 7 March 1822, and was awarded the first Royal Medal in 1826.[99] Besides this, a simple search of the Royal Society catalogue shows that 20 papers were published in the *Philosophical Transactions* by authors from Manchester between 1800 and 1840. Manchester and its scientific activities were not unknown or despised by the Royal Society.

Although he recorded Joule's comment, Schuster did not agree with Joule's mordant interpretation, and instead attempted to understand the event in a careful and neutral way.

> The paper was read on the 17th December, and in due course a short abstract appeared in the *Proceedings*, which gave the final result arrived at, and hence secured Joule's priority. It is, therefore, not quite correct to say that the paper was rejected. The difficulty arose in connexion [*sic*] with its publication *in extenso*. The paper was short—it would not have taken up more than four or five pages in the *Proceedings*—and it was perhaps considered that such far-reaching results could not be proved by the comparatively few experiments conducted by Joule. Criticisms were also made on the ground that previous investigations on the same subject were not mentioned. …
>
> It is not my desire to acquit the Royal Society of all blame, but mitigating circumstances might be urged. Joule's experiments no doubt appear conclusive to us, but the very simplicity of his experimental arrangements, and the comparatively few numerical results given, may have raised doubts which were perhaps excusable. The heat generated was determined by the rise of temperature of a measured quantity of water in which a coil of uncovered wire was inserted, and no cooling correction was applied. Though Joule gave good reasons why these simplifications did not affect the result, such

97 *Ibid.*, p. 170.

98 Schuster, *op. cit.* (note 69), p. 201.

99 Harold Brewer Hartley, 'John Dalton, F. R. S. (1766–1844) and the atomic theory: a lecture to commemorate his bicentenary', *Proc. R. Soc. Lond. A*, **300**(1462), 291–315 (1967), p. 291.

> cavalier treatment of the minor sources of error may have shocked the academically-trained mind, as showing want of respect for the dignity of the problem.[100]

According to Schuster's analysis, the paper was simply not good enough to deserve publication. All his remarks are well founded, although one of his comments is perplexing, since he states as a fact (not as conjecture) that 'criticisms were also made on the ground that previous investigations on the same subject were not mentioned'. How could he have known that these criticisms had been made? Did he have access to a report on Joule's manuscript, or to some letter sent to him? It is impossible to ascertain the basis of Schuster's statement.

The next section of this article analyses the extant evidence concerning the analysis and rejection of Joule's paper by the Royal Society.

The Royal Society and Joule's 1840 paper

Joule's manuscript was received by the Royal Society on 16 October 1840. This took place during the long vacation period, when there were no meetings at the Society, and works that had been submitted were accumulating. The next meeting was held on 19 November; four weeks later, Peter Roget read Joule's article to the Society.

According to the rules of the Royal Society, only Fellows had the right to read their own works at meetings, although they could also read papers written by someone else which, in their opinion, deserved to be presented to the Society. However, those who did not belong to the Society and who did not have a sponsor had no guarantee that their works would be communicated at a meeting. The initial screening of the manuscripts sent by 'outsiders' was made by the President and the Secretaries of the Royal Society,[101] and a paper was 'communicated' if and only if it was read at a meeting. In the specific case of Joule's paper, preliminary acceptance for communication was given by Peter Roget, one of the two Secretaries of the Society.

The rules of the Royal Society that were in force at that time stated that each weekly meeting should last for one hour, and discussions were forbidden.[102] Typically, one or two papers were read, alongside other business such as the election of new members and communications by the President and other officers. At the 17 December meeting, when Joule's paper was read, the President, Spencer Joshua Alwyne Compton (1790–1851), Marquis of Northampton, read the congratulations that had been sent on behalf of the Society to Queen Victoria and Prince Albert of Saxe-Coburg and Gotha on the birth of Princess Victoria (21 November 1840), their first offspring. There were four communications: 'Present state of the diamond mines of Golconda' by Thomas John Newbold (1807–1850), an 'outsider', which was communicated by Samuel Hunter Christie (1784–1865), the Second Secretary of the Royal Society; 'Magnetic-term observations made at Milan' by Francesco Carlini (1783–1862), 'Magnetic-term observations made at

100 Schuster, *op. cit.* (note 69), pp. 201–202 and 203–204.

101 Noah Moxham and Aileen Fyfe, 'The Royal Society and the prehistory of peer review, 1665–1965', *Hist. J.* **61**(4), 863–889 (2017), p. 872.

102 Aileen Fyfe and Noah Moxham, 'Making public ahead of print: meetings and publications at the Royal Society, 1752–1892', *Notes Rec. R. Soc.* **70**, 361–379 (2016), p. 363. Discussions were introduced only after a reform of the statutes in June 1845: *ibid.*, p. 376.

Prague' by Karl Kreil (1798–1862) and 'On the production of heat by voltaic electricity' by Joule were communicated by Peter Mark Roget (1779–1869), the First Secretary of the Royal Society. In the *Proceedings of the Royal Society of London*, we find abstracts of the works by Newbold and Joule. No abstracts were published for the two contributions concerning magnetic observations, and these were probably not read, since they essentially contained sets of data. Hence, during the 17 December meeting, most of the available time was devoted to the reading of Newbold's and Joule's papers. It is likely that Joule's paper was read in full, since it was relatively short (about 1,400 words).

All papers read at a meeting were regarded as suitable candidates for full publication in the *Philosophical Transactions*, and only papers communicated at a meeting could be published.[103] The final decision depended on the Committee of Papers. After 1832, any resolution of the committee needed to be backed by at least one written referee report.[104] Hence, after the 17 December meeting, Joule's paper could be expected to have been submitted to examination by at least one Fellow. Donald Cardwell guessed that '[it] is possible that his work was uncongenial to Snow Harris who may have been a referee and his mathematical style was not acceptable to Faraday who may also have been a referee'.[105] The archives of the Royal Society have preserved the referees' reports since 1832, but none could be found for Joule's 1840 paper, meaning that Cardwell's conjecture is unsupported by documentary evidence.

Another suggestion concerning a referee of Joule's 1840 paper was made by James Crowther:

> Presumably in reference to this paper Lyon Playfair wrote: 'When Joule first sent an account of his experiments to the Royal Society, the paper was referred among others to Sir Charles Wheatstone, who was my intimate personal friend. Wheatstone was an eminently fair man and a good judge, but the discovery did not then commend itself to his mind. For a whole Sunday afternoon we walked on Barnes Common, discussing the experiments and their consequences, if true, to science. But all my arguments were insufficient to convince my friend, and I fear that then the Royal Society did not appreciate and publish the researches.'[106]

However, Playfair's reminiscence cannot refer to the 1840 manuscript. He met Joule for the first time in 1842, and the above recollection is part of a letter written by Playfair to James Dewar on 20 January 1890, relating some of his memories of Joule from between 1842 and 1845.[107]

Although the Committee of Papers met on 7 January 1841, Joule's paper was not dealt with.[108] The issue of the *Proceedings of the Royal Society of London* containing information about the meetings held between 10 December 1840 and 25 February 1841 was published at the beginning of March, and part of its content, including the abstract of Joule's work, was reproduced by the weekly journal *The Athenæum* in its 6 March issue. At this point, there had been no formal decision on the destination of Joule's paper.

103 Fyfe and Moxham, *op. cit.* (note 102), pp. 362–363.
104 Moxham and Fyfe, *op. cit.* (note 101), pp. 876–877.
105 Cardwell, *op. cit.* (note 1), p. 294, n. 36.
106 Crowther, *op. cit.* (note 74), p. 167.
107 Thomas Wemyss Reid, *Memoirs and correspondence of Lyon Playfair* (Harper, New York, 1899), pp. 73–74.
108 Minutes of a meeting of the Committee of Papers, 7 January 1841, Royal Society Library, CMB/90/3/130.

There is also a mystery surrounding one detail of the published abstract. Its last sentence states that 'He [Joule] also infers from his researches that the heat produced by the combustion of zinc in oxygen is likewise the consequence of resistance to electric conduction'.[109] However, the 1840 manuscript does not contain a single word on this subject, and it is dealt with only in the 1841 paper. Hence, Roget could not have become aware of this conclusion from the original manuscript, and we must infer that, some time before the publication of the corresponding issue of the *Proceedings*, Joule had sent to the Royal Society some additional information concerning his new research. In principle, the abstract should only have summarized the content of the paper read on 17 December, and Roget's inclusion of this last sentence was unorthodox.

The next meeting of the Committee of Papers was held on 11 March, and it was decided that Joule's paper should be sent to the archives, meaning that it was not accepted for publication in the *Philosophical Transactions*.[110] One may surmise that the author was informed of this decision only after this resolution.

The meeting examined 15 papers in total, 2 of which were withdrawn by their authors, 1 was postponed, 6 were sent to referees, 1 was accepted for publication in the *Transactions* and 4 were sent to the archives—that is, rejected. It is peculiar that the only work approved on this occasion was by Edward Sabine (1788–1883), a member of the committee who had participated in the meeting.

The four rejected papers were by James Blake (1815–1895), on the physiological effects of some chemical substances, which had been read on 21 January; by John Davy (1790–1868), a Fellow, on the torpedo, which had been read on 11 March; and those by Joule and Newbold, read on 17 December. No referee report can be found for any of these works. Notice also that John Davy's paper had been communicated on that very night, and there could therefore have been no time to produce a statement about it.

Thus, it seems that the Committee of Papers sometimes broke the rules. The President and the two Secretaries were present at the 11 March meeting, making this infringement even more serious. According to Noah Moxham and Aileen Fyfe, only half of the papers communicated to the Society around 1840 were sent to referees to be reported, and a selection was made in advance, possibly by the Committee of Papers.[111]

Two of the rejected papers had been communicated by Roget and one by Christie, both of whom had participated in the decisions at the 11 March meeting of the Committee of Papers. It is possible that the two Secretaries had deemed those manuscripts interesting enough to be brought to the attention of the members of the Royal Society, but not sufficiently outstanding for full publication. If this was their opinion, this might explain why no referee report was demanded for those papers.

However, there is an additional puzzle. The minutes of another meeting of the Committee of Papers of the Royal Society, held on 24 June 1841, state that Joule had withdrawn a paper entitled 'On the heat evolved by metallic conductors of electricity'.[112] This was not the title of

109 Joule, *op. cit.* (note 5), p. 281.

110 Minutes of a meeting of the Committee of Papers, 11 March 1841. Royal Society Library, CMB/90/3/131.

111 Moxham and Fyfe, *op. cit.* (note 101), p. 877. On the other hand, a study of the refereeing process of mathematical papers in the nineteenth century showed that in the specific cases that were investigated, the rules were duly followed: Sloan Evans Despeaux, 'Fit to print? Referee reports on mathematics for the nineteenth-century journals of the Royal Society of London', *Notes Rec. R. Soc. Lond.* **65**(3), 233–252 (2011).

112 Minutes of a meeting of the Committee of Papers, 24 June 1841, Royal Society Library, CMB/90/3/134.

his 1840 manuscript, but it does correspond to the beginning of the title of the *Philosophical Magazine* paper: 'On the heat evolved by metallic conductors of electricity, and in the cells of a battery during electrolysis'.

It is therefore likely that Joule submitted a second paper to the Royal Society, and this may have been the same article that was published in the *Philosophical Magazine*. According to William Gee, Joule's experiments on electrolysis took several additional months after he had completed the study of metallic conductors. They were probably finished by March, since Joule submitted the larger version of his paper to the *Philosophical Magazine* on 25 March 1841. Hence, during the period between October 1840 and March 1841, he completed his experiments on electrolysis and revised the manuscript on the production of heat in metallic conductors. Although he did not insert descriptions of additional experiments in the first part of this article, he made significant changes, as a comparison between the two versions shows. These changes were made before he was officially informed of the rejection of the 1840 paper. According to Schuster,

> It may be surmised that some correspondence took place in the three months between the date at which the paper was read and that at which it was committed to the archives. On being informed of the objections raised, Joule may have prepared a more complete account to be substituted for the paper originally submitted; but the Royal Society having finally declined to print the original paper *in extenso*, it is quite likely that he forwarded the amplified version to the *Philosophical Magazine*, the reference to the Society in the opening paragraph being left standing by an oversight.[113]

Unfortunately, the hypothetical correspondence mentioned by Schuster has never been found. The preamble to the 1841 paper indeed suggests that Joule planned to submit it to the Royal Society: 'I have hoped, therefore, that the results of my careful investigations on the heat produced by voltaic action, are of sufficient interest to justify me in laying them before the Royal Society'.[114] This statement would make no sense if Joule had written the article with the intention of sending it only to the *Philosophical Magazine*. It is likely that he wrote the paper and submitted it to both journals simultaneously in March 1841, a practice that was not prohibited at the time. He withdrew the paper from the Royal Society in June, probably because he received notification of its acceptance by the *Philosophical Magazine*. Unfortunately, it has been impossible to find the manuscript that was withdrawn in the archives of the Royal Society. It was probably returned to the author, since the archives only kept manuscripts that were either communicated or published in full.[115]

Cardwell offers another suggestion: 'A possible explanation is that Joule may have referred the second, extensive paper to Roget for guidance before a formal submission. Roget may then have recommended publication in *Phil. Mag.*'[116] This suggestion cannot be accepted, however. We know for certain that Joule submitted the longer paper to the Royal Society and that he withdrew it in June. We also know that he submitted the paper to the *Philosophical Magazine* in March, meaning that the paper was indeed submitted for publication at the same time to both media. Further evidence that Joule submitted more

113 Schuster, *op. cit.* (note 69), p. 203.

114 Joule, *op. cit.* (note 6), p. 260.

115 Raymond K. Bluhm, 'A guide to the archives of the Royal Society and to other manuscripts in its possession', *Notes Rec. R. Soc. Lond.* **12**(1), 21–39 (1956), p. 25.

116 Cardwell, *op. cit.* (note 1), p. 294, n. 36.

than one paper to the Royal Society is his own comment, published in 1842: 'In the papers which I had some time ago the honour of communicating to the Royal Society, I related an investigation concerning the calorific effects of voltaic electricity'.[117]

Roget and Joule

It has already been mentioned that the physician Peter Mark Roget was the First Secretary of the Royal Society at the time when Joule submitted his paper, and that he was the person who read the paper to the Society in December 1840. The relationship between Joule and Roget was much more extensive than this, however.

Peter Roget is nowadays almost exclusively known as the author of the famous *Roget's Thesaurus*, a resource that was widely used before the development of word processors that included synonym resources.[118] Roget studied medicine in Edinburgh, where he was a student of Joseph Black, one of the foremost researchers in calorimetry. He was greatly interested in chemistry, and in 1798 obtained his degree as a medical doctor with a thesis entitled 'Tentamen physicum inaugurale, de chemicæ affinitatis legibus'.

The beginning of his professional life was directly linked to Manchester. In 1801, he was hired by a wealthy cotton manufacturer, John Philips, as a tutor to his two sons and to take them to the continent on a cultural tour. This trip was unfortunate, since Roget and the two teenagers were in Geneva when Napoleon began military hostilities against England. After being arrested and detained for a few weeks, Roget was released and took the two boys back to England, passing through Germany. A short time later, Roget obtained a medical position in Manchester, where he stayed from 1805 to 1808. During this period, besides his medical practice at the Infirmary, he delivered lectures on anatomy and physiology at the hospital, and on comparative physiology at the Manchester Lit. & Phil. It was also in Manchester that he began the elaboration of his famous *Thesaurus*. Towards the end of 1808, he moved to London.

Roget therefore knew Manchester very well, although this was a few decades before Joule sent his paper to the Royal Society. He was aware of the strong intellectual and scientific tradition in Manchester, and would not have thought or said that nothing good could come from that town. Instead, he would probably have empathized with a young man from Manchester who was trying to publish his first scientific contributions.

Peter Roget was interested in a wide range of subjects, including mathematics. In 1814, he designed a new kind of sliding rule that could give the powers and roots of numbers. The paper in which he communicated this invention was presented to the Royal Society, and it was because of this contribution, rather than his many medical works, that he was elected a Fellow of the Society in the following year.

117 James Prescott Joule, 'On the electric origin of the heat of combustion', *Lond, Edinb. Dubl. Phil. Mag. J. Sci.* (ser. 3) **20**, 98–113 (1842), p. 98; *Ann. Electr. Magn. Chem. Guard. Exp. Sci.* **8**, 302–315 (1842), p. 302. This paper was submitted to the *Philosophical Magazine* on 5 October 1841, and was read before the Manchester Literary and Philosophical Society on 2 November 1841.

118 On Roget's life, the reader may consult: Donald Lewis Emblen, *Peter Mark Roget: the word and the man* (Longman, London, 1970); Joshua C. Kendall, *The man who made lists: love, death, madness, and the creation of Roget's thesaurus* (Berkley Books, New York, 2009). See also the very informative anonymous obituary: 'Peter Mark Roget, M.D.', *Proc. R. Soc. Lond.* **18**, xxviii–xl (1870).

In 1826, Roget published his first paper on electromagnetism, and in the subsequent years he wrote four treatises for the *Library of useful knowledge* on electricity, galvanism, magnetism and electromagnetism.[119] These were first released as separate booklets (as were all other works in this *Library*) in 1827, 1829 and 1831, and were later published with other works by different authors as a single book.[120] Roget's four treatises were also published as an independent book in 1832.[121] In these works, he divided the text into numbered paragraphs, a practice that would be later used by both Faraday and Joule. In the same period, he wrote an article on galvanism for the *Encyclopædia metropolitana* (1830).[122] In all of these works, he reviewed the heat produced by electric currents, and was therefore very well prepared to assess Joule's 1840 manuscript. One peculiar fact is that Roget described an experiment in which water, contained in a vessel with a thermometer, was heated by a voltaic current—the same method used by Joule, and which had not been described by other researchers of that time.[123] It is possible that Joule was acquainted with Roget's works, a copy of which is extant in the Library of the University of Manchester.[124]

In 1812, Roget had become a member of the London Royal Institution, and lectured there on comparative physiology and zoological classification. He carried out some experimental research in chemistry with Humphry Davy, and later became acquainted with Davy's young assistant, Michael Faraday. On at least one occasion, Faraday assisted Roget in one of his galvanic experiments.[125] Roget later became the first Fullerian professor of physiology and comparative anatomy of the Royal Institution, in 1834; Michael Faraday had become the first Fullerian professor of chemistry in 1833.

Although Roget's works on galvanism were written a decade before Joule's research on this subject, he retained an interest in electricity and its effects. On 19 March 1840, Michael Faraday read his seventeenth series of *Experimental researches in electricity* at the Royal Society.[126] This included a discussion of the contact theory of voltaic electricity,

119 For information about this project, see Carol H. Weiss, 'Reflections on 19th-century experience with knowledge diffusion', *Knowledge* **13**(1), 5–16 (1991).

120 Library of Useful Knowledge. *Natural philosophy II* (Baldwin & Cradock, London, 1832). The volume contained the following tracts, each with a different pagination: 'Popular introductions to natural philosophy' (by Jane Marcet); 'Sir Isaac Newton's optics' (by D. Lardner); 'A description of optical instruments' (by A. Pritchart); 'The thermometer and pyrometer' (by T. S. Traill); and the four treatises on electricity, galvanism, magnetism and electro-magnetism by Peter M. Roget.

121 Peter Mark Roget, *Electricity, galvanism, magnetism, and electro-magnetism* (Robert Baldwin, London, 1832). Another edition was published in the same year by a different printer, with the same pagination but with a different title page: Peter Mark Roget, *Treatises on electricity, galvanism, magnetism, and electro-magnetism* (Baldwin & Cradock, London, 1832). There was also a German translation: Peter Mark Roget, *Darstellung der Elektricität, des Magnetismus, des Galvanismus und des Elektromagnetismus* (trans. Franz Kottenkamp) (Expedition der Wochenbäude, Stuttgart, 1847).

122 Peter Mark Roget, 'Galvanism', in *Encyclopædia Metropolitana; or universal dictionary of knowledge* (ed. Edward Smedley), vol. 4 (second division, vol. 2), pp. 173–224 (Baldwin & Cradock, London, 1830). This article is much more detailed than the corresponding treatise of 1829, and contains a profusion of bibliographic references.

123 Roget, *op. cit.* (note 120), 'Galvanism', at §29, p. 12; Roget, *op. cit.* (note 121), at §22, p. 179 and §41, p. 184. In the latter paragraph, Roget described an experiment in which different wires were connected in series to a galvanic battery and the rise in temperature was measured. No reference was given, and the experiment may have been carried out by Roget himself. A previous author described the heating by electricity observed in water: 'Indeed a platina wire may be kept ignited in vacuo, for an unlimited time, by Voltaic electricity. Water, surrounding a wire so placed, may be made to boil briskly.' William Henry, *The elements of experimental chemistry* (Baldwin, Cradock & Joy, & R. Hunter, Successor to J. Johnson, London, 1818), p. 193. However, Henry did not suggest any experiments to measure the heating effect of electric currents, as Roget did.

124 Available from John Rylands Library, University of Manchester, James R. Partington Collection (#925), https://www.librarysearch.manchester.ac.uk/permalink/44MAN_INST/1lr7mpn/alma9913113884401631 (accessed 20 January 2020).

125 Ian Lemco, 'P. M. Roget: some electrical connections', *Eng. Sci. Educ. J.* **3**(5), 229–233 (1994), p. 231.

126 Michael Faraday, 'Experimental researches in electricity: seventeenth series', *Phil. Trans. R. Soc. Lond.* **130**, 93–128 (1840).

which Faraday criticized. On 27 March, Roget wrote to him calling his attention to a similar argument that he had published in 1829,[127] and Faraday acknowledged the importance and precedence of this contribution, which he quoted fully in a footnote to his paper dated 29 March 1840.[128] Roget's ideas are relevant because they are associated with the later development of the principle of energy conservation.[129] In his treatise on galvanism, after presenting several arguments against the contact theory and in defence of the chemical interpretation of voltaic batteries, Roget remarked:

> Were any further reasoning necessary to overthrow it, a forcible argument might be drawn from the following consideration. If there could exist, a power having the property ascribed to it by the hypothesis, namely, that of giving continual impulse to a fluid in one constant direction, without being exhausted by its own action, it would differ essentially from all the other known powers in nature. All the powers and sources of motion, with the operation of which we are acquainted, when producing their peculiar effects, are expended in the same proportion as those effects are produced; and hence arises the impossibility of obtaining by their agency a perpetual effect; or, in other words, a perpetual motion. But the electro-motive force ascribed by Volta to the metals when in contact, is a force which as long as a free course is allowed to the electricity it sets in motion, is never expended, and continues to be exerted with undiminished power, in the production of a never-ceasing effect. Against the truth of such a supposition, the probabilities are all but infinite.[130]

Although Roget never produced a full account of energy conservation (or conservation of forces, as it was called), he was one of several authors who published claims concerning the impossibility of perpetual motion using any kind of physical force, and he admitted the proportionality between cause and effect in the transformation of 'powers'.[131] Faraday accepted Roget's ideas, which were similar to his own, and Joule acknowledged in 1843 that he was proposing ideas similar to those of Faraday and Roget.

Given this background, any claim that the Royal Society could not accept Joule's ideas since the concept of conservation and transformation of forces was new and unexpected, as claimed by Rosenfeld, does not hold up. The very person who communicated Joule's 1840 paper to the Royal Society was one of the forerunners of the principle of conservation of energy.

127 Letter from Peter Roget to Michael Faraday, 27 March 1840 (letter 1258), in *The correspondence of Michael Faraday, volume 2, 1832–1840* (ed. Frank A. J. L. James), pp. 644–645 (Institution of Electrical Engineers, London, 1993).

128 Helge Kragh, 'Confusion and controversy: nineteenth-century theories of the voltaic pile', in *Nuova Voltiana, studies on Volta and his times* (ed. Fabio Bevilacqua and Lucio Fregonese), vol. 1, pp. 133–157 (Universitá degli studi di Pavia, Pavia, 2000), p. 146.

129 Kenneth L. Caneva, *Robert Mayer and the conservation of energy* (Princeton University Press, Princeton, 1993), pp. 179, 372.

130 Roget, *op. cit.* (note 121), treatise on galvanism, at §113, p. 32. A similar statement can be found in Roget, *op. cit.* (note 122), at §181, p. 212. Michael Faraday regarded this argument as decisive. In a later paper, he again discussed the theory of the voltaic pile, and then asked the editor of the *Philosophical Magazine* to reproduce the paragraphs of the *Experimental researches in electricity* in which he had presented both his own arguments and Roget's against the contact theory. Michael Faraday, 'On Dr. Hare's second letter, and on the chemical and contact theories of the voltaic battery', *Lond. Edinb. Dubl. Phil. Mag. J. Sci.* **22**, 268–269 (1843); Michael Faraday, 'On the chemical and contact theories of the voltaic battery', *Lond. Edinb. Dubl. Phil. Mag. J. Sci.* **22**, 477–480 (1843).

131 Thomas S. Kuhn, 'Energy conservation as simultaneous discovery', in *Critical problems in the history of science* (ed. Marshall Clagett), pp. 321–356 (University of Wisconsin Press, Madison, 1959), pp. 328, 342 and 345; Vance M. D. Hall, 'The contribution of the physiologist, William Benjamin Carpenter (1813–1885), to the development of the principles of the correlation of forces and the conservation of energy', *Med. Hist.* **23**(2), 129–155 (1979), p. 131. Faraday's and Roget's ideas and arguments on the conservation of forces strongly influenced Joule. Shaul Katzir, 'Employment before formulation: uses of proto-energetic arguments', *Hist. Stud. Nat. Sci.* **49**(1), 1–40 (2019), p. 30.

In 1827, Roget was elected First Secretary of the Royal Society, on the retirement of John Herschel. His tasks included editing the *Proceedings* of the Society, and the production of the abstracts of the papers that were read. He performed this function until his retirement from this role in 1848. He was undoubtedly the author of the abstract of Joule's paper, which faithfully presents the content of Joule's manuscript except for its last sentence, as already remarked. It is also known that Roget read Joule's paper (excluding the abstract) at the meeting of the Royal Society on 17 December 1840. It is therefore likely that he favoured Joule's research, although the paper had several shortcomings and was not ready for publication.

In all probability, there was an exchange of letters between Roget and Joule. Indeed, many years later, in a footnote to another of his works published in 1881 in the *Scientific papers*, Joule remarked that 'This abstract was made, I believe, by Dr. Roget, who took a kind interest in my early papers'.[132] Observe the use of the plural 'early papers'; this is noteworthy, and may refer to both the 1840 manuscript and the 1841 paper.

Roget never published any comment on Joule's articles, and this 'kind interest' must therefore have been transmitted to Joule either by letter or via a common acquaintance. If this kind of communication did take place, then it is possible that Roget made some suggestions for the improvement of Joule's work.

Samuel Christie and Joule's manuscript

Some of the pencil annotations to the manuscript were probably made by Roget. However, a careful analysis shows that it was also examined by Samuel Hunter Christie (1784–1865), professor of mathematics at the Royal Military Academy. Christie's main experimental research concerned terrestrial magnetism, but in 1833 he presented the Bakerian Lecture at the Royal Society on the currents produced by electromagnetic induction.[133] In that paper, he proposed a new and very delicate method for comparing the electrical resistance of metallic wires. Christie confirmed with high accuracy that the conducting power varies inversely as the length and directly as the square of the diameter of the conducting wire. He also compared the relative resistances of several metals and reviewed previous papers on this subject.

On the reverse of the last sheet of Joule's manuscript, we find the following annotation in Christie's handwriting: 'Archives / 11 March 1841 / S.H.C.'[134] Since Christie was the Second Secretary of the Royal Society, this might have been simply a note on the decision of the Committee of Papers, on that very day. The digit '8' in '1841' is written in a very unusual way, similar to the Greek letter δ. The same distinctive symbol can be found in a pencil annotation at the foot of folio 3 *recto* of the manuscript, containing a calculation intended to check one of Joule's quantitative results. Roget did not write '8' in this way, so it seems

132 Joule, *op. cit.* (note 2), p. 171n.

133 See the anonymous obituary of Samuel Hunter Christie: 'Obituary notices of Fellows deceased between 30th Nov. 1864 and 30th Nov. 1865', *Proc. R. Soc. Lond.* **15**, i–xlvii (1867), at pp. xi–xiv.

134 It was possible to check that the initials 'S.H.C.' were written by Samuel Hunt Christie by comparing them with his signature on several proposals for new Fellows to which he subscribed, such as those of Justus Liebig, Jacob Karl Franz Sturm, Jean-Baptiste-André Dumas (all of them in 1840) and Heinrich Friedrich Link (in 1842). Royal Society Library, manuscripts EC/1840/22, EC/1840/24, EC/1840/25, EC/1842/15.

that this annotation was made by Christie, and that he carried out a careful analysis of the paper.[135]

It has already been pointed out that there was a footnote in Joule's paper in which he stated: 'The relative conducting powers of my specimens of metal of equal length and weight were Mercury, 1; Iron, 11.6; Copper, 36.6' (f. 3r). Few researchers had measured the conducting power (or resistance) of mercury, but copper and iron wires were very widely used in electrical experiments. In 1833, Christie had compared their conductivities, and had also provided a table in his published paper showing previous results obtained by other researchers.[136] Taking the conductivity of copper as 100, the values for iron varied from 14.5 (Davy) to 24.3 (Cumming), while Christie's own result was 22.3. Joule's result, taking copper's conducting power as 100, would give a value of 31.7 for iron—a result that was quite different from those obtained by all previous researchers.[137] Christie did not measure the conductivity of mercury, and hence did not include that metal in his comparative table. However, one of the authors he cited, Antoine-César Becquerel, had obtained a value of 3.45, while Joule's result was 2.73, 20% below the measurement of the French physicist.[138] Christie would probably have noticed that Joule had not described the method he used to compare the electric conductivities, and that his estimates were unreliable. It is quite possible that he told Roget or the Committee of Papers about this problem, which would have undermined the publication of Joule's full paper.

Christie would also have been fully qualified to carry out a quantitative analysis of the consistency of Joule's data as shown above. The single calculation that appears at the foot of one page of the manuscript shows that he was indeed checking Joule's results, although a lack of further annotations precludes us from inferring which other details he examined.

Both Roget and Christie participated in the decisions made at the 11 March meeting of the Committee of Papers which rejected Joule's paper. We know that both men carefully read the manuscript, and no written referee's report is available for the paper. We may surmise that they agreed that the paper was inadequate for full publication, and that their oral opinion was accepted by their colleagues.

The evolution of a man of science

We may presume that Joule discussed his 1840 manuscript with members of his scientific circle and, especially, with William Sturgeon, and that this interaction with his fellows helped to produce the improved 1841 version. It is even possible that Sturgeon acted as an intermediate between Joule and Samuel Hunt Christie concerning the paper submitted to

135 The proposals for new Fellows that were analysed (note 134) show the decision 'Elected' written by Roget, with his initials and the date. The numeral '8' that appears in all of them is clearly different from the peculiar '8' written at the back of Joule's manuscript.

136 Christie, *op. cit.* (note 93), p. 139.

137 Copper and iron wires were widely used in technical applications at the time. The ratio between their conducting powers was accepted to be nearly 6:1; that is, the conductivity of iron was about 17 if that of copper was taken as 100. Steinheil, *op. cit.* (note 94), p. 509.

138 Antoine-César Becquerel, 'Du pouvoir conducteur de l'électricité dans les métaux, et de l'intensité de la force électro-dynamique en un point quelconque d'un fil métallique qui joint les deus extrémités d'une pile', *Ann. Chim. Phys.* (ser. 2) **32**, 420–430 (1826), p. 428. Another more recent measurement had been published in Sturgeon's journal, giving the electrical conductivity of mercury as 4.66 for copper = 100. Heinrich Friedrich Emil Lenz, 'On the production of cold by the voltaic current', *Ann. Electr. Magn. Chem. Guard. Exp. Sci.* **3**, 380–385 (1839), p. 383.

the Royal Society. Sturgeon had become acquainted with Christie 15 years earlier in Woolwich and they still kept in contact at this time.[139] Those guesses cannot be substantiated, however, because of the lack of pertinent documentation.

The focus of this paper is Joule's 1840 research on the heat produced by electricity in metallic conductors. In the following years, his experimental skill steadily improved. His scientific circle gradually increased, and he conducted research in collaboration with William Scoresby (1789–1857), Lyon Playfair (1818–1898) and William Thomson (1824–1907).[140] He was progressively accepted by scientific institutions, beginning in 1842 with the Manchester Lit. & Phil., where he assumed an important role, and he became a Fellow of the Royal Society of London in 1850. The expansion of his invisible circle of assistants is harder to assess, of course.

Ten years after Joule submitted his first manuscript to the Royal Society, he was a respected man of science. During this period his ideas concerning the transformation and equivalence of the different physical forces became mature;[141] and he concentrated much of his experimental effort on ascertaining the exact value of the mechanical equivalent of heat. His early measurements varied so widely that it was initially impossible to establish the proportionality between mechanical work and heat production.[142] He devoted much time to the improvement of his famous paddle-wheel experiment and in 1850 he was able to publish very important and convincing results.[143] His experimental difficulties and the skill he had attained at this time became evident when there were attempts to reproduce that experiment, in the late twentieth century.[144] Even his careful 1850 research had some gaps, however, such as problems connected with the highly sensitive thermometers he used; and he was led to improve and to repeat that experiment in 1878.[145]

Joule's 1840 study of the heat production by electric currents was clearly crude. It was superseded by the very careful experimental investigation of the same phenomenon by Heinrich Lenz.[146] In 1844 Joule repeated his early experiments with improved instruments

139 In 1836, a few days after Sturgeon's communication to the Royal Society was read, Christie studied the manuscript and contacted the author, making some suggestions. William Sturgeon, 'Researches in electro-dynamics, experimental and theoretical', *Ann. Electr. Magn. Chem. Guard. Exp. Sci.* **2**, 1–24 (1837), p. 20.

140 Those joint projects were published in James Prescott Joule, *Joint scientific papers* (Taylor & Francis, London, 1887).

141 Before 1843, Joule's publications contained no clear allusion to those ideas. They were only fully presented in 1847. E. C. Watson, 'Joule's only general exposition of the principle of conservation of energy', *Am. J. Phys.* **15**(5), 383–390 (1947); Kargon, *op. cit.* (note 18), p. 53. The role of theoretical principles implicit in Joule's early work is a matter of controversy: Gordon Jones, 'Joule's early researches', *Centaurus* **13**(2), 198–219 (1968).

142 In one of his first attempts to find the correspondence between mechanical work (pounds risen to one foot) and heat (increase of one Fahrenheit degree of one pound of water), Joule obtained 13 different values that varied between 587 and 1,040. James Prescott Joule, 'On the calorific effects of magneto-electricity, and on the mechanical value of heat', *Lond. Edinb. Dubl. Phil. Mag. J. Sci.* (ser. 3) **23**, 263–276, 347–355, 435–443 (1843), pp. 437–439. Two years later, he was able to improve his experiments, but even then Helmholtz deemed that his method and results were unreliable: Hermann von Helmholtz, 'On the conservation of force', in *Scientific memoirs, selected from the transactions of foreign academies of science, and from foreign journals* (ed. John Tyndall and William Francis), vol. 7, pp. 114–162 (Taylor & Francis, London, 1853), p. 131.

143 James Prescott Joule, 'On the mechanical equivalent of heat', *Phil. Trans. R. Soc. Lond.* **140**, 61–82 (1850). At the end of this paper (p. 82), he concluded: 'That the quantity of heat capable of increasing the temperature of a pound of water (weighed in vacuo, and taken at between 55° and 60°) 1° Fahr., requires for its evolution the expenditure of a mechanical force represented by the fall of 772 lbs. through the space of one foot'.

144 Sibum 1995, *op. cit.* (note 17); Peter Heering, 'On J. P. Joule's determination of the mechanical equivalent of heat', in *The history and philosophy of science in science education* (ed. S. Hills), pp. 502–504 (Queen's University, Kingston, 1992).

145 Sibum 1998, *op. cit.* (note 17).

146 Heinrich Friedrich Emil Lenz, 'Ueber die Gesetze der Wärmeentwicklung durch den galvanischen Strom', *Bull. Classe Physico-math. Acad. Imp. Sci. Saint-Pétersbourg* (ser. 2) **2**, c161–c188 (1844).

and using a different method, and he provided a better confirmation of the relation between current, electric resistance and heat generation.[147]

Final comments

Every historian dreams of a fully catalogued archive in which everything that was ever written is preserved. This fantastic collection would allow us to solve innumerable historical puzzles. Coming back to reality, we see that some scientific archives, such as that of the Royal Society, have kept a huge number of documents that have not yet been fully explored; although not every document produced or received has been kept, those that have been maintained can be used to answer many historical queries.

In the specific investigation reported here, which focuses on a single scientific paper written by James Prescott Joule, a detailed examination of the 1840 manuscript and a comparison with the 1841 published paper allowed us to understand several aspects of Joule's struggle to produce his first scientific paper. The story that unfolds is not the heroic tale once told of the self-made scientist endowed with immense skill and precision by providence, but treated unfairly by the Royal Society. Joule's experiments and his analysis were far from perfect, and when he perceived some of the shortcomings of his manuscript, he removed the steps that would have shown his mistakes, rather than redoing his work. We can see that Roget and Christie, who examined his manuscript, had a very good knowledge of the subject of this paper, and that its rejection was even-handed. The communication of the paper to the Fellows by Roget, and the publication of its abstract, on the other hand, can be regarded as a sympathetic favour to the young Mancunian researcher.

Acknowledgements

The author acknowledges support from the Brazilian National Council for Scientific and Technological Development (CNPq) for the development of this research (Research Fellowship #302661/2017-4). He is likewise grateful to Ellen Embleton, Picture Curator of the Royal Society, for her help in identifying and providing copies of relevant unpublished material. He also acknowledges permission granted by the Royal Society to publish the transcription of the manuscript and its two figures.

147 The 1844 experiments were published only eight years later: James Prescott Joule, 'On the heat disengaged in chemical combinations', *Lond. Edinb. Dubl. Phil. Mag. J. Sci.* (ser. 4) **3**(21), 481–504 (1852). In this improved version of his experiments, Joule took several precautions, such as the use of distilled water instead of rain water. See Martins and Silva, *op. cit.* (note 7).