Chapter 3

James and William Thomson: the creation of thermodynamics

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Abstract
In his Belfast notebooks, James Thomson demonstrated deep thinking about the nature of heat and work, and his analysis of the lowering of the freezing point of water under pressure was a crucial step towards a complete theory. Throughout this period, James and William Thomson worked together, but when this theory emerged as thermodynamics, it was credited to William, and to Rudolf Clausius and MacQuorn Rankine. James’s claim to be added to the list of founders of the theory is discussed.

1 James Thomson and the Belfast notebooks: the air engine

Enthused by many aspects of the Carnot theory, in April 1848 James had started what we shall call the Belfast notebooks [1]. These consist of three fairly substantial exercise books, crammed with a considerable variety of investigations into aspects of Carnot’s theorem, with the overall title Motive Power of Heat. It will be remembered that, at this period of his life, James had returned to Glasgow because of ill-health, but was working on a variety of projects. On the front cover of Book A is a note that James attached in September 1863, which says: ‘This book contains, at page 58, under date May 1848, my original manuscript investigation of the Amount of the Lowering of the freezing Point of Water by Pressure’.

On the inside cover of Book A, James has written for reference the basic laws of gases, essentially the laws for the ideal gas, another reminder that in this period these were still far from standard. Indeed, the correction between the Centigrade scale and the air thermometer scale is originally given as 267°, later replaced by 273° as more accurate results became available.

Book 1 starts with a full account of the contents of the Clapeyron paper, titled Air or Gas Engine Imagined by Carnot: Preparatory Investigation. James starts by giving what he calls an important formula combining the laws of Mariotte and Gay Lussac, which James writes as $PV = P_0V_0 (1 + Et)$, again essentially the ideal gas law. He then works through the Carnot theory in considerable mathematical detail. He is working within the caloric theory and makes a point of arguing explicitly that the heat given out to the cold reservoir is equal to that gained from the hot reservoir. James’s isothermal processes are actually taken to be infinitely close together, and so are his adiabatics, as was standard for both Carnot and Clapeyron. Thus, James says that each pair ‘must be parallel, being portions of curves lying infinitely near one another’.
James then calculates the total work gained as $\frac{dV}{V}(P_0 V_0 ET)$, and says: ‘Now this work has been obtained by the passage from $t$ to $t-T$ of a quantity of heat which we have denoted by $dQ$ and is the quantity absorbed or evolved by the gas when with a constant temperature $t$ or $t-T$ it alters its volume by $dV’$. The other quantities, including $E$ are as in the equation above. A little analysis leads him to the result that the work gained from $H$ units of caloric is given by $\frac{1}{C}HT$

where $\frac{1}{C} = \frac{EP_0 V_0}{V dQ dV}$. C, he says, is a ‘remarkable quantity, to be determined experimentally, constant for all gases but varying with the temperature’.

In this way, James has made the Carnot theory very much his own, and, of course, he is extremely interested in it as a fundamental set of ideas of tremendous importance. However, the notebooks make it absolutely clear that, as an engineer, and indeed as an engineer who was particularly interested in the question of efficiency and waste at both the theoretical and the practical levels, and as an engineer who must have been extremely keen to make some money and pay his way at home, he also clearly regarded it as a technical challenge. Carnot had sketched in a very general way the means of maximising the amount of work obtained from a given amount of fuel; James decided to design an engine at least broadly along these lines. Much of the content of his notebooks consists of detailed descriptions of the engine as it evolved, together with comprehensive diagrams of practically draughtsman standard. It was to be an air-engine.

The significance of the choice of air-engine is as follows. In his paper based on the work of Carnot [2], Clapeyron had stressed that, for maximum efficiency of any engine, the work produced should correspond to maximum possible fall of temperature of the vapour. This implied that the lowest temperature reached by the vapour in the condenser should be as near as possible to that of the atmosphere. More importantly, though, it implied that its highest temperature should be as high as possible, and that the fact that the temperature of the boiler was between $1000^\circ C$ and $2000^\circ C$ below that of the furnace corresponded to a major waste of heat.

It would be dangerous to increase the boiler temperature of the steam engine because of the high pressures that would be produced and the corresponding risk of explosion. It was, therefore, clear, then, that an air-engine might in principle be much more efficient than a steam engine because it could utilise a much greater fall of temperature. These ideas had been mentioned by Carnot, and in his 1849 paper, Kelvin [3] also had a short section titled ‘Comparison of the relative advantages of the air-engine and the steam-engine’. He commented that: ‘A “perfect air-engine” would be a much more valuable instrument than a “perfect steam-engine”, but admitted that: ‘Neither steam-engines nor air-engines, however, are nearly perfect; and we do not know in which of the two actual kinds of machine the nearest approach to perfection may be actually attained’.

Kelvin went on to mention that: ‘The beautiful engine of Mr Stirling of Galston may be considered as an excellent beginning for the air-engine’, though obviously capable of much further improvement. Rev. Robert Stirling (Fig. 1), who was a Presbyterian minister from the town of Galston in Ayrshire, 20 miles from Glasgow, had taken out his first patent for an air-engine as early as 1816, and only 2 years later an engine that he had built was used for pumping water.
in a quarry. The engine incorporated an ‘economizer’ or ‘regenerator’ designed to make use of heat that would otherwise be wasted. This idea was independently invented by John Ericsson (Fig. 2) in the 1830s [4, p. 296], and a range of Ericsson engines, very similar to Stirling engines have been designed and built. Robert Stirling and his brother James, who was a civil engineer, produced further patents in 1827 and 1840, and an engine of 45 horsepower was used in James’s factory in Dundee [5, p. 48]. The reason for using this engine was to avoid the disastrous consequences of an explosion of a steam-engine, all too common at that period. However, the Stirling engine itself failed three times in 4 years and unfortunately had to be replaced with a steam engine.

In the late 1820s, the Stirling brothers had presented a model Stirling engine to the Natural Philosophy Department at Glasgow University, and in 1847, this engine was discovered by William Thomson in a store crammed with old and unwanted equipment, clogged with dust and oil. William immediately set about having it cleaned and put into a working condition.

The principle of the Stirling engine (Fig. 3) is very simple. It works with a fixed mass of gas and can use any source of heat. Today’s engines may use, for example, solar power, waste heat or nuclear energy; a table-top demonstration version uses a candle or the heat from a coffee cup. The engine has a sealed cylinder with one part hot and one part cold and the working gas is moved from the hot side to the cold side. When the gas is on the hot side, it expands and pushes up on a piston; when it is on the cold side, it contracts and pulls down the piston. The job of the displacer in Fig. 3 is to move the air from the hot side to the cold side as the engine runs. The regenerator is a temporary heat store – a lump of matter arranged so that the working fluid
passes through it first in one direction and then in the other; it enables heat to be retained in the system that would otherwise be lost to the environment. (It may be mentioned that some Stirling engines today have an extra piston rather than the displacer; this makes them effectively equivalent to the Ericsson engine.)

We shall return to Robert Stirling and his engine shortly. For the moment we may say that by 1847, Lewis Gordon was attempting to set up a substantial company for the manufacture of the Stirling engine. Smith says that: ‘Over the next few years, Joule, Rankine and [Robert] Napier (Fig. 4) would all take up the challenge set by Carnot’s theory to find a workable air-engine which could bring fame and fortune to its designer [5, p. 95]’. James Thomson also took up that challenge.
On 24 April 1848, he wrote that

I have just found out a new air engine which leaves nothing farther to be wished for in the theory of the action of air, gas or vapour, in developing motive power from a fall of heat from the highest temperature which a vessel (without rubbing parts) will bear to as low a temperature as we have at command ... I have been led to the new air engine from having seen a fundamental fault theoretical and practical in Stirling's air engine, and from having observed that Ericsson's Air Engine is subject to the same objection, as well as having found the fundamental difference between the theory of Carnot's imaginary engine and these two real ones.

The following day, James wrote that:

Until this week I had for a long time (more than a year) seen various faults in Stirling's Air Engine ... But I could not see in every respect the mode by which the motive power was obtained from the fall, or rather I failed in all attempts to make it out to be an apparatus, not perfect in operation indeed, but still an apparatus for putting in practice the ideal action required in Carnot's imaginary engine. It was only yesterday, however, that I found that the principle of Stirling's and Ericsson's is fundamentally different from Carnot's. I then was able to alter this principle in such a way as to produce a new imaginary engine quite distinct from Carnot's, but perfect as well as it; one namely which would utilise all the fall of heat supplied at one temperature and removed at another. Then this new ideal engine has the character that, with the materials we possess, an apparatus can be constructed which will practically almost quite fulfil all the conditions required by the theory of the ideal engine. I have often tried to adopt an apparatus to the four processes of Carnot's but have failed.

James went to considerable effort to understand, establish and explain the differences between his own engine, and the proposed one of Carnot on the one hand, and those of Stirling and Ericsson on the other. In part, this may have been for his own understanding, but it is quite obvious that he hoped to patent his proposed engine, and for that purpose he would have to demonstrate the differences between his own engine, and the published one of Carnot, and the patented ones of Stirling and Ericsson.

It will be noted that the Stirling cycle follows Carnot in using two isothermal processes, but replaces the adiabatic processes of Carnot by isochoric processes – processes at constant volume. In the latter, heat does, of course, flow under a difference of temperature in conflict with the Carnot criterion; the regenerator was included to minimise not only heat loss but also this dissipation. In theory, the Stirling cycle was still capable of providing an efficiency very little reduced from that of Carnot, but in practice materials were not available to allow the high temperatures required to take full advantage of the fall in temperature of the caloric, and limitations of design meant that efficiencies were very low.

James considered that the Stirling engine was better than that of Ericsson; his criticism of both engines focused around the fraction given by volume of the cylinder through which the displacer moved relative to that of the entire volume of gas used. If the fraction is large, then it is not possible to build up a significant increase of pressure, because only a small part of the
volume of gas will become hot, and there will also be large heat losses from the heat passing round the displacer to the air around it. This loss could be reduced if the strokes were quick enough, but then there would be insufficient time for the heat to pass from the air to the water. On the other hand, if the fraction is small enough to avoid these problems, then the work done at each stroke will be very small. James’s summing-up of the Stirling engine was that:

In almost every respect in Stirling’s air engine it turns out that if we make the best adaptations for any one condition we make the worst for some other or others and what is of great importance when we take a middle course we violate all the conditions greatly.

In contrast, James considered that: ‘In mine the best adaptations in various respects can be made at the same time’.

James also stressed that his engine was different from that of Carnot. On 25 October 1848, he included a slip of paper titled: ‘Paper showing that the engines imagined by Carnot and used by him for assisting in his investigations as regarding the Motive Power of Heat are different in principle from mine’. He drew a detailed diagram comparing the two schemes, with his own engine in red and Carnot’s in black. James, in fact, replaces Carnot’s adiabatics by isobaric processes – processes at constant pressure. He comments that:

If Carnot were to use an infinite number of infinitely small engines to perform the work represented [by James’s figure], still every one of these engines will be different in principle from mine … So I do not at present admit that my engine is a practical apparatus for working out Carnot’s principles when taken to embrace even their widest range.

James then stressed the most important difference between his scheme and that of Carnot:

Carnot reduces the temperature by expanding while I reduce it by abstracting caloric in such a way that it may be restored … If in any of Carnot’s investigations he goes on the supposition that the lines for II and IV [which for Carnot are adiabatics] may be of any form whatever, then these investigations will comprehend my own.

In James’s engine, the main change from Stirling was that the displacer was made to follow the motion of the piston; in his description, rather than the term ‘displacer’; in fact, he uses the term ‘plunger. In later versions of his design, the piston has become conical, and he uses the work ‘cap’. He writes that:

The fundamental principle then which I adopt is to retain the pressure constant during the stroke of the plunger by permitting a proper change of volume at that period, and to obtain the necessary change of pressure on the piston not by heating and cooling of the air due to the motion of the plunger, but by the operation of Mariotte’s Law, the cylinder being made so that the plunger can follow the piston and this expel all the air from the cylinder to the hot part there.

**process 1** Take in heat by raising plunger. Volume to more than double, but raise plunger along with piston so as to keep all air in hot place. Pressure follows Mariotte’s Law.

**process 2** Let down plunger and allow volume to diminish by descent of piston [cap] pressure remaining constant but the whole air being transformed from hot to cold.
process 3 Push down piston [cap] pressure following Mariotte’s Law heat being given out at the lower temperature.

process 4 Raise plunger the piston rising at the same time (but more slowly) to retain the pressure constant. The process ends when plunger by overtaking piston has put all the air from cold to hot.

‘By keeping pressure constant and making the plunger conical’, he writes, ‘the action of the regenerator is rendered theoretically perfect and in practice almost perfect’.

Actually in May 1848, James met Robert Stirling in person. James warned Stirling that he was working on similar ideas himself, so asked him not to report anything that might be regarded as confidential. Stirling did say that he was making improvements to his engine but was quite relaxed in his conversation, remarking that anything he might say was already in the public domain. James, though, took care to have witnesses: his father, William and William’s classroom assistant, Robert Mansell. Fortunately or unfortunately, though, James was to realise that he had little to gain from Stirling’s cooperation, but equally little to fear from his competition. James wrote in his notebook that: ‘I found that, as I had previously thought, he does not understand his own engine; not knowing at all the way in which the heat is expended in generating work’.

For a start, Stirling did not appear to appreciate the significance of the changes of temperature produced by changes in pressure. In today’s theory, these would be explained by the fact that work would have been done either on or by the gas, but at the time they would have been described as a change between latent and sensible caloric. More particularly, Stirling had convinced himself that though heat might be lost at some stages of his cycle, an average might be struck, so that through the complete cycle, and if friction and similar effects might be ignored, all the heat might be retained.

James pointed out that there must be some loss of heat, or some flow of heat between furnace and condenser, as otherwise he would be in possession of a perpetual motion machine. Even this did not worry Robert Stirling unduly; surely, he said, if friction is ignored, perpetual motion is not out of the question. James replied that that is the case, but such machines cannot be perpetual sources of power! It may be noted that with our current understanding of conservation of energy, anyone holding Stirling’s belief that a certain quantity of heat could provide essentially an infinite amount of work would certainly be regarded as invincibly ignorant. Such a belief was not so ridiculous for believers in caloric, for whom, in the production of work, heat was not transformed but merely changed from a state of ‘concentration’ to one of ‘diffusion’. It was, perhaps, by no means out of the question that the regenerator could reverse this change in the caloric, so that it could be used to produce more work.

The exchange must have convinced James that he and his brother were probably in possession of far more understanding of the theoretical and experimental facts than all but a few people in the world, and they realised that it was certainly very worthwhile continuing their discussions and James continuing his attempts to design and build a useful air-engine.

James’s design work continued for several years. Sections of the book were marked as written at the Causeway Head and at Cupar, Fife, so the work must have continued through family holidays. At Blair Logie, he had written a section on ‘Calculations and Data to the Power of the Engine’. Finally, on 7 March 1850, he wrote: ‘I have just finished the drawing of the air engine began in 1848’.
Certainly, the work must have benefitted James immensely in terms of his understanding of the science involved, and in experience in engineering design. It certainly helped him in the important work to be described in the next section. Sadly, though, from the point of view of actually building the engine, and possibly making a great deal of money, all the effort seems to have been abortive.

Smith [5, pp. 157–165] tells the immediate story of the air-engine project in detail. In 1852, John Ericsson was in the course of powering a ship of nearly 2000 t, The Caloric Ship Ericsson (Fig. 5) using an enormous Ericsson engine, which had perhaps the largest piston ever built. When he heard about this ship, Rankine approached the industrialist, Napier, and together they planned a strategy involving basic engineering science to be carried on by Rankine, with engine construction, market know-how and capital to be provided by Napier. The use of isochoric processes backed up with the regenerator, rather than adiabatic processes, meant that the volume of the engine could be small, as required for operation on a ship; this should have been a massive advantage over Ericsson’s own huge engine, and also much less heat would be wasted than on Stirling’s scheme. By 1853, all seemed set fair; contracts had been exchanged both for a non-functioning model, to be built by James White, the Glasgow instrument-maker, who worked so closely with William Thomson for many years, and for an experimental working air engine.

It was not to be. By 1854, White had not delivered the model, and the experimental engine did not yet work. The Ericsson had a sea trial in which the engine ran well, but it seemed to be under-powered. Then some time after the trial, the ship was hit by a tornado coming out of a cloudless sky, and it sank. It was raised, but only for its air-engine to be removed and replaced by a steam engine. For at least the foreseeable future, it was the end for air-engines, and James Thomson’s stood very little chance even of gaining attention.

Over the subsequent years, Stirling engines have had a chequered history. Through the first half of the nineteenth century, as steam power became safer and cheaper, they found it difficult to compete. However, in the second half of the century, they were often used when a reliable source of low-to-medium power was required, such as raising water or providing air for church
organs. They were inefficient, but, unlike a steam engine, they could be operated by anybody who could light and control a fire. Then during most of the twentieth century, they were unable to compete with electric motors and small internal combustion engines, but they have recently experienced a revival in interest because of increased fuel prices and environmental concerns. Stirling engines are compatible with all fuel sources, including renewables, and applications today include space-based astronautics, and generation of electricity from solar energy, biomass and domestic refuse. It should be mentioned that the Stirling engines of today are by no means always air-engines; a variety of different gases are used.

2 The Belfast notebooks: lowering of the freezing point of water

The credit for the important discovery of the lowering of the freezing point of water under pressure is normally allocated to James Thomson [6] for the theoretical part and William for the experimental part. Since the issue is open to some debate, we shall leave that till later in this section and concentrate for the moment on the physics. The issue emerged during discussion between the brothers on the means of operation of the Stirling engine, but in fact it was a direct application of the Carnot paper itself. In fact, both Carnot and Clapeyron, as well as dealing with the general gas case we have discussed so far, dealt with the case of phase change, but it was specifically William Thomson who argued as follows [4, pp. 296–298; 6].

Let us consider a Stirling engine, with the usual lower hot reservoir and upper cold reservoir, but to which no heat is applied; we also assume that there are no losses due to friction. Essentially the engine would work opposite to the usual way; the hot reservoir would cool and the cold reservoir would heat up until their temperatures were equal; the engine would be turned but without expenditure of work. If we wished to continue this process, to cause a situation in which the upper reservoir is hotter than the lower one, we would need to provide work; we would at this point essentially be taking advantage of the reversibility of the engine by working it in reverse. If, however, we keep both reservoirs at the melting point of ice, we may keep a stream of water running across the upper part of the engine and immerse the lower part in a basin of water at the same temperature. By turning the engine forwards, heat would constantly be taken from the lower region and deposited in the stream above, so the water in the basin would be converted gradually to ice, but no work would be required.

We may express the situation in terms coming more directly from Carnot’s work. We may have a Carnot cycle with both isothermals at the melting point of water. Heat will be systematically moved from lower to upper reservoir, but because there is no difference in temperature between the reservoirs, no work will be required.

At this point, though, one or both of the brothers (to be discussed later) saw the following objection to the scheme. In fact, since ice is less dense than water, water expands when it freezes, and clearly this expansion could be used to produce useful work. Thus, in operating the engine as above, there would be a net gain of work and possibly a perpetual motion machine could be constructed.

The only answer to this dilemma that the brothers could come up with was that, in fact, the temperature in the two isothermal processes could not be the same. It will be appreciated that if the freezing water does external work, this must come from work done to turn the engine. This necessitates the pressure of the gas being higher when it is expanding than when it is being
compressed. For net work to be performed by the person turning the engine, and hence by the engine itself in order to freeze the water, the Carnot principle tells us that the temperature must have been lower during the expansion at higher pressure than during the contraction at lower pressure. In other words pressure must lower the freezing point of water. One may obtain the same result directly from a Carnot cycle; to produce net work, a Carnot cycle must be between two different temperatures.

It is certain that it was James who then performed the necessary calculations in the Belfast notebooks in May 1848. In the model he used, as usual he followed four processes, using a cylinder that contains ice at 0°C. In the first, the cylinder is put into contact with an infinitely large lake at 0°C, and the motion of the piston has the effect of melting enough ice in the cylinder to produce a cubic foot of water. In this process, heat is gained from the lake. In the second process, the cylinder is removed from the lake, and the piston is pushed down a little further until the pressure is increased by a quantity \( p \). During the process the temperature decreases; at any moment it is equal to that corresponding to the instantaneous pressure; at the end of the process, it is \(-t^°C\).

In the third process, the cylinder is in contact with a second infinitely large lake at a temperature of \(-t^°C\). During this process, the pressure and temperature remain constant, and the water increases its volume by freezing. Then, in the fourth process, the cylinder is removed from the lake and the piston is moved back to the position that it occupied at the beginning of the first process.

The work produced in the process could be calculated in a straightforward way in terms of \( p \), but using figures from experiments of Regnault, James could also calculate the work produced from knowledge of the fall in temperature \( t^°C \). He produced the formula \( t = 1/138 \, n \), where \( t \) is the lowering of the freezing point in degrees centigrade and \( n \) is the pressure above atmospheric pressure in atmospheres. For an excess pressure of 1 atmosphere, clearly the lowering of the freezing point is around 0.0073°C, an extremely small quantity to attempt to measure.

There then followed the publication by James of the famous paper in the Transactions of the Royal Society of Edinburgh [6]. This paper had been communicated to the Society on 2 January 1849. The calculations in the paper follow those in the notebooks quite closely, though with very minor changes in data and results, and James also provided a full explanation of the ideas involved. Having demonstrated the origin of the effect in general terms, but before providing the calculations, James commented that:

The fact of the lowering of the freezing point being thus demonstrated, it becomes desirable, in the next place, to find what is the freezing point of water for any given pressure. The most obvious way to determine this would be by direct experiment with freezing water. I have not, however, made any attempt to do so in this way. The variation to be appreciated is extremely small, so small in fact as to afford sufficient reason for its existence never having been observed by any experimenter. Even to detect its existence, much more to arrive at its exact amount by direct experiment, would require very delicate apparatus which would not be easily planned out or procured.

It is for this reason, he contented himself with the calculations.
This makes even more memorable the fact that by the end of 1849, William Thomson had constructed the necessary apparatus and performed the experiment with great success. William, it may be remarked, had only been in position as a professor of natural philosophy (Physics) for around 3 years, previously having been mostly regarded as a mathematician with very little practical experience. Robert Mansell constructed an ether thermometer for him [5, p. 95], which was, William assured his class at Glasgow, ‘the most delicate that ever there was made, there being 71 divisions [and between 2 and 3 inches] in a single degree of Fahrenheit’ [4, p. 282]. At first, not surprisingly, there were great problems – it was difficult to compare the thermometer with any other. However, before the end of the year, he was triumphant. He told his class:

A pressure of 18 atmospheres was applied and the temperature was lowered by $17 \frac{1}{2}$ of the divisions, 0.246 was the lowering according to experiment; 0.2295 according to theory … Under a pressure of 8.1 atmospheres the temperature was lowered $7 \frac{1}{2}$ divisions. 0.106 is the lowering by experiment; 0.109 by theory. This is within 1/3000 of a Fahrenheit degree [4, p. 282].

William mentioned James’s calculations in his paper on Carnot [3]. His own experimental results were read to the Royal Society of Edinburgh in January 1850 and published in 1851 [7].

Clearly, the work of the Thomson brothers had been exceptionally important and successful. As well as providing theoretical and experimental knowledge of great significance in its own right, it was, or at the very least appeared to be, a triumphant confirmation of the truth of the Carnot argument. Certainly, it would now be difficult to deny that at least the main thrust of Carnot’s argument had been validated. Still, though, the brothers felt that at least some parts of Joule’s argument had also to be included in a final synthesis, but for the moment they were unable to see their way through to a final resolution.

Let us, though, briefly return to the question of the division of credit between the brothers. It is quite clear, of course, that the credit for the experimental work goes to William, that for the calculations to James. It has been usual to give the credit for the very idea that there is a reduction in the freezing point to James also. This is natural in any case, as the fact was announced in his paper together with the calculations.

To study the situation in more detail, we note that, at the start of this paper, he made it clear that William had given the argument for freezing without the necessity to provide work, as published earlier [8]. He then remarked that: ‘This at first appeared to me to involve an impossibility …’, proceeding to the argument already given in this paper that, because of the expansion of water on freezing, it seemed that work could actually be produced by the process. He then says that: ‘To avoid the absurdity of supposing that mechanical work could be got out of nothing, it occurred to me further that is necessary farther to conclude that the freezing point becomes lower as the pressure to which the water is subjected is increased [italics in original]’.

This seems to establish that the idea itself is due to James. However, Smith and Wise [4, p. 298] have drawn attention to a letter William wrote to Forbes in December 1848 in which he appeared to claim credit for himself. He wrote that: ‘In conversing with my brother James about this proposition, it struck me that we may prove as a consequence that the freezing point of water under heavy pressure must be lower than 32’. Smith and Wise suggest three explanations for
the seeming contradiction: ‘(i) that William allowed James the credit in the published paper as a form of recompense for his elder brother’s other disappointments in health and career and as an acknowledgement of his guidance in the subject; or (ii) that William had simply given an erroneous account to Forbes; or (iii) that the idea was so much the product of both minds that a separation of their contributions was impossible’.

William could indeed have been generous, but James was a man of the greatest integrity, and it seems unlikely that he would have explicitly claimed individual credit that he knew he was not due. It seems likely that (iii) is nearest the truth, that in the course of their discussions the idea reached each of their minds independently. William was then delegated to do the experiments, James to do the detailed calculations and it naturally followed that James would go on to write the paper giving the account of the idea. It is thus suggested that both brothers were telling nothing but the truth – as, indeed, one would expect.

James may thus have to share the credit for this important discovery with his brother, but it is clear that, over the entire course of their work, his contribution was immense. Another contribution was to follow. As was common in these days, his paper [6] was reprinted in the Cambridge and Dublin Mathematical Journal in November 1850, but for this republication he introduced a small but important change.

In the first version, he stated, broadly following Carnot and Clapeyron, that the third process may be described as follows: ‘Continue the motion till all the heat has been given out to the second lake at \(-t^\circ\), which was taken in during [Process 1] from the first lake at 0\(^\circ\). This follows the usual description of the Carnot process in terms of a caloric theory: all the heat gained by the system from the hot reservoir in the first isothermal process is deposited in the cold reservoir in the second isothermal process.

As a footnote in this original version, he says that:

This step, as well as the corresponding ones in Carnot’s investigations, it must be observed, involves difficult questions, which cannot as yet be satisfactorily answered, regarding the possibility of the absolute formation or destruction of heat as an equivalent for the destruction or formation of other agencies, such as mechanical work; but in taking it, I go on the almost universally adopted supposition of the perfect conservation of heat.

However, by the time the paper was reprinted, he had come to an important decision. In his description of the third process, he inserted the words:

Continue the motion till so much heat has been given out to the second lake at \(-t^\circ\), as that if the whole mass contained in the cylinder were allowed to return to its original volume without any introduction or abstraction of heat, it would assume its original temperature and pressure. This, if Carnot’s principles be admitted, as they are supposed to be throughout the present investigation, is the same as to say, – Continue the motion … [continuing as in the first version]

Thus, James avoids the assumption of conservation of heat by continuing the third process until we reach a point where the fourth process will take us back to the original state of the system.
That James’s change was looked on as extremely important is seen from the fact that, when William prepared his Carnot paper [3] for publication in his collected papers (MPP), he inserted a note of 5 November 1881 in his description of the third process: ‘The specification of this operation … is the only item in which Carnot’s temporary and provisional assumption of the materiality of heat has effect. To exclude this hypothesis, Prof. James Thomson gave the following corrected specification for the third operation [continuing in analogy at James’s words above …]’ [3 MPP, pp. 122–123, also 127–128].

Truesdell’s opinion [10, p. 86] may be noted, that, with this change James was actually correcting not Carnot but William, but, from any point of view, we see yet again how highly William regarded James’s sustained contributions to thermodynamics. However, we conclude this section with another application of Carnot’s ideas, this time due to William alone.

Through the latter part of the eighteenth century and the early part of the nineteenth, a vast amount of work was performed on thermometry, with particular attention being paid to the heating of gases [11]. By mid-nineteenth century, it was fair to conclude that the gas thermometer was the most satisfactory, because the behaviour of gases when heated fell into a number of distinct classes, depending, as we would recognise today, on the atomicity of the particular gas. In retrospect again, we would be clear that the behaviour of ideal gases was particularly simple, and could, in principle, be used as the basis of an absolute scale of temperature. In practice, though, no gas would be ideal, different gases would behave differently when heated, and so the gas thermometer was extremely useful but could not be an absolute one.

By 1848, when William Thomson had assimilated the ideas of Carnot, he realised that it would be possible to use a Carnot cycle to define an absolute temperature scale. At this stage, of course, William was working within a caloric theory, so his account of his ideas [12] was incorrect in detail in modern terms, but the basic idea certainly worked. He wrote that:

> The characteristic property of the scale which I now propose is, that all degrees have the same value; that is, that a unit of heat descending from a body \( A \) at the temperature \( T^o \) of this scale, to a body \( B \) at the temperature \( (T-1)^o \), would give out the same mechanical effect, whatever be the number \( T \). This may justly be termed an absolute temperature scale, since its characteristic is quite independent of the physical properties of any specific substance.

The argument was indeed perfectly satisfactory, but an awkward feature is that the scale does not approximate to any gas scale. We are not here implying that gas scales should be correct; the
point of the argument is that they are not correct and should be compared to an absolute scale. But we would like them to be nearly correct. This may be achieved with a different definition of an absolute scale, as later discussed by William in his magisterial account of the new theory of thermodynamics, written between 1851 and 1855, and collected together in MPP [12].

Here (MPP, p. 235) he wrote that: ‘The temperatures of two bodies are proportional to the quantities of heat, respectively, taken in and given out in localities at one temperature and at the other, respectively, by a material system subjected to a complete cycle of perfectly reversible thermodynamic operations’. To explain the idea simply, in modern thermodynamics we assume absolute temperatures, $T_h$ and $T_c$ for the temperatures of the hot and cold reservoirs of a Carnot engine, and deduce that the quantities of heat taken in at the hot reservoir and deposited in the cold reservoir, $Q_h$ and $Q_c$, respectively, are related by $\frac{Q_c}{Q_h} = \frac{T_c}{T_h}$.

What William Thomson does is essentially the opposite process to this. He measures $Q_h$ and $Q_c$, and then defines $T_h$ and $T_c$ by using the equation $\frac{T_c}{T_h} = \frac{Q_c}{Q_h}$. With one suitably chosen fixed point – today we would use the triple point of water, an absolute scale has been set up, independent of the properties of any substance. However, the scale is identical to the (hypothetical) ideal gas thermometer, so it will be close to any thermometer using a real gas.

To return to 1848, it was clear at that point how useful the Carnot cycle could be, but still Joule’s ideas could not be ignored. In the next section, we shall explore a short section of the Belfast notebooks where James made what may in retrospect seem a rather desperate attempt to reconcile the two without giving up the idea of caloric.

3 The Belfast notebooks: reconciling Carnot and Joule

On 20 May 1848, James sketched in his notebook an extraordinary attempt to reconcile Carnot and Joule by an analogy with mechanics. [This section of his notes has already been included in the paper of Smith [13].] James presented two columns, one containing mechanical terms, the other terms related to the relationship between heat, temperature and work. As well as momentum, the mechanical side stressed what we would now call kinetic energy, though that term had not yet been coined, and James still called it (half) *vis viva*.

James stressed that momentum, or mass multiplied by velocity, which he also called quantity of motion, is not lost in impact [in a collision], but some *vis viva*, in general, is lost [except in an elastic collision]. ‘During the passage of a given quantity of motion from a velocity $V$ to a velocity $v$', James said, ‘work is given out’. ‘Work’, he spelled out, ‘is equivalent to force multiplied by space, equivalent of $\frac{1}{2}$ *vis viva*, equivalent to $\frac{1}{2}$ mass of body multiplied by velocity$^2$’. So: ‘By impact or mutual frictions: *vis viva* is lost, but quantity of motion is not lost. $\frac{1}{2}$ *vis viva* is locked up in velocity’.

He then established his analogy as follows. The equivalent of the conserved quantity of motion, or momentum, is the conserved quantity of caloric. James says that the quantity of motion in a mass or set of masses is equal to the momentum that would be given out in stopping ‘so as to be at rest with reference to a thing regarded as stationary’. By analogy, the quantity of caloric in a set of masses is ‘the caloric given out in descending to 0°C’.
James explicitly stated that the analogy of mass is capacity [which is what we today would call thermal capacity], and the equivalent of velocity is temperature. Thus, just as the quantity of motion is mass multiplied by velocity, the quantity of caloric is [or is proportional to] capacity multiplied by temperature.

To any believer in a caloric theory, that is to say, anybody who believes that one may talk of the heat of a set of masses, these equations are little more than obvious. However, to a modern reader no less than to James Joule, the analogy between caloric and momentum must seem dangerous in the extreme; caloric or heat, we and Joule will say, should be equivalent to energy, and might better be the analogy of vis viva than of momentum.

However, James was able to attain some formal consistency in his next step. We saw earlier, for example in Fig. 2, that on a caloric theory, the motive power or work produced is proportional not to temperature but to its square. Therefore, James wrote that motive power or work is equivalent to half the capacity of the body multiplied by the square of its temperature, directly analogous to the mechanical quantity we had earlier, half the mass of the body multiplied by square of its velocity.

James has said that the motive power on the mechanical side of the analogy is known as vis viva. By analogy, he gave the name of the motive power on what we may call the thermodynamic side as vis calida. Just as in the passage of a given quantity of motion from one velocity to another, work is given out, so, in the passage of a given quantity of heat from a temperature $T$ to a temperature $t$, work is also given out, and in the two cases, the work is equivalent to $\frac{1}{2}$ vis viva and $\frac{1}{2}$ vis calida. Just as vis viva may be lost but quantity of motion or momentum is not lost, so ‘by conduction or radiation: vis calida is lost, but quantity of heat is not lost’. Just as $\frac{1}{2}$ vis viva is work locked up in velocity, so ‘$\frac{1}{2}$ vis calida is work locked up in temperature’.

At this point in the argument, James was ready to claim that it does more than just establish an analogy: he has shown, he suggests, that vis viva and vis calida are mutually convertible. Both are equivalent to work, or we may say, may be transformed into work. He considered that his argument was an authoritative statement of the position of William and himself, and continued by stating, based on this argument, ‘the question at issue between Joule and ourselves’. The question posed is ‘Is a certain quantity of heat equivalent to a certain vis viva the two being mutually convertible?’ This was certainly Joule’s position – work could be transformed to heat, and vice versa.

For James, though, since vis viva and vis calida were equivalent, Joule’s position was that a certain quantity of heat was equivalent to and convertible to a certain vis calida. But for James this implied a contradiction. He had defined the quantity of heat as the product of capacity and temperature, but vis calida as $\frac{1}{2}$ the product of capacity and the square of the temperature. Clearly, it was completely impossible that the two could be equivalent, as, ignoring the factor of $\frac{1}{2}$, James wrote ‘quantity of heat = vis calida/temperature’. This was an argument, he said, that ‘a certain vis calida/temperature [should be] equivalent and convertible to vis calida, which appears to be nonsense’, as he rightly says. He added in brackets: ‘Is my reasoning good?’

James immediately turned to a consideration of Davy’s experiment in which had he melted two pieces of ice by their mutual friction, which suggested that work or vis calida actually produced a quantity of heat, and thus very much supported Joule against the position reached in James’s
notes; as he said ‘[It] would be incompatible with my view of the subject’. He asked himself in brackets: ‘Is the experiment to be trusted?’

Ironically, within a few years, when William had fully accepted a dynamical theory of heat as an important component of his theoretical construction of thermodynamics, he was to acknowledge Davy’s work in particular, even more than that of Rumford, as that which had established the theory.

As to James’s own analysis, one may delight in its boldness and its inventiveness. From a modern perspective, it has the difficulty that seems inevitable in a caloric theory: the work obtained is proportional not to the temperature difference through which the heat has dropped, but to its square. It will be remembered that, in Williams’ view, the ‘blunder’ in Joule’s presentation at the BAAS was to disagree with this rule, and exactly the same factor is involved in James’s difference with Joule in these notes. In retrospect, the cure for the problems faced by the brothers was to dispense neither with Carnot nor with Joule, but to bring them together and to expel caloric.

4 The denouement

By the spring of 1850, the Thomson brothers and Joule had been circling the problem of heat and work for a number of years. The Thomsons had shown themselves willing to question the caloric theory, with its conservation of heat and its upholding of heat as a state function, but they were seemingly unable to move effectively beyond it, and thus were not able to discuss waste or dissipation in a useful way. Joule had moved beyond caloric in his full acceptance of what we would today call conservation of energy, but was still unable to understand fully what happened to energy that seemed to have been wasted. It was in this spring that Joule wrote to William [4, p. 316] saying that:

[A]s your brother said, there ought to be some connecting link between the results I have arrived at and those deduced from Carnot’s theory. Perhaps you will succeed before long in discovering it. For my own part it quite baffles me.

However, this honour was not to go to William but to Rudolf Clausius (Fig. 6), then only 28 years old. Clifford Truesdell was not one to shower compliments on the creators of thermodynamics. He describes William’s paper on Carnot [3] as ‘enthusiastic, rambling, and sometimes vague [10, p. 168]’ and his 1851 paper [14] as ‘verbose and rambling [10, p. 224]’. Of Clausius, he asserts that: ‘Few mathematical physicists have shown so little sense of the right mathematics for the job [10, p. 206]’. So we must take Truesdell very seriously when he [10, p. 204] says that:

There is no doubt that Clausius with this paper created classical thermodynamics. Compared with his work here, all preceding except Carnot’s is of small moment. Clausius exhibits here the quality of a great discoverer: to retain from his predecessors major and minor – in this case, from Laplace, Poisson, Carnot, Mayer, Holtzmann, Helmholtz, and Kelvin – what is sound while frankly discarding the rest, to unite disparate theories, and by one simple if drastic change to construct a complete theory that is new yet firmly based upon previous partial successes.
Clausius’s paper (included in Ref. [2]), published in April 1850, was titled ‘On the motive power and on the laws which are deducible from it for the theory of heat’, and the ‘simple if drastic change’ was to reject the conservation of heat, and to replace it by the equivalence of work and heat.

Clausius gave great credit to William Thomson for his analysis [3] of the state of the subject at the end of the 1840s, and his explanation of the problems that he discussed. Indeed, Clausius admitted that his knowledge of the work of Carnot was only through the accounts given by Clapeyron and by William Thomson. Clausius made the following comments on Carnot’s work:

[Carnot] says expressly that no heat is lost in the process, but that the \textit{quantity of heat} remains unchanged, and adds: ‘This fact is not doubted; it was assumed at first without investigation, and then established in many cases by calorimetric measurements. To deny it would overthrow the whole theory of heat, of which it is the foundation’. I am not aware, however, that it is has been sufficiently proved by experiments that no loss of heat occurs when work is done; it may, perhaps, on the contrary, be asserted with more correctness that even if such a loss has not been proved directly, it has yet been shown by other facts to be not only admissible, but highly probable.

To this end, Clausius discussed the work of Joule, in which, he said: ‘Heat is produced in several different ways by the application of mechanical work’. These experiments, he said, ‘have almost certainly proved … the law that the quantity of heat developed is proportional to the work expended in the operation’. Various experiments, he said, had suggested that heat was not a
substance, but consisted in the motion of ‘the least parts of bodies’, so Clausius argued that such a motion may be transformed to work, the loss of *vis viva* being proportional to the work accomplished.

Having supported Joule, Clausius then gave an eloquent account of William’s difficulties in reconciling his work with that of Carnot:

[Thomson] speaks of the obstacles which lie in the way of the unrestricted assumption of Carnot’s theory, calling special attention to the researches of Joule, and also raises a fundamental objection which may be made against it. Though it may be true in the case of the production of work, when the working body has returned to the same condition as at first, that heat passes from a warmer to a colder body, yet on the other hand it is not generally true that whenever heat is transferred work is done. Heat can be transferred by simple conduction, and in all such cases, if the mere transfer of heat were the true equivalent of work, there would be a loss of working power in Nature which is hardly conceivable. Nevertheless, he concludes that in the present state of the science, the principle adopted by Carnot is still to be taken as the most probable basis for an investigation of the motive power of heat, saying: ‘If we abandon this principle, we meet with innumerable other difficulties – insuperable without further experimental investigation – and an entire reconstruction of the theory of heat from its foundations’.

However, Clausius then suggested that we should not be daunted by these difficulties, but rather familiarise ourselves with the consequences of the idea that heat is a motion. Indeed, he actually did not feel the difficulties were as serious as William Thomson had suggested. Carnot’s theory did not have to be discarded, a step which Clausius said, in agreement with William, we would find difficult to take, since it has been amply verified by experiment. Clausius’s new method, he claimed, was not in contradiction to the essential principle of Carnot, only to the subsidiary statement that no heat is lost. Clausius then summed up his new theory, very much the basis of today’s thermodynamics, as follows:

It may well be the case that at the same time a certain quantity of heat is consumed and another quantity transferred from a hotter to a colder body, and both quantities of heat stand in a definite relation to the work that is done.

Clausius also introduced two other central components of thermodynamics. The first, which he stated explicitly, was, in the terms we have been using, that heat was not a state variable:

It is common to speak of the *total heat* of bodies, especially of gases and vapours, by which term is understood the sum of the free and latent heat, and to assume that this is a quantity dependent only on the actual conditions of the body considered, so that, if all other physical properties, its temperature, its density, etc., are known, the total heat contained in it is totally determined. This assumption, however, is no longer admissible if our principle is adopted.

Clausius gave detailed examples of how the concept of total heat broke down. If, for example, a gas moved from one physical state to another along different routes on a *PV*-diagram, then in general different amount of work would be done on or by the gas along the different routes.
It follows then also that different amounts of heat will have been lost by or given to the gas during the two processes, so the idea that the gas ‘contains’ specific amounts of heat at the beginning and at the end of the process is untenable. (Kelvin had actually recently made similar remarks [4, pp. 312–314] but had not quite known how to follow through his ideas.) Of ‘latent heat’, Clausius wrote that: ‘[It] is not merely, as its name implies, concealed from our perception, but it is nowhere present; it is consumed during the changes in doing work’.

Clausius showed, though, that there was an important new state function. When heat is provided to a system, he said that, in his terminology at the time, two types of work may be done. The first is what we today would just call ‘work’, though Clausius called it ‘external work’; this may be a gas expanding or a liquid being vapourised and having to push back the external pressure to make room for itself. This clearly depends on the external pressure, so ‘work’ for us, ‘external work’ for Clausius, is, like heat, not a state function. However, what Clausius called ‘internal work’ related to overcoming the mutual attraction of the particles, and so was independent of pressure and was a state function. Today it would be called ‘internal energy’, and with our increased knowledge of the kinetic theory of gases, we would wish to make it clearer than Clausius that it included what we would call kinetic energy as well as potential energy. For an ideal gas, of course, the potential energy is zero, and so internal energy is solely kinetic energy.

Ever since Clausius, the quantity has been given the symbol \( U \). Clausius said explicitly that it is a function of volume and temperature, ‘and of being therefore fully determined by the initial and final conditions of the gas, between which the transformation has taken place’. His paper, in fact, included the equation:

\[
dQ = dU + A R \frac{a + t}{v} dv
\]

where \( A \) is the heat equivalent of work, \( R \) is the gas constant, \( a = 273 \) and \( t \) is the temperature in degree centigrade. This is our present first law of thermodynamics for the case of an ideal gas. (Here, \( U \) is clearly to be considered in units of heat.) Clausius’s adaptation of Carnot’s work essentially also provided the second law.

If we should say that Clausius had laid lasting foundations for classical thermodynamics, we must admit that there was still much for Macquorn Rankine (Fig. 7) and William Thomson to do. In fact, even before Clausius’s work had been published, Rankine, on the basis of rather elaborate and perhaps unconvincing models involving the motion of atmospheric particles forming vortices around atomic nuclei, had produced detailed results quite analogous to many of those of Clausius. William Thomson was to have a stimulating debate with Rankine through 1850 and into 1851 [5, pp. 102–106].

Thomson knew of Clausius’s theory as early as August 1950, and indeed discussed it, at least in general terms, with Rankine from that time. However, he was to write, in an early draft of his important paper of 1851 [4, p. 324]:

The same conclusion has been arrived at by Clausius, to whom the merit of having first enunciated and demonstrated it is due. It is with no wish to claim priority that the author of the present paper states that more than a year ago he had gone through all the fundamental investigations depending on it which are at present laid before the Royal Society.
By February 1851, it is clear that he felt he had reconciled Carnot and Joule in his own mind, and was persuaded by Joule to lose no time in writing an account of his new theory. Thus were written the first three parts of what would eventually become the great paper of 159 pages eventually put together and published in Thomson’s collected papers [14]. (The title used in the collected papers is the title of these first three parts.) The fourth and fifth parts followed in 1851 and the sixth, which dealt with thermoelectric currents, an area where Thomson made exceedingly important discoveries, in particular, the so-called Thomson effect, in 1854. The seventh, on thermoelastic and thermomagnetic properties of matter, was mostly written in 1855, though major additions were made for the collected papers. Lastly, appendices in the version included in the collected papers had originally been published in 1851, 1853 and 1854.

Here, we may concentrate on just a few notions from the beginning of this paper. It should incidentally be made clear that, like his 1849 paper [3], he regarded it as less a statement of a new discovery, more a general coherent account of the significance of the new theory, and a full working out of the conclusions that might be drawn from it. Thus, he acknowledged the contributions of Rankine and Clausius at the outset. He then spelled out what he called two propositions. The first, which he attributed to Joule, was that whenever heat is produced from thermal sources, or lost in thermal effects, equal amounts of heat are put out of existence or generated. He, thus, completely abandoned the caloric theory, accepting Joule’s ideas in their entirety, and becoming perhaps the main advocate from that moment of what he called the dynamical theory of heat. In this role, he was to celebrate the memory of the work of Davy and Rumford half a century before; why, he asked, had scientists largely ignored their proofs of the dynamical nature of heat? (Thomas Kuhn [15] made the fair rejoinder that Kelvin himself had been using the caloric theory only a year earlier!)

The second proposition, attributed by William to Carnot and Clausius, states that the most efficient engine acting between particular temperatures of source and refrigerator is a reversible one. William expressed this proposition in his own terms a little later in the paper: ‘It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects’. This became his famous statement of the second law of thermodynamics.

So far he had broadly repeated the concepts of Clausius, and to an extent, of Rankine. But the difference between them was that Clausius’s interests were rather narrow, being largely restricted to the theory of heat engines [5]; it might be said that his paper was an admittedly brilliant but technical solution to a technical problem. Rankine too was a comparative newcomer to the topic. William, in contrast, had spent the best part of a decade worrying practically incessantly about the conceptual and cosmological significance of the problems he had been considering. With the solutions now understood in principle, he had much to say about their implications. He began in the remainder of this paper, and continued for the rest of his life.

First, he became the apostle for the new idea of energy. Until that time, physics had been constructed around the Newtonian idea of force, which was immensely useful in mechanics but not so useful elsewhere in physics. With the understanding that heat, light, sound, electricity and magnetism could all be expressed in terms of energy, with the full appreciation of what we now call kinetic and potential energy, and paying regard to the idea of transformations between the various types of energy, it became clear, at least to William, that all the various areas in physics could very fruitfully be discussed in the new paradigm of energy. The treatise he and Tait wrote
together [16] is a celebration of this new belief, and today their evangelism has been so successful that it is almost regarded as tautological to describe physics as the science of energy.

Even more significant was William’s long concern, together with James, with the question of waste. What for Clausius was little more than a logical explanation of what happened when heat could have produced work but had failed to do so – extra heat was deposited in the cold reservoir, for William became the solution to his central conceptual problems, and the key to his new worldview. This heat was not lost in the material world, thus satisfying William’s demand that only God could create or destroy. Nevertheless, this energy is ‘lost to man irrecoverably’. Thus, William’s worldview was one of dissipation and irreversibility, with an arrow of time leading to the so-called heat death, where everything is at the same temperature, and any interesting features in the universe have been lost. It was a most beautiful solution to the worries that William and James had shared over many years, and this major conceptual development played a deservedly large part in building up William’s towering reputation through the second half of the nineteenth century [17].

We may only sketch the subsequent history of thermodynamics. For many years after 1851, Clausius showed little interest in the idea of energy, but in the 1860s he turned his thoughts to such general matters, and then, around 1865, introduced the idea of ‘entropy’. Entropy remains constant in a reversible process but increases in an irreversible one; indeed, such is the content of one of the most useful statements of the second law of thermodynamics.

However, it was at about this time that Tait, who had taken on the self-appointed task of William Thomson’s bulldog, apparently decided that William’s initial support of Clausius had been premature and unwise. It is true that William himself felt that Clausius’s own proofs of some of the most fundamental portions of thermodynamics had been unsatisfactory, in particular being restricted to the case of ideal gases; he felt that his own proofs were the first satisfactory and general ones. Tait [18] took the process much further, arguing consistently against Clausius’s priority for thermodynamics, and also the suitability and correctness of his methods [5, pp. 255–260]. At least to an extent Maxwell appeared to support Tait, and Clausius had to resort to complain of Maxwell’s Theory of Heat [19] that:

[In Maxwell’s book] my writings are left quite unmentioned; and my name occurs only once, when it is said I introduced the word entropy; but it is added that the theory of entropy had already been given by W. Thomson. Hence anyone who derives his knowledge of the matter solely from this book must conclude that I have contributed nothing to the development of the mechanical theory of heat.

It seems to have been Josiah Willard Gibbs (Fig. 8), the American physicist, who was the first to endorse fully Clausius’s achievements. Gibbs himself, of course, was to contribute massively to foundational studies in thermodynamics [5, pp. 260–261].

The other member of the triumvirate credited with the founding of thermodynamics, Rankine, was active on both technical and theoretical fronts in the years after 1851 [5, pp. 150–166]. We have already seen his efforts to develop an air-engine, and he was also involved in the production of a marine compound engine, attempting to use the new thermodynamics to produce the most efficient and compact heat engine for long-distance navigation [5, p. 5]. On the
theoretical side, he wrote extensively on the structure of thermodynamics, though always from an engineering perspective. Rankine is credited with bringing the actual word thermodynamics into general use in the subject in 1859, though William Thomson had introduced it in a more restricted context a few years earlier. Rankine also established the terminology of the first and second laws of thermodynamics and was responsible for the term kinetic energy. In 1855, all this work gained Rankine the prestigious Chair of Engineering at Glasgow in succession to Lewis Gordon, and, of course, it was Rankine’s early death in 1872 that paved the way for James Thomson’s own return to Glasgow. Mention of James brings us back to the central topic of this paper and encourages us to ask – in the final emergence of modern thermodynamics, why was James’s name absent?

5 James Thomson and thermodynamics

From the beginning of their interest in the study of work and heat right up to 1849, it is quite clear that James and William worked as a partnership. This applied to the discussion of their early dilemmas, though their reception of the work of Carnot, and later of that of Joule, and their later approach to these theories and their attempts to weld them into a single unified theory. It also applied to the discovery of the lowering of the freezing point of water under pressure.

Indeed it may be remarked that not only was there a partnership but, if either of them should be described as the senior partner, it was certainly James. He had perhaps the greater desire and ability to reach the most fundamental aspects of any set of ideas, and also his practical engineering experience, and particularly his intense interest in the means of minimising waste and maximising efficiency in any process, made his interest in, and his appreciation of, what was to become the subject of thermodynamics extremely strong.
For example, Smith [13] remarks that: ‘[M]uch of the early interest in Carnot and Clapeyron lay with James, and it appears to have been his enthusiasm which inspired William to go beyond a mere passive awareness of Clapeyron’s memoir’; and that: ‘James as the ‘theoretical engineer’ had rather more specialised and concentrated interests relating to the motive power of heat’ than William, whose interests ‘were much wider in the field of natural philosophy and mathematics generally’. It may also be remarked that, at least during the latter part of the 1840s, as a newly appointed professor, and even before then as an aspirant to the position, William was especially busy, while James was at home with much more free time. He was, of course, recovering from illness, but, as we have seen, after presumably a period during which work was not easy, he actually was able to work hard and extremely constructively.

Yet from 1850, it appears that William was working effectively on his own. The initial parts of his paper On the Dynamical Theory of Heat, which were completed in 1851, do not mention James. Since his claim to be regarded as one of the founders of thermodynamics rests mainly on that paper, it follows that James’s contribution has been scarcely known, and again it should be mentioned that Crosbie Smith’s work in drawing attention to his contribution has been of the utmost importance.

Why did this separation of the interests of the brothers occur? It will be remembered that it was at this period that, following the death of their father, James left Glasgow in a desire to avoid any breakdown of family harmony over his decision to become a Unitarian. He only returned to his family, and then to Belfast not Glasgow, at the request of Anna in 1851. It is certainly the case that William stayed with James in London for over a month in the summer of 1849, and of course at this time the discussions on the lowering of the freezing point and its experimental prediction were ongoing. However, in the subsequent 2 years or so, it may be that not only were the brothers not living in the same house, but communication by post was somewhat curtailed. Ironically, these years, so important in the sudden flowering of thermodynamics, were perhaps the one time during their lives that the brothers may not have been in very close contact either in person or by mail. It is still difficult to believe William would not have wished to discuss the work of Rankine and Clausius with James, but there does not seem to be evidence that this actually happened.

When William wrote the paper, it will be remembered that, ostensibly at least, he regarded it as a means not of providing new results, but rather of expressing the results of others in an informed and informative form. Thus, it might be argued that, since, again it must be said ostensibly, he was not attempting to claim credit for himself for the work reported, by refraining from mentioning James, he was not denying James any credit either.

Yet, this argument seems unconvincing. William must have been aware that there was much that was new in his paper. As well as the fact that he considered the mathematical proofs much more satisfactory than those of Clausius, there were, in particular, the arguments on waste, dissipation and the future of the Universe, which went well beyond anything Clausius had provided or indeed was interested in. And much of the basis of this work was the discussions of the two brothers over the years up to 1849. It is undoubtedly a simplification, but perhaps not a gross one, to suggest that many of William’s most important contributions to thermodynamics were based on the answers provided by Clausius to the profound questions discussed by William and James through the 1840s. James could really have hoped for more credit.
So where does our argument leave James’s position in the history of science? It is clear that his contribution to his brother’s important publications was crucial. Whether that actually entitles him to be regarded as one of the founders of thermodynamics must be a matter of opinion, depending on the extent to which one is prepared to take into account intellectual support and guidance, as distinct from crucial publication. In more general terms, though, it is clear that he should be recognised as a major figure in the history of science and technology in his own right, with a wide range of discoveries and achievements, not merely as worthy of attention only as the brother of Lord Kelvin.

In a more local context, he may be regarded as an important contributor to the nineteenth-century Irish tradition in mathematics and science, or, to put this more precisely, the tradition built up between the northeast of Ireland and the southwest of Scotland [20]. What Crosbie Smith said of Tait [21] may amply be said of James Thomson: ‘[H]e formed an integral part of a North British scientific intelligentsia, whose members often circulated around academic posts and institutions in Ulster and in Scotland but whose influential networks of science and engineering extended far beyond these shores’.

Acknowledgements

I thank Deirdre Wildy, Senior Subject Librarian at Queen’s University Belfast, for permission to use and quote from the material in the Special Collections in the Library, and Diarmuid Kennedy, Special Collections Librarian and his staff for their courtesy and help in obtaining material from the James Thomson archive. I also thank Joan Whitaker, Physics Subject Librarian, and the other staff of the Science Library at Queen’s University Belfast, for considerable support in obtaining books and papers.

I also like to acknowledge Mark McCartney for many interesting and useful conversations on the topics of James and William Thomson and other aspects of nineteenth century physics.

Finally, I am grateful to Michael Collins for suggesting that I should write this paper.

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