Chapter 4

James Thomson, an engineer and scientist: the path to thermodynamics

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Abstract

James Thomson, brother of William Thomson who later became Lord Kelvin, was also a highly creative practical engineer but also an extremely deep thinker in many areas of science. After early difficulties in his career due to ill-health, he was a professor of civil engineering at Queens University Belfast from 1853 to 1873, and then at the University of Glasgow until 1889, and he also became a Fellow of the Royal Society. Through the 1840s, the brothers worked together to analyse the logical problems of the then current theories of heat and work, and also together they studied the seemingly contrasting ideas of Sadi Carnot and James Joule, in the desire to combine them to create a new successful theory.

1 Introduction

By any criterion but one, James Thomson (1822–1892) (Fig. 1) was indisputably one of the very most able and important engineers and scientists of the nineteenth century. As a creative engineer, he was responsible for a large number of interesting and useful technical inventions. Much of his engineering work related to a wide range of studies of fluid motion, and his interest in this topic extended from engineering into the more strictly scientific field, while other important scientific achievements included work on the relations between the gaseous, liquid and solid states of matter; on elasticity and the strength of materials and on various aspects of freezing. In the latter category was his famous paper on the lowering of the freezing point of water by pressure [1], which was exceptionally important, not just as a major discovery in its own right, but also because of its central role in the establishment of the formal structure of thermodynamics.

His fertile imagination extended well beyond engineering and physics to what might best be described as geophysics, and he studied such problems as the flow of rivers, glaciation, and the prismatic structure of the Giant’s Causeway, in all cases providing novel approaches, and clear and convincing analysis.

In career terms, he spent 20 years as a professor of Civil Engineering in Belfast, his birthplace, and a further 16 in Glasgow where he occupied what had been the first chair of engineering in the world, and was still one of the very most prestigious. His teaching was recognised as being devoted and inspiring. He became a Fellow of the Royal Society and was chosen to deliver its important Bakerian Lecture in the year of his death, and he received honorary degrees from the Queen’s University, the University of Glasgow and the University of Dublin.
Outside academia, he worked as a successful consulting civil engineer, occupying a number of important civic posts. At a time when Belfast was expanding rapidly from a fairly small town to a large industrial city, with all the social problems that the process involved, James was an engineer to the Water Commissioners, and as such was responsible for the crucial task of providing fresh water, and for the removal of sewage; he became a well-known authority on such matters. He was also influential in the provision of public parks.

(The most notable account of James Thomson’s life and work is the collaboration between the famous physicist Joseph Larmor, and James’s son, also called James; this account is included in his Collected Papers [2]. More recent shorter articles are by Bernard Crossland and John Moore [3], and by Peter Bowler [4]. Apart from a discussion of Ref. [1], none of these accounts provide much information on Thomson’s contributions to thermodynamics, the main topic of the present paper, for which, see later.)

By any criterion bar one, then, James was a highly successful man. Unfortunately, the criterion by which he has most often been judged is by a comparison of his achievements with those of his younger brother, William Thomson, later Lord Kelvin (1824–1907) (Fig. 2) [5–7]. Kelvin was, of course, not only a scientist and engineer of spectacular achievement, but also extremely important in the development of scientific education as the founder of the first undergraduate laboratory. His technical work made him extremely well known and indeed rich, mostly as a result of his work on the first Atlantic telegraph cable, and after that on a range of important inventions. He worked closely with government on committees and inquiries, and, partly at least as a result of his political activities, became Lord Kelvin. He was to be buried in Westminster Abbey. It would have been impossible for James to compete with Kelvin on Kelvin’s own ground!

But James had his own considerable strengths, many of which were in areas where William was comparatively weak. While this will emerge during the rest of this paper, some preliminary
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remarks may be made at this point. With two brothers, one, William, nominally a scientist, the other, James, nominally an engineer, one might expect that it would be the engineer who would be less wedded to the study of fundamental principle, but with the Thomsons it was, if anything, the other way round. Larmor writes of James as ‘the philosopher, who plagued his pragmatisical brother’ [6, pp. 282–283]. As an early example, when the brothers made toy boats in their boyhood, James was not satisfied until a toy boat he might be making was perfect in every detail, while William was happy as soon as his floated at all [2, pp. xvii–xviii]!

Similarly in later life, before James would publish a paper or make public an invention, he would have considered every detail long and carefully. Thus, his theories stood the task of investigation, and his engineering inventions worked. The less patient William, on the other hand, often threw off ideas at white heat, and, though they may often have been brilliant, they frequently required substantial correction or amendment.

Smith and Wise [6, p. 282] contrast James’ ‘determined single-mindedness, manifested in his compulsive involvement with engineering problems’ with ‘William’s impulsive attacks on a variety of loosely related problems’. They also quote from a letter of Hermann Helmholtz to his wife as follows: ‘James is a level-headed fellow, full of good ideas, but cares for nothing but engineering, and talks about it ceaselessly all day and all night, so that nothing else can be got in when he is present. It is really comic to see how the two brothers talk at one another, and neither listens, and each holds forth about quite different matters. But the engineer is the most stubborn, and generally gets through with his subject.’

Other interesting comments come from John Nichol. Nichol was the son of John Pringle Nichol, who was a professor of astronomy at The University of Glasgow when James Thomson’s father, yet another James whom we shall call James Senior, was a professor of mathematics. John Pringle Nichol was particularly responsible for William becoming a devotee of the work of Joseph Fourier, who was to be an important influence on him right the way through his career. John Nichol was friendly with the Thomson children when they were young, and later he served with James Thomson when John himself was a professor of English Language and Literature at Glasgow from 1862 to 1889, so his opinions are of great interest.

John Nichol [2, p. xxxviii] wrote: ‘Of the sons I liked James best. He was crotchety and apt to be sulky with those who would not enter into his crotchets; here his faults end. He was steadfast, independent, straightforward, quiet, unobtrusive … His passion was engineering; he was always on the eve of something that was going to revolutionize trade. He used to show me lots of models, and often he would walk out to try his boats on the streams, as a chemist goes to make an experiment that will test the worth or worthless of years of toil … I believe that some of the inventions were excellent, but there was always some practical obstacle which prevented their bringing to the inventor either the fame or the fortune they merited. James was an idealist in his way’.

We have now met some of the facets of James’ mind, intelligence and character. In the following two sections, we shall look briefly at his life and then give a summary of his general achievements. After that, the rest of the paper is devoted to his contributions to the development of thermodynamics. It is true, of course, that this was the area of pure science for which his brother William has deservedly become most famous. It is usual to regard him with Rudolf Clausius and MacQuorn Rankine as the three founders of the subject. In apparent contrast, apart from
Ref. [1], James Thomson published next to nothing in the area. However, it is very much to the credit of Crosbie Smith [8, 9; see also 6] that he has investigated in detail James’s contributions to William’s own work, and found them to be extremely important. It would seem that James’s determination and clear-sightedness blended well with William’s more impulsive and perhaps imaginative qualities. James also gathered considerable experience in practical working with machines and studying their various efficiencies. Smith’s publications have stimulated the question that this paper is designed to study, even if it may not actually succeed in answering: Were James Thomson’s contributions to the emergence of thermodynamics so great that he deserves to be ‘promoted’ and regarded as one of its founders?

As has been implied, Thomson’s contributions to science and engineering in general, let alone thermodynamics, where most of his main contributions were admittedly unpublished, have been less well known than they should have been. In the Preface to his collected papers [2, p. v], which was published as soon after his death as 1912, it is said that the importance of his scientific activity is well known, but ‘chiefly through references to his work by other physicists such as his brother Lord Kelvin, Thomas Andrews, James Clerk Maxwell, Osborne Reynolds [and] J Willard Gibbs’.

Yet one man – the highly perceptive John Perry [10], who had been a brilliant pupil of James Thomson at Belfast, and who in his turn became an exceptional engineer, physicist and educator – was clear-sighted enough to raise precisely the topic of this paper. ‘As early as 1847’, Perry has written [2, p. li], ‘[James] applied Carnot’s cycle and first principles to the study of the pressure and temperature of melting ice, and he may be said to have really shown to Clausius, Rankine and his brother (Lord Kelvin) the method of attack which has led to the development of thermodynamics.’

2 A sketch of the life of James Thomson

James and William were the third and fourth children of James Thomson Senior, a man perhaps as remarkable as either of his two more famous sons. James and William had two elder sisters, Elizabeth (1818–1896) and Anna (1820–1857), while a younger sister, Margaret, died as an infant. Anna, in particular, was to play a crucial role in James’s life. Of their two younger brothers, John (1826–1847) became a medical student, but sadly, while still young, he contacted fever in the course of his duties and died. Robert (1829–1905) entered the insurance profession and emigrated to Australia. Here, he was to suffer mixed fortunes. As actuary to a life insurance company he was renowned for his enthusiasm, but he was dismissed when he was unable to account for considerable sums of money, and he became bankrupt several times in the 1860s. He was subsequently able, though, to rebuild his actuarial career with great success, being renowned as ‘a man of genius but erratic working habits’ [11].

James Senior (1786–1849) had begun life as a farmhand on a family-owned farm near Ballynahinch, around 15 miles south of Belfast, but, as a man of great mathematical ability and enormous determination, he was able to receive a basic education locally, and then to assist with the teaching, a job he was able to maintain in the summer months while studying for a degree at the University of Glasgow in the winter months. Having obtained his degree in 1812, his natural progression would have been to a ministry in the Presbyterian church, but for an exciting academic opportunity that presented itself.
In 1798, under the United Irishmen, a rebellion had taken place in Ireland, effectively bringing together Catholics and Presbyterians [dissenters], and uniting them broadly against the combination of the British state and members of the established church in Ireland, the Irish Church, which could be regarded as the branch of the Anglican Church in Ireland. At this time, only members of the Irish Church had full political rights. The rebellion had been inspired by the American and French revolutions, and its ideals were those of liberty, equality and freedom from sectarian discrimination. These were very much James Thomson Senior’s own beliefs, which he passed on to a very great extent to his children, and he strongly supported at least the principles of the United Irishmen. The rebellion was crushed, and, as is well-known, the tentative unity of Catholics and dissenters was fairly soon broken down, giving way to the strong division between Catholic on the one hand, and Protestant (Presbyterian and Anglican) on the other which has largely persisted till the present day.

However, in Belfast, in particular, where the United Irishmen had been mostly Presbyterian rather than Catholic, the ideals of tolerance and non-sectarianism were to remain pronounced for roughly the first third of the nineteenth century. Central to ‘liberal Belfast’ was the establishment of the Belfast Academical Institution, universally known as ‘Inst’. This had school and college departments (the latter being effectively a Scottish university on a small scale), and was avowedly non-sectarian, its secretary stating that: ‘The subscribers to the Institution are composed of all religious persuasions’. As well as its liberal perspective, Inst aimed at providing a practical education for its students, and in this and indeed every way James Thomson was ideal for Inst, and Inst was ideal for James Thomson. He became both a teacher of mathematics in the school department (also teaching geography) from 1814, and a professor of mathematics in the college department from 1815, and he immediately became a strong political presence in the administration of Inst. Wherever he was employed, he was to become a leader of men, coupling clear ideas of what should be done, with the political ability actually to get it done in the majority of cases.

As well as his teaching duties, Thomson used his courses as the basis of a series of practical and commercially oriented textbooks, to write which he got up around 4 O’clock in the morning before his day at Inst began. Fortunately, these books sold extremely well and thus made him a lot of money. He certainly felt no need to apologise for his comparative affluence, believing that the labourer was worthy of his hire, another belief that was passed on to his sons. William was clearly the more successful in money making, though John Nichol’s comments in the previous section suggest that James was by no means averse to using his engineering knowledge for commercial purposes, though not as successful as his younger brother.

James Senior married Margaret Gardner in 1817, and the family listed above were born over the next 10 years or so; sadly the hard-working but happy and prosperous existence the family enjoyed came to a sudden end when Margaret died in May 1830, and by December 1831 James Thomson Senior had been appointed as the chair of mathematics at the University of Glasgow. It was a new start for the family, in which, particularly in the absence of a mother, the influence of the father on his children was to be immense and of supreme importance.

James Senior himself adapted very quickly to the new situation. The university was at the time something of a hotbed of conservatism, privilege, patronage and nepotism. In particular, religious tests ensured that only members of the (Presbyterian) Church of Scotland could be
appointed to chairs. James set himself the task of reform, but this was only achieved slowly, mostly by the death of the reactionary professors and their replacement by those more willing to encompass fresh ideas; the process was to take the remainder of his life and longer, and in fact the religious tests were only to be abolished in 1858.

Due to a misunderstanding, Professor Thomson was initially not nearly as well off in Glasgow as he had expected, and as a result the children were taught at home. Neither James nor William was to regret this, as they felt the free flow of ideas when working directly with their father easily surpassed the routine drill they might have been subject to at school. In fact, they had already attended some of their father’s lectures informally before, in 1834, they both matriculated at the university. James was 12 and William 10; it is true that the age of matriculation was lower then, but not normally younger than 14.

Despite their youth, both boys were regular prize winners over the next few years, often finishing first and second in the various lists [5]. But inevitably, it was William, the younger boy who was first, with James second. Larmor writes [2] that ‘no cloud of jealousy ever marred the friendship of the pair’, as ‘the younger brother was even then adored by all the family as a genius’, and one may believe him and yet still wonder whether there may have been a dampening effect on James’ confidence, which may not have been restored until the early 1850s.

James took a BA in 1839, and an MA in mathematics and natural philosophy in 1840. Then, following a family holiday in Germany, including a walking tour of the Black Forest, obsessed with engineering as he was, he entered the Dublin office of John McNeil, a professional engineer. William studied in Glasgow a further year and could have taken his MA, but did not do so as he was to enter Cambridge University as an undergraduate in 1841, and was not sure whether a holder of an MA was eligible. Despite James’s devotion to engineering, it may have occurred to him at the time that William was to have the more comfortable billet, a feeling that would be expressed on at least one occasion in the next few years.

In fact, even before William left for Cambridge, James had returned home after around 3 weeks, as a result of a knee injury he had suffered while walking in the Black Forest that had left him quite lame. As it turned out, he was able to return to the University of Glasgow to study engineering under the new professor, Lewis Gordon for the 1841–1842 session; Gordon was actually the first Professor of Engineering in the world.

Having improved considerably in health, James was able to spend some time in 1842 working under the guidance of John McClean in Walsall. McClean and James had been fellow students in his father’s class, and McClean was already advancing in his engineering career that was to bring him to an important position in the early days of railway construction. At the time, he was in charge of a number of canals, and James took the opportunity of visiting recently constructed locks, and measuring and drawing their various components.

Then, early in 1843 James went to the Horseley Iron Works, Tipton, Staffordshire, where he stayed for a few months as a pupil in the drawing office. The first iron steamer to put to sea had been constructed in Horseley, rather strangely [6, p. 288] as it is very far from the sea! James, however, was highly critical of some of the methods the firm adopted; the costing of an extremely substantial contract was delegated to young apprentices, and quantities were estimated by eye rather than measured precisely. Perhaps not surprisingly the firm encountered severe financial difficulties.
James, though, moved in the autumn of 1843 to Millwall in London, where William Fairbairn had set up a very substantial shipbuilding works, a subsidiary of his Manchester engineering firm, which were the first millwrights and among the first engine makers in Britain. James was a premium apprentice, an arrangement for which his father paid a fee of £100 (perhaps the equivalent of £10,000 today). He learned about heavy engineering techniques, studied marine engines and iron ships and made drawings for engines. In fact, his studies of marine engines in Millwall led him to consideration of their fundamental principles, and he had some important and deep discussions with William, which would bear fruit within a few years with the development of thermodynamics.

While at Millwall, James modified the dynamometer of the French engineer, A.J. Morin, and he suggested that this device could be used to measure the work produced by Fairbairn’s steam engine. The idea was that the excess work produced by the engine, rather than being wasted, could be sold to neighbours who needed motive power, and James’s dynamometer could be used to ascertain exactly how much work they used so they might be charged accordingly [9, p. 40]. Again we see James’s intense interest in the efficient use of the power derived from natural resources, and indeed his time in Millwall was useful for him in very many ways. However, he found the atmosphere at Millwall damp and unhealthy, and he suffered from a practically perpetual cold.

The Millwall works were not profitable and by the summer of 1844 they were being run down, prior to being sold, and in the autumn James himself was moved to the Manchester works where he would work more closely with Fairbairn, concentrating on the building of steam-engines. He seems to have become fairly despondent, writing to William that: ‘I am not getting settled in any works. Just when I begin to take root I have to leave the place.’ Writing again before William’s final examinations at Cambridge, he said that: ‘I wish my apprenticeship was as nearly done as yours – but when it is done, I fear I shall have no comfortable berth to step into as that which is probably waiting for you’ [6, p. 292].

Indeed, James’s immediate prospects were far from good. His health became steadily worse. There was some derangement of the action of the heart; his pulse was far too fast, and he felt weak and ill. At the end of the year he had to return to Glasgow, where his Calvinist doctor gave him the traumatic news that he had heart disease and might die at any moment.

One would have imagined that he would have collapsed under this news. If he did, such a collapse was temporary, because in fact rather remarkably in the next few years he was to develop a variety of intellectual pursuits, many of which were to be continuing themes through the rest of his life and will be discussed in more detail in the remaining sections of this paper. They included the recovery of pure water from sea water; the properties of whirling fluids; the horizontal waterwheel or vortex turbine; the problem of design in creation; the expansion of ice; the glacial period and glacial markings; the formation of lakes; the parallel roads of Lochaber; elasticity and the strength of materials and atmospheric circulation. The extreme breadth of his interests is remarkable, from engineering design to more general questions of engineering science, and on to study of problems of general scientific interest.

Part of his interest in most of these topics, and always in the background, was the fundamental question about the relationships between force, work, heat and temperature, and how these relationships were seen in the working of engines and the properties of different substances. An important document is what we shall call his 1848 Belfast notebooks [12], which summarise
James's ideas at that time. The centrepiece of this work may be said to be the important paper on the depression of the freezing point of water by pressure [1].

In 1846, William was appointed to the Chair of Natural Philosophy at Glasgow to the delight of his father and the whole family. There is no doubt that James shared in this delight as he was never jealous of his younger brother, but he would not have been human if he had not felt the contrast between the positions of the brothers, one established in a position of considerable prestige, the other older one an invalid, perhaps unlikely to work again and maybe not expected to live long.

In fact, James's prospects improved markedly in 1848 when a new doctor told him that his health was vastly improved, and there was no reason why he should not regain his strength and move to a normal life. It is not to be supposed that the rather outlandish treatments and draconian diets suggested by the previous doctor had been successful; probably, the preceding prolonged exposure to noisy and dirty machinery, noxious substances and drastically uncomfortable surroundings had left him in a severely unhealthy state, and a considerable amount of time was required for recovery. Though he was keen to return to engineering, he was careful never to place himself in the type of environment that had caused his breakdown in health.

While this improvement of health clearly great news for James, the late 1840s were still not a time of much happiness for him. First in 1847, his younger brother John, who had become a medical student, died of typhus, probably as a result of overworking during an uncontrollable outbreak of fever. Perhaps even worse, in 1849, James Thomson Senior, on whom all the family, but perhaps particularly James, had relied so much, died of cholera.

Even before this second death, James had been passing through a severe moral crisis. He was an extremely devout man, but he had come to the conclusion that he could not accept much that would be consider essential in the Presbyterian faith in which he had been brought up. Though believing in God, he did not believe that Jesus was the Son of God. In fact, he had become in fact a Unitarian, believing in only one person of the godhead, rather than a Trinitarian believing in three (Father, Son and Holy Ghost). Smith [9, p. 310] suggests that it was ‘[T]he imperfect world of harsh Calvinism’ that had driven James to take this step. He had confided his belief to his father, but following the death of the latter, and after a brief visit to his sister Anna, who had married William Bottomley and was living back in Belfast, he determined to move to London where he would perform some collaborative work with Lewis Gordon, hoping to produce a patent for the water-wheel he had designed [6, p. 301].

James had felt that if he was to disclose his views on religion to his family he would cause great distress, and probably a break in relations might be inevitable. So: ‘However painful the effort was, and gloomy the way before me, I tore myself away from my friends [2, p. xl].’ He then wrote to his brothers and sisters and a very few close friends saying: ‘I believe in one God, the Creator, the Preserver, and the Governor of all other things; and I believe that He has given us, while we are in this world, no other revelation of Himself but that contained in his wonderful works, and in the ideas which He has implanted in our minds, or rather which He has adapted our minds to receive. I think we should never neglect to pray to Him...’ An interesting aspect of this ‘confession’ was that he felt no requirement to ‘sugar the pill’ by giving even lip-service to the idea of Jesus as a unique human being, worthy of the greatest admiration, even if not worship as a member of the godhead. Much as he must have longed
for his family at least to appreciate his views, he was too honest to dissemble even in the smallest way.

In fact, he was delighted to receive letters from his family ‘indicating such a spirit of true kindness and love on their part as has made me quite happy’. Yet he stayed in London until 1851, and it was only then that he felt able to agree to Anna’s plea to come back to Belfast where he could work as a civil engineer. She had done him a great sisterly service, for this was the final turning-point in his life. Having recovered from his illness, with his religious dilemma solved – he was never to question the conclusion he had reached, and his position was accepted by his family – he was able to rebuild his life. For all his past difficulties he was still only 29, and he had before him nearly 40 years of success and happiness until the almost inevitable trials of old age. (Sadly, though, Anna herself was to die in 1857.)

It has been said [2, p. xl] that in Belfast ‘conditions were more favourable for his inventions, coal being dearer and less used than in England, and water power more available’. In fact, a very efficient version of the type of turbine invented by Benoît Fourneyron in France in 1827 had been produced by the Macadam Brothers in Belfast with considerable success in the 1840s [13]. (An example of the MacAdam-Fourneyron turbine from the Catteshall paper mill in Godalming, Surrey has been preserved.)

In Belfast, James’s engineering practice gradually grew; in particular, in 1853 he became a resident engineer to the Belfast Water Commissioners, a post he held until 1857. In this capacity, he became involved with the measurement of flow of water in rivers and streams, and proposed simple methods that gave excellent results demonstrating ‘Dr Thomson’s genius as an experimenter’ [2, p. xlv]. His expertise was called on by authorities in several other towns and cities, and he also explored many other interesting and significant ideas to do with water flow, which will be described in later sections. In 1855, William wrote to him from Glasgow saying:

People often ask about you here, and I say that I believe you are getting a good deal of business…. The water-wheels, too, I hear sometimes spoken of, and I hope they are going to get a name yet of practical value. Have they begun to be profitable? I hope they will sooner or later.

As well as a fruitful working environment, in Belfast James found himself among, not just family but friends. He joined a number of cultural societies – the Literary Society, of which his father had been President some 30 years before, the Belfast Social Enquiry Society, the Belfast Natural History Society, and on its foundation a few years later, the Belfast Naturalists’ Field Club. Attendance at these meetings stimulated James’s interest in the very wide range of academic topics that has already been noted. He was to give a number of papers, including one to the Belfast Social Enquiry Society on the provision of parks in growing cities; this paper was to lead to the provision of the very large Ormeau Park in Belfast. He also met congenial people, in particular Thomas Andrews, Professor of Chemistry and Vice-President of Queen’s College Belfast, with whom James was later to carry out exceptionally important work, and Neilson Hancock, Professor of Political Economy, whose sister, Elizabeth, he was to marry in 1853. The marriage was extremely happy and they were to have three children.

As it happened, their honeymoon was interrupted by a request from Queen’s (Fig. 3) that James should stand in for Professor John Godwin in the latter’s absence from the Chair of Engineering,
and 4 years later he was appointed to the Chair permanently when Godwin retired. (The formal name of the Chair was Civil Engineering and Surveying.) There can be no doubt that in this position, James had found his métier. He gave up the great majority of his other work, the exceptions being mostly public positions where he worked for the general good, and he dedicated himself to his students and his research. His predecessor Godwin had been very much a railway engineer; he had been actively involved in introducing the railway into Ulster as a resident engineer and manager to the Ulster Railway since 1837. In fact, his college work had been rather a side-line, as his salary from his railway employers was nearly three times the one that he drew from Queen’s; his lecturing was restricted to 2 hours on 2 days a week, plus a substantial drawing-class held in his office at the railway company on Saturdays [14, pp. 119, 173]. James took on much heavier teaching duties, partly because the engineering curriculum at Queen’s was much extended in 1859. As a teacher, he was concerned and sympathetic, though also a stickler for rectitude.

These qualities come out in comments made in later life by John Perry [2, pp. li–lii]. Perry had entered Queen’s without the intention of taking a degree, and, when he decided he wished to do so, it was discovered that some of the requirements were lacking. First, while at Queen’s he had not produced enough engineering drawings; Thomson went to the bother of chasing down and studying drawings that Perry had made while employed in a foundry in Belfast, and was able to pronounce them a satisfactory alternative. However, Perry had also registered too low an attendance at classes, and on this ground, Thomson could not be persuaded, despite the efforts of Andrews, to give him the required credit. It was only when a right of the President to exercise leniency in such matters was discovered, or perhaps invented, that Perry was allowed to take the examination. Nevertheless, Perry declared Thomson to be ‘one of the kindest of men’, though one with ‘a very strong sense of duty’.

With a guaranteed salary, Thomson was able to devote his research talent to problems of interest and importance across a wide spectrum of academic and industrial topics, and this work will be sketched in the following section. A particular interest that started when he moved to a new...
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James Thomson’s years at Queen’s were successful and happy, and when in 1872 MacQuorn Rankine, who had become Professor of Civil Engineering at the University of Glasgow, died at the early age of 52, Thomson had little inclination to apply for the post. He felt he was carrying out successful and important research, particularly in his collaboration with Andrews. He himself enjoyed his work very much, and in addition he did not wish to uproot his wife and youngest daughter, neither of whom was physically strong. He may also have recalled that 40 years before when his father had made his own move from Belfast to Glasgow, financial difficulties had occurred; Glasgow was a more expensive city than Belfast, so to maintain the same standard of living, a higher salary would be required.

His brother, though, now Sir William Thomson as a result of his success with the Atlantic cable, was determined that James should join him in Glasgow. He overcame all James’s objections; what probably clinched the matter for James was the mere fact that his brilliant younger brother did indeed feel that he was the best man for the post. Next, William organised the campaign for James, just as their father, James Senior had done for William himself 26 years before. In contrast to his father’s demand in the previous campaign that William obtained as many testimonials as possible, for his brother’s campaign William was content with five, which were all of the highest quality. It is interesting that, for a chair in Engineering, they were all from physical scientists – Maxwell, Andrews, Joule, Helmholtz and Tait (Figs 4–8). Three of these are quoted in the Biographical Sketch [2, pp. lxvi–lxix].

Hermann von Helmholtz wrote that: ‘I regard Mr James Thomson as a man of very sound and unusually acute judgement in questions relating to physical, mechanical, and mathematical science, and a very extended amount of knowledge in these same branches. His theoretical prediction of the alteration of the freezing temperature of water by pressure was an original idea of first-rate importance. Also his hydrodynamical considerations, and their practical applications for the construction of water wheels for his jet pumps, for the theory of oceanic and atmospheric

Figure 4: James Clerk Maxwell.

Figure 5: Thomas Andrews [Queen’s University Belfast].

Figure 6: James Joule.
currents, are of a very original turn of mind, and prove that he is able to follow out his specu-
lations to the very fundament of the question. Besides I know from personal acquaintance his
scrutinizing way to penetrate into the very heart of scientific questions, and I should think that
such a man ought to be the very best teacher for young engineers’.

P.G. Tait listed much the same set of achievements, adding that: ‘I was for some years his col-
league in Belfast, and I can thus state of my own knowledge that he is an excellent teacher, and
works in thorough harmony with his brother Professors. As an original discoverer in Physics,
and especially in the Dynamic Theory of Heat, he holds a very high place indeed… I certainly
do not believe that there can be found in Britain any one so well qualified as he is to fill the place
of our lamented friend Rankine.’

When James Joule was asked to give a testimonial, he noted that he had already written one for
another candidate, as he had no idea that James would be able to leave Belfast. He added that:
‘In the event of his seeking the Chair of Glasgow College, it is impossible that there can be any
serious competition, inasmuch as [James’s] European fame as a discoverer combined with his
eminent success as a teacher of Engineering science, are such as to place him altogether without
a rival.’

James Thomson was duly appointed to the Chair, and commenced his duties in November
1873. In a letter to William, Thomas Andrews noted that: ‘Your brother will indeed be a sad loss
to us all here – and to no one more that to myself. I have long been greatly attached to him. He
is indeed a rare character, so truthful and gentle and wise withal.’

The marked divisions in the professoriate at Glasgow between conservatives and reformers,
which had dominated so much of the efforts of James’s father in particular 40 years before, had
disappeared; his battles had long been won, and the newly appointed James Thomson was to
be as happy and successful in Glasgow as he had been in Belfast. His renown also increased; he
became a Fellow of the Royal Society in 1877, and 7 years later he was elected the president of
the Institution of Engineers and Shipbuilders in Scotland. Like his brother, he was a convinced
opponent of Gladstone’s adoption of Home Rule for Ireland as the official policy of the Liberal
Party, and James and William both became ardent members of the breakaway Liberal Unionist Party; this was to lead eventually to William’s peerage as Lord Kelvin.

However, in 1889, James was struck with a rapid deterioration of his eyesight. His retina became detached in the middle, so he could barely read and could write only on large sheets of cartridge paper which his wife or one of his daughters would then copy. Fortunately, he never became totally blind, but could only to a certain extent recognise the faces of friends. He resigned his chair, but was actually able to continue to work at his research, in particular at a topic he had long been interested in, that of atmospheric circulation. He wrote a new paper recapitulating much of his work over 30 years or so, and this was given the honour of being selected by the Royal Society as its Bakerian Lecture for 1892. William was then president of the Royal Society, and he read it on James’s behalf at the meeting of the Society on 10th March. Sadly, though, even before this paper was published, on 8th May James Thomson died. Within a week, his wife and daughter Bessie also died, all of what was termed ‘a severe cold’, though it is now suspected of pneumonia. Figure 9 shows a plaque erected in his memory in Queen’s College Belfast.

The last few years had not been pleasant, and of course there was the period of great concern earlier in his life, but in both cases James was able to use the time to great profit. In between, there had been a considerable period of happiness and achievement.

As a last note, it may be mentioned that James had sympathies in some areas that could be termed today broadly leftward. As well as his work for healthy and sanitary conditions in housing, and his calls for the setting up of public parks, which have already been mentioned, he wrote an article for the Northern Whig, arguing for the nationalisation of public works, in particular railways; this paper was included with his Collected Papers [15].

3 James Thomson’s achievements in engineering and science

In this section, we sketch those contributions of James to engineering and science that lie outside the specific area of the creation of thermodynamics, the area which will then occupy the
remainder of the paper. It will become clear, though, that much of this work outside the specific area was actually quite closely connected with his work on thermodynamics – for example, his studies of engines of different types, in which he was always concerned with the efficiency [or ‘economy’] of operation, his collaboration with Thomas Andrews on the properties of liquids and gases, and his studies of glaciation, which were essentially applications of his important work on the lowering of the freezing point under pressure.

Perhaps, his most important work in engineering was the invention of his vortex turbine. James had always had a strong interest in the utilisation of natural sources of power in rivers and waterfalls, and was particularly impressed by the waterwheels of Holland. He also learned about the French turbine from Gordon’s classes in Glasgow; this was a horizontal rotating waterwheel, in which the water enters at the inside of the wheel, flows along the buckets and escapes at the outer circumference. Thus, the centrifugal force fulfils the role played by gravity in a vertical waterwheel.

The idea of the horizontal rather than vertical waterwheel was to satisfy the criteria announced by Lazare Carnot (Fig. 10) in 1803 for maximising the economy of any waterwheel [9, p. 35; 16, p. x]. These are that the water must enter the wheel without ‘shock’ and must leave it without velocity. Today, we may easily explain the meaning of the criteria in terms of the requirement to make the greatest possible use of the kinetic energy of the incoming water. Kinetic energy will be lost if there is an inelastic initial collision (or ‘shock’) between the water and the blade, and of course any kinetic energy still possessed by the departing water is wasted. But at the beginning of the nineteenth century, there was at most a rudimentary understanding of these ideas; any genuine appreciation of the idea of energy and its conservation came with the ideas of thermodynamics and the central importance of this quantity throughout physics was particularly due to William Thomson, Lord Kelvin.

Lazare Carnot’s criteria are in many ways analogous to, and in some ways maybe the basis of, the celebrated criterion reached two decades later by his son, Sadi Carnot (Fig. 11) [16], and discussed and developed as a centrepiece of thermodynamics by Émile Clapeyron (Fig. 12), the Thomsons and Clausius. Sadi Carnot’s criterion is that, for maximisation of the efficiency of a heat engine, one must ensure that heat never flows between sub-systems between which there
is any difference in temperature greater than an infinitesimal one. Sadi Carnot’s work will be extremely important through the remainder of this paper.

In 1846, James made a small model of his own design of horizontal waterwheel, which resembled the French turbine; his first ideas had actually occurred to him as early as 1842. The essential new feature was that pivoted inlet guide vanes were used, which were controlled by an external linkage; this allowed the flow of water into the turbine to be controlled and thus the output power to be varied [3]. To avoid jarring, one of Lazare Carnot’s criteria demanded that the water was injected at the same speed as the wheel but at high pressure. As the water moves inwards, the vanes curved from radial to nearly tangential, so the water was effectively always kept as close as possible to a state of equilibrium; the pressure decreased as the water moved inwards, and it left at the centre with an extremely low velocity, again to satisfy Lazere Carnot’s criteria [6, pp. 412–413]. The flow of the water was clearly analogous to a natural vortex.

In 1847, while visiting a friend, Walter Crum, who ran a large engineering works, James had been able to have a much larger model constructed and carefully tested. Despite some defects in the manufacture of the model, it still produced around 70% of the total work available in the flow of water, which compared with around 75% obtained from the best overshot wheels. (An overshot wheel is a vertical wheel in which the water passes over the top of the wheel. Thus, the gravitational energy of the water, as well as its original kinetic energy is available for the provision of useful power. So, overshot wheels should be the most effective vertical wheels, though careful design is required; if Lazare Carnot’s criteria are not obeyed at least approximately, much, even most, of the initial energy may be wasted in jarring or in the final kinetic energy of the water.)

At this time, James’s health was still not good, but by 1850, with his health much improved, he was able to take out a patent, titled ‘Hydraulic Machinery and Steam Engines’, on what he now called a vortex water-wheel; it would later be called a vortex turbine. He had received much help from William, who, on his travels in France, had discussed James’s wheel [17] with Jean-Victor Poncelet, a leading authority on waterwheels and turbines. James came to an agreement with Williamson and Brothers of Kendal, which later became the internationally well-known firm of Gilkes and Gordon, to make his turbine [3], and in 1856 Williamson produced the first turbine to his design. This was known as the Vortex Williamson Number 1, described in the Williamson catalogue as ‘designed by Professor James Thomson’. It produced 5 horsepower under a fall of 31 ft with a 9 inch diameter supply [13].

At the South Kensington International Exhibition of 1862, the turbine was awarded a medal for ‘originality of design, practical success, and good work’, and in the article on Hydro-mechanics in the 1881 edition of the Encyclopaedia Britannica, SG Greenhill and WC Unwin wrote [2, p. xxxiii] that: ‘James Thomson’s inward flow or vortex turbine (Fig. 13) has been selected as the type of reaction turbines. It is one of the best even in normal conditions of working, and the mode of regulation introduced is decidedly superior to that in most reaction turbines; it might almost be said to be the only mode of regulation which satisfies the conditions of efficient working, and it has been adopted in a modified form in the Leffel turbine, which is now largely used in America.’ It may perhaps be surmised that the vortex turbine was at least a partial exception to John Nichol’s comment, mentioned in the first section, that for James’s inventions there was always some practical obstacle preventing fame or fortune.
Many turbines were made under this patent, and they were erected in England, Scotland and Ireland, as well as in many other countries. In fact, the basic design of James’s vortex turbine has been exploited right up to the present for large, power generating, water turbines. However, they are usually called Francis Turbines, being named after JB Francis, who designed a similar turbine with fixed guide vanes [3].

James’s work on the vortex turbine led to more fundamental studies of vortex motion, and he called his 1852 study of exact hydrodynamic observation and theory the ‘vortex of free mobility’. In such a scheme, essentially each particle had the same total energy, however, far it might be from the central axis. This prevented the necessity of work being performed by centrifugal forces when particles moved, which would lower the efficiency of any device. These considerations came 6 years before Helmholtz’s own major work on vortex motion [2, p. xlii].

This concept was used when James was involved in the design of extremely large centrifugal pumps. In particular, around 1858, he designed and constructed such a pump with exterior whirlpool for drainage of lands in Jamaica, and also in Demerara in British Guiana, lands which lay generally below the level of high tide. The provision of the whirlpool is to make use of the kinetic energy of the water leaving the pump. James stated [18] that: ‘The fluid revolving
assumes the condition of a vortex or whirlpool, which has been designated by the writer as the Vortex or Whirlpool of free mobility.’

Another important development was his so-called jet pump, or ‘apparatus for drawing up water by the power of a jet’. This was originally designed to drain the pits of his turbines when they required cleaning, but its greater importance was in draining swamps and shallow lakes [19]. The jet pump has no moving parts and only needs a high head of water [3]. In Ref. [18], James reports that a jet pump had recently been erected by William Forster of Ballynure near Clones, and the pump had behaved very successfully in removing the water from wet lands adjacent to his house.

James’s involvement with turbines and his position as an engineer to the Belfast Water Commissioners led him to an interest in the measurement of the flow of water in streams and rivers. His great achievement here was the proposal to replace rectangular notches by triangular or V-shaped ones. V-shaped notches are amenable to exact calculation, because the issuing jet is of the same shape; however, much of the notch that is occupied by the stream. James was in fact able to deduce that quantities of water flowing through V-shaped notches are proportional to the $\frac{5}{2}$-power of their linear dimensions [20]. His experiments were performed on open streams with the simplest of equipment, and it is interesting that when, nearly 50 years later, far more sophisticated and expensive experiments on the same problem were carried out at Glasgow, the experimenter reported that, for the gauge notch used by James, his results were remarkably near to the true value. He concluded that: ‘It is a proof of Dr Thomson’s genius as an experimenter that, with the crude apparatus at his disposal, he was able to arrive at such an accurate result [2, p. xlv].’

James was also interested in the accurate measurement of power, and he constructed a new form of friction brake dynamometer, which he showed to his classes as Glasgow. It consisted of fast and loose pulleys on a shaft, with a rope which was attached to the loose pulley but lapped partially round the fast pulley with weights at each end. The loose pulley adjusted itself to give the required arc of contact, winding or unwinding the cord on the running pulley. Much later William Ayrton was to say that: ‘Another plan was to vary the arc of contact. It was curious that every person who had arrived at that idea seemed to regard it as his own. It had been invented over and over again… [I believe] it was due initially to Professor James Thomson [2, pp. xlv–xlvii].’

James’s interest in fluid flow was further exhibited in a major study of the flow of water through orifices [21], and he also addressed fundamental questions about the winding of rivers in alluvial plains [22]. The process of river meanders and the formation of oxbow lakes are usually imagined to occur, he says, through the water rushing at the outer bank and washing it away, at the same time causing deposition on the inner bank. James pointed out, however, that this simple explanation could not be true. The hydraulic principle, which he suggested is not generally known, says that a stream flowing into a curve must flow with diminished velocity along the outer bank, and an increased one along the inner bank, this suggesting that it is the inner bank that would wear away the most.

James’s solution was that the water turning the bend is subject to a centrifugal force, but a thin layer of the water at the bottom is retarded by friction with the bottom, and so it is subject to
a smaller centrifugal force than the great body of the water flowing over it. Consequently, the
bottom layer flows across the channel towards the inner bank and rises up between the inner
bank and the rapidly flowing water. It protects the inner bank from the rubbing of the rapid
water and brings with it sand and detritus from the bottom of the stream, which it deposits
along the inner bank. James also said that the same phenomena take place in connection with
flow in pipes, with considerable effect on the behaviour of frictional resistance. He was later to
set up a simple model with a small artificial river in which were anchored at various depths
threads of suitable length.

A last topic in this general area of fluid motion is that of atmospheric circulation. James’s first
work on this subject was in 1846, when his father asked him to write a paragraph on trade
winds for a new edition of his Geography. He published his developing views through his life,
and, as has been said, his Bakerian lecture of 1892 was based on the ideas he had in his
last few years. In the lecture, he prefaced these ideas with a comprehensive and extremely use-
ful historical survey of the topic, discussing the contributions of, among others, Martin Lister,
Edmund Halley, George Hadley and John Dalton.

Another range of research topics emerged from James’s famous freezing point paper; these
were the melting of ice and its importance in the movement of glaciers. These studies brought
him into discussion with many of the leading British scientists of the day, glaciology being a
special interest of several British scientists, including James Forbes and John Tyndall, while
Michael Faraday had been extremely interested in the transition between water and ice. James’s
first paper, published in 1857, discussed regelation (a term introduced by Tyndall) – the
phenomenon that occurs when a fine wire is looped over a block of ice, and a heavy weight is
attached to it. The ice will melt under the wire, and the wire may thus move right through the
block, though the ice freezes again after the wire has passed, and the block remains solid at the
end of the experiment. James called this phenomenon the plasticity of ice and showed that it is
a fairly direct result of his own discovery of the lowering of the freezing point under pressure.

These ideas are clearly relevant to the movement of glaciers. There were differing opinions on
the mechanism involved in this movement; some scientists thought that they slid like solids,
others that they flowed like viscous liquids, others still that they crawled by alternate expansion
and contraction, or by alternate freezing and melting. Forbes, who argued that ice was viscous,
became implacably opposed to Tyndall, who believed in regelation. (This controversy was
the second for Forbes in the area of glaciology; earlier he had been an equally virulent opponent
of Louis Agassiz.) James Thomson’s intervention did little to calm the situation, and, in fact,
today it would be accepted that many of the proposed explanations play a role in the overall
behaviour. In 1865, no less an authority than Hermann Helmholtz had adopted James’s theory,
or, as it might be said, his branch of Tyndall’s theory.

A related problem investigated by James was the phenomenon of ground or anchor ice, and its
effects on navigation in the St Lawrence River. Ground ice is ice that forms from the bottom of
a river, in contrast to the usual surface ice we are familiar with on the surface of a lake. James’s
explanation was that the crystals of ice are frozen from the water at any depth where cold may
be introduced by contact, or by loss of energy by radiation; they may also be supplied by snow.
These ice crystals may be whirl ed about in currents or eddies until they come into contact with
fixed objects to which they may adhere – rocks, stones, pieces of ice jammed in crevices or ground
ice already growing. Such pieces of ice, Thomson says, will adhere together with a continually increasing firmness, even when the surrounding water is above the temperature of freezing.

Rather earlier James had become engrossed by another problem involving glaciers – the so-called Parallel Roads of Lochaber in Scotland. These were horizontal indentations on the upper levels of valleys, and it had been realised that they were the remains of ancient beaches. Charles Darwin had deduced that, at the time of their production, Scotland must have been totally sunk below the ocean, but this, he was later to admit, was his greatest mistake [4]. The concept of an ice age had been introduced by Agassiz in the 1840s, and, as we have already seen, many British scientists had become extremely interested in the topic of glaciation.

James had never visited the district, but when he studied the subject in detail from books and maps, he came to the conclusion, universally accepted today, that the ‘roads’ or beaches had been formed by glacial lakes, created when glaciers dammed the lower reaches of the valleys. When the ice melted, the water drained away. James was able to show that certain locations of glacial barriers would explain the positions of all the terraces. His paper [27] was read to the Royal Society of Edinburgh in March 1848; it was his first paper he had written, and it received an enthusiastic reception, the Secretary inviting him to send further papers. This must have been extremely pleasing for the young man, still regarded as an invalid, and his biographers [2, p. xxxiv] make the interesting comment that: ‘Thus encouraged, he turned his mind more and more to pure science rather than to practical engineering.’

We now turn to the topic of the liquefaction of gases. Following his work on the lowering of the freezing point, James was to retain his interest in phase transitions for the remainder of his life, and while he occupied his chair in Belfast, he was fortunate to collaborate between 1861 and about 1872 with Thomas Andrews, an exceedingly gifted practical chemist, whose talents matched those of James in an extremely profitable way [28]. James was fully able to interpret the results of Andrews’ brilliant experiments. A number of exceedingly important conclusions were to emerge from their work. First [29] was what is usually simply called Andrews’ Experiment, which demonstrated the essential difference between a gas and a vapour: below its so-called critical temperature, a substance is a vapour and may be liquefied by pressure alone, while above this temperature it is a gas and cannot be liquefied using only pressure. While this is obviously a crucial practical point for the technical task of liquefying gases, the existence of the critical point is also exceptionally important for the general understanding of the nature of substances and their different phases.

A second very important result of this collaboration was James Thomson’s pointing out that there was a unique value of temperature and pressure where a substance can exist together in gaseous, liquid and solid states. He [30] gave this point in the now famous name of the triple point. The third result that James also introduced and discussed informatively [31, 32] was what he called ‘a difficulty of making a beginning of [a] change of state’, or what would now be called superheating and supercooling.

While the above is sufficient to show the great success of the collaboration between James and Andrews, Rowlinson [28] points out that, but for a variety of factors, even more might have emerged. Andrews was ill in 1872 and again in the late 1870s, and he resigned his chair in 1879, and James Thomson, of course, moved to Glasgow in 1873. After Andrews’ death in...
1885, P.G. Tait published some of their remaining results posthumously, and it has gradually become clear that Andrews and Thomson had anticipated some of the important results of the Dutch physicists, Johannes van der Waals and Heike Kamerlingh Onnes, both of whom were to be awarded the Nobel Prize for Physics in the new century. Andrews and Thomson did indeed draw diagrams showing what would now be called the van der Waals loops; Rowlinson comments that: ‘The two ideas [of Andrews and Thomson] were combined in a masterly away in the equation of state that van der Waals put forward in his thesis at Leiden in 1873, and for which he borrowed the title of [Andrews’] Bakerian Lecture.’ And many of the results obtained by Andrews in the late 1870s were reproduced and explained by Kamerlingh Onnes in the 1890s.

Another interesting and important piece of work concerned the prismatic structure of basalt as seen at the Giant’s Causeway (Fig. 14) in the North of Ireland and elsewhere. James concluded that the older theories could not be correct, and he developed his own theory [33] over a period of years. His account explained the columnar structure as the result of shrinking and cracking during solidification, and said that the cross joints are circular fractures commencing in the centre of the column and moving out, or ‘flashing’, towards the circumference. He provided a detailed discussion of many more special features of the Causeway, and most of his suggestions are now taken as standard.

Yet another interesting piece of work concerned integrating machines, which had in principle many uses, but their most important one was predicting the tides. Around 1874, William was interested in the possibilities of designing and constructing such machines, and James told him of a machine he himself had designed many years before, probably, in fact, around 1845. It was called a disk–ball–cylinder integrator, and it enabled any fraction of the motion of a revolving disk to be communicated to a cylinder pivoted above it, by the intermediation of a heavy metal ball that rested on the disk and pressed against the cylinder.

Figure 14: The Giant’s Causeway, County Antrim in Victorian times.
William immediately realised that it could be used not only to perform simple integrations but also to solve differential equations and to perform harmonic analysis, by which any complicated periodic motion can be broken down into a sum of sinusoidal terms, the last application being central for study of tidal and meteorological phenomena. James published a paper [34] on his invention, and William followed this paper with several on conceivable applications. The so-called Harmonic Analyser for calculating the elements of tides was actually constructed [5, pp. 692–694, 730–731]; the first working model had five disk–ball–cylinders, and later instruments could provide a curve giving the rise and fall of the tide at a particular location for a whole year, the computation taking a mere 24 minutes.

An early topic of interest to James was the distillation of fresh water from seawater on steamships. While this could be achieved very effectively using coal, it must be remembered that space is at a premium on such ships, as the amount of cargo must naturally be maximised. Thus, James suggested two economical methods of obtaining fresh water but was particularly pleased with the one that used the labour of sailors rather than coal, thus saving space for extra money-earning cargo.

Both methods involved using the heat given out in the condensation of the steam to generate more steam. In the first method, this was achieved in a fairly straightforward way using coal. In the second method, which James favoured, the labour of the sailors was used to pump steam from the seawater boiler to a vessel within it, thus increasing the pressure and temperature of the steam. Heat was conducted from this vessel to the boiler, so that more steam would be generated in the latter, while condensation of steam in the vessel created fresh water [9, p. 43]. James had shrewdly noticed that, while on sailing ships the sailors were often busy in adjusting the sails in order to obtain maximum advantage from the wind, on steamships they had little to do except when entering or leaving port or at times of bad weather, so they had plenty of time to provide the pumping and save coal.

We will conclude this section by mentioning briefly a few other tasks and achievements of James at different periods of his life. When he was 16 or 17, he invented a mechanics for feathering the floats of the paddles of paddle steamers [1, p. xcii]; engineers on the Clyde were extremely interested in the idea, but unfortunately it turned out that the same idea had been invented and patented a few months earlier. A few years later, he proposed [1, p. xx] the idea of such a steamer with paddles actuating jointed legs reaching to the bottom of the river, thus being able to propel the steamer upstream against the current. A completely different topic was that of the refraction of light passing through the atmosphere, to which James also made important contributions [1, pp. lxv–lxvi]. Lastly, we may mention that James invented many new scientific terms [1, pp. ci–cii]; among those which have stood the course of time are ‘radian’, ‘interface’, ‘ergometer’ and ‘torque’.

4 James and William: early dilemmas and discussions

Many of the fundamental difficulties that James and William were to discuss over several years, and which were eventually to lead to their work on thermodynamics, emerged much earlier and in very different guises, some involving the ideas of heat and work themselves, some based on more general scientific ideas and some indeed more to do with religion than science.
We will start with the question of the long-term future of the Universe. While it may be said that Newton’s mathematics demonstrated that the solar system acted like a clockwork model, Newton himself insisted that God was required to ‘operate’ as well as to create the Universe [35]. Yet, by the first decade of the nineteenth century, with the famous work of Laplace [36], it appeared to have been proved that the solar system would remain stable eternally. It seemed natural to assume, and it generally was assumed, that the Universe itself would last for ever, that changes would be cyclic in nature, that there was no direction to the development of the Universe, no end-point to which the Universe was heading, no ‘arrow of time’ [6, pp. 89–92].

Yet, this comforting belief was challenged towards the beginning of the eighteenth century by observations on Encke’s comet. This comet was actually discovered by Jean-Louis Pons in 1805, and when he suspected that an apparently new comet of 1818 was a reappearance of his 1805 discovery, it was Johann Encke who was able to analyse the orbit and confirm Pons’s belief. Thus, Encke’s comet is one of the few to be named after someone other than its discoverer! At that time, all known comets had orbital periods of at least 70 years, for example, that of Halley’s comet, the best-known, being 76 years, and their aphelia lay beyond Uranus. In contrast, the period of Encke’s comet was as low as 3.3 years, and its aphelion lay within the orbit of Jupiter. Thus, great interest was shown in the comet, particularly when in 1832 Encke was able to demonstrate the surprising result that its period decreased by 212 hours in each revolution.

This apparently showed that there must be a frictional medium in interplanetary space. In turn, this implied that all motion would be retarded and eventually cease, and that the solar system would in time collapse into the sun, and thus it would follow that there was a direction to the behaviour of the Universe, an arrow of time heading towards an end-point of the Universe. Along with all other scientists and mathematicians, James and William worried about these matters. Around 1841, for example, James argued that the tides on earth caused by the motion of the moon around it must lead to retardation either in the motion of the moon itself, or in the rotation of the earth [6, p. 285].

The dilemma had a parallel in a theological debate of the period [6, pp. 92–99], again concerned with a contrast between conservation and decay. On the one hand, it would be generally believed that God had created the Universe, and that man had the power neither to create nor to destroy. This was a clear call for conservation, and in scientific terms it was the credo of James Joule and was to become that of William Thomson and his brother. Joule was to write [6, p. 306] of: ‘Believing that the power to destroy belongs to the Creator alone, I entirely coincide with Roget and Faraday in the opinion that any theory which, when carried out, demands the annihilation of force [to be interpreted as what would now be called ‘energy’] is necessarily erroneous,’ and William explicitly agreed.

Yet equally, common experience and the Bible spoke clearly of decay – human beings died, buildings crumbled and fell down, dynasties disappeared. In the draft of one of his first accounts of thermodynamics, William [6, pp. 330–331] was to write: ‘Everything in the material world is progressive. The material world could not come back to any previous state without a violation of the laws which have been manifested to man, that is, without a creative act or an act possessing similar power. I believe the tendency in the material world is for motion to become diffused, and that as a whole the reverse of concentration is gradually going on.’ He quoted the Biblical lines: ‘The earth shall wax old like a garment’; analogously the hymn-writer says: ‘Change and decay in all around I see.’
This dichotomy seems to have a considerable effect on William’s eventual formulation of thermodynamics. Of course in saying that theological ideas influenced scientific beliefs, one is not suggesting that raw undigested religious ideas were or should have been inserted into scientific theories in an *ad hoc* way. However, nearly all the British men involved in the creation of thermodynamics were strongly religious, and they very naturally developed a worldview that used the fruits of observation, contemplation and experimentation within a religious framework. It turned out that this worldview was extremely helpful in creating the scientific paradigm based on energy that is at the centre of our ideas of physics today.

The third area [6, pp. 285–288] where the brothers had intense discussions, which were again to be extremely fruitful, was in the contrast between different situations where what the brothers were happy to call ‘power’ was provided to a system. In some cases, a useful ‘effect’ resulted; in others, it seemed that the power was simply wasted. In a letter of 1862 to William, James referred to a particular discussion they had had around 1842 when James was working at Walsall. Watching the barges go through the locks on one of the local canals, they observed that the purpose of the efforts of the lock-keeper or traveller in raising water was, of course, to provide power to raise the barge, but for much of the time the raised water merely cascaded over the lock gates and it seemed that no useful ‘work’ was performed. Similarly, water falling from a height could turn a water-wheel and thus provide useful work, or alternatively just fall under gravity with no useful result.

In part, the ideas of the brothers followed those of William Whewell (1794–1866), the important Cambridge mathematician, which had recently been discussed in his book *The Mechanics of Engineering* [37]. Whewell had effectively put together a set of ideas corresponding to much of the strictly mechanical side of our present understanding of conservation of energy. ‘Labouring force’ or an input of ‘work’ would be provided to a system. This amount of work could be equated to half the *vis viva* which it could produce; *vis viva* was a time-honoured term, literally ‘living force’, which was equal to $mv^2$, and so this was a broad appreciation of the idea of what would today be called kinetic energy. The result of the process could be useful work – a weight raised or a spring extended. Some or all of the work, however, might be ‘wasted’ in frictional processes.

However, when discussing this ‘waste’, Whewell was thinking principally of solids, where friction could be explained as permanent wearing or distortion of the solids in contact, for example, overcoming cohesion, or permanently changing the elastic properties. For the Thomson brothers, thinking of water rather than of a solid, it seemed inconceivable that friction could be explained in this way, but they had no alternative set of ideas with which to replace those of Whewell.

The problem was analogous to that concerning the steam-engine that was to give the brothers much cause for thought. Heat leaving the boiler of a steam-engine and eventually reaching the condenser was responsible for the production of external work. Yet equally, one could arrange affairs so that the heat could travel directly from the boiler to the condenser by conduction and no work would be produced. One was bound to ask the question: In the second case, what happened to the work that would have been produced in the first case?

The various dilemmas discussed in this section were only to be resolved in the creation of the subject of thermodynamics, its first and second laws, which included, of course, a new
understanding of the nature of heat. It should be pointed out that, in the discussion of the steam engine in the previous paragraphs, it is not to be supposed that the brothers thought in terms of transformation of heat to work. Rather they were working within the ‘caloric’ theory in which heat was conserved, but its fall from high to low temperature was, where appropriate, associated with the provision of external work. We discuss this topic further in the following section.

5 Ideas of heat in the first half of the nineteenth century; the caloric theory

It is easy to say that, with the coming of thermodynamics, the view of heat held by William Thomson, and indeed the majority of other scientists changed from a theory of ‘caloric’ to a ‘dynamical’ theory. However, it is essential to clarify what is implied (and indeed what is not implied) by these terms.

Today, the caloric theory is usually taken to be explicitly a theory, widely held between about 1780 and about 1850, in which ‘heat’ or ‘caloric’ consisted of particles of a subtle, weightless and highly elastic fluid. The presence of these particles in the pores of the atoms of a solid could be used to explain thermal expansion, while their emission from surfaces explained the well-known Newton’s law of cooling. One of the features of caloric theories that made them popular is that there were several other highly developed theories in which physical phenomena were explained in terms of similar subtle elastic fluids; the phenomena so explained included electricity, magnetism and light. Thus, many of the main branches of science were explained in a unified way [38, pp. 14–15].

It is today taken for granted that the caloric theory stood in opposition to what we shall here call a ‘kinetic’ or ‘vibrational’ theory in which heat was taken to be the motion of the particles of the particular medium. It is widely held that this type of theory is very much the precursor of, and in fact practically identical to, today’s dynamical theory.

It is further usually believed that the caloric theory, like, in fact, the other ‘fluid’ theories, was naive and, even for its day, old-fashioned and foolish. Textbooks tell us that by the beginning of the nineteenth century the caloric theory had been refuted by the 1898 experiments of Count Rumford, in which repeated boring of a cannon produced a seemingly inexhaustible amount of heat, and those of Humphry Davy around 1799, in which he is supposed to have melted two pieces of ice by rubbing them together. It is implied that the scientific authorities of the time were almost wilfully blind in ignoring these ‘refutations’ of the caloric theory.

However, Mendoza [16, p. xv] suggests that the current accounts of the caloric theory and its clash with the kinetic theory have been derived fairly directly from those given by William Thomson in his later years. Mendoza suggests that, perhaps wishing to distance himself as much as possible from his earlier beliefs, Thomson tended to emphasis the contrasts between the caloric theory and more modern ones, and to pour scorn on the caloric theory. Thus, he argues, our current perspective is distorted. In this section, we shall attempt to provide a more balanced account.

Incidentally, Smith [9, pp. 170, 186] suggests a rather different motive for Thomson, and in particular for that of his friend P.G. Tait in his Sketch of Thermodynamics [39]. By raising the
profile of the Rumford and Davy experiments, and thus implying that the dynamical theory of heat had been established as early as the late 1790s, they took away any possibility that the work of Julius Mayer in the 1840s should be awarded any priority. In 1862, Mayer's work had been championed by John Tyndall, the scientific opponent of Thomson and Tait. Thomson and Tait themselves were keen that the priority for the proof that heat and work were interchangeable should be shared between Rumford and Davy for the early suggestions, and James Joule, their own colleague, for his own work in the late 1840s, which was much more authoritative than that of Mayer.

We shall now say a little more about the caloric theory. It is clear that it may handle readily the situation where two bodies at different temperatures come into contact; caloric is believed to repel itself, so some caloric moves from the hotter body where it is more dense to the cooler one where its is less dense. As it does so, the hotter body cools and the cooler body heats up.

However, there are situations which are much more difficult to explain on the simplest ideas of caloric. These would include adiabatic compression or expansion of a gas, in which there is a change of temperature although no heat or caloric may leave or enter the system [38, pp. 39-60]. They also include isothermal expansion or compression, in which heat does leave or enter the system but there is no resultant change of temperature.

This apparent anomaly is answered by the statement that the total quantity of caloric, or absolute heat, of a body is composed of two components, the 'free' or 'sensible' or 'perceptible caloric' which affects a thermometer, and the 'latent' or 'combined caloric', which does not [38, pp. 104-156]. The word 'combined' indicates that the caloric was supposed to be combined, perhaps even chemically, with the matter of the gas, but could be 'squeezed out' by compression. The difficult situations of the previous paragraph may then be handled trivially by the transfer of caloric between the two categories. In an adiabatic compression, for example, an amount of caloric is transferred from the latent component to the perceptible component.

Such a technical manoeuvre may be said to typify the behaviour of 'scientific research programmes' in the theory of Imre Lakatos [40]. A band of 'auxiliary' or 'protective hypotheses' may be concocted to avoid any clash between the central theory and experimental results. In the present case, it may be suspected that such manoeuvres will be available for practically any suggested 'anomaly' of the theory. Appropriate movement of caloric from the latent to the perceptible category or vice versa would seem to be able to repel practically any criticism.

Indeed, Fox [38, p. 121] says that:

With caloric being called upon to explain such a wide diversity of phenomena as radiation, temperature, specific heat, expansion, change of state, and the repulsive forces between gas particles, some degree of complexity was to be expected, but the multiplicity of forms that it was thought caloric could adopt was truly formidable around 1800. The easiest of solutions was, after all, simply to postulate as many sorts of caloric, each of them with its own characteristic properties, as there were phenomena to explain, and although no one went quite as far as this, the current terminology reflects how attractive this type of solution was. Thus we find such terms as combined, latent, radiant, sensible, and free heat (or caloric) all in common use, to say nothing of the quantities absolute
and specific heat. However it is important to note that in all versions of caloric theory only one fluid was concerned and its properties were merely modified by its state of combination with ordinary matter.

A possible exception to the idea that the theory of caloric could not be refuted might appear to be Rumford’s experiment which seemed to suggest that an infinite amount of heat must be present in latent form. However, Cardwell [41, p. 102] sums up the response of the supporters of the caloric theory:

Rumford’s challenge to the accepted theory of heat stirred up a hornet’s nest of counter-argument and experimental refutations. The consensus would seem to be that while the generation of frictional heat was rather difficult to explain on the caloric theory it was not necessarily impossible to do so, and in any case there were very grave objections to any alternative theory. If heat can be created de novo, what happens to the fundamental axiom of the conservation of heat on which the whole science rested? No scientific theory is ever flawless, and the caloric doctrine was no exception. Rumford pointed to a weakness in the doctrine, but that was by no means the same as refuting it.

We will move on to consider how such arguments did finally lose ground in the middle of the century.

First, though, we point out that the direct physical understanding of caloric was not universal. Truesdell [42] follows Fox [38] in saying that: ‘The term ‘Caloric Theory’ when current meant very different things to different schools of thought. Some of these are now strange even to historians of physics; still familiar are some particular models such as subtle fluids or atoms of caloric.’ For example, Cardwell [41, p. 143] mentions the belief by some workers in ‘caloric of space’. So, certainly we should attempt to follow broad features of caloric theories rather than particular facets, or particular physical models.

It may be said that the most fundamental aspect of the caloric theory was actually that heat was a conserved quantity. One may read the important works of those highly convinced of the caloric theory, such as Sadi Carnot and Clapeyron [16] and the early James Thomson [12], and this is the point that is constantly mentioned, perhaps more often implicitly than explicitly because it was broadly taken for granted. Clifford Truesdell, in his masterly but often savage account of the creation of thermodynamics [42, p. 34], for example, writes that ‘For Laplace, heat is never created nor destroyed. We shall refer to this assumption by the traditional term Caloric Theory of heat.’

If heat is always conserved, it would seem difficult to interpret those processes which today we describe by saying that heat is converted to work. Before the Carnot cycle, which we shall explain shortly, became well known, the most important example was the steam engine. As Cardwell stresses [41, Chapter 3], around the nineteenth century water power and steam power were the two important and rival sources of power for industry, the former being gradually displaced by the latter. Thus, it was unsurprising that the conceptual analysis taken for granted for water power was assumed to apply also to its rival. In water power, whether it be a water-wheel or a column-of-water engine [41, pp. 85–88, 183–185] in which an engine is driven by the weight or pressure of a column of water, it is water that is the source of the work produced by the system, but, of course, it would not be suggested that water is ‘converted’ to work! Rather,
in the course of its passage through the system, the ‘state’ of the water is changed and it is that change that is responsible for the production of work.

Similarly, it was taken for granted that it was the passage of heat from high temperature to low that caused the production of work in the stream engine, but it was certainly not supposed that heat was, in any sense, converted to work. One may speak of a ‘fall’ of heat from ‘high intensity’ to ‘low intensity’, or from a state of ‘concentration’ to one of ‘diffusion’ [6, p. 243]. Thus, a change in the nature of the heat could be responsible for the production of work, but it was not to be assumed that there was a simple relationship between the amount of heat, its fall in temperature, and the amount or work produced. We shall see how James Thomson in particular produced a detailed analysis along these lines in his Belfast notebooks.

While conservation of heat is certainly the crucial idea in the theory of caloric, it may be expressed in an even more potent way. This is that we may talk of the ‘amount of heat’ in a system, or say that, in today’s terms, heat is a ‘state function’. This implies that, for a gas, for example, we may take it round cycles which may be as complicated as we wish, containing, for example, adiabatic or isothermal expansions or compressions, processes at constant volume or constant pressure, and so on. However, we must assume that if the system is returned to the same state at the end of the cycle as it had at the beginning, the total amount of heat lost is the same as the total amount gained, so the amount of heat ‘in the system’ at the end is the same as that at the beginning.

Carnot [16, p. 19], for example writes that:

We tacitly assume in our demonstration, that when a body has experienced any changes, and when after a certain number of transformations it returns to precisely its original state, that is, to the state considered in respect to density, to temperature, to mode of aggregation – let us suppose, I say, that this body is found to contain the same quantity of heat that it contained at first, or else that the quantities of heat absorbed or set free in these different transformations are exactly compensated. This fact has never been called into question. It was first admitted without reflection, and verified afterwards in many cases by experiments with the calorimeter. To deny it would be to overthrow the whole theory of heat of which it serves as a basis.

William Thomson was to quote this remark in his 1849 paper in which he explained his dilemmas at that time, dilemmas which would be resolved over the next two years.

Similarly, Truesdell [42, p. 85] writes that: Carnot introduces a heat function $H_C(V, \theta)$, the quantity of a heat in a fluid body, to within an additive constant. (There must be an additive constant because, like several other conserved quantities in physics, it is only differences in the function that may be discussed or measured, not absolute values.) Truesdell goes on to say that in his book (or ‘tragicomedy’ as he calls it), the term Caloric Theory will refer to the existence of a heat function, not to broader ideas. It must be said, incidentally, that Truesdell appears to have put his own gloss on what Carnot actually said.

Smith and Wise [6, pp. 315–316] stress that William Thomson could not have thought of heat as a substance; even when he still supported the theory of caloric, as late as the spring of 1850, he had no problem in accepting the conversion of work to heat. Rather they say that
he treated heat as a ‘state function’, implying that no heat could be converted into work in a cyclic process.

Overall then, we shall regard the idea of heat as a state function, or the right to talk of a heat function or total heat of a system as the fundamental postulate of the caloric theory. We shall see that, in his crucial paper announcing the new thermodynamics, Clausius [16] spent much effort in showing that it is this idea that much be sacrificed in the new theory.

Earlier we looked at what we called kinetic theories and clearly these theories were in strong contrast with those caloric theories in which heat consists of particles or a fluid. However, they may now be seen to be good examples of our broader classification of caloric theories, based on conservation of heat and heat being a state function. For it is trivial to construct theories in which the *vis viva* or, in today’s terms, the total kinetic energy is conserved, and may certainly be regarded as a state function.

Thus, it is clear that while the physical picture presented by those kinetic theories based on experiments such as those of Rumford and Davy are superficially analogous to today’s dynamical theory of heat, they may miss the essential point that, in the modern theories, heat is not a state function. In the following sections, we shall follow how ideas of caloric were central to the extremely important work of Sadi Carnot and Clapeyron, and the early work of the Thomson brothers, but did not apply in the new theory of thermodynamics produced in the middle of the nineteenth century.

First though we make one last point. We said that in the caloric theories, the quantity of latent heat was available to explain discrepancies in the caloric bookkeeping. Clearly, this was a somewhat different concept from that of latent heat used in changes of phase today. The latter is defined in our dynamical theory in terms of energy transmitted rather than energy stored. Nevertheless, it may be said that terms such as ‘latent heat’, ‘specific heat’ and ‘thermal capacity’, though hallowed by time, may be somewhat misleading and more appropriate for a caloric type of theory.

### 6 The theory of Sadi Carnot and its reception by the Thomsons

Sadi Carnot’s theory, published in his *Reflections on the Motive Power of Fire* [16], which was centred round his so-called Carnot cycle, was a triumph of the use of fairly abstract and general argument to make crucially important statements, which on the one hand were directly applicable to such practical objects as steam engines, but which on the other could be extended to encompass the widest range of phenomena involving heat and work. The theory was clearly pre-thermodynamic in that it was based on the theory of caloric, and was, in any case, produced in 1824, just over a quarter of a century before the emergence of thermodynamics. Yet, as well as having a direct importance for those designing air-engines and steam engines, such as the young James Thomson, it played a crucial part in the development of thermodynamics itself. With some adaptation, in particular to meet the demise of the caloric theory, it remains a crucial part of any account of the subject today, and may be said to have stimulated, fairly specifically, the production of the all-important second law. (William Thomson, for example, was to ascribe the second law to Carnot and Clausius [6, p. 329].)
It may be mentioned that Carnot, who was born in 1796 the son of Lazare Carnot, studied at the famous École Polytechnique, which was a training school for army engineers [16]. For much of his short life he served as an army officer, but, with the bewilderingly rapid changes in French politics in the period, Sadi’s social status oscillated dramatically according to that of his famous father, whose fate was intimately tied in with the rising and plummeting fortune of Napoleon himself. Frustrated by this, he retired from the army on half-pay in 1820, and after much further study wrote his famous treatise. Subsequently, he had a brief spell back in the army, and produced further writing, some aspects of which will be referred to briefly later in this section, but he was to die from cholera in 1832. As Mendoza [16, p. xiv] points out, by a strange coincidence another young French genius, the mathematician, Evariste Galois (1811–1832), had died just a few days before.

Joseph Larmor has described Carnot’s work as ‘perhaps the most original in physical science’. For a reader of today, it is, at first sight, difficult to appreciate the full significance of Carnot’s achievement, but this is only because so many of his insights have become the central components of thermodynamics today, and thus are easy to take for granted.

His first insight was the necessity to consider a *cycle*, in which, for a gas, successive changes of temperature, pressure and volume would take place, but the final state of the system would be identical to its initial state. Clearly, this approach was able to model actual engines working through many cycles. Equally importantly from a theoretical point of view, it focussed attention on the crucial question of what quantities should be regarded as state functions and so would have a value that must clearly be the same at the end of each cycle. We saw in the previous section that these ideas became the best definition of a caloric theory, and so were to become the best description of its eventual failure. Carnot, as we have said, was a believer in the caloric theory, so, of course, for him it is the heat of the system that must have the same value at the beginning and end of each cycle. His terminology [16, p. 8] is that: ‘[I]n steam-engines the motive-power [or work] is due to a re-establishment of equilibrium in the caloric.’

His cycle, described for a gas, consisted of two isothermal stages, (12) and (34) in Fig. 15, in which the pressure and volume change; the stage (12) corresponds to a higher temperature than (34). In a steam engine, the temperatures are those of the boiler and the condenser, respectively;

![Figure 15: The Carnot cycle.](image-url)
it should be emphasised that, for all his theoretical and conceptual powers, Carnot was entranced by practical engines. In a more abstract approach, these isothermals may be regarded as ‘thermal reservoirs’ of effectively infinite extent, so that they may transfer heat to objects with which they are in thermal contact, without their own temperature changing.

The isothermals are joined by two adiabatic stages, (23) and (41), in which no flow of heat takes place between the gas and its surroundings but there are changes in the temperature, pressure and volume of the gas. The gas is expanding and thus doing work in the stages (12) and (23), while in stages (34) and (41), work is done on the gas. However, from the point of view of work, these stages do not cancel each other out; overall, net work is done by the gas, and the amount is equal to the volume inside the cycle in Fig. 14. Physically, we may say that this is because the expansion is performed at higher pressures than the contraction; alternatively, we may say that the gas expands at higher temperatures than those at which it contracts.

Heat flows into the gas in the stage (12) and out at (34). Our current theory, of course, tells us that the flow in is greater than that out, the balance being taken up by the work done by the system, all according to conservation of energy. For Carnot, though, all the heat gained in the stage (12) must be lost in stage (34) as heat is conserved. Nevertheless, there are a number of crucial insights obtained by Carnot. First is the point that the very requirement for a cycle of this nature implies that there must be two reservoirs, a cold one as well as a hot one, and that at least some heat must be deposited in the cold one. Once today’s dynamical theory is understood, Carnot’s argument is trivially translated into our second law of thermodynamics, in which not all heat taken in by the system from the hot reservoir may be transformed into useful work; some must be wasted by being deposited into the cold reservoir. (This is usually called the Kelvin statement of the second law.)

Another central point in today’s thermodynamics is that, as mentioned earlier, for maximum efficiency, there must be no heat flow between systems at different temperatures, that is to say no conduction of heat. Carnot expresses this fact in the following way: ‘The necessary condition of the maximum is, then, that in the bodies employed to realize the motive power of heat there should not occur any change of temperature which may not be due to a change in volume’ [italics in original]. Carnot brilliantly achieves this condition by separating the changes of temperature in stages (23) and (41), which as has been said, are achieved by changes of volume and pressure and not by flow of heat, from stages (12) and (34), where there is flow of heat but no change of temperature. Heat flow is always between systems at the same temperature, or to be precise one may say that an infinitesimal temperature difference must be allowed to cause the actual flow.

Carnot introduces another central feature of today’s thermodynamics in his discussion of maximising the effectiveness of such an engine – that of reversibility. His own cycle is reversible, and he puts together a simple but brilliant argument to show that no cycle can be more effective than his own. For that let there be a more effective one. Then, it may be used to run a Carnot cycle backwards. But this hypothetical engine, being more efficient than the Carnot cycle, will produce more power than is required to do so, or in other words, a net gain of work will be produced, so that the two machines together are acting as a perpetual motion machine, invariably regarded as impossible in principle. Therefore, our assumption of an engine more effective than that of Carnot must be incorrect.
It may be noted that in the previous paragraph the word ‘effectiveness’ has been used rather than ‘efficiency’ as the latter would seem to be less appropriate in a theory based on the ideas of caloric than in our present one, in which one may naturally think of the amount of work done as a fraction of the heat provided. Of course, though, the ideas translate immediately to our present ones when one changes from a caloric theory to a dynamical theory of heat.

Once we have reached the idea of a maximum effectiveness, it is clear that this will be independent of the type of engine being considered. This important fact is stated by Carnot as follows: ‘The motive power of heat is independent of the agents to realize it; its quantity is fixed solely by the temperatures of the bodies between which is effected, finally, the transfer of the caloric.’

Carnot’s ability to come to more detailed conclusions, like that of his followers until at least the middle of the century, was limited, not just because he was using the caloric theory, but because a detailed understanding of the behaviour of gases was only developing along with the basic ideas of thermodynamics. Carnot, Clapeyron and the Thomsons all include substantial sections in their workbooks and papers struggling with the laws of Mariotte (or Boyle for English people of today!) and Gay-Lussac, and the experimental results of workers such as Dulong and Petit. Most of those involved specialised almost entirely to the case of an ideal gas, but even so the going was difficult, and it was really only with William Thomson’s definition of an absolute temperature following from the idea of the Carnot cycle (as adapted for the dynamical theory of heat) that there was a completely rigorous basis for working with gases. (See the end of Section 2 of the companion paper to this one [43].)

It is abundantly clear to us today, though, that Carnot had grappled with and considerably illuminated issues of the deepest scientific and technical significance. Sadly it was far from obvious to the engineers and scientists of the today. His book received one excellent review in the Revue Encyclopédique but was otherwise ignored; probably, it was too abstract and conceptual for the practising engineer, but its subject was too applied for most scientists and mathematicians.

For the remainder of his life, Carnot continued to ponder on these ideas, and it is clear that his belief in the caloric theory faded considerably. Mendoza [16, p. 46] suggests that this process had actually begun before the actual publication of the Reflections. At one point, while in the manuscript he wrote: ‘The fundamental law that we proposed to confirm seems to us to have been placed beyond doubt, both by the reasoning which served to establish it, and by the calculations which have just been made.’ In the published version, however, these words are replaced by: ‘The fundamental law that we proposed to confirm seems to us to require, however, in order to be placed beyond doubt, new verifications. It is based upon the theory of heat as it is understood today, and it should be said that this foundation does not appear to be of unquestionable solidity. New experiments alone can decide the question.’

In his later manuscripts which were published posthumously, he went much further. He wrote [16, p. 67]: ‘Heat is simply motive power which has changed its form. It is a movement among the particles of bodies. Wherever there is destruction of motive power, there is at the same time production of heat in quantity exactly proportional to the quantity of motive power destroyed,’ and he suggested many experiments to check these ideas, including repeating Rumford’s experiment, but measuring the motive power consumed as well as the heat produced. However,
Carnot’s sad and untimely death prevented any possible substantive work along these lines being performed.

The content of Carnot’s Reflections was saved from total obscurity only through the labours of Émile Clapeyron [16], another graduate of the École Polytechnique, who took it upon himself to present Carnot’s results in a way more accessible to academic physicists and mathematicians. Carnot’s work had been non-mathematical, but that of Clapeyron was mathematical throughout. Also Carnot’s work had not used any diagrams, but Clapeyron presented the Carnot cycle on the so-called indicator diagram, or graph of pressure against volume as we have shown in Fig. 15, which gave a very direct impression of what was involved. Even so, Clapeyron’s work, which was published in 1834, received very little attention until it was translated into German in 1843 [16, p. xv]. (It had already been translated into English in 1837 [6, p. 289].)

It appears to have been shortly after the publication of the German translation that news of the ideas of Carnot and Clapeyron reached the Thomson brothers, though it may well have been the English translation that they studied. In a letter to William written in August 1844, James, at that time working at the Fairbairn shipyard, inquired who it was that had proved that a definite quantity of mechanical effort was given out in the passage of heat from one body to another [6, p. 289–290]. It was clearly the work of Carnot and Clapeyron that he was referring to, and James showed that he already had a very good grasp of the issues.

In this letter, James showed himself already highly familiar with the analogy between the water-wheel and the heat engine, which had been heavily used by Carnot and Clapeyron – indeed Smith and Wise [6, p. 841] call it ‘the water-wheel analogy of Sadi Carnot’. James, in fact, introduced a new practical point of analogy. In principle, the water should fall from as high a temperature as possible, almost, as James says, at the source of the river; similarly the heat should ‘fall’ from as high a temperature as possible, and from that point of view it was a pity that the boiler in a steam engine was at a much lower temperature than the actual burning fuel.

In each case, there was a drawback to this ideal maximal situation; for the water case, tributary streams providing water at a lower level would be missed, while, for the steam engine, most of the heat would pass up the chimney and likewise be lost. As we shall see, James would continue to study the Carnot–Clapeyron argument for several years, analysing both its fundamental concepts, and its practical possibilities and difficulties. It will be remembered that it was in this period that, as a semi-invalid in Glasgow, James invented his vortex turbine, the purpose of which was explicitly to maximise the power produced by the device.

Over the next few years, the brothers became steadily more interested and, indeed captivated, by Carnot’s argument. While he was in Paris in 1845, William searched in vain for a copy of Carnot’s original treatise. For the Paris book-sellers, ‘Carnot’ meant either Lazare, Sadi’s father, or Hippolyte, his brother, author of a book on social studies. However, the brothers were able to locate and read Clapeyron’s paper in the original French, and became convinced further of its very great merit. James commented that it was ‘a very beautiful piece of reasoning, and of course perfectly satisfactory’, while William said that ‘nothing in the whole range of Natural Philosophy is more remarkable than the establishment of general laws by such a process of reasoning’. William was not to obtain a copy of Carnot’s original work until late 1848 when he received a copy from Gordon. It was then that he prepared his first important paper on thermodynamics.
in which he gave a detailed account of Carnot’s theory [44]. Rather amusingly, when Clausius presented his own exceptionally significant paper [16] in 1850, he had to admit that he had not yet seen Carnot’s paper and was basing his comments on that paper on the descriptions of the paper provided in the works of Clapeyron and Thomson. This explains, at least in part, Cardwell’s suggestion that Clausius could be regarded as ‘almost a disciple, at one remove, of Kelvin’; much of Clausius’ highly admirable work was based on the profound analysis of William Thomson.

James became so enthusiastic about Carnot’s work that even earlier in 1848 he had commenced the production of an astounding piece of work which we shall call the Belfast notebooks. These contained a considerable amount of information about his own air-engine, which was very much a result of Carnot’s ideas, study of the Stirling engine, the first calculations of the lowering of the freezing point under pressure [1], and, perhaps most important of all from our point of view, important analysis of what would soon be called thermodynamics. This seminal work is discussed in Ref. [43].

Here we shall study the ideas of James Joule and the response to them of the Thomson brothers. As has been said, from the time of their discovery of the work of Carnot and Clapeyron, the brothers were convinced that it should be a crucial component of the sought-after understanding of the relations between heat and work or, as we may say, the future thermodynamics. Yet, they also became steadily, if rather unwillingly, convinced that Joule’s work must also provide part of the final understanding, despite the fact that, as they saw it, it was inherently contradictory to that of Carnot.

7 The work of Joule, and the response of James and William Thomson

We saw earlier to what extent the caloric theory had made itself invulnerable to criticism. One important aspect of the theory was the conservation of heat or caloric, but the willingness to include the concept of ‘latent caloric’ [38] meant that it must be immensely difficult to disprove the theory. One might inquire whether it was falsifiable in a Popperian sense [45]. The most natural answer may be that the work of Joule, the Thomsons and Clausius did not actually falsify the caloric theory, but did show that the dynamical theory of heat and the idea of conservation of energy provided a more attractive explanation for the wide range of phenomena studied.

Before we describe the work of Joule himself, we should refer to Julius Mayer, who, as a result of his writings in the 1840s, is sometimes credited with priority over Joule in the idea of the mechanical equivalent of heat. His claim was particularly asserted in the early 1860s by John Tyndall, always an opponent of what Crosbie Smith [9, Chapter 9] calls the North British grouping centred around William Thomson, and including his brother, Joule, Rankine, Tait and the rather younger Maxwell. Smith regards this grouping as keen to assert their own priority in the establishment of what he calls The Science of Energy [9], which may be taken to include not only thermodynamics but also, for example, electromagnetism. We do not need to judge the significance of Mayer here, since we are studying specifically the development of the ideas of the Thomsons, and it is certain that their inspiration was Joule and not Mayer.

In view of the difficulty mentioned above of understanding the nature of heat by study of its use in engines of different types in producing work, it is interesting that Joule first approached the question from other angles [9, Chapter 4]. His first significant result, published in 1840 and still
important today, was that the heat produced by an electric current was proportional to $i^2$ and to $R$, where $i$ and $R$ are the current and resistance, respectively, of the circuit. Today, this heating effect is still known as 'Joule heat'.

Joule also studied the amount of work that might be produced by an electromagnetic engine, and moved on, by 1843, to study of electrochemistry. He was able to show that, in the electrolysis of water, all the ‘caloric’ produced could be traced back precisely to chemical changes. The use of the word ‘caloric’ suggested that he was not at the point willing to oppose the caloric theory, but clearly he was at the very least extremely close to a claim of ‘conservation of energy’. He noted, for example, that both the mechanical and the heating effects of a current were proportional to the electromotive force, suggesting a relationship between these types of effect. He also remarked that if an electrical generator is inserted into an electrical circuit, there would be a decrease in the heat evolved by the circuit, precisely equal to the mechanical effort produced by the generator.

Soon afterwards he was able to show that an electrical generator was indeed a generator of heat as well as electricity, and suggested that heat should be viewed, not as a ‘substance’ but as a ‘state of vibration’, claiming, in this belief, that he was a follower of Rumford. Very importantly, he introduced a ‘mechanical value of heat’, making it clear his belief that heat was transformed to work, not just related in some way to it, and, on the basis of his experiments, he was able to come up with a range of results for this value. Later experiments included measuring the heat produced by compressing a gas, the heat lost when it expands, and the heat produced by paddle-wheels rotating in water. In all cases, there was a broad though not particularly convincing agreement in the inferred value for the mechanical value of heat. It will be appreciated incidentally that, from the point of view of this paper, the actual physical nature of heat is much less important than the statement that it is not conserved.

Another interesting experiment was to allow a gas to flow into a vacuum, when Joule believed that there should be no increase in temperature because no work was done. This is today known as the Joule effect. Joule’s expectation of no change in temperature is not quite borne out because the gases are not exactly ideal. In the next few years, Joule and William were to carry out a long series of experiments investigating a related effect, in which a gas moves from one pressure to another through a porous plug. Today, this is called the Joule–Kelvin or Joule–Thomson effect.

It may be noted that, even at this date, Joule was aware of the Clapeyron paper, and, recognising that Carnot’s ideas conflicted substantially with his own, he took the opportunity to criticise them severely. Carnot, he said, stated that the fall of the heat from the temperature of the boiler to that of the condenser was responsible for the production of work. But in that case, the substantial fall of the heat from the temperature of the hot fuel to that of the boiler presented severe difficulties; the work or vis viva as Joule calls it, that could have been produced by this fall, appeared to have been lost. ‘Believing that the power to destroy belongs to the Creator alone’, Joule felt that Carnot’s theory must be erroneous [9, p. 69]. He wrote that: ‘I conceive that this theory, however ingenious, is opposed to the recognized principles of philosophy, because it leads to the conclusion that vis viva may be destroyed by an improper disposition of the apparatus [6, p. 306].’ It will be noted that this objection was identical to the worries that the Thomson brothers had shared at Rugby some years before.
For several years, Joule’s claims attracted very little attention. From the point of view of the scientific community, he was something of an outsider, coming from a brewing background. Many of the temperature increases he measured were so small as to be unconvincing, and his measured values for the mechanical value of heat covered quite a wide range. More important, perhaps, was the fact that most scientists, very much including the Thomson brothers, found it difficult to move away from the caloric theory with its conservation of heat and its water–wheel analogy. So several of Joule’s most important papers were rejected by the Physical Transactions of the Royal Society, being published instead in the Philosophical Magazine. Even in the June 1947 meeting of the British Association or the Advancement of Science (BAAS) in Oxford, he was only allowed to give an abbreviated presentation of his paper.

However, it was at this meeting that his work attracted the attention of William Thomson, whose support, limited and almost grudging as it was at first, was crucial in, as Joule later put it, rescuing him from obscurity [6, p. 304], and assuring the eventual general acceptance of his views, and his high place in scientific history. Also important was the interest of James Thomson, who was immediately informed of the content of Joule’s paper by William, and also of George Stokes, William’s close friend and correspondent.

The (extremely) positive component of William’s response to Joule concerned the transformation of work to heat, in particular via fluid friction; William and Stokes were greatly concerned with the theory of hydrodynamics, and Joule’s ideas and his experiments appeared to have the capacity to make an important contribution to that subject. He later said that he became convinced that ‘Joule had certainly a great truth and a great discovery, and a most important measurement [6, p. 302]’.

However, there were also very negative components of William’s response, mostly, at least in retrospect, explained by William’s adherence, instinctive as much as reasoned, to the idea of caloric, but there was also the point that Joule’s ideas, certainly correct in themselves, missed one aspect of Carnot’s work – directionality. As William was later to express it, Joule gave science the first law of thermodynamics, but the second was produced by Carnot as elaborated by Clausius.

During Joule’s 1847 BAAS talk, William’s main disagreement with the speaker came with Joule’s statement that the work required to warm water must be proportional to the quantity of heat involved in the process, an obvious result of the assumption of a mechanical value of heat. William was sure, on the basis of Carnot’s work, that it must rather be proportional to the square of the quantity of heat.

Smith and Wise [6, pp. 302–304] suggest that William’s confidence stemmed from the belief that, when applying heat to a body to increase its temperature, one is both increasing the amount of heat ‘in the body’ and raising the temperature of the heat – very much a notion inspired by the caloric theory. The situation is at least broadly analogous to the study of the work done against gravity in building a wall steadily higher and higher, the work in this case is definitely proportional to the square of the height of the wall. Similarly, the work done in heating the body, on the Carnot–Thomson argument, must be proportional to the square of the temperature difference, or the square of the heat produced. A beautiful picture of this idea was given by John Dalton,
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a great believer in the caloric theory, in a book published in 1808 and included here as Fig. 16. (Incidentally and ironically, Joule was a disciple of Dalton, both being from Manchester, but Joule’s attack on caloric commenced in the period in which Dalton died.)

After Joule’s talk, William had fruitful discussions with him and was given copies of several of his published papers. After the meeting, he wrote to his father that he felt Joule was ‘wrong in many of his ideas, but he seems to have discovered some facts of extreme importance, as for instance that heat is developed by the friction of fluids in motion [6, p. 302]’. He promised to write to James about Joule’s ideas and to send him a set of Joule’s papers. This is yet again evidence that James was at the very least an equal partner in the exchange of ideas between the brothers.

In his letter to James, William [2, p. xxviii] said that

I enclose Joule’s papers which will astonish you. I have only had time to glance through them as yet. I think at present that some great flaws must be found. Look especially to the rarefaction and condensation of air, where something is decidedly neglected, in estimating the total change effected, in some of the cases. Keep all the papers carefully together and give them to me when I return.

In reply, James did indeed raise some issues over Joule’s analysis of his experiment on the free expansion of gases. The difference of opinion seems to have been a result of implicit differences in assumptions. In the experiment, the temperature of a vessel at first rose but, by the end of the experiment, had returned to its original value. For Joule, since there was no overall increase in temperature, there could have been no expenditure of work; for James, though, the raising of the temperature of the caloric of itself required the expenditure of work.
However, James was extremely interested in Joule’s general position:

Some of his views have a slight tendency to unsettle one’s mind as to the accuracy of Clapeyron’s principles. If some of the heat can absolutely be turned into mechanical effect, Clapeyron may be wrong. I think, however, that before coming to a conclusion, we would need to define more accurately what we mean by *a certain quantity of heat* as applied to two bodies at different temperatures. Perhaps Joule would say that if a hot pound of water lose a degree of heat to a cold one, the cold one may receive a greater absolute amount of heat than that lost by the hot one; the increase being due to the mechanical effect which might have been produced during the fall of heat from the high temperature to the low one.

James’ suggestion made at the end of the letter has been fleshed out by Smith and Wise [6, pp. 307–309] and by Smith [9, pp. 79–80]. It retains Carnot’s conservation of caloric in so far as, for the Carnot cycle case, the heat, $Q$, deposited in the cold reservoir is equal to that gained from the hot reservoir; work $W$ is also done. However, for the case of conduction of heat directly from hot to cold reservoir, the mechanical work that could have been obtained in the Carnot cycle case is not lost but is effectively transformed to heat, and the heat deposited in the cold reservoir is equal to $Q + W$. (In adding an amount of heat to an amount of work, we are, of course, taking advantage of Joule’s mechanical value of heat.)

From the comfort of today’s hindsight, James’s suggestion may seem a clumsy attempt to splice the ideas of Carnot and Joule. $W$ comes from a fall in caloric, rather than being a transformation of caloric, but if it does not arrive at all, conservation appears to dictate that fresh caloric appears instead. However, the real significance of the remarks is to show how extremely keen both brothers were to bring together the seemingly contrasting ideas of Carnot and Joule in a single theory.

Over the next year or so, this struggle continued. On the one hand, William carried out the kind of experiments that Joule had pioneered – using a paddle-wheel to heat water; indeed, in what might be described as typical Kelvin fashion, he was determined to go much further than Joule and actually boil the water [6, p. 308]! However he and James still had difficulties with both theories. Carnot could not handle the conduction case in anything like a satisfactory way. In one sense, Joule could; heat that was not transformed into work could just remain as heat. But for Joule, conservation of energy implied that there was no reason why that heat could not be transformed to work; for William, heat was an end-product or waste-product of any process, and he could not believe useful work could be obtained from it.

Within a few years, he would realise that low temperature heat is an end-product of a reversible cycle such as the Carnot cycle; since the cycle *is* reversible, it must be possible to transform this heat back to work in a straightforward way by running the engine backwards. However, where there is dissipation, the corresponding process must be irreversible and extra heat is produced. Waste, or we may say increase in entropy, is necessarily involved in this process. But for the moment, William could not see his way to solving the problem.

In 1848, when he finally received his copy of Carnot’s original treatise, William wrote an account of Carnot’s theory for the Royal Society of Edinburgh [44]; he had long promised this...
to James Forbes, Professor of Natural Philosophy at the University of Edinburgh. In this paper he gave an account of Carnot’s theory, providing in addition experimental support from the important experiments of Victor Regnault on the behaviour of steam. He referred to ‘Carnot’s fundamental principle’ whereby ‘the quantity of heat … discharged during a complete revolution (or double stroke) of the engine must be precisely equal to that which enters the water of the boiler’, commenting that: ‘So generally is Carnot’s principle tacitly admitted as an axiom, that its application in this case has never, as far as I am aware, been questioned by practical engineers [44, p. 117].’

In a footnote in this paper, William praised the contributions of Joule and mentions that Joule himself had ‘strongly urged’ that one should abandon Carnot’s fundamental axiom. However, William responded that:

If we do so, however, we meet with innumerable other difficulties – insuperable without farther experimental investigation, and an entire reconstruction of the theory of heat from its foundation. It is in reality to experiment that we must look – either for a verification of Carnot’s axiom, and an explanation of the difficulty [as described above here] we have been considering; or for an entirely new basis of the Theory of Heat.

William and James had made great progress together, but still faced very real difficulties in reaching a theory that would satisfy all their requirements.

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