A complete account of our knowledge of energy and its transformations would require an exhaustive treatise on every branch of physical science, for natural philosophy is simply the science of energy.

William Garnett, *Encyclopaedia Britannica* (9th edition), 1879

The Science of Energy

A Cultural History of Energy Physics in Victorian Britain

CROSBIE SMITH

THE UNIVERSITY OF CHICAGO PRESS

CHAPTER 7

'The Epoch of Energy': the New Physics and the New Cosmology

> But the scientific importance of the principle of the conservation of energy does not depend merely on its accuracy as a statement of fact, nor even on the remarkable conclusions which may be deduced from it, but on the fertility of the methods founded on this principle. . . . .It gives us a scheme by which we may arrange the facts of any physical science as instances of the transformation of energy from one form to another. It also indicates that in the study of any new phenomenon our first inquiry must be, How can this phenomenon be explained as a transformation of energy? What is the original form of the energy? What is its final form? and What are the conditions of transformation?
>
> *Maxwell judges the significance of the principle of energy conservation for scientific practice (1876–7)*[1]

On 20 January 1852 William Thomson saw for the first time Hermann Helmholtz's 'admirable treatise on the principle of mechanical effect' published nearly five years earlier as *Über die Erhaltung der Kraft*. In a footnote added to the 1852 *Phil. Mag.* reprint of part one of his paper 'On the Dynamical Theory of Heat', Thomson stated that had he 'been acquainted with it in time', he should have had occasion to refer to it with respect to thermo-electric questions and 'on numerous other points of the dynamical theory of heat, the mechanical theory of electrolysis, the theory of electro-magnetic induction, and the mechanical theory of thermo-electric currents in various papers communicated to the Royal Society of Edinburgh' and to the *Phil. Mag.*[2] Far from seeing Helmholtz as a threat to British priorities, however, Thomson would rapidly appropriate the German physiologist's essay to the British cause, deploying it ultimately as a means of enhancing the international credibility of the new 'epoch of energy'.

For his part Helmholtz too derived dramatic gains in credibility from Thomson's enthusiastic recognition of the value of the 1847 essay which had hitherto received rather mixed reactions from German physicists. John Tyndall,

whom Helmholtz first met in August 1853, translated the essay for *Scientific Memoirs. Natural Philosophy* (edited by Tyndall and the publisher William Francis) in the same year. Also in 1853, Helmholtz travelled to England for the Hull meeting of the British Association where he met William Hopkins whose presidential address did much to promote, especially on Thomson's behalf, the new doctrines of heat. He became acquainted with other members of Thomson's circle, notably Stokes and the Belfast chemist Thomas Andrews (1813–85), though it was not until 1855 that he met Thomson in person. By 1853 he could therefore write that his *Erhaltung der Kraft* was 'better known here [in England] than in Germany, and more than my other works'. As a result, Helmholtz's essay acquired the status of a major contribution to physical science.[3]

Reviewing Helmholtz's career in 1877, James Clerk Maxwell claimed that in the 1847 essay Helmholtz had shown 'if the forces acting between material bodies were equivalent to attractions or repulsions between the particles of these bodies, the intensity of which depends only on the distance, then the configuration and motion of any material system would be subject to a certain equation which, when expressed in words, is the principle of the conservation of energy'.[4] Maxwell's appraisal vividly illustrates the nature of the British re-reading of Helmholtz's essay from 1852, a re-reading which would render Helmholtz as an ally in the establishment of the new 'science of energy':

> [Helmholtz] is not a philosopher in the exclusive sense, as Kant, Hegel . . . are philosophers, but one who prosecutes physics and physiology, and acquires therein not only skill in discovering any desideratum, but wisdom to know what are the desiderata, e.g., he was one of the first, and is one of the most active, preachers of the doctrine that since all kinds of energy are convertible, the first aim of science at this time should be to ascertain in what way particular forms of energy can be converted into each other, and what are the equivalent quantities of the two forms of energy.[5]

So powerful has this re-reading been that Helmholtz scholars have tended to interpret the 1847 essay in terms of the extent to which it measured up to a British 'energy' perspective rather than as a treatise firmly grounded in German traditions of *Kraft*, understood as separation in space.[6]

In this chapter I begin with Helmholtz's *Erhaltung der Kraft* in its original context of German physiology and physics. Re-read by Thomson as the author of a treatise on the 'principle of mechanical effect', Helmholtz found himself feted in North Britain as a key contributor to the principle of energy conservation, now construed as a basic and independent doctrine whose truth rested not on any rational deduction from more fundamental principles but upon Joule's experimental measurements. Their confidence boosted by this latest and not unwilling recruit, the North British scientists of energy moved quickly from local to national contexts via the BAAS. At successive British Association meetings throughout the 1850s, they and their allies among the 'old lions' promoted a new physics and a new system of the world which together would constitute a powerful but by no means unproblematic 'science of energy'.

The second section of this chapter tracks the promotion of Scottish-centred energy physics at annual meetings of the British Association. Transforming the science of work from its initial industrial contexts, the promoters of the science of energy gave it universal character and universal marketability transcending local customs and conventions. The science of energy became the intellectual property of the elite scientific practitioner, especially in Section A (Mathematics and Physics) of the British Association. These practitioners presented themselves as responsible for interpreting and directing the grand economy of nature upon which the wealth of nations ultimately depended. From the mid-1850s, the new scientific elite also brought quantitative estimates of the age of the sun and earth to bear upon debates about the origin of the solar system and life on earth. Adherents to the new energy doctrines thus sought to police the theoretical and empirical scope of the geological and biological sciences. In particular, the fundamental assumptions of Charles Lyell and Charles Darwin were subjected to critical scrutiny and their claims to have laid bare the natural history of the earth and its inhabitants challenged. What Thomson, addressing Section A at the fiftieth annual meeting of the British Association in 1881, called 'the epoch of energy' therefore forms the principal subject of the present chapter.

Physics and physiology: Hermann Helmholtz's Erhaltung der Kraft

As with Mayer, Helmholtz's professional career lay largely in medical science rather than physics. Unlike Mayer, however, much of Helmholtz's scientific work took place firmly within the institutional context of German universities, first as professor of physiology at Königsberg (1849–55), then in the chair of anatomy and physiology at Bonn (1855–8), and thirdly as professor of physiology at Heidelberg (1858–71). Only at the age of fifty did he become professor of experimental physics in Berlin (1871–88) and finally president of the new *Physikalisch-technische Reichsanstalt*, founded to promote science in the service of German industry and empire, until his death in 1894. Physiology, rather than physics, thus stood at the core of his professional career until the 1870s.[7]

Helmholtz and Mayer also differ strikingly in the degree and manner of credibility afforded to their research. The difference was in large part due to what might be termed Helmholtz's 'scientific cosmopolitanism'. Through his early association with the Berlin Physical Society (*Physikalische Gesellschaft*), through his succession of academic locations in the German-speaking world, and especially through his British acquaintances, Helmholtz exploited to the full the advantages offered by such social contexts for the promotion of his researches. Yet the initial reception of his 1847 essay had inauspicious. Rejected by Poggendorff's *Annalen*, Helmholtz published locally with no guarantee of a mainstream scientific readership. Furthermore, open hostility over the assumptions of his essay soon broke out between Helmholtz and his fellow-German Rudolf Clausius in the early 1850s.[8]

Born in Potsdam not far from the great Prussian capital, Berlin, Helmholtz grew up in a family limited in wealth but rich in culture. In due course, he attended the Potsdam Gymnasium (1832–8) where he studied subjects from mathematics to classics. Given his father's limited income, he required external funding if he were to enter university. The Royal Friedrich-Wilhelm Institut of Medicine and Surgery offered just the kind of state support which Hermann needed, in return for eight years service as an army surgeon.

In his first term he reported to his parents that he had used a spare interval to read Homer, Byron, Biot and Kant. Here indeed was a wide range of texts, from the classical Homer to the 'romantic' Byron and from the French physics of J.B. Biot (1774–1862) to the critical philosophy of Immanuel Kant. Biot's four-volume physics text, with its commitment to Laplace's style of physics, had been translated into German in 1824–5. In his second term Helmholtz apparently devoted more spare time to studies of Kant, whose writings served as a major philosophical resource for the introduction to *Erhaltung der Kraft*.[9]

It was in this second term, however, that he first became inspired by the physiological lectures of Johannes Müller. Although he had been introduced to French physiology from François Magendie's textbook (*Précis élémentaire de physiologie*) prior to his four-year course at the Institut, it was Müller who was to provide one of the most enduring social and intellectual foundations for Helmholtz's scientific career. By 1841–2 he 'lived entirely in the circle of Müller's pupils, since he had already formed a friendship with the physiologists [Ernst] Brücke and [Emil] du Bois-Reymond, who were two years senior to him, and like him devotedly attached to their teacher'.[10]

Appointed an army surgeon in 1843 and conveniently stationed in neighbouring Potsdam, Helmholtz continued to move in the Müller circle throughout the 1840s and beyond. His first publication, 'On the Nature of Fermentation and Putrefaction' appeared in Müller's *Archiv für Anatomie und Physiologie* (1843). It was in part based on work carried out by Helmholtz in his master's laboratory where Liebig's latest investigations of the physiology of respiration and nutrition were being appraised.[11]

This paper demonstrated his early opposition to Müller's views on *Lebenskraft* as an expression of purposive organization. Arguing from experiments in which he claimed to have eliminated the presence of micro-organisms, Helmholtz concluded that putrefaction was a purely chemical process which involved the breakdown of organic materials. Consequently, he argued, chemical processes alone, without any notion of *Lebenskraft*, were sufficient to replicate processes previously thought to require living organisms. Fermentation, on the other hand, was 'a form of putrefaction bound to and modified by the presence of an organism'. Helmholtz's demonstration that the laws of chemistry and physics could account for *all* the phenomena of life was therefore far from complete.[12]

Two years later Helmholtz targeted what he called 'One of the most important questions of physiology, one immediately concerning the nature of the *Lebenskraft* . . . [that is], whether the life of organic bodies is the effect of a special self-generating, purposive force or whether . . . the mechanical force and heat generated in the organism can be completely deduced from the process of material exchange or not'.[13] He chose to attack the problem through a specific investigation of muscle activity.

As a practising physiologist he had a particular enthusiasm for froglegs. He thus explained to du Bois Reymond early in 1847, the year of his *Erhaltung der Kraft*, that he was waiting 'impatiently for the spring and the frogs'.[14] Helmholtz's experiments involved the removal of the pairs of legs from several frogs. Taking one leg from each pair, weighing them, and finally arranging the legs end to end in series, he connected this set of froglegs to a Leyden jar which subjected the set to electric discharges until the legs showed no further 'irritability'. The other set, after weighing, was not subjected to electrical action. Both sets then had the muscles removed from the bones. Helmholtz found the difference in total weight between the two sets to be negligible but, after quantitative chemical analysis, he found a significant difference between the electrified and non-electrified sets: a material, chemical exchange had apparently taken place in the electrified set of froglegs. The analysis seemed to confirm that an increase in one kind of constituent was balanced by the decrease in another kind. Thus he concluded that muscle action involved chemical exchange or transformation within the muscle.[15]

Published by the Medical Faculty of the University of Berlin, the *Encyclopaedia of Medical Knowledge* (1845) contained a contribution from Helmholtz on 'Physiological Heat'. Here for the first time he considered rival theories of heat. Rejecting the caloric (material) theory on account of its inability to explain the continuous production of heat by friction, he explicitly supported a mechanical theory:

> the relationships of free and latent heat set forth in the language of the materialistic theory remain the same if in place of the quantity of matter we put the constant quantity of motion in accordance with the laws of mechanics. The only difference enters where it concerns the generation of heat through other motive forces and where it concerns the equivalent of heat that can be produced by a particular quantity of a mechanical or electrical force.

Bringing heat under the rule of mechanics entailed, in Helmholtz's argument, that 'the forces present in an organised body can only produce a particular quantity of heat motion, and . . . that a particular quantity of a motive force [*bewegende Kraft*] can only produce the same definite quantity [of heat] whatever the complication of its mechanism might be'.[16] Given that animals liberated heat, the issue was whether the chemical and physical forces arising from the intake of food and oxygen were alone sufficient to account for this animal heat or whether a special *Lebenskraft* with the capacity to supply force indefinitely was needed.

Helmholtz then engaged directly with Liebig's views that animal heat, a purely chemical process, was due to the oxidation of organic substances containing carbon and hydrogen in the lungs and was equivalent to the heat generated by combustion in a free state. Instead of dismissing earlier experimental work by Dulong and Despretz that the total quantity of animal heat exceeded such generation by respiration, Helmholtz widened the site of heat production from respiration in the lungs to the animal tissues, including the muscles. While confessing that more accurate quantitative determinations were necessary, he claimed that 'we must be temporarily satisfied with the fact that at least very nearly as much heat is generated through chemical processes in the animal as we find in it and its output, and that we must regard it as experimentally demonstrated that by far the greatest part of organic heat is a product of chemical forces'. Clearly, though, there remained the possibility that at least some of the animal heat derived not from chemical processes but from (for example) the activity of the nervous system which could be linked to *Lebenskraft*.[17]

The Berlin Physical Society, founded in 1845 by du Bois Reymond, Brücke and others including the electrical engineer Werner Siemens, counted among its members a new generation of German physiologists desirous of presenting their researches in purely physical terms, free from all remnants of *Geist* and *Lebenskraft*. Here, then, was a forum of like-minded young researchers with ambitions for the radical advancement of physiology along mechanistic lines and whose careers were being launched in the context of the growth of the German university system and the decline of *Naturphilosophie*. By 1847, Helmholtz was presenting his research before the Physical Society, reporting on physiological heat to the Society's *Progress of Physics* (*Fortschritte der Physik*) from its first volume and arguing for new and rigorous foundations for physiological science.[18] Those new foundations were to be provided by his *Erhaltung der Kraft*.

In February 1847 Helmholtz wrote to his friend du Bois Reymond inviting private criticism of a draft of the introduction to his forthcoming memoir '*Über die Erhaltung der Kraft*': 'I want to know if you think the style in which it is written [is] one that will go down with the physicists. I pulled myself together at the last reading, and threw everything overboard that savoured of philosophy, wherever it was not absolutely essential'. Helmholtz's private uncertainties reveal his concerns about the kind of reception which his ambitious memoir could expect, not least from physicists like Gustav Magnus (1802–70) in whose Berlin physical laboratory Helmholtz had recently carried out some physiological research. Because such physicists wielded considerable patronage, power and prestige, Helmholtz was especially desirous of winning their approval.[19]

When he read the memoir before the Physical Society on 23 July 1847, the younger generation of Helmholtz's friends and contemporaries were enthusiastic. The physicists were less impressed. Magnus, to whom Helmholtz sent the essay in the hope of obtaining support for its publication in Poggendorff's *Annalen*, was only persuaded to provide that support through the intervention of du Bois Reymond. In Poggendorff's opinion, however, the work was not sufficiently experimental to justify publication in his *Annalen*: 'The *Annalen* is necessarily dependent above all on experimental investigations'.[20] The largely-theoretical memoir, with no new experimental results, had thus to find publication outside the official scientific journals. Backed by strong support from his Berlin circle, Helmholtz not only persuaded a local publisher, G.A. Reimer, to produce the work, but was even paid an honorarium for the 71-page text.[21]

Helmholtz's *Erhaltung der Kraft* opened with the statement that 'The principal contents of the present memoir show it to be addressed to physicists [*Physiker*] chiefly'. He thus stressed that he was going to lay down the 'fundamental principles' of the memoir 'purely in the form of a physical premise, and independent of metaphysical considerations [*philosophischen Begründung*]'. He would then 'develope the consequences of these principles, and submit them to a comparison with what experience has established in the various branches of physics'.[22]

In spite of his denial of 'philosophical foundations', the 'Introduction' has often been interpreted in its philosophical, notably 'Kantian', context. What, then, did he mean by its independence from 'philosophical foundations'? The answer lies in the physiological context discussed in the previous section: that Helmholtz was above all concerned to dispose of all traces of *Geist* and *Lebenskraft* from natural science and to establish a mechanical approach to physiology on persuasive rational and empirical foundations. In this quest, of course, we see him pursuing a 'metaphysical' and philosophical goal which transcends purely physical and empirical investigation. But for Helmholtz and his contemporaries 'philosophical' carried the disreputable connotations of 'speculative', 'imaginary' and ultimately sterile *Naturphilosophie*.[23]

At the outset Helmholtz made clear that he was concerned to demonstrate that the propositions contained in his memoir were based upon and deducible from either of two maxims or assumptions. First, 'that it is not possible by any combination whatever of natural bodies to derive an unlimited amount of mechanical force', that is, perpetual motion is impossible. And second, 'that all actions in nature can be ultimately referred to attractive or repulsive forces, the intensity of which depends solely upon the distances between the points by which the forces are exerted'.[24]

Helmholtz further promised that in due course he would show the identity of these maxims. But in his 'Introduction' he set out to consider their bearing on what he termed 'the true aim of the [physical] sciences'. Arguing that the final goal of theoretical physical science was to 'refer back the phænomena of nature to unchangeable final causes', Helmholtz now transformed this requirement into a search for 'unchangeable forces'. He then invoked a conception of motion and force, rooted in German scientific culture, in terms of 'spatial relationship' [*räumliche Verhältnisse*] and synthesized it with a Laplacian perspective on the forces of attraction and repulsion acting centrally between material points.[25] First, 'motion is the alteration of spatial relationship'. From experience we know that motion can only appear as a change in the spatial relationship of at least two material bodies. Second, force, as the cause of motion, can 'only be conceived of as referring to the relation of at least two material bodies towards each other'. Force was therefore 'to be defined as the endeavour of two masses to alter their relative position':

> But the force which two masses exert upon each other must be resolved into those exerted by all their particles upon each other; hence in mechanics we go back to forces exerted by material points. The relation of one point to another, as regards space, has reference solely to their distance apart: a moving force, therefore, exerted by each upon the other, can only act so as to cause an alteration of their distance, that is, it must be either attractive or repulsive.

The problem of physical science therefore became simply that of referring 'natural phænomena back to unchangeable attractive and repulsive forces, whose intensity depends solely upon distance'.[26]

Helmholtz also explained that many of the general mechanical principles applicable to systems of bodies (the principle of virtual velocities, the conservation of motion of the centre of gravity, and the conservation of *vis viva* found in most of the existing treatises and textbooks on rational mechanics) were 'only valid for the case that these bodies operate upon each other by unchangeable attractive or repulsive forces'. Indeed, he claimed that virtual velocities was just a special case of *vis viva* which 'therefore must be regarded as the most general and important consequence of the deduction we have made'.[27]

His 'Introduction' complete, Helmholtz had thus far endeavoured to arrive at a 'rational' foundation for physical science in the form of a claim that all natural phenomena ultimately depended upon unchangeable attractive and repulsive forces. It was not the intensity of these forces which remained constant, however, for he had made their intensity depend upon the distance apart of the two or more material points between which the forces acted. Already he had hinted that it was to be *vis viva* which would be the principal measure of the conserved *quantity* of force. Helmholtz, however, was not presenting a formulation of the principle of conservation of energy as an independent, fundamental principle. Rather, his foundations rested very decisively on a commitment to a system of attractive and repulsive forces, from which the principle of conservation of *vis viva* followed as the most important consequence, a measure of 'conservation of force' [*Erhaltung der Kraft*].

In the first principal section of his essay Helmholtz began from the assumption of the impossibility of perpetual motion, that is, that 'it is impossible, by any combination whatever of natural bodies, to produce force continually from nothing'. He cited in particular Carnot and Clapeyron's deductions from this assumption of a series of laws regarding the specific and latent heats of natural bodies. Clapeyron's 1834 memoir had been partly translated for Poggendorff's *Annalen* in 1843 (ch.3). Now Helmholtz wanted to render the same assumption applicable throughout the whole range of physical phenomena.

Generalizing the Carnot–Clapeyron cycle of operations for reversible processes far beyond its previous application to gases, Helmholtz argued that if a system of natural bodies, acted upon by forces 'mutually exerted among themselves', were thereby moved to a new position with the net production of external work, the same quantity of work would be required to restore the system to its original position: 'For were the quantity of work greater in one way than another, we might use the former for the production of work and the latter to carry the bodies back to their primitive positions, and in this way produce an indefinite amount of mechanical force [work]'. In other words, we would have constructed 'a *perpetuum mobile* which could not only impart motion to itself, but also to exterior bodies'.[28]

The mathematical expression of this principle was, Helmholtz stated, to be found in the 'known law of the conservation of *vis viva*' but followed the comparatively recent (particularly French engineering) practice of restyling the traditional *vis viva* as $\frac{1}{2}mv^2$ (ch.3). He then devoted the remainder of this section to showing that the principle 'is alone valid where the forces in action may be resolved into those of material points which act in the direction of the lines which unite them, and the intensity of which depends only upon the distance', that is, that the principle was not applicable to all possible kinds of forces, but only to central forces.[29]

In the second section of the memoir, Helmholtz sought to transform the principle of conservation of *vis viva* into a much more general law, 'the principle of the conservation of force', for mechanical systems subject to the condition of centrally acting forces. Thus if ϕ were the intensity of the force acting in the direction r (positive for attraction; negative for repulsion), then he showed that for a material point of mass m and when Q and q and R and r represent corresponding tangential velocities and distances:

$$\frac{1}{2}mQ^2 - \frac{1}{2}mq^2 = -\int_r^R \phi dr$$

This equation, Helmholtz explained, expressed on the left-hand side the difference of *vires vivae* of m at two different distances. The quantity on the right-hand side had to be interpreted as 'the sum of the intensities of the forces which act at all distances between R and r'. He therefore named the forces which tended to move m, before the motion actually occurred, *tensions* [*Spannkräfte*] by contrast to *vires vivae* [*lebendige Kräfte*]. Thus the law of the conservation of force would be stated as: 'The increase of *vis viva* of a material point during its motion under the influence of a central force is equal to the sum of the tensions which correspond to the alteration of its distance'.[30] The choice of the term *Spannkraft*, consistent with the notion of spatial relationships, carried the meaning of increased tension or intensity of force with increased distance, analogous to the stretching of an elastic string.

Helmholtz moved to a still more general mathematical statement of the law for any number whatever of material points. Expressed verbally, the law became:

> In all cases of the motion of free material points under the influence of their attractive and repulsive forces, whose intensity depends solely upon the distance, the loss of [quantity of] tension is always equal to the gain in *vis viva*, and the gain in the former equal to the loss in the latter. Hence *the sum of the existing tensions and vires vivae is always constant*. In this most general form we can distinguish our law as *the principle of the conservation of force*.

He immediately proceeded to show that the most general law of statics, the principle of virtual velocities, followed from his principle of conservation of force, that is, that all the laws of statics may be deduced from the equations he had set forth.[31]

Having established his foundations, Helmholtz began a wide-ranging and systematic application of the principle of conservation of force [*Das Princip von der Erhaltung der Kraft*]. Initial applications were simple: for example, he considered briefly the application of the principle (as conservation of *vis viva*) to the motions of heavenly and terrestrial bodies under the influence of gravitational force. Provided friction or inelastic collision did not occur, the *vis viva* acquired by a body falling through a height would be sufficient to carry it back to the same height.[32]

Following a preliminary discussion of the nature of inelastic collision and friction, Helmholtz claimed in the fourth part of his memoir that the conventional representation of friction as a force which acts against existing motion had been only made to facilitate calculation and was a very inadequate expression of the complex processes of action and reaction of molecular forces. As such it led to the inference that by friction (and by inelastic collision) '*vis viva* was absolutely lost'. But the heat developed also represented 'a force by which we can develope mechanical actions; the electricity developed, whose attractions and repulsions are direct mechanical actions, and the heat it excites, an indirect one, has also been neglected'.[33]

The problem then to be solved was 'whether the sum of these forces always corresponds to the mechanical force which has been lost'. In cases which avoided molecular changes and electrical effects, the question became: 'whether for a certain loss of mechanical force a definite quantity of heat is always developed, and how far can a quantity of heat correspond to a mechanical force'. In response to the first part of this question, the development of heat from work, Helmholtz pronounced upon the value of Joule's experiments (ch.4):

> For the solution of [this] first question but few experiments have yet been made. Joule has measured the heat developed by the friction of water in narrow tubes, and that developed in vessels in which the water was set in motion by a paddle-wheel; in the first case he found that the heat which raises 1 kilogramme of water 1°, was sufficient to raise 452 kilogrammes through the height of 1 metre; in the second case he found the weight to be 521 kilogrammes. His method of measurement however meets the difficulty of the investigation so imperfectly, that the above results can lay little claim to accuracy. Probably the above numbers are too high, inasmuch as in his proceeding a quantity of heat might have readily escaped unobserved, while the necessary loss of mechanical force in other portions of the machine is not taken into account.[34]

Although Helmholtz questioned the accuracy of Joule's method and results, he nevertheless placed considerably more reliance upon Joule's investigations as evidence against a material theory, and in favour of a mechanical theory of heat, than has generally been recognized.[35]

Turning to the converse process, the development of work from heat, Helmholtz set out the Carnot–Clapeyron theory of the motive power of heat and its connection with a caloric theory:

> The material theory of heat must necessarily assume the quantity of caloric to be constant; it can therefore develope mechanical forces only by its effort to expand itself. In this theory the force-equivalent of heat can only consist in the work produced by the heat in its passage from a warmer to a colder body; in this sense the problem has been treated by Carnot and Clapeyron, and all the consequences of the assumption, at least with gases and vapours, have been found corroborated.[36]

Having rejected the material theory in favour of a mechanical theory in his 1845 encyclopaedia article, he now highlighted the principal problem which the material theory continued to face: that of accounting for the heat developed by friction. In particular he dismissed an attempt to explain frictional heating by conduction: 'if it were true, then in the neighbourhood of the rubbed portions a cold proportionate to the intense heat often developed must be observed'.

Helmholtz also put considerable emphasis on the development of heat from electricity. He examined two cases of the production of electricity by mechanical means, those of electrostatic induction and of electromagnetic machines. In the first case, an electrophorus, as an insulated conductor, is brought near an insulated charged body and thus acquires an induced charge which may be discharged into a Leyden jar which can then be itself discharged to produce heat without the production of corresponding cold. In this manner we 'have consumed a certain amount of force, for at each removal of the negatively-charged conductor from the inducing body the attraction between both is to be overcome'. Similarly, in magneto-electric machines 'heat *ad infinitum* may be developed by the bodies constituting the machine, while it nowhere disappears. That the magneto-electric current developes heat instead of cold, in the portion of the spiral directly under the influence of the magnet, Joule has endeavoured to prove experimentally'.[37]

Such cumulative evidence appeared decisive against all caloric theories: 'it follows that the quantity of heat can be absolutely increased by mechanical forces, that therefore calorific phænomena cannot be deduced from the hypothesis of a species of matter, the mere presence of which produces the phænomena, but that they are to be referred to changes, to motions, either of a peculiar species of matter, or of the ponderable or imponderable bodies already known, for example of electricity or the luminiferous aether'. Under this interpretation, quantity of heat would express firstly 'the quantity of *vis viva* of the calorific motion' (corresponding to the old term 'free heat') and secondly 'the quantity of those tensions between the atoms which, by changing the arrangement of the latter, such a motion can develope' (corresponding to 'latent heat').[38]

Instances of collision, friction, and chemical processes, however, tended to focus on the production of heat rather than on the 'disappearance' of heat. Thus 'Whether by the development of mechanical force heat disappears, which would be a necessary postulate of the conservation of force, nobody has troubled himself to inquire' – with the possible exception of Joule:

> I can only in respect to this cite an experiment by Joule, which seems to have been carefully made. He found that air while streaming from a reservoir with a capacity of 136.5 cubic inches, in which it was subject to a pressure of 22 atmospheres, cooled the surrounding water 4°.085 Fahr. when the air issued into the atmosphere, and therefore had to overcome the resistance of the latter. When, on the contrary, the air rushed into a vessel of equal size which had been exhausted of air, thus finding no resistance and exerting no mechanical force, no change of temperature took place.[39]

Helmholtz made no mention of his little-known countryman Mayer (nor indeed of Gay-Lussac) with regard to this experiment (ch.4). Once again, in spite of reservations of the accuracy of Joule's results, Helmholtz placed considerable credibility upon the Englishman's researches.

In the final part of this section, Helmholtz summarized Clapeyron's theory of the motive power of heat (ch.3). Noting that Clapeyron's deduction of the 'law' that the maximum mechanical effect is the same for all bodies 'can only be admitted when the quantity of heat is regarded as unchangeable', Helmholtz nevertheless accepted that Clapeyron's specific formulae for gases were supported by experiment as well as following from K.H.A. Holtzmann's more recent formula.

Holtzmann, on the other hand, had begun from the assumption that a quantity of heat entering a gas either increases the temperature or causes an expansion in volume of the gas: 'The quantity of work thus produced by the heat he [Holtzmann] assumed to be the mechanical equivalent of heat; he calculated from the experiments of Dulong upon sound, that the heat which raises the temperature of 1 kilogramme of water 1° Centigrade, would raise a weight of 374 kilogrammes 1 metre'. Accordingly, Helmholtz noted that the free expansion of a gas must exhibit no change of temperature, an inference which 'indeed appears to follow from the above-mentioned experiment of Joule'. Thus 'the increase and diminution of temperature by compression and dilatation would, under ordinary circumstances, be due to the excitation of heat by mechanical force, and *vice versa*'. Joule's own experimental results, however, for the mechanical equivalent (the 'force-equivalent of heat') were in the range from 452 to 521, considerably higher than Holtzmann's value of 374. Apart from commenting upon the fact that the latter figure, unlike those of Joule, had been calculated 'from the experiments of others', Helmholtz made no attempt to account for the discrepancy.[40]

Sections five and six of the memoir ranged widely and systematically across the whole of electrical science, beginning with statical electricity and continuing through galvanism, thermoelectricity, and magnetism to conclude with electromagnetism. In these investigations he was guided by the conservation principle throughout an impressive range of electrical conversion processes which brought together chemical, thermal, mechanical and electrical phenomena. He again drew extensively on recent experimental research, including that of Joule, Faraday, Ohm and Lenz. His stated aim was 'to draw conclusions regarding laws which are as yet but imperfectly known . . . and thus to indicate the course which the experimenter must pursue'.[41]

Helmholtz's *Erhaltung* ended with brief reference to the physiological context which had generated it. In that original context the memoir had been constructed to function as a theoretical and experimental foundation which would demonstrate the fallacy of any doctrine involving *Lebenskraft*. This 'policing role' had become vastly extended in the course of the memoir itself. His strategy had been to show that the principle was applicable not simply within rational mechanics but had universal applicability to all the forces of nature. He had therefore assembled a substantive weight of physical, chemical and physiological evidence drawn from a wide range of experimental resources in support of that claim. Now the theoretical physicist could assert mastery over all the branches of physical and chemical science.[42]

Within a very few years this role was strengthened when *Erhaltung* ceased to be the locally published property of a German physiologist. Having 'discovered' Helmholtz's treatise on 20 January 1852, Thomson began reflecting on the theoretical and experimental foundations for his paper 'On a Universal Tendency in Nature to the Dissipation of Mechanical Energy'. In a draft for the opening paragraphs he recognized that in the past the principle of mechanical effect had been restricted to 'conservative' cases, that is, where the effects are independent of path between initial and final states:

> The principle of mechanical effect first stated in all its generality by Newton at the conclusion of the scholium to his 3^d Law of Motion, and enunciated more or less <completely> explicitly by subsequent writers in the two propositions commonly called 'the principle of virtual velocities' and 'the principle of the conservation of vis viva', is first deduced from the fundamental axioms of mechanics as a theorem applicable to cases in which either working or resisting forces may be regarded as arbitrarily applied.[43]

Thomson continued, however, that 'as soon as it is established in Natural History that all the working or resisting forces of inanimate matter, as well as all the mechanical actions of living Creatures either are due to the inertia of matter, or are mutual forces between material particles which, when overcome through any spaces are always ready to restore the work spent, by working backwards through the same spaces, the postulate . . . [that a potential function exists for every force] assumed in the theorem of the "conservation of vis viva" becomes known as a Universal Truth'. In this laden sentence Thomson expressed his conviction that the empirical establishment of 'conservation of energy' *throughout* nature would guarantee the existence of a potential function for all the forces of nature, including apparently non-conservative ones such as friction. Thus the effect of every force would be independent of path and the principle of mechanical effect raised to the status of a completely general variational equation from which the principle of virtual velocities and other equations of motion could be derived.[44]

In his draft Thomson was in effect reworking Helmholtz's arguments in the light of his own fundamental convictions. At first sight the analyses appear identical. But Helmholtz's commitment to a basic physics of attractive and repulsive forces acting at a distance contrasted strikingly with Thomson's early preference for continuum approaches to physical agencies such as electricity and magnetism.

Erhaltung der Kraft as conservation of force, whose quantity is measured in terms of *vis viva* and whose intensity is expressed in terms of attractive or repulsive forces acting at a distance, was now being read as an independent 'Universal Truth', 'conservation of mechanical energy', whose quantity is measured as mechanical effect and whose intensity is understood in terms of a potential gradient.[45]

Reworking Helmholtz further, Thomson drafted his own range of empirical evidence in support of this 'Universal Truth' that mechanical energy could not be altered in quantity by any natural agency:

> This state of certainty we may regard as now reached, in consequence of the recent advancement of science in the establishment of the dynamical theory of heat and light; in the discoveries of Rumford, Davy, and Joule regarding the thermal effects of the friction of solids, fluids, and electric currents, and the aggregate thermal and non-thermal mechanical effects of electric currents employed in electrolysis or in raising weights [as in an electromagnetic engine]; in the discoveries of Priestley, Sennebier, and Davy regarding the influence of sunlight on vegetation; and in the ample confirmation of the mechanical energy of animals afforded by the researches of Dulong, Dumas, and Liebig. *We may consequently regard it as certain that, neither by natural agencies of inanimate matter, nor by the operations arbitrarily effected by animated Creatures, can there be any change produced in the amount of mechanical energy in the Universe*; and the belief that Creative Power alone can either call into existence or annihilate mechanical energy, enters the mind with perfect conviction.[46]

Very soon afterwards Rankine formally restyled the 'principle of mechanical effect' as 'the law of the conservation of energy', that 'the sum of the actual and potential energies in the universe is unchangeable'.[47] The new language, developed by Thomson and Rankine, signified their concern not merely to avoid ambiguities in speaking about 'force' and energy' in physics and engineering, but to reinforce a whole new way of thinking about and doing science.

Rankine's 'On the General Law of the Transformation of Energy' (originally communicated to the Glasgow Philosophical Society on 5 January 1853 and reprinted in the *Phil.Mag.*) explained that 'the term *energy* is used to comprehend every affection [state] of substances which constitutes or is commensurable with a power of producing change in opposition to resistance, and includes ordinary motion and mechanical power, chemical action, heat, light, electricity, magnetism, and all other powers, known or unknown, which are convertible or commensurable with these'. As with Thomson's 1851 axiom, Rankine's language here read like that of a legal document which in a sense it was. Associated through personal and family connections with lawyers, commercial gentlemen and inventors, Rankine and Thomson were quick to apply the language of patents to scientific property which could then be marketed to a scientific public in return for increased credibility. Like any product in a capitalist economy, the science of energy would be gauged by the extent of its market appeal. Again in legalistic style, Rankine distinguished all conceivable forms of energy into two kinds, 'actual' or 'sensible', and 'potential' or 'latent'.[48]

In the preceding article in the same issue of the *Phil. Mag.*, Thomson added a note referring to the 'admirable terms "potential" and "actual" introduced by Mr Rankine in his paper "On the Transformation of Energy" . . . to designate the two kinds of energy which I had previously distinguished by the inconvenient adjectives of "statical" and "dynamical"'. Joule too firmly endorsed the new perspectives. As he told Thomson in February 1853: 'The term energy employed by you seems admirably suited as the expression of anything which might ultimately by proper transformations be exhibited in the form of, say, heat'. Furthermore, 'The adjectives potential and actual employed by Mr Rankine seem to deserve the approbation you express in reference to them'.[49] From now on, the new terminology of energy would replace Thomson's 'principle of mechanical effect' and Helmholtz's 'law of conservation of force' as well as various older versions of conservation of *vis viva*.

Yet the North British scientists of energy did not always present a wholly united public image. During the preparation of their *Treatise on Natural Philosophy* (1867), Thomson and Tait substituted 'kinetic' for 'actual' in 1862 (ch.10). Rankine himself publicly criticized the change as undesirably restrictive and seized upon Maxwell's remark in his *Theory of Heat* (1871) that 'We cannot even assert that all energy must be either potential or kinetic, though we may not be able to conceive any other form'. In Rankine's view 'this was the very reason which induced me in 1853 to propose the word "actual" for denoting energy that is not potential, rather than any word expressly denoting motion, and which still induces me to prefer the word "actual" to the word "kinetic" for that purpose'.[50]

The British Association and the advancement of energy

By the mid-1840s the annual meetings of the British Association for the Advancement of Science had become a familiar ritual of Victorian life. But leadership of the Association was slowly changing. Many members of the founding generation of Oxbridge-dominated Anglicans – polymaths such as William Whewell, geologists such as Adam Sedgwick and William Buckland, and astronomers such as Sir John Herschel – no longer played such a central role after 1845. And although Section A could boast a distinctive style of scientific practice (modelled upon a mythical 'Newtonian' inductive approach of data, laws and general theory), the Association had never constructed a universal science of its own to rival, for example, that of Laplacian physics. For a younger, more entrepreneurial generation there thus existed a gap in this large scientific market which a credible new physics might readily fill.[51]

A contemporary spectator at the Ipswich meeting (1851), however, would scarcely have discerned significant changes in the Association's hierarchical character. The Cambridge-educated Astronomer Royal, George Biddell Airy, delivered the presidential address. Astronomy, hitherto 'Queen of the sciences', predominated. But Airy referred briefly to 'the investigations which have lately been made by able engineers regarding the Mechanical Equivalent of Heat. The subject, in this form, is yet new; but I think that the importance of an accurate determination cannot be over-rated'.[52] Unnamed, these 'able engineers' (probably Joule and Rankine) were not accorded the status of the Cambridge-trained mathematicians, natural philosophers and astronomers of Section A. Yet Airy, sympathetic to engineering concerns, had nonetheless highlighted here the need for 'accurate determination' (the hall-mark of his own astronomical practice at the Royal Greenwich Observatory) of the mechanical equivalent of heat.

For the 1852 Belfast meeting the BAAS president was a very different figure. A particularly astute soldier of science, Colonel Edward Sabine (1788–1883), senior member, treasurer and vice-president of the Royal Society, was best known for his role in promoting the so-called 'Magnetic Crusade' in the late 1830s. Once held up by Babbage as an embodiment of the Royal Society's failings, Sabine had become one of the most powerful patrons of young Cambridge mathematicians, such as Archibald Smith and William Thomson, who had little liking for the older generation of Oxbridge lions.[53]

Almost certainly well primed by Thomson, Sabine pronounced from the presidential chair a highly favourable verdict on the new theory of heat in general and upon Joule in particular. Given the Royal Society's reluctance to accord him a place in its *Transactions* until 1850, Joule may well have felt that Sabine's judgment had come somewhat late in the day. Nevertheless Joule was now being represented within the BAAS as a model gentleman of science who, employing all the correct procedures of experimental and numerical accuracy, had placed the new theory of heat beyond controversy.[54] Sabine also drew attention to the 'Mathematical developments of the theories of heat and electro-dynamics, in accordance with these [Joule's] principles . . . given in various papers by MM. Helmholtz, Rankine, Clausius and Thomson, published principally within the last two years'. He noted with special satisfaction that Section A 'will have a great advantage in being presided over by the last-named of these gentlemen, a native of Belfast, who at so early an age has attained so high a reputation, and who is taking a leading part in the investigations to which I have referred'.[55]

The 28-year-old Glasgow professor of natural philosophy, elevated for the first time to the presidency of the prestigious Section A, commanded unrivalled authority in the field. His series of papers 'On the Dynamical Theory of Heat' had been appearing both in the *Transactions of the Royal Society of Edinburgh* and in the *Phil. Mag.*, as had his recent paper 'On the Universal Tendency in Nature to the Dissipation of Mechanical Energy'. He was receiving presidential patronage from Sabine. And his now-frequent public usage of the term 'energy' linked together the investigations of a formidable range of practitioners: some, like Joule, Rankine and his brother, part of a well-established personal network; others, like Clausius and Helmholtz, a means to elevate the significance of the new science from merely local relevance to international and universal importance.

Although Thomson read at least four papers to Section A during the 1852 meeting, his dissipation paper was not one of them. Instead, Rankine presented a summary of the new physics and cosmology under the provocative title 'On the Reconcentration of the Mechanical Energy of the Universe'. He began by observing that:

> it has long been conjectured, and is now being established by experiment, that all forms of physical energy, whether visible motion, heat, light, magnetism, electricity, chemical action, or other forms not yet understood, are mutually convertible; that the total amount of physical energy in the universe is unchangeable, and varies merely its condition and locality, by conversion from one form to another, or by transference from one portion of matter to another.[56]

These transformations and transferences, he added to his *Phil. Mag.* version, constituted 'the phænomena which are the objects of experimental physics'. Thus experimental physics could be re-read and redefined as the study of energy conversions (ch.9).

Rankine noted that Professor Thomson had pointed out that 'in the present condition of the known world there is a preponderating tendency to the conversion of all the other forms of physical energy into heat, and to the equable diffusion of all heat; a tendency which seems to lead towards the cessation of all phænomena'.[57] Here Rankine was taking further Thomson's original third conclusion which had merely referred to the finite character of the earth as a place fit for human life (ch.6). Two years later, Helmholtz also broadened Thomson's original claim when he reflected that 'we must admire the sagacity of Thomson, who, in the letters of a long-known little mathematical formula which speaks only of the heat, volume, and pressure of bodies, was able to discern consequences which threatened the universe, though certainly after an infinite period of time, with eternal death'.[58]

While not disputing Thomson's claim 'to represent truly the present condition of the universe, so far as we know it', Rankine refused to accept the pessimistic conclusion. He therefore speculated that radiant heat – 'the ultimate form to which all physical energy tends' – might be totally reflected at the boundaries of the very interstellar medium through which the radiation had been transmitted and diffused. This energy might then be 'ultimately re-concentrated into foci; at one of which, if an extinct star arrives, it will be resolved into its elements, and a store of energy reproduced'. Rankine therefore concluded:

> Thus it appears, that although, from what we can see of the known world, its condition seems to tend continually towards the equable diffusion, in the form of radiant heat, of all physical energy, the extinction of the stars, and the cessation of all phenomena; yet the world, *as now created*, may possibly be provided within itself with the means of reconcentrating its physical energies, and renewing its activity and life.[59]

Although we as yet know too little of Rankine's specific religious position within presbyterian culture, the key phrase 'as now created' is consistent with the evidence that he placed great store on the teachings of the Bible. Outside the growing liberal presbyterianism of Glasgow College until 1854, Rankine may have felt that Thomson's temporal vision of the universe came too perilously close to the deistic undertones in *Vestiges* (ch.6) and that what was needed instead was a universe of recent creation functioning as a perfectly reversible thermo-dynamic engine. It is therefore possible that Rankine sought to embody Thomson's notion of a perfect thermo-dynamic engine in a universe 'as now created' in the image of a God of perfection. 'Dissipation' once again related to human beings' inefficient design of engines rather than to any ultimate imperfections in nature, the apparent tendency towards 'the cessation of all phænomena' being in reality only one phase of a larger reversible cycle of the created world.[60]

Wielding vastly greater scientific authority and shamelessly recruiting BAAS presidential allies in the guise of William Hopkins to his cause (below), Thomson ensured that it was his, not Rankine's vision, which held sway. Indeed, Rankine was never again to air such a 'cyclical' speculation in public. So strong was Thomson's commitment to a directional universe that he was apt to dismiss all such hypotheses of 'compensation' and 'balance' as wholly unwarranted by the evidence of nature and inconsistent with a liberal reading of scripture (ch.6). As he clarified his position in 1862:

> The result would be a state of universal rest and death, [only] if the universe were finite . . . But it is impossible to conceive a limit to the extent of matter in the universe; and therefore science points rather to an endless progress, through an endless space, of action involving the transformation of potential energy into palpable motion and thence into heat, than to a single finite mechanism, running down like a clock, and stopping for ever.

Thomson's version of the new energy cosmology, then, ruled out both the possibility of reconcentration *and* a view of the universe as a simple mechanical clock winding down to an inevitable cessation of motion. Furthermore, his public phrase 'an endless progress', echoing his private claim in 1851 that 'Everything in the material world is progressive', was symptomatic of the authority of the new science of energy. In particular, Thomson could now speak confidently of the universe in terms of an endless flow, implying a perfect infinite creation rather than an imperfect finite mechanism. Equally, he and his allies could attempt to break the monopoly on 'progress' held by evolutionary theorists such as Herbert Spencer who in 1857 had enunciated a fundamental natural law: 'that in which Progress essentially consists, is the transformation of the homogeneous into the heterogeneous'.[61] Such doctrines of onwards and ever-upwards evolutionary progress were indeed anathema to the North British scientists of energy (ch.9).

Very conveniently, the BAAS president at the Hull meeting (1853) was none other than Thomson's former Cambridge mathematical coach, William Hopkins, who had recently served as president of the Geological Society of London. Respected for his researches in physical geology, he was known as the 'wrangler-maker' on account of his ability to produce many of the top-ranking Cambridge mathematical graduates classed from 'Senior Wrangler' downwards. Ideally placed to give credibility to the new doctrines of heat, he devoted at least half of his BAAS address to a wide-ranging review of the subject.[62]

His enthusiasm for geological science originally fired by the Cambridge professor of geology, Adam Sedgwick, around 1833, Hopkins had read to the Cambridge Philosophical Society his first major scientific memoir within two years. He there introduced a new subject, 'Researches in Physical Geology', which would bring the power and prestige of mathematical analysis to bear on geological phenomena. Geological science would thereby be raised to the same high status as physical astronomy. As with the mythologized history of physical astronomy, the new geology would have three stages: first, a geometrical description of the relevant motions (analogous to Kepler's laws of planetary motion); second, postulation of a very general force that would cause the motions (analogous to gravitational force); and third, derivation of the actual motions from the general force according to dynamical principles.[63]

The formulation of geometrical laws of faults, fissures, mineral veins and the like constituted the goal of this memoir. In subsequent investigations, Hopkins sought the very general cause, an 'elevatory force', responsible for all such phenomena wherever and whenever they occurred. He found that cause in the widely held doctrine of central heat whereby the earth, assumed to have been formed as a hot fluid mass that subsequently solidified, had undergone, and continued to experience, a progressive (directional) cooling in time. Like the doctrine of universal gravitation in its generality and simplicity, central heat became for Hopkins the fundamental agency of geological dynamics.[64]

Hopkins's preferred model was of a largely solid earth consisting of cavities. Hot vapours or fluids forced into those cavities from below would produce elevatory pressures in local regions. This model clashed directly with the steady-state (non-progressionist) geological theory of Lyell (ch.6) which denied the doctrine of primitive heat while upholding the notion of a largely liquid interior supporting a thin terrestrial crust less than 100 miles thick. Most of Hopkins's subsequent investigations were directed towards justifying the adequacy of his model and extending its explanatory power. Between 1838 and 1842 a series of papers for the Royal Society argued on mathematical grounds that the observed behaviour of the terrestrial planet around its axis (precession and nutation) was consistent with a solid, but inconsistent with a liquid, interior. A long report to the BAAS (1847) treated the phenomena of earthquakes and volcanoes within the same framework.[65]

Hopkins's 1853 presidential address enlisted the new theory of heat in support of geological theory. A scientific lifetime's credibility invested in geological researches had brought increasing returns to Hopkins: presidency of the Geological Society in 1851, presidency of the BAAS in 1853, and now seemingly irrefutable support from the new physics of his one-time pupil. He had indeed emerged as the most scientifically active of the older generation of Cambridge lions, an outsider who had entered the University relatively late in life and who had been excluded by marriage from a College fellowship. His alliance with the rising generation had been quickly cemented by his institution of a series of experiments, supported by Royal Society grants, guided by Thomson's thermo-dynamic

expertise and aided in practice by Joule and Fairbairn, in Manchester to determine the effects of enormous pressures on the melting points of substances, the results of which he interpreted as supporting the solidity of the earth.[66]

In his address, Hopkins deployed the new theory of heat in the Cambridge war against Lyellian geology. He defined the controversy as one of 'progression' *versus* 'non-progression' (rather than, as Whewell had done earlier, as 'catastrophism' versus 'uniformitarianism') in geological theory. Progressionists held that 'the matter which constitutes the earth has passed through continuous and progressive changes from the earliest state in which it existed to its actual condition at the present time'. The earliest state (fluid or gaseous) was one of 'enormous primitive heat of the mass', the gradual loss of which caused progressive change. On the other hand, non-progressionists recognize 'no primitive state of our planet differing essentially from its existing state'. The only changes they recognized were 'those which are strictly periodical, and therefore produce no permanent alteration in the state of our globe'.[67]

The theory of non-progression, however, remained in serious doubt: 'the theory [cannot] derive present support . . . by an appeal to any properties of inorganic matter or physical laws, with which we are acquainted'. In contrast, Hopkins presented Thomson's cosmological views as resting upon well-founded physical laws. The new theory of heat asserted 'the exact equivalence of heat and motive power; and that a body [such as the sun], in sending forth heat, must lose a portion of that internal motion of its constituent particles on which its thermal state depends'. Furthermore, 'no mutual action of these constituent particles can continue to generate motion which might compensate for the loss of motion thus sustained'. This 'simple deduction from dynamical laws and principles' was 'independent of any property of terrestrial matter which may possibly distinguish it from that of the sun'. In the absence of adequate external supplies of 'thermal energy', Hopkins concluded, 'the heat of the sun must ultimately be diminished, and the physical condition of the earth therefore altered, in a degree inconsistent with the theory of non-progression'.[68]

Hopkins' indictment here of non-progressionists extended also to Rankine whom he damned with faint praise for having 'ingeniously suggested an hypothesis according to which the reconcentration of heat is conceivable'. He rejected Rankine's 'very ingenious, though, perhaps, fanciful hypothesis' as 'affording a sanction to the theory of *non-progression*'. By contrast, he stated that his own convictions 'entirely coincide with those of Prof. Thomson'. Thus, Hopkins concluded, 'If we are to found our theories upon our knowledge, and not upon our ignorance of physical causes and phænomena, I can only recognize in the existing state of things a passing phase of the material universe'.[69]

Hopkins's reading of the new theory of heat itelf bore a distinctive Cambridge stamp. Invoking no appeal to unobservable particles, he defined a 'dynamical theory of heat' as that which 'proposes to explain the thermal agency by which motive power is produced, and to determine the numerical relations between the *quantities* of heat and the *quantity* of mechanical effect produced by it'. He even included Carnot's original theory under this 'macroscopic' definition: 'Carnot was the first to give such a theory a mathematical form'. Such 'dynamical theories', macroscopic in character, were characteristic of the new generation at Cambridge, exemplified particularly in Stokes's hydrodynamical and optical memoirs of the period.[70]

With respect to the experimental basis for the new theory, Hopkins acknowledged Rumford's 'rough attempt' to determine a mechanical equivalent of heat. But, he stressed, 'it was reserved for Mr Joule to lay the true foundation of this theory by a series of experiments, which, in the philosophical discernment with which they were conceived, and the ingenuity with which they were executed, have not often, perhaps, been surpassed'. Once again Joule had been unreservedly endorsed as a model gentleman of science within the Association's elite. Hopkins even reminded his audience of Joule's links with one of the BAAS's early heroes: 'we have in Mr Joule a pupil, a friend, and fellow-townsman of Dalton'.[71]

Hopkins also proclaimed that the new dynamical theory was 'in perfect harmony with the opinions now very generally entertained respecting *radiant heat*'. It had been 'established beyond controversy' that 'light is propagated through space by the vibrations of an exceedingly refined ethereal medium . . . and it is now supposed that radiant heat is propagated in a similar manner' whereby the particles of a heated body are in a state of vibration.

In this context he referred to Mr. Rankine's 'ingenious paper on a molecular theory of heat'. Professor Thomson, on the other hand, had 'given a clear and compendious mathematical exposition of the new dynamical theory of heat, founded on Mr. Joule's principle of the exact equivalence of heat and mechanical effect', a theory which was not, 'like Mr. Rankine's, a *molecular* theory, but one which must henceforth take the place of Carnot's theory'.[72] Although Hopkins here referred approvingly to the 'ingenuity' of Joule's experimental technique as well as to that of Rankine's theoretical manipulations, he reserved the highest accolade for his own former pupil, *Professor* Thomson, whose academic status, Cambridge mathematical training, and critical attitude towards speculative hypotheses together qualified him for leadership of Section A, alongside the likes of Stokes and Hopkins himself. Concomitantly, while Rankine would remain a central member of the energy network on account of his engineering expertise, his ventures into molecular physics and speculative cosmology earned him less credibility from among his scientific peers. As Thomson told Tait privately in 1864, 'Rankine . . . gave a somewhat magnificent but very imperfect & lame molecular theory (read & judge). Only less lame than all its predecessors'.[73]

With this presidential address Hopkins had set up Thomson as the BAAS's 'real man of armour' for whom the geologist Roderick Murchison had once called as a bulwark against the kind of dangerous and populist speculations embodied in *Vestiges* (ch.6). Although Thomson himself was not at the Hull meeting, the presidential address had opened the way for his fuller account of energy cosmology to Section A at the Liverpool meeting (1854). He also read the same paper, 'On the Mechanical Antecedents of Motion, Heat and Light', to the French Academy of Sciences, prompting the Abbé Moigno to comment critically on the views of the popular young British *savant*, the 'spoilt child of English science'.[74]

Reminding the BAAS audience of his 1852 discussion of animal heat and its origins (ch.6), Thomson recalled his verdict that, with minor exceptions, 'every kind of motion . . . that takes place naturally, or that can be called into existence through man's directing powers on this earth, derives its mechanical energy either from the sun's heat or from motions and forces among bodies of the solar system'. In a series of papers read to the RSE in the spring of 1854, he had further discussed the nature and cause of the sun's heat. The conclusions owed much to intensive discussions with his Cambridge friend Stokes.[75]

Thomson rejected two possible ways of accounting for the sun's heat: that of a 'primitive' store of heat in the sun and that of intrinsic (chemical) combustion. Neither hypothesis, he argued, could supply the required quantity of heat even over a 6,000-year period, that is, over the period of human history. Instead, he claimed to have 'shown that the sun's heat is probably due to friction in the atmosphere between his surface and a vortex of vapours, fed externally by the evaporation of small planets, in a region of very high temperature round the sun, which they reach by gradual spiral paths, and falling in torrents of meteoric rain, down from the luminous atmosphere of intense resistance, to the sun's surface'.[76]

While acknowledging that J.J. Waterston (1811–83), one-time civil engineer and now naval instructor for the East India Company in Bombay, had put forward such a 'meteoric' view at the Hull meeting, Thomson argued that, unlike Waterston's 'extra-planetary' origin for the meteoric matter, his own view was consistent with planetary dynamics in supposing the meteoric matter to lie within the earth's orbit. Indeed, he believed this matter was visible as a 'tornado of dust' or 'Zodiacal Light' whirling around the sun. He also estimated the sun's age as some 32,000 years and that 'Sunlight cannot last as at present for 300,000 years'. This dramatic vision, of meteors retarded in their orbits by an aetherial medium and progressively spiralling inwards towards the sun, was analogous to James Thomson's designs for vortex turbines with maximum economy, though in the one case the result was heat, in the other work (ch.3).[77]

Thomson, however, now asked a further question: 'from what source do the planets, large and small, derive the mechanical energy of their motions?' He regarded this question as one which could be answered by the application of mechanical reasoning 'for we know that from age to age the potential energy of the mutual gravitation of those bodies is gradually expended, half in augmenting their motions, and half in generating heat'. This kind of action could be traced either backwards or forwards in time:

> backwards for a million of million of years with as little presumption as forwards for a single day. If we trace them forwards, we find that the end of this world as a habitation for man, or for any living creature or plant at present existing in it, is *mechanically inevitable*; and if we trace them backwards according to the laws of matter and motion . . . we find that a time must have been when the earth, with no sun to illuminate it, the other bodies known to us as planets, and the countless smaller planetary masses at present

> seen as the zodiacal light, must have been indefinitely remote from one another and from all other solids in space.

But Thomson now checked his unbridled enthusiasm for 'mechanical' reasoning backwards in time. 'All such conclusions', he emphasized, 'are subject to limitations, as we do not know at what moment a creation of matter or energy may have given a beginning, beyond which mechanical speculations cannot lead us'. Because the omnipotent God could have chosen to create matter or energy out of nothing at any time, human reasoning from present to past time was by no means infallible.

A striking instance of this point occurred with respect to the earth's history. Mechanical reasoning showed that the earth had been once without tenants. In Thomson's view science could not point to any antecedent or past state for the appearance of life on earth 'except the Will of a Creator', a view which he modified in 1871 to allow for the arrival by meteors of primitive life on earth. With these limitations in mind, Thomson nevertheless urged that 'we may legitimately push [our speculations] . . . into endless futurity, and we can be stopped by no barrier of past time' unless we arrived at a state of matter in some finite past epoch 'derivable from no antecedent by natural laws'.

Thomson concluded 'that the bodies now constituting our solar system have been at infinitely greater distances from one another in space than they are now' or that '*the potential energy of gravitation may be in reality the ultimate created antecedent of all the motion, heat, and light at present in the universe*'. He used this conclusion to attack the 'ordinarily stated' nebular theory (promoted and popularized by his old Glasgow teacher, John Pringle Nichol) which assumed primitive matter in a gaseous state. In contrast the new view showed evaporation of matter into a gaseous state to be 'a necessary consequence of heat generated by collisions and friction'.[78]

At this stage Thomson referred to Helmholtz's recent Königsberg lecture, 'On the Interaction of Natural Forces' (February 1854), in which the particles constituting the sun's present mass were supposed drawn together by gravitational attraction from a state of infinite diffusion, though not from an originally gaseous state. Helmholtz, who had attended the Hull meeting, would have been well acquainted with Hopkins's presentation of the new 'North British' views. Against this context, he had now constructed his own cosmological theory. His estimate of the heat generated, Thomson noted, yielded some 20 million times the amount of heat at present radiated by the sun in one year. But Thomson claimed that most of this heat would have been radiated off immediately, leaving not enough to account for the sun's present store of heat. He therefore reiterated preference for a meteoric theory of compensation.

Although maintaining here an independent position from that of Helmholtz, Thomson largely abandoned his meteoric theory in the early 1860s on the grounds that an adequate supply of such meteors within the earth's orbit seemed too much at variance with the observed motion of comets passing close to the sun. By 1861 he had adopted instead Helmholtz's 'contraction' model of the sun's heat in which

the progressive shrinkage of the sun under gravitational attraction released vast quantities of heat over immense periods of time. Thomson therefore informed the 1861 Manchester meeting of the BAAS that 'the sun is probably an incandescent liquid mass, radiating away heat without any appreciable compensation by the influx of meteoric matter'.[79]

Taken together, these British Association meetings in the period 1851–4 transformed the new energy physics and cosmology from hitherto largely local contexts in Manchester, Glasgow and Edinburgh to national and even international contexts. With the patronage of Sabine, representing the Royal Society and a powerful imperial strand in British science, and Hopkins, representing the latest 'progressionist' crusade of Cambridge geometrical geology, Thomson had staged little short of a *coup d'etat*. Having appropriated in turn the insights of Carnot and Helmholtz, among other continental *savants*, Thomson and his allies had launched their promotion of energy physics and cosmology at a time when the older generation of British Association lions had failed to construct and sustain a distinctively British natural philosophy. With a new science of energy in the making, Section A's 'men of armour' could now effectively discipline other, often very active sections such as geology, and police wayward popularizers such as the author of *Vestiges*. Fresh opportunities for such disciplinary authority were not long in coming (ch.9).