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Energy Conservation as an Example of Simultaneous Discovery

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Between 1842 and 1847, the hypothesis of energy conservation was publicly announced by four widely scattered European scientists—Mayer, Joule, Colding, and Helmholtz—all but the last working in complete ignorance of the others.[1] The coincidence is conspicuous,

1. J. R. Mayer, "Bemerkungen über die Kräfte der unbelebten Natur," *Ann. d. Chem. u. Pharm.*, vol. 42 (1842). I have used the reprint in J. J. Weyrauch's excellent collection, *Die Mechanik der Wärme in gesammelten Schriften von Robert Mayer* (Stuttgart, 1893), pp. 23–30. This volume is cited below as Weyrauch, I. The same author's companion volume, *Kleinere Schriften und Briefe von Robert Mayer* (Stuttgart, 1893), is cited as Weyrauch, II.

James P. Joule, "On the Calorific Effects of Magneto-Electricity, and on the Mechanical Value of Heat," *Phil. Mag.*, vol. 23 (1843). I have used the version in *The Scientific Papers of James Prescott Joule* (London, 1884), pp. 123–59. This volume is cited below as Joule, *Papers*.

L. A. Colding, "Undersögelse on de almindelige Naturkraefter og deres gjensidige Afhaengighed og isaerdeleshed om den ved visse faste Legemers Gnidning udviklede Varme," *Dansk. Vid. Selsk.* 2 (1851): 121–46. I am indebted to Miss Kirsten Emilie Hedebol for preparing a translation of this paper. It is, of course, far fuller than the unpublished original, which Colding read to the Royal Society of Denmark in 1843, but it includes much information about that original. See also, L. A. Colding, "On the History of the Principle of the Conservation of Energy," *Phil. Mag.* 27 (1864): 56–64.

yet these four announcements are unique only in combining generality of formulation with concrete quantitative applications. Sadi Carnot, before 1832, Marc Séguin in 1839, Karl Holtzmann in 1845, and G. A. Hirn in 1854, all recorded their independent convictions that heat and work are quantitatively interchangeable, and all computed a value for the conversion coefficient or an equivalent.[2] The convertibility of heat and work is, of course, only a

H. von Helmholtz, *Ueber die Erhaltung der Kraft. Eine physikalische Abhandlung* (Berlin, 1847). I have used the annotated reprint in *Wissenschaftliche Abhandlungen von Hermann Helmholtz* (Leipzig, 1882), 1: 12–75. This set is cited below as Helmholtz, *Abhandlungen*.

2. Carnot's version of the conservation hypothesis is scattered through a notebook written between the publication of his memoir in 1824 and his death in 1832. The most authoritative version of the notes is E. Picard, *Sadi Carnot, biographie et manuscript* (Paris 1927); a more convenient source is the appendix to the recent reprint of Carnot's *Réflexions sur la puissance motrice du feu* (Paris, 1953). Notice that Carnot considered the material in these notes quite incompatible with the main thesis of his famous *Réflexions*. In fact, the essentials of his thesis proved to be salvageable, but a change in both its statement and its derivation was required.

Marc Séguin, *De l'influence des chemins de fer et de l'art de les construire* (Paris, 1839), pp. xvi, 380–96.

Karl Holtzmann, *Über die Wärme und Elasticität der Gase und Dämpfe* (Mannheim, 1845). I have used the translation by W. Francis in *Taylor's Scientific Memoirs*, 4 (1846):189–217. Since Holtzmann believed in the caloric theory of heat and used it in his monograph, he is a strange candidate for a list of discoverers of energy conservation. He also believed, however, that the same amount of work spent in compressing a gas isothermally must always produce the same increment of heat in the gas. As a result he made one of the early computations of Joule's coefficient and his work is therefore repeatedly cited by the early writers on thermodynamics as containing an important ingredient of their theory. Holtzmann can scarcely be said to have caught any part of energy conservation as we define that theory today. But for this investigation of simultaneous discovery the judgment of his contemporaries is more relevant than our own. To several of them Holtzmann seemed an active participant in the evolution of the conservation theory.

G. A. Hirn, "Etudes sur les principaux phénomènes que présentent les frottements médiats, et sur les diverses manières de déterminer la valeur mécanique des matières employées au graissage des machines," *Bulletin de la société industrielle de Mulhouse* 26 (1854): 188–237; and "Notice sur les lois de la production du calorique par les frottements médiats," ibid., pp. 238–77. It is hard to believe that Hirn was completely ignorant of the special case of energy conservation, but the generality lacking in this second group of announcements occurs elsewhere in the literature of the period. Between 1837 and 1844, C. F. Mohr, William Grove, Faraday, and Liebig all described the world of phenomena as manifesting but a single "force," one which could appear in electrical, thermal, dynamical, and many other forms, but which could never, in all its transformations, be created or destroyed.[3] That so-called force is the one known to later scientists as energy.

work of Mayer, Joule, Helmholtz, Clausius, and Kelvin when he wrote the "Études" in 1854. But after reading his paper, I find his claim to independent discovery (presented in the "Notice") entirely convincing. Since none of the standard histories cite these articles or even recognizes the existence of Hirn's claim, it seems appropriate to sketch its basis here.

Hirn's investigation deals with the relative effectiveness of various engine lubricants as a function of pressure at the bearing, applied torque, etc. Quite unexpectedly, or so he says, his measurements led to the conclusion that: "The absolute quantity of caloric developed by mediated friction [e.g., friction between two surfaces separated by a lubricant] is directly and uniquely proportional to the mechanical work absorbed by this friction. And if we express the work in kilograms raised to the height of one meter and the quantity of caloric in calories, we find that the ratio of these two numbers is very nearly 0.0027 [corresponding to 370 kg.m./cal.], whatever the velocity and the temperature and whatever the lubricating material" (p. 202). Until almost 1860 Hirn had doubts about the validity of the law for impure lubricants or in the absence of lubrication (see particularly his *Récherches sur l'équivalent mécanique de la chaleur* [Paris, 1858], p. 83.) But despite these doubts, his work obviously displays one of the mid-nineteenth-century routes to an important part of energy conservation.

3. C. F. Mohr, "Ueber die Natur der Wärme," *Zeit. f. Phys.* 5 (1837): 419–45; and "Ansichten über die Natur der Wärme," *Ann. d. Chem. u. Pharm.* 24 (1837): 141–47.

William R. Grove, *On the Correlation of Physical Forces: Being the Substance of a Course of Lectures Delivered in the London Institution in the Year 1843* (London, 1846). Grove states that in this first edition he has introduced no new material since the lectures were delivered. The later and more accessible editions are greatly revised in the light of subsequent work.

Michael Faraday, *Experimental Researches in Electricity* (London, 1844), 2:101–4. The original "Seventeenth Series" of which this is a part was read to the Royal Society in March, 1840.

Justus Liebig, *Chemische Briefe* (Heidelberg, 1844), pp. 114–20. With this work, as with Grove's, one must beware of changes introduced in editions published after the conservation of energy was a recognized scientific law.

The history of science offers no more striking instance of the phenomenon known as simultaneous discovery.

Already we have named twelve men who, within a short period of time, grasped for themselves essential parts of the concept of energy and its conservation. Their number could be increased, but not fruitfully.[4] The present multiplicity sufficiently suggests that in

4. Since a few of my conclusions depend upon the particular list of names selected for study, a few words about the selection procedure seem essential. I have tried to include all the men who were thought by their contemporaries or immediate successors to have reached independently some significant part of energy conservation. To this group I have added Carnot and Hirn, whose work would surely have been so regarded if it had been known. Their lack of actual influence is irrelevant from the viewpoint of this investigation.

This procedure has yielded the present list of twelve names, and I am aware of only four others for whom a place might be claimed. They are von Haller, Roget, Kaufmann, and Rumford. Despite P. S. Epstein's impassioned defense (*Textbook of Thermodynamics* [New York, 1937], pp. 27–34), von Haller has no place on the list. The notion that fluid friction in the arteries and veins contributes to body heat implies no part of the notion of energy conservation. Any theory that accounts for frictional generation of heat can embrace von Haller's conception. A better case can be made for Roget, who did use the impossibility of perpetual motion to argue against the contact theory of galvanism (see note 27). I have omitted him only because he seems unaware of the possibility of extending the argument and because his own conceptions are duplicated in the work of Faraday, who did extend them.

Hermann von Kaufmann probably should be included. According to Georg Helm his work is identical with Holtzmann's (*Die Energetik nach ihrer geschichtlichen Entwickelung* [Leipzig, 1898], p. 64). But I have been unable to see Kaufmann's writings, and Holtzmann's case is already somewhat doubtful, so that it has seemed better not to overload the list. As to Rumford, whose case is the most difficult of all, I shall point out below that before 1825 the dynamical theory of heat did not lead its adherents to energy conservation. Until mid-century there was no necessary, or even likely, connection between the two sets of ideas. But Rumford was more than a dynamical theorist. He also said: "It would follow necessarily, from [the dynamical theory] . . . that the sum of the active forces in the universe must always remain constant" (*Complete Works* [London, 1876], 3:172), and this does sound like energy conservation. Perhaps it is. But if so, Rumford seems totally unaware of its significance. I cannot find the remark applied or even repeated elsewhere in his works. My inclination, therefore, is to regard the sentence as an easy echo, appropriate before a French audience, of the eighteenth-century theorem about the conservation of *vis viva.* Both Daniel Bernoulli and Lavoisier and Laplace had applied that theorem to the dynamical theory before (see note 95) without obtainthe two decades before 1850 the climate of European scientific thought included elements able to guide receptive scientists to a significant new view of nature. Isolating these elements within the works of the men affected by them may tell us something of the nature of simultaneous discovery. Conceivably, it may even give substance to those obvious yet totally unexpressive truisms: "A scientific discovery must fit the times," or "The time must be ripe." The problem is challenging. A preliminary identification of the sources of the phenomenon called simultaneous discovery is therefore the main objective of this paper.

Before proceeding toward that objective, however, we must briefly pause over the phrase "simultaneous discovery" itself. Does it sufficiently describe the phenomenon we are investigating? In the ideal case of simultaneous discovery two or more men would announce the same thing at the same time and in complete ignorance of each other's work, but nothing remotely like that happened during the development of energy conservation. The violations of simultaneity and mutual influence are secondary. But no two of our men even said the same thing. Until close to the end of the period of discovery, few of their papers have more than fragmentary resemblances retrievable in isolated sentences and paragraphs. Skillful excerpting is, for example, required to make Mohr's defense of the dynamical theory of heat resemble Liebig's discussion of the intrinsic limits of the electric motor. A diagram of the overlapping passages in the papers by the pioneers of energy conservation would resemble an unfinished crossword puzzle.

Fortunately no diagram is needed to grasp the most essential differences. Some pioneers, like Séguin and Carnot, discussed only a special case of energy conservation, and these two used very different approaches. Others, like Mohr and Grove, announced a universal conservation principle, but, as we shall see, their occasional attempts to quantify their imperishable "force" leave its concrete significance in doubt. Only in view of what happened later can we say that all these partial statements even deal with the same aspect of nature.[5] Nor is this problem of divergent discoveries restricted

ing anything like energy conservation. I know of no reason to suppose that Rumford saw further than they.

5. This may well explain why the pioneers seem to have profited so little from each other's work, even when they read it. Our twelve men were not, in fact, strictly independent. Grove and Helmholtz knew Joule's work and

to those scientists whose formulations were obviously incomplete. Mayer, Colding, Joule, and Helmholtz were not saying the same things at the dates usually given for their discoveries of energy conservation. More than *amour propre* underlies Joule's subsequent claim that the discovery he had announced in 1843 was different from the one published by Mayer in 1842.[6] In these years their papers have important areas of overlap, but not until Mayer's book of 1845 and Joule's publications of 1844 and 1847 do their theories become substantially coextensive.[7]

cited it in their papers of 1843 and 1847 (Grove, *Physical Forces*, pp. 39, 52; Helmholtz, *Abhandlungen*, 1:33, 35, 37, 55). Joule, in turn, knew and cited the work of Faraday (*Papers*, p. 189). Liebig, though he did not cite Mohr and Mayer, must have known their work, for it was published in his own journal. (See also G. W. A. Kahlbaum, *Liebig und Friedrich Mohr, Briefe, 1834–1870* [Braunschweig, 1897], for Liebig's knowledge of Mohr's theory.) Very possibly more precise biographical information would disclose other interdependencies as well.

But these interdependencies, at least the identifiable ones, seem unimportant. In 1847 Helmholtz seems to have been unaware both of the generality of Joule's conclusions and of their large-scale overlap with his own. He cites only Joule's experimental findings, and these very selectively and critically. Not until the priority controversies of the second half-century, does Helmholtz seem to have recognized the extent to which he had been anticipated. Much the same holds for the relation between Joule and Faraday. From the latter Joule took illustrations, but not inspiration. Liebig's case may prove even more revealing. He could have neglected to cite Mohr and Mayer simply because they provided no relevant illustration and did not even seem to be dealing with the same subject matter. Apparently the men whom we call early exponents of energy conservation could occasionally read each other's works without quite recognizing that they were talking about the same things. For that matter, the fact that so many of them wrote from different professional and intellectual backgrounds may account for the infrequency with which they even saw each other's writings.

6. J. P. Joule, "Sur l'équivalent mécanique du calorique," *Comptes rendus* 28 (1849): 132–35. I have used the reprint in Weyrauch, II, pp. 276–80. This is only the first salvo in the priority controversy, but it already shows what the controversy is going to be about. Which of two (and later more than two) different statements is to be equated with *the* conservation of energy?

7. J. R. Mayer, *Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel* (Heilbronn, 1845) in Weyrauch, I, pp. 45–128. Most of Joule's papers between 1843 and 1847 are relevant, but particularly: "On the Changes of Temperature Produced by the Rarefaction and Condensation

In short, though the phrase "simultaneous discovery" points to the central problem of this paper, it does not, if taken at all literally, describe it. Even to the historian acquainted with the concepts of energy conservation, the pioneers do not all communicate the same thing. To each other, at the time, they often communicated nothing at all. What we see in their works is not really the simultaneous discovery of energy conservation. Rather it is the rapid and often disorderly emergence of the experimental and conceptual elements from which that theory was shortly to be compounded. It is these elements that concern us. We know why they were there: Energy *is* conserved; nature behaves that way. But we do not know why these elements suddenly became accessible and recognizable. That is the fundamental problem of this paper. Why, in the years 1830–50, did so many of the experiments and concepts required for a full statement of energy conservation lie so close to the surface of scientific consciousness?[8]

This question could easily be taken as a request for a list of all those almost innumerable factors that caused the individual pioneers to make the particular discoveries that they did. Interpreted in this way, it has no answer, at least none that the historian can give. But the historian can attempt another sort of response. A contemplative immersion in the works of the pioneers and their contemporaries may reveal a subgroup of factors which seem more significant than the others, because of their frequent recurrence, their specificity to the period, and their decisive effect upon individual research.[9] The depth of my acquaintance with the literature permits, as yet, no definitive judgments. Nevertheless, I am already quite sure about two such factors, and I suspect the relevance of a third. Let me call them the "availability of conversion processes," the "concern with engines," and the "philosophy of nature." I shall consider them in order.

The availability of conversion processes resulted principally from the stream of discoveries that flowed from Volta's invention of the battery in 1800. According to the theory of galvanism most prevalent, at least, in France and England, the electric current was itself gained at the expense of forces of chemical affinity, and this conversion proved to be only the first step in a chain.[10] Electric cur-

of Air" (1845) and "On Matter, Living Force, and Heat" (1847) in *Papers*, pp. 172–89, 265–81.

8. This formulation has at least one considerable advantage over the usual version. It does not imply or even permit the question, "Who *really* discovered conservation of energy first?" As a century of fruitless controversy has demonstrated, a suitable extension or restriction in the definition of energy conservation will award the crown to almost any one of the pioneers, an additional indication that they cannot have discovered the same thing.

The present formulation also bars a second impossible question, "Did Faraday (or Séguin, or Mohr, or any one of the other pioneers, at will) really grasp the concept of energy conservation, even intuitively? Does he really belong on the list of pioneers?" Those questions have no conceivable answer, except in terms of the respondent's taste. But whatever answer taste may dictate, Faraday (or Séguin, etc.) provides useful evidence about the forces that led to the discovery of energy conservation.

9. These three criteria, particularly the second and third, determine the orientation of this study in a way that may not be immediately apparent. They direct attention away from the *prerequisites* to the discovery of energy conservation and toward what might be called the *trigger-factors* responsible for simultaneous discovery. For example, the following pages will show implicitly that all of the pioneers made significant use of the conceptual and experimental elements of calorimetry and that many of them also depended upon the new chemical conceptions derived from the work of Lavoisier and his contemporaries. These and many other developments within the sciences presumably had to occur before conservation of energy, as we know it, could be discovered. I have not, however, explicitly isolated elements like these below, because they do not seem to distinguish the pioneers from their predecessors. Since both calorimetry and the new chemistry had been the common property of all scientists for some years before the period of simultaneous discovery, they cannot have provided the immediate stimuli that triggered the work of the pioneers. As prerequisites for discovery, these elements have an interest and importance all their own. But their study is unlikely to illuminate very much the problem of simultaneous discovery to which this paper is directed. [This note has been added to the original manuscript in response to points raised during the discussion that followed the oral presentation.]

10. Faraday provides scarce and useful information about the progress of the significant controversy between the exponents of the chemical and contact theories of galvanism (*Experimental Researches*, 2: 18–20). According to his account, the chemical theory was dominant in France and England from at least 1825 on, but the contact theory was still dominant in Germany and Italy when Faraday wrote in 1840. Does the dominance of the contact theory in Germany account for the rather surprising way in which both Mayer and Helmholtz neglect the battery in their accounts of energy transformations?

rent invariably produced heat and, under appropriate conditions, light as well. Or, by electrolysis, the current could vanquish forces of chemical affinity, bringing the chain of transformations full circle. These were the first fruits of Volta's work; other more striking conversion discoveries followed during the decade and a half after 1820.[11] In that year Oersted demonstrated the magnetic effects of a current; magnetism, in turn, could produce motion, and motion had long been known to produce electricity through friction. Another chain of conversions was closed. Then, in 1822, Seebeck showed that heat applied to a bimetallic junction would produce a current directly. Twelve years later Peltier reversed this striking example of conversion, demonstrating that the current could, on occasions, absorb heat, producing cold. Induced currents, discovered by Faraday in 1831, were only another, if particularly striking, member of a class of phenomena already characteristic of nineteenth-century science. In the decade after 1827, the progress of photography added yet another example, and Melloni's identification of light with radiant heat confirmed a long-standing suspicion about the fundamental connection between two other apparently disparate aspects of nature.[12]

Some conversion processes had, of course, been available before 1800. Motion had already produced electrostatic charges, and the resulting attractions and repulsions had produced motion. Static generators had occasionally engendered chemical reactions, including dissociations, and chemical reactions produced both light and heat.[13] Harnessed by the steam engine, heat could produce motion, and motion, in turn, engendered heat through friction and percussion. Yet in the eighteenth century these were isolated phenomena; few seemed of central importance to scientific research; and

11. For the following discoveries see Sir Edmund Whittaker, *A History of the Theories of Aether and Electricity*, vol. 1, *The Classical Theories*, 2d ed. (London, 1951), pp. 81–84, 88–89, 170–71, 236–37. For Oersted's discovery see also, R. C. Stauffer, "Persistent Errors Regarding Oersted's Discovery of Electromagnetism," *Isis* 44 (1953): 307–10.

12. F. Cajori, *A History of Physics* (New York, 1922), pp. 158, 172–74. Grove makes a particular point of the early photographic processes (*Physical Forces*, pp. 27–32). Mohr gives great emphasis to Melloni's work (*Zeit. f. Phys.* 5 [1837]: 419).

13. For the chemical effects of static electricity see Whittaker, *Aether and Electricity*, 1:74, n. 2.

those few were studied by different groups. Only in the decade after 1830, when they were increasingly classified with the many other examples discovered in rapid succession by nineteenth-century scientists, did they begin to look like conversion processes at all.[14] By that time scientists were proceeding inevitably in the laboratory from a variety of chemical, thermal, electrical, magnetic, or dynamical phenomena to phenomena of any of the other types and to optical phenomena as well. Previously separate problems were gaining multiple interrelationships, and that is what Mary Sommerville had in mind when, in 1834, she gave her famous popularization of science the title, *On the Connexion of the Physical Sciences*. "The progress of modern science," she said in her preface, "especially within the last five years, has been remarkable for a tendency to . . . unite detached branches [of science, so that today] . . . there exists such a bond of union, that proficiency cannot be attained in any one branch without a knowledge of others."[15] Mrs. Sommerville's remark isolates the "new look" that physical science had acquired between 1800 and 1835. That new look, together with the discoveries that produced it, proved to be a major requisite for the emergence of energy conservation.

Yet, precisely because it produced a "look" rather than a single clearly defined laboratory phenomenon, the availability of conversion processes enters the development of energy conservation in an immense variety of ways. Faraday and Grove achieved an idea very close to conservation from a survey of the whole network of conversion processes taken together. For them conservation was quite literally a rationalization of the phenomenon Mrs. Sommerville described as the new "connexion." C. F. Mohr, on the other hand, took the idea of *conservation* from a quite different source, probably metaphysical.[16] But, as we shall see, it is only because he attempted to elucidate and defend this idea in terms of the new

14. The single exception is significant and is discussed at some length below. During the eighteenth century steam engines were occasionally regarded as conversion devices.

15. Mary Sommerville, *On the Connexion of the Physical Sciences* (London, 1834), unpaginated Preface.

16. Reasons for distinguishing Mohr's approach from that of Grove and Faraday will be examined below (note 83). The accompanying text will consider possible sources of Mohr's conviction about the conservation of "force."

conversion processes that Mohr's initial conception came to look like conservation *of energy.* Mayer and Helmholtz present still another approach. They began by applying their concepts of conservation to well-known older phenomena. But until they extended their theories to embrace the new discoveries, they were not developing the same theory as men like Mohr and Grove. Still another group, consisting of Carnot, Séguin, Holtzmann, and Hirn, ignored the new conversion processes entirely. But they would not be discoverers of energy conservation if men like Joule, Helmholtz, and Colding had not shown that the thermal phenomena with which these steam engineers dealt were integral parts of the new network of conversions.

There is, I think, excellent reason for the complexity and variety of these relationships. In an important sense, though one which will demand later qualification, the conservation of energy is nothing less than the theoretical counterpart of the laboratory conversion processes discovered during the first four decades of the nineteenth century. Each laboratory conversion corresponds in the theory to a transformation in the form of energy. That is why, as we shall see, Grove and Faraday could derive conservation from the network of laboratory conversions itself. But the very homomorphism between the theory, energy conservation, and the earlier network of laboratory conversion processes indicates that one did not have to start by grasping the network whole. Liebig and Joule, for example, started from a single conversion process and were led by the connection between the sciences through the entire network. Mohr and Colding started with a metaphysical idea and transformed it by application to the network. In short, just because the new nineteenth-century discoveries formed a network of connections between previously distinct parts of science, they could be grasped either individually or whole in a large variety of ways and still lead to the same ultimate result. That, I think, explains why they could enter the pioneers' research in so many different ways. More important, it explains why the researches of the pioneers, despite the variety of their starting points, ultimately converged to a common outcome. What Mrs. Sommerville had called the new connections between the sciences often proved to be the links that joined disparate approaches and enunciations into a single discovery.

The sequence of Joule's researches clearly illustrates the way in which the network of conversion processes actually marked out the experimental ground of energy conservation and thus provided the essential links between the various pioneers. When Joule first wrote in 1838, his exclusive concern with the design of improved electric motors effectively isolates him from all the other pioneers of energy conservation except Liebig. He was simply working on one of the many new problems born from nineteenth-century discovery. By 1840 his systematic evaluations of motors in terms of work and "duty" establishes a link to the researches of the steam engineers, Carnot, Séguin, Hirn, and Holtzmann.[17] But these connections vanished in 1841 and 1842, when Joule's discouragement with motor design forced him to seek instead a fundamental improvement in the batteries that drove them. Now he was concerned with new discoveries in chemistry, and he absorbed entirely Faraday's view of the essential role of chemical processes in galvanism. In addition, his research in these years was concentrated upon what turned out to have been two of the numerous conversion processes selected by Grove and Mohr to illustrate their vague metaphysical hypothesis.[18] The connections with the work of other pioneers are steadily increasing in number.

In 1843, prompted by the discovery of an error in his earlier work with batteries, Joule reintroduced the motor and the concept of mechanical work. The link to steam engineering was thus established, and simultaneously Joule's papers began, for the first time, to read like investigations of energy relations.[19] But even in 1843 the resemblance to energy conservation was incomplete. Only as

17. The first eleven items in Joule's *Papers* (pp. 1–53) are exclusively concerned with improving first motors and then electromagnets, and these items cover the period 1838–41. The systematic evaluations of motors in terms of the engineering concepts, work and "duty," occur on pp. 21–25, 48. For Joule's earliest published use of the concept work or its equivalent, see p. 4.

18. Joule's concern with batteries and more particularly with the electrical production of heat by batteries dominates the five major contributions in *Papers*, pp. 53–123. My remark that Joule was led to batteries by his discouragement with motor design is a conjecture, but it seems extremely probable.

19. See note 1. This is the paper in which Joule is usually said to have announced energy conservation.

Joule traced still other new connections during the years 1844–47 did his theory really encompass the views of such disparate figures as Faraday, Mayer, and Helmholtz.[20] Starting from an isolated problem, Joule had involuntarily traced much of the connective tissue between the new nineteenth-century discoveries. As he did so, his work was linked increasingly to that of the other pioneers, and only when many such links had appeared did his discovery resemble energy conservation.

Joule's work shows that energy conservation could be discovered by starting from a single conversion process and tracing the network. But, as we have already indicated, that is not the only way in which conversion processes could effect the discovery of energy conservation. C. F. Mohr, for example, probably drew his initial concept of conservation from a source independent of the new conversion processes, but then used the new discoveries to clarify and elaborate his ideas. In 1839, close to the end of a long and often incoherent defense of the dynamical theory of heat, Mohr suddenly burst out: "Besides the known 54 chemical elements, there is, in the nature of things, only one other agent, and that is called force; it can appear under various circumstances as motion, chemical affinity, cohesion, electricity, light, heat, and magnetism, and from any one of these types of phenomena all the others can be called forth."[21] A knowledge of energy conservation makes the import of these sentences clear. But in the absence of such knowledge, they would have been almost meaningless except that Mohr proceeded immediately to two systematic pages of experimental examples. The experiments were, of course, just the new and old conversion processes listed above, the new ones in the lead, and they are essential to Mohr's argument. They alone specify his subject and show its close similarity to Joule's.

Mohr and Joule illustrate two of the ways in which conversion processes could affect the discoverers of energy conservation. But, as my final example from the works of Faraday and Grove will indicate, these are not the only ways. Though Faraday and Grove reached conclusions much like Mohr's, their route to the conclusions includes none of the same sudden leaps. Unlike Mohr, they seem to have derived energy conservation directly from the experimental conversion processes that they had already studied so fully in their own researches. Because their route is continuous, the homomorphism of energy conservation with the new conversion processes appears most clearly of all in their work.

20. See note 7.

21. *Zeit. f. Phys.* 5 (1837): 442.

In 1834, Faraday concluded five lectures on the new discoveries in chemistry and galvanism with a sixth on the "Relations of Chemical Affinity, Electricity, Heat, Magnetism, and Other Powers of Matter." His notes supply the gist of this last lecture in the words: "We cannot say that any one [of these powers] is the cause of the others, but only that all are connected and due to one common cause." To illustrate the connection, Faraday then gave nine experimental demonstrations of "the production of any one [power] from another, or the conversion of one into another."[22] Grove's development seems parallel. In 1842 he included a remark almost identical with Faraday's in a lecture with the significant title, "On the Progress of Physical Science."[23] In the following year he expanded this isolated remark into his famous lecture series, *On the Correlation of Physical Forces.* "The position which I seek to establish in this Essay is," he said, "that [any one] of the various imponderable agencies . . . viz., Heat, Light, Electricity, Magnetism, Chemical Affinity, and Motion, . . . may, as a force, produce or be convertible into the other[s]; thus heat may mediately or immediately produce electricity, electricity may produce heat; and so of the rest."[24]

This is the concept of the universal convertibility of natural powers, and it is not, let us be clear, the same as the notion of conservation. But most of the remaining steps proved to be small and rather obvious.[25] All but one, to be discussed below, can be taken by applying to the concept of universal convertibility the perennially serviceable philosophic tags about the equality of cause and

22. Bence Jones, *The Life and Letters of Faraday* (London, 1870), 2:47.

23. *A Lecture on the Progress of Physical Science since the Opening of the London Institution* (London, 1842). Though the title page is dated 1842, the date is immediately followed by "[Not Published]." I do not know when the actual printing took place, but a prefatory remark of the author's indicates that the text itself was written very shortly after the lecture was delivered.

24. *Physical Forces*, p. 8.

25. Reasons for calling the remaining steps "obvious" are given in the closing paragraphs of this paper (see note 92).

effect or the impossibility of perpetual motion. Since any power can produce any other *and be produced by it*, the equality of cause and effect demands a uniform quantitative equivalence between each pair of powers. If there is no such equivalence, then a properly chosen series of conversions will result in the creation of power, that is, in perpetual motion.[26] In all its manifestations and conversions, power must be conserved. This realization came neither all at once, nor fully to all, nor with complete logical rigor. But it did come.

Though he had no general conception of conversion processes, Peter Mark Roget, in 1829, opposed Volta's contact theory of galvanism because it implied a creation of power from nothing.[27] Faraday independently reproduced the argument in 1840 and immediately applied it to conversions in general. "We have," he said, "many processes by which the form of the power may be so changed that an apparent *conversion* of one into another takes place. . . . But in no cases . . . is there a pure creation of force; a production of power without a corresponding exhaustion of something to supply it."[28] In 1842 Grove devised the argument once more in order to prove the impossibility of inducing an electric current from static magnetism, and in the following year he generalized still further.[29] If it were true, he wrote, "that motion [could] be subdivided or changed in character, so as to become heat, electricity, etc.; it ought to follow, that when we collect the dissipated and changed forces, and reconvert them, the initial motion, affecting the same amount of matter with the same velocity, should be reproduced, and so of the change of matter produced by the other forces."[30] In the context of Grove's exhaustive discussion of the known conversion processes, this quotation is a full statement of all but the quantitative components of energy conservation. Furthermore, Grove knew what was missing. "The great problem that remains to be solved, in regard to the correlation of physical

26. Strictly speaking, this derivation is valid only if all the transformations of energy are reversible, which they are not. But that logical shortcoming completely escaped the notice of the pioneers.

27. P. M. Roget, *Treatise on Galvanism* (London, 1829). I have seen only the excerpt quoted by Faraday, *Experimental Researches*, 2:103, n. 2.

28. *Experimental Researches*, 2:103.

29. *Progress of Physical Science*, p. 20.

30. *Physical Forces*, p. 47.

forces, is," he wrote, "the establishment of their equivalent of power, or their measurable relation to a given standard."[31] Conversion phenomena could carry scientists no further toward the enunciation of energy conservation.

Grove's case brings this discussion of conversion processes almost full circle. In his lectures energy conservation appears as the straightforward theoretical counterpart of nineteenth-century laboratory discoveries, and that was the suggestion from which I began. Only two of the pioneers, it is true, actually derived their versions of energy conservation from these new discoveries alone. But because such a derivation was possible, every one of the pioneers was decisively affected by the availability of conversion processes. Six of them dealt with the new discoveries from the start of their research. Without these discoveries, Joule, Mohr, Faraday, Grove, Liebig, and Colding would not be on our list at all.[32] The other six pioneers show the importance of conversion processes in a subtler but no less important way. Mayer and Helmholtz were late in turning to the new discoveries, but only when they did so, did they become candidates for the same list as the first six. Carnot, Séguin, Hirn, and Holtzmann are the most interesting of all. None of them even mentioned the new conversion processes. But their contributions, being uniformly obscure, would have vanished from history entirely if they had not been gathered into the larger network explored by the men we have already examined.[33] When con-

31. Ibid., p. 45.

32. I am not quite sure that this is true of Colding, particularly since I have not seen his unpublished paper of 1843. The early pages of his 1851 paper (note 1) contain many examples of conversion processes and are thus reminiscent of Mohr's approach. Also, Colding was a protegé of Oersted, whose chief renown derived from his discovery of electromagnetic conversions. On the other hand, most of the conversion processes cited explicitly by Colding date from the eighteenth century. In Colding's case, I suspect a prior tie between conversion processes and metaphysics (see note 83 and accompanying text). Very probably neither can be viewed as either logically or psychologically the more fundamental in the development of his thought.

33. Carnot's notes were not published until 1872 and then only because they contained anticipations of an important scientific law. Séguin had to call attention to the relevant passages in his book of 1839. Hirn did not bother to claim credit, but only attached a note denying plagiarism to his 1854 paper. That paper was published in an engineering journal that I

version processes did not govern an individual's work, they often governed that work's reception. If they had not been available, the problem of simultaneous discovery might not exist at all. Certainly it would look very different.

Nevertheless, the view which Grove and Faraday derived from conversion processes is not identical with what scientists now call the conservation of energy, and we must not underestimate the importance of the missing element. Grove's *Physical Forces* contains the layman's view of energy conservation. In an expanded and revised form it proved to be one of the most effective and sought-after popularizations of the new scientific law.[34] But this role was achieved only after the work of Joule, Mayer, Helmholtz, and their successors had provided a full quantitative substructure for the conception of force correlation. Anyone who has worked through a mathematical and numerical treatment of energy conservation may well wonder whether, in the absence of such substructure, Grove would have had anything to popularize. The "measurable relation to a given standard" of the various physical forces is an essential ingredient of energy conservation as we know it, and neither Grove, Faraday, Roget, nor Mohr was able even to approach it.

The quantification of energy conservation proved, in fact, insuperably difficult for those pioneers whose principal intellectual equipment consisted of concepts related to the new conversion processes. Grove thought he had found the clue to quantification in Dulong and Petit's law relating chemical affinity and heat.[35] Mohr believed he had produced the quantitative relationship when

have never seen cited by a scientist. Holtzmann's paper is the exception in that it was not obscure. But if other men had not discovered conservation of energy, Holtzmann's memoir would have continued to look like another one of the extensions of Carnot's memoir, for that is basically what it was (see note 2).

34. Between 1850 and 1875 Grove's book was reprinted at least six times in England, three times in America, twice in France, and once in Germany. The extensions were, of course, numerous, but I am aware of only two essential revisions. In the original discussion of heat (pp. 8–11), Grove suggested that macroscopic motion appears as heat only to the extent that it is *not* transformed to microscopic motion. In addition, of course, Grove's few attempts at quantification were quite off the track (see below).

35. *Physical Forces*, p. 46.

he equated the heat employed to raise the temperature of water 1° with the static force necessary to compress the same water to its original volume.[36] Mayer initially measured force by the momentum which it could produce.[37] These random leads were all totally unproductive, and of this group only Mayer succeeded in transcending them. To do so he had to use concepts belonging to a very different aspect of nineteenth-century science, an aspect to which I previously referred as the concern with engines, and whose existence I shall now take for granted as a well-known by-product of the Industrial Revolution. As we examine this aspect of science, we shall find the main source of the concepts—particularly of mechanical effect or work—required for the quantitative formulation of energy conservation. In addition, we shall find a multitude of experiments and of qualitative conceptions so closely related to energy conservation that they collectively provide something very like a second and independent route to it.

Let me begin by considering the concept of work. Its discussion will provide relevant background as well as opportunity for a few essential remarks on a more usual view about the sources of the quantitative concepts underlying energy conservation. Most histories or prehistories of the conservation of energy imply that the model for quantifying conversion processes was the dynamical theorem known almost from the beginning of the eighteenth century as the conservation of *vis viva*.[38] That theorem has a distinguished role in the history of dynamics, and it also turns out to have been a special case of energy conservation. It could have provided a model. Yet I think the prevalent impression that it did so is misleading. The conservation of *vis viva* was important to Helmholtz's derivation of energy conservation, and a special case (free fall) of the same dynamical theorem was ultimately of great assistance to Mayer. But these men also drew significant elements from a second generally separate tradition—that of water, wind,

36. *Zeit. f. Phys.* 5 (1837): 422–23.

37. Weyrauch, II, pp. 102–5. This is in his first paper, "Ueber die quantitative und qualitative Bestimmung der Kräfte," sent to Poggendorf in 1841 but not published until after Mayer's death. Before he wrote his second paper, the first to be published, Mayer had learned a bit more physics.

38. It would be more precise to say that most prehistories of energy conservation are principally lists of anticipations, and these occur particularly often in the early literature on *vis viva*.

and steam engineering—and that tradition is all important to the work of the other five pioneers who produced a quantitative version of energy conservation.

There is excellent reason why this should be so. *Vis viva* is mv^2, the product of mass by the square of velocity. But until a late date that quantity appears in the works of none of the pioneers except Carnot, Mayer, and Helmholtz. As a group the pioneers were scarcely interested in energy of motion, much less in using it as a basic quantitative measure. What they did use, at least those who were successful, was $f{\cdot}s$, the product of force times distance, a quantity known variously under the names mechanical effect, mechanical power, and work. That quantity does not, however, occur as an independent conceptual entity in the dynamical literature. More precisely it scarcely occurs there until 1820, when the French (and only the French) literature was suddenly enriched by a series of theoretical works on such subjects as the theory of machines and of industrial mechanics. These new books did make work a significant independent conceptual entity, and they did relate it explicitly to *vis viva.* But the concept was not invented for these books. On the contrary it was borrowed from a century of engineering practice where its use had usually been quite independent of both *vis viva* and its conservation. That source within the engineering tradition is all that the pioneers of energy conservation required and as much as most of them used.

Another paper will be needed to document this conclusion, but let me illustrate the considerations from which it derives. Until 1743 the general dynamical significance of the conservation of *vis viva* must be recaptured from its application to two special sorts of problems: elastic impact and constrained fall.[39] Force times distance has no relevance to the former, since elastic impact numerically conserves *vis viva.* In other applications, for example, the bachistochrone and isochronous pendulum, vertical displacement rather than force times distance appears in the conservation theorem. Huyghen's statement that the center of gravity of a system of masses can ascend no higher than its initial position of rest is typical.[40] Compare Daniel Bernoulli's famous formulation of 1738: Conservation of *vis viva* is "the equality of actual descent with potential ascent."[41]

The more general formulations, inaugurated by d'Alembert's *Traité* in 1743, suppress even vertical displacement, which might conceivably be called an embryonic conception of work. D'Alembert states that the forces acting on a system of interconnected bodies will increase its *vis viva* by the amount $\Sigma m_i\, u_i^2$, where the u_i are the velocities that the masses m_i would have acquired if moved freely over the same paths by the same forces.[42] Here, as in Daniel Bernoulli's subsequent version of the general theorem, force times distance enters only in certain particular applications to permit the computation of individual u_i's; it has neither general significance nor a name; *vis viva* is the conceptual parameter.[43] The same parameter dominates the later analytic formulations. Euler's *Mechanica*, Lagrange's *Mécanique analytique*, and Laplace's *Mé-*

39. The early eighteenth-century literature contains many general statements about the conservation of *vis viva* regarded as a metaphysical force. These formulations will be discussed briefly below. For the present notice only that none of them is suitable for application to the technical problems of dynamics, and it is with those formulations that we are here concerned. An excellent discussion of both the dynamical and metaphysical formulations is included in A. E. Haas, *Die Entwicklungsgeschichte des Satzes von der Erhaltung der Kraft* (Vienna, 1909), generally the fullest and most reliable prehistory of energy conservation. Other useful details can be found in Hans Schimank, "Die geschichtliche Entwicklung des Kraftbegriffs bis zum Aufkommen der Energetik," in *Robert Mayer und das Energieprinzip, 1842–1942*, ed. H. Schimank and E. Pietsch (Berlin, 1942). I am indebted to Professor Erwin Hiebert for calling these two useful and little known works to my attention.

40. Christian Huyghens, *Horologium oscillatorium* (Paris, 1673). I have used the German edition, *Die Pendeluhr*, ed. A. Heckscher and A. V. Oettingen, Ostwald's Klassiker der Exakten Wissenschaften, no. 192 (Leipzig, 1913), p. 112.

41. D. Bernoulli, *Hydrodynamica, sive de viribus et motibus fluidorum, commentarii* (Basel, 1738), p. 12.

42. J. L. d'Alembert, *Traité de dynamique* (Paris, 1743). I have been able to see only the second edition (Paris, 1758), where the relevant material occurs on pp. 252–53. D'Alembert's discussion of the changes introduced since the first edition give no reason to suspect he has altered the original formulation at this point.

43. D. Bernoulli, "Remarques sur le principe de la conservation des forces vives pris dans un sense général," *Hist. Acad. de Berlin* (1748), pp. 356–64.

canique céleste give exclusive emphasis to central forces derivable from potential functions.[44] In these works the integral of force times differential path element occurs only in the derivation of the conservation law. The law itself equates *vis viva* with a function of position coordinates.

Not until 1782, in Lazare Carnot's *Essai sur les machines en général*, did force times distance begin to receive a name and a conceptual priority in dynamical theory.[45] Nor was this new dynamical view of the concept work really worked out or propagated until the years 1819–39, when it received full expression in the works of Navier, Coriolis, Poncelet, and others.[46] All these works are concerned with the analysis of machines in motion. As a result, work—the integral force with respect to distance—is their fundamental conceptual parameter. Among other significant and typical results of this reformulation were the introduction of the term "work" and of units for its measure, the redefinition of *vis viva* as $\frac{1}{2}mv^2$ to preserve the conceptual priority of the measure work, and the explicit formulation of the conservation law in terms of the equality of work done and kinetic energy created.[47] Only when thus reformulated did the conservation of *vis viva* provide a

44. L. Euler, *Mechanica sive motus scientia analytice exposita*, in *Opera omnia* (Leipzig and Berlin, 1911–), ser. 2, 2:74–77. The first edition was St. Petersburg, 1736.

J.-L. Lagrange, *Mécanique analytique* (Paris, 1788), pp. 206–9. I cite the first edition because the second, as reprinted in volumes 11 and 12 of Lagrange's *Oeuvres* (Paris, 1867–92), contains a very significant change. In the first edition, the conservation of *vis viva* is formulated only for time-independent constraints and for central or other integrable forces. It then takes the form $\Sigma m_i v_i^2 = 2H + 2\Sigma m_i \pi_i$, where H is a constant of integration and the π_i are functions of the position coordinates. In the second edition, Paris, 1811–15 (*Oeuvres*, 11:306–10), Lagrange repeats the above but restricts it to a particular class of elastic bodies in order to take account of Lazare Carnot's engineering treatise (note 45), which he cites. For a fuller account of the engineering problem treated by Carnot, he refers his readers to his own *Théorie des fonctions analytiques* (Paris, 1797), pp. 399–410, where his version of Carnot's engineering problem is formulated more explicitly. That formulation makes the impact of the engineering tradition quite apparent, for the concept work now begins to appear. Lagrange states that the increment of *vis viva* between two dynamical states of the system is $2(P) + 2(Q) + \ldots$, where (P)—Lagrange calls it an "aire"—is $\Sigma_i \int P_i \, dp_i$, and P_i is the force on the i'th body in the direction of the position coordinates p_i. These "aires" are, of course, just work.

P. S. Laplace, *Traité de mécanique céleste* (Paris, 1798–1825). The relevant passages are more readily found in *Oeuvres complètes* (Paris, 1878–1904), 1:57–61. Mathematically, this treatment of 1798 actually resembles Lagrange's 1797 formulation rather than the earlier 1788 form. But, as in the pre-engineering formulations, the conservation law which includes a work integral is rapidly passed over in favor of the more restricted statement employing a potential function.

45. L. N. M. Carnot, *Essai sur les machines en général* (Dijon, 1782). I have consulted this work in Carnot's *Oeuvres mathématiques* (Basel, 1797) but rely principally on the expanded and more influential second edition, *Principes fondementaux de l'équilibre et du mouvement* (Paris, 1803). Carnot introduces several terms for what we call work, the most important being, "force vive latent" and "moment d'activité" (ibid., pp. 38–43). Of these he says, "The kind of quantity to which I have given the name *moment of activity* plays a very large role in the theory of machines in motion: for in general it is this quantity which one must economize as much as possible in order to derive from an agent [i.e., a source of power] all the [mechanical] effect of which it is capable" (ibid., p. 257).

46. A useful survey of the early history of this important movement is C. L. M. H. Navier, "Détails historiques sur l'emploi du principes des forces vives dans la théorie des machines et sur diverses roues hydrauliques," *Ann. Chim. Phys.* 9 (1818): 146–59. I suspect that Navier's edition of B. de F. Belidor's *Architecture hydraulique* (Paris, 1819) contains the first developed presentation of the new engineering physics, but I have not yet seen this work. The standard treatises are: G. Coriolis, *Du calcul de l'effet des machines, ou considérations sur l'emploi des moteurs et sur leur évaluation pour servir d'introduction à l'étude speciale des machines* (Paris, 1829); C. L. M. H. Navier, *Résumé des leçons données a l'école des ponts et chaussées sur l'application de la mécanique à l'établissement des constructions et des machines* (Paris, 1838), vol. 2; and J.-V. Poncelet, *Introduction à la mécanique industrielle*, ed. Kratz, 3d ed. (Paris, 1870). This work originally appeared in 1829 (part had appeared in lithoprint in 1827); the much enlarged and now standard edition from which the third is taken appeared in 1830–39.

47. The formal adoption of the term *work* (*travail*) is often credited to Poncelet (*Introduction*, p. 64), though many others had used it casually before; Poncelet also (pp. 74–75) gives a useful account of the units (*dynamique, dyname, dynamie*, etc.) commonly used to measure this quantity. Coriolis (*Du calcul de l'effet des machines*, p. iv) is the first to insist that *vis viva* be $\frac{1}{2}mv^2$, so that it will be numerically equal to the work it can produce; he also makes much use of the term *travail*, which Poncelet may have borrowed from him. The reformulation of the conservation law proceeds gradually from Lazare Carnot through all these later works.

convenient conceptual model for the quantification of conversion processes, and then almost none of the pioneers used it. Instead, they returned to the same older engineering tradition in which Lazare Carnot and his French successors had found the concepts needed for their new versions of the dynamical conservation theorem.

Sadi Carnot is the single exception. His manuscript notes proceed from the assertion that heat is motion to the conviction that it is molecular *vis viva* and that its increment must therefore be equal to work done. These steps imply an immediate command of the relation between work and *vis viva*. Mayer and Helmholtz might also have been exceptions, for both could have made good use of the French reformulation. But neither seems to have known it. Both began by taking work (or rather the product of weight times height) as the measure of "force," and each then rederived something very like the French reformulation for himself.[48] The other

48. As soon as he considers a quantitative problem in his first published paper, Mayer says: "A cause, which effects the raising of a weight, is a force; since this force brings about the fall of a body, we shall call it fall-force [Fallkraft]" (Weyrauch, I, p. 24). This is the engineering, not the theoretical dynamical, measure. By applying it to the problem of free fall, Mayer immediately derives $\frac{1}{2}mv^2$ (note the fraction) as the measure of energy of motion. The very crudeness of his derivation together with its lack of generality indicates his ignorance of the French engineering texts. The one French text he does mention in his writings (G. Lamé, *Cours de physique de l'école polytechnique*, 2d ed. [Paris, 1840]) does not deal with *vis viva* or its conservation at all.

Helmholtz uses the terms *Arbeitskraft, bewegende Kraft, mechanische Arbeit*, and *Arbeit* for his fundamental measurable force (Helmholtz, *Abhandlungen*, I, 12, 17–18). I have not as yet been able to trace these terms in the earlier German literature, but their parallels in the French and English engineering traditions are obvious. Also, the term *bewegende Kraft* is used by the translator of Clapeyron's version of Sadi Carnot's memoir as equivalent to the French *puissance motrice* (*Pogg. Ann.* 59 [1843]: 446), and Helmholtz cites this translation (p. 17, n. 1). To this extent the tie to the engineering tradition is explicit.

Helmholtz was not, however, aware of the French theoretical engineering tradition. Like Mayer, he derives the factor of ½ in the definition of energy of motion and is unaware of any precedent for it (p. 18). More significant, he fails completely to identify $\int Pdp$ as work or *Arbeitskraft*, and instead calls it the "sum of the tensions" (*Summe der Spannkräfte*) over the space dimension of the motion.

six pioneers who reached or came close to the quantification of conversion processes could not even have used the reformulation. Unlike Mayer and Helmholtz, they applied the concept work directly to a problem in which *vis viva* is constant from cycle to cycle and therefore does not enter. Joule and Liebig are typical. Both began by comparing the "duty" of the electric motor with that of the steam engine. How much weight, they both asked, can each of these engines raise through a fixed distance for a given expenditure of coal or zinc? That question is basic to their entire research programs as it is to the programs of Carnot, Séguin, Holtzmann, and Hirn. It is not, however, a question drawn from either the new or old dynamics.

But neither, except for its application to the electrical case, is it a novel question. The evaluation of engines in terms of the weight each could raise to a given height is implicit in Savery's engine descriptions of 1702 and explicit in Parent's discussion of water wheels in 1704.[49] Under a variety of names, particularly mechanical effect, weight times height provided the basic measure of engine achievement throughout the engineering works of Desagulier, Smeaton, and Watt.[50] Borda applied the same measure to hydraulic machines and Coulomb to wind and animal power.[51] These ex-

49. The unit implicit in Savery's work is really the horsepower, but this includes weight times height as a part. See H. W. Dickinson and Rhys Jenkins, *James Watt and the Steam Engine* (Oxford, 1927), pp. 353–54. Antoine Parent, "Sur le plus grande perfection possible des machines," *Hist. Acad. Roy.* (1704), pp. 323–38.

50. J. T. Desagulier, *A Course of Experimental Philosophy*, 3d ed., 2 vols. (London, 1763), particularly 1:132, and 2:412. This posthumous edition is practically a reprint of the second edition (London, 1749).

John Smeaton, "An Experimental Inquiry concerning the Natural Powers of Water and Wind to Turn Mills, and Other Machines, depending on a Circular Motion," *Phil. Trans.* 51 (1759): 51. Here the measure is weight times height per unit time. The time dependence is, however, dropped in his "An Experimental Examination of the Quantity and Proportion of Mechanic Power Necessary to be Employed in Giving Different Degrees of Velocity to Heavy Bodies," *Phil. Trans.* 66 (1776): 458.

For Watt see Dickinson and Jenkins, *James Watt*, pp. 353–56.

51. J. C. Borda, "Mémoires sur les roues hydrauliques," *Mem. l'Acad. Roy.* (1767), p. 272. Here the measure is weight times vertical speed. Height replaces speed in C. Coulomb, "Observation théorique et expérimentale sur l'effet des moulins à vent, et sur la figure de leurs ailes," ibid. (1781), p. 68, and "Resultat de plusieurs expériences destinée à determiner la quantité

amples, drawn from all parts of the eighteenth century, but increasing in density toward its close, could be multiplied almost indefinitely. Yet even these few should prepare the way for a little noted but virtually decisive statistic. Of the nine pioneers who succeeded, partially or completely, in quantifying conversion processes, all but Mayer and Helmholtz were either trained as engineers or were working directly on engines when they made their contributions to energy conservation. Of the six who computed independent values of the conversion coefficient, all but Mayer were concerned with engines either in fact or by training.[52] To make the computation they needed the concept work, and the source of that concept was principally the engineering tradition.[53]

The concept work is the most decisive contribution to energy conservation made by the nineteenth-century concern with engines. That is why I have devoted so much space to it. But the concern with engines contributed to the emergence of energy conservation in a number of other ways besides, and we must consider at least

d'action que les hommes peuvent fournir par leur travail journalier, suivant les differentes manières dont ils emploient leurs forces," *Mem. de l'Inst.* 2 (1799): 381.

52. Mayer states that he loved to build model water wheels as a boy and that he learned the impossibility of perpetual motion in studying them (Weyrauch, II, p. 390). He could have learned simultaneously the proper measure of the product of machines.

53. Professor Hiebert asks if the concept of mechanical work may not have emerged from elementary statics and particularly from the formulation that derives statics from the principle of virtual velocities. The point needs further research, but my present response must be at least equivocally negative. The elements of statics were an important item in the equipment of all eighteenth-century engineers and the principle of virtual velocities, or an equivalent, therefore recurs in eighteenth-century writings on engineering problems. Quite possibly the engineers could not have evolved the concept work without the aid of the pre-existing static principle. But, as the preceding discussion may indicate, if the eighteenth-century concept work did emerge from the far older principle of virtual velocities, it did so only when that principle was firmly embedded in the engineering tradition and only when that tradition turned its attention to the evaluation of power sources such as animals, falling water, wind, and steam. Therefore, reverting to the vocabulary of note 9, I suggest that the principle of virtual velocities may have been a prerequisite for the discovery of energy conservation but that it can scarcely have been a trigger. [This note added to original manuscript in response to points raised during discussion.]

a few of them. For example, long before the discovery of electrochemical conversion processes, men interested in steam and water engines had occasionally seen them as devices for transforming the force latent in fuel or falling water to the mechanical force that raises weight. "I am persuaded," said Daniel Bernoulli in 1738, "that if all the *vis viva* hidden in a cubic foot of coal were called forth and usefully applied to the motion of a machine, more could be achieved than by the daily labor of eight or ten men."[54] Apparently that remark, made at the height of the controversy over metaphysical *vis viva*, had no later influence. Yet the same perception of engines recurs again and again, most explicitly in the French engineering writers. Lazare Carnot, for example, says that "the problem of turning a mill stone, whether by the impact of water, or by wind, or by animal power . . . is that of consuming the maximum possible [portion] of the work delivered by these agents."[55] With Coriolis, water, wind, steam, and animals are all simply sources of work, and machines become devices for putting this in useful form and transmitting it to the load.[56] Here, engines by themselves lead to a conception of conversion processes very close to that produced by the new discoveries of the nineteenth century. That aspect of the engine problem may well explain why the steam engineers—Hirn, Holtzmann, Séguin, and Sadi Carnot—were led to the same aspect of nature as men like Grove and Faraday.

The fact that engines could and occasionally did look like conversion devices may also explain something more. Is this not the reason why engineering concepts proved so readily transferable to the more abstract problems of energy conservation? The concept work is only the most important example of such a transfer. Joule and Liebig reached energy conservation by asking an old engi-

54. *Hydrodynamica*, p. 231.

55. *De l'équilibre et du mouvement*, p. 258. Notice also that as soon as Lagrange turns to Carnot's problem (note 44), he speaks in the same way. In the *Fonctions analytiques*, he says that waterfalls, coal, gunpowder, animals, etc., all "contain a quantity of *vis viva*, which one can harness but which one cannot increase by any mechanical means. One may [therefore] always regard a machine as intended to destroy a given quantity of *vis viva* [in the load] by consuming some other given *vis viva* [from the source]" (*Oeuvres*, 9:410).

56. *Du calcul de l'effet des machines*, chap. 1. For Coriolis the conservation theorem applied to a perfect machine becomes the "Principle of the Transmission of Work."

neering question, "What is the 'duty'?" about the new conversion processes in the battery-driven electric motor. But that question—how much work for how much fuel?—embraces the notion of a conversion process. In retrospect, it even sounds like the request for a conversion coefficient. Joule, at least, finally answered the question by producing one. Or consider the following more surprising transfer of engineering concepts. Though its fundamental conceptions are incompatible with energy conservation, Sadi Carnot's *Réflexion sur la puissance motrice du feu* was cited by both Helmholtz and Colding as the outstanding application of the impossibility of perpetual motion to a nonmechanical conversion process.[57] Helmholtz may well have borrowed from Carnot's memoir the analytic concept of a cyclic process that played so large a role in his own classic paper.[58] Holtzmann derived his value of the conversion coefficient by a minor modification of Carnot's analytic procedures, and Carnot's own discussion of energy conservation repeatedly employs data and concepts from his earlier and fundamentally incompatible memoir. These examples may give at least a hint of the ease and frequency with which engineering concepts were applied in deriving the abstract scientific conservation law.

My final example of the productiveness of the nineteenth-century concern with engines is less directly tied to engines. Yet it underscores the multiplicity and variety of the relationships that make the engineering factor bulk so large in this account of simultaneous discovery. I have shown elsewhere that many of the pioneers shared an important interest in the phenomenon known as adiabatic compression.[59] Qualitatively, the phenomenon provided an ideal demonstration of the conversion of work to heat; quantitatively, adiabatic compression yielded the only means of computing a conversion coefficient with existing data. The discovery of adiabatic compression has, of course, little or nothing to do with the interest in engines, but the nineteenth-century experiments which the pioneers used so heavily often seem related to just this practical concern. Dalton, and Clément and Désormes, who did important early work on adiabatic compression, also contributed early fundamental measurements on steam, and these measurements were used by many of the engineers.[60] Poisson, who developed an early theory of adiabatic compression, applied it, in the same article, to the steam engine, and his example was immediately followed by Sadi Carnot, Coriolis, Navier, and Poncelet.[61] Séguin, though he uses a different sort of data, seems a member of the same group. Dulong, to whose classic memoir on adiabatic compression many of the pioneers referred, was a close collaborator of Petit, and during the period of their collaboration Petit produced a quantitative account of the steam engine that antedates Carnot's by eight years.[62] There is even a hint of government interest. The prize offered by the French *Institut national* and won in 1812 by the classic research on gases

57. Helmholtz, *Abhandlungen*, 1:17. Colding, "Naturkraefter," *Dansk. Vid. Selsk.* 2 (1851): 123–24. Particularly interesting evidence about the apparent similarities between the theory of energy conservation and Carnot's incompatible theory of the heat engine is provided by Carlo Matteucci. His paper, "De la relation qui existe entre la quantité de l'action chimique et la quantité de chaleur, d'électricité et de lumière qu'elle produit," *Bibliothèque universelle de Genève, Supplement*, 4 (1847): 375–80, is an attack upon several of the early exponents of energy conservation. He describes his opponents as the group of physicists who "have tried to show that Carnot's celebrated principle about the motive force of heat can be applied to the other imponderable fluids."

58. Helmholtz, *Abhandlungen*, 1:18–19, gives Helmholtz's initial abstract formulation of the cyclic process.

59. T. S. Kuhn, "The Caloric Theory of Adiabatic Compression," *Isis* 49 (1958): 132–40.

60. John Dalton, "Experimental Essays on the Constitution of Mixed Gases; on the Force of Steam or Vapour from Water and Other Liquids in Different Temperatures, Both in a Torricellian Vacuum and in Air; on Evaporation; and on the Expansion of Gases by Heat," *Manch. Mem.* 5 (1802): 535–602. The second essay, though it grew out of Dalton's meteorological interests, was immediately exploited by both British and French engineers.

Clément and Désormes, "Mémoires sur la théorie des machines à feu," *Bulletin des sciences par la société philomatique* 6 (1819): 115–18; and "Tableau relatif à la théorie général de la puissance mécanique de la vapeur," ibid. 13 (1826): 50–53. The second paper appears in full in Crelle's *Journal für die Baukunst* 6 (1833): 143–64. For the contributions of these men to adiabatic compression, see my paper, cited in note 59.

61. S. D. Poisson, "Sur la chaleur des gaz et des vapeurs," *Ann.. Chim. Phys.* 23 (1823): 337–52. For Navier, Coriolis, and Poncelet, all of whom devote chapters to steam engine computations, see note 46.

62. A. T. Petit, "Sur l'emploi du principe des forces vives dans le calcul de l'effet des machines," *Ann. Chim. Phys.* 8 (1818): 287–305.

of Delaroche and Bérard may well have grown in part from government interest in engines.[63] Certainly Regnault's later work on the same topic did. His famous investigations of the thermal characteristics of gas and steam bear the imposing title, "Experiments, undertaken by order of the Minister of Public Works and at the instigation of the Central Commission for Steam Engines, to determine the principal laws and the numerical data which enter into steam engine calculations."[64] One suspects that without these ties to the recognized problems of steam engineering, the important data on adiabatic compression would not have been so accessible to the pioneers of energy conservation. In this instance the concern with engines may not have been essential to the work of the pioneers, but it certainly facilitated their discoveries.

Because the concern with engines and the nineteenth-century conversion discoveries embrace most of the new technical concepts and experiments common to more than a few of the discoverers of energy conservation, this study of simultaneous discovery might well end here. But a last look at the papers of the pioneers generates an uncomfortable feeling that something is still missing, something that is not perhaps a substantive element at all. This feeling would not exist if all the pioneers had, like Carnot and Joule, begun with a straightforward technical problem and proceeded by stages to the concept of energy conservation. But in the cases of Colding, Helmholtz, Liebig, Mayer, Mohr, and Séguin, the notion of an underlying imperishable metaphysical force seems prior to research and almost unrelated to it. Put bluntly, these pioneers seem to have held an idea capable of becoming conservation of energy for some time before they found evidence for it. The factors previously discussed in this paper may explain why they were ultimately able to clothe the idea and thus to make sense of it. But the discussion does not yet sufficiently account for the idea's existence. One or two such cases among the twelve pioneers might not be troublesome. The sources of scientific inspiration are notoriously inscrutable. But the presence of major conceptual lacunae in six of our twelve cases is surprising. Though I cannot entirely resolve the problem it presents, I must at least touch upon it.

We have already noted a few of the lacunae. Mohr jumped without warning from a defense of the dynamical theory of heat to the statement that there is only one force in nature and that it is quantitatively unalterable.[65] Liebig made a similar leap from the duty of electric motors to the statement that the chemical equivalents of the elements determine the work retrievable from chemical processes by either electrical or thermal means.[66] Colding tells us that he got the idea of conservation in 1839, while still a student, but withheld announcement until 1843 so that he might gather evidence.[67] The biography of Helmholtz outlines a similar story.[68] Séguin confidently applied his concept of the convertibility of heat and motion to steam engine computations, even though his single attempt to confirm the idea had been totally fruitless.[69] Mayer's leap has repeatedly been noted, but its full size is not often remarked. From the light color of venous blood in the tropics, it is a

63. F. Delaroche and J. Bérard, "Mémoire sur la determination de la chaleur specifique des differents gaz," *Ann. Chim. Phys.* 85 (1813): 72–110, 113–82. I know of no direct evidence relating the prize won by this memoir to the problems of steam engineering, but the Academy did offer a prize for improvement in steam engines as early as 1793. See H. Guerlac, "Some Aspects of Science during the French Revolution," *The Scientific Monthly* 80 (1955): 96.

64. In *Mém. de l'Acad.* 21 (1847): 1–767.

65. See note 21 and accompanying text.

66. *Chemische Briefe*, pp. 115–17.

67. Colding, "History of Conservation," *Phil. Mag.* 27 (1864): 57–58.

68. Leo Koenigsberger (*Hermann von Helmholtz*, tr. F. A. Welby [Oxford, 1906], pp. 25–26, 31–33) implies that Helmholtz's ideas about conservation were complete as early as 1843, and he states that by 1845 the attempt at experimental proof motivated all of Helmholtz's research. But Koenigsberger gives no evidence, and he cannot be quite correct. In two articles on physiological heat written during 1845 and 1846 (*Abhandlungen*, 1:8–11; 2:680–725), Helmholtz fails to notice that body heat may be expended in mechanical work (compare the discussion of Mayer, below). In the second of these papers he also gives the usual caloric explanation of adiabatic compression in terms of the change in heat capacity with pressure. In short, his ideas were by no means complete until 1847 or shortly before. But the papers of 1845 and 1846 do show that in these years Helmholtz was concerned to combat vitalism, which he thought implied the creation of force from nothing. Also they show that he already knew the work of Clapeyron and of Holtzmann, which he thought relevant. To this extent, at least, Koenigsberger must be right.

69. *Chemins de fer*, p. 383. Séguin had tried unsuccessfully to measure the difference in the quantities of heat abstracted from the boiler and delivered to the condenser of a steam engine.

small step to the conclusion that less internal oxidation is needed when the body loses less heat to the environment.[70] Crawford had drawn that conclusion from the same evidence in 1778.[71] Laplace and Lavoisier, in the 1780s, had balanced the same equation relating inspired oxygen to the body's heat losses.[72] A continuous line of research relates their work to the biochemical studies of respiration made by Liebig and Helmholtz in the early 1840s.[73] Though Mayer apparently did not know it, his observation of venous blood was simply a rediscovery of evidence for a well-known, though controversial, biochemical theory. But that theory was not the one to which Mayer leaped. Instead Mayer insisted that internal oxidation must be balanced against *both* the body's heat loss *and* the manual labor the body performs. To this formulation, the light color of tropical venous blood is largely irrelevant. Mayer's extension of the theory calls for the discovery that lazy men, rather than hot men, have light venous blood.

The persistent occurrence of mental jumps like these suggests that many of the discoverers of energy conservation were deeply predisposed to see a single indestructible force at the root of all natural phenomena. The predisposition has been noted before, and a number of historians have at least implied that it is a residue of a similar metaphysic generated by the eighteenth-century controversy over the conservation of *vis viva*. Leibniz, Jean and Daniel Bernoulli, Hermann, and du Châtelet, all said things like, "*Vis* [*viva*] never perishes; it may in truth appear lost, but one can always discover it again in its effects if one can see them."[74] There are a multitude of such statements, and their authors do attempt, however crudely, to trace *vis viva* into and out of nonmechanical phenomena. The parallel to men like Mohr and Colding is very

70. Weyrauch, I, pp. 12–14.

71. E. Farber, "The Color of Venous Blood," *Isis* 45 (1954): 3–9.

72. A. Lavoisier and P. S. Laplace, "Mémoire sur la chaleur," *Hist. de l'Acad.* (1780), pp. 355–408.

73. Helmholtz touches on much of this research in his paper of 1845, "Wärme, physiologisch," for the *Encyclopädische Wörterbuch der medicinischen Wissenschaften* (*Abhandlungen*, 2:680–725).

74. Haas, *Erhaltung*, p. 16, n. Quoted from *Institutions physiques de Madame la Marquise du Chastellet adressés à Mr. son Fils* (Amsterdam, 1742).

close. Yet eighteenth-century metaphysical sentiments of this sort seem an implausible source for the nineteenth-century predisposition we are examining. Though the technical *dynamical* conservation theorem has a continuous history from the early eighteenth century to the present, its metaphysical counterpart found few or no defenders after 1750.[75] To discover the *metaphysical* theorem, the pioneers of energy conservation would have had to return to books at least a century old. Neither their works nor their biographies suggest that they were significantly influenced by this particular bit of ancient intellectual history.[76]

Statements like those of both the eighteenth-century Leibnizians and the nineteenth-century pioneers of energy conservation can, however, be found repeatedly in the literature of a second philosophical movement, *Naturphilosophie*.[77] Positing organism as the fundamental metaphor of their universal science, the *Naturphilosophen* constantly sought a single unifying principle for all natural phenomena. Schelling, for example, maintained "that magnetic, electrical, chemical, and finally even organic phenomena would be interwoven into one great association . . . [which] extends over the

75. Haas, *Erhaltung*, p. 17.

76. None of the pioneers mention the eighteenth-century conservation literature in their original papers. Colding, however, says that he got his first glimpse of conservation while reading d'Alembert in 1839 (*Phil. Mag.* 27 [1864]: 58), and Koenigsberger says that Helmholtz had read d'Alembert and Daniel Bernoulli by 1842 (*Helmholtz*, p. 26). These two counterexamples do not, however, really modify my thesis. D'Alembert omitted all mention of the metaphysical conservation theorem from the first edition of his *Traité*, and in the second he explicitly disowned the view (Paris, 1758, beginning of the "Avertissement" and pp. xvii–xxiv). In fact, d'Alembert was among the first to insist on freeing dynamics from what he considered to be mere metaphysical speculations. To take his ideas from this source Colding would still have required a strong predisposition. Bernoulli's *Hydrodynamica* is a more appropriate source (see, for example, the text that accompanies note 54), but Koenigsberger makes the very plausible point that Helmholtz consulted Bernoulli in order to work out his preexisting conception of conservation.

77. The roots of *Naturphilosophie* can, of course, be traced back through Kant and Wolff to Leibniz, and Leibniz was the author of the metaphysical conservation theorem about which both Kant and Wolff wrote (Haas, *Erhaltung*, pp. 15–18). The two movements are not, therefore, entirely independent.

whole of nature."[78] Even before the discovery of the battery he insisted that "without doubt only a single force in its various guises is manifest in [the phenomena of] light, electricity, and so forth."[79] These quotations point to an aspect of Schelling's thought fully documented by Bréhier and more recently by Stauffer.[80] As a *Naturphilosoph*, Schelling constantly sought out conversion and transformation processes in the science of his day. At the beginning of his career chemistry seemed to him the basic physical science; from 1800 on he increasingly found in galvanism "the true border-phenomenon of both [organic and inorganic] natures."[81] Many of Schelling's followers, whose teaching dominated German and many neighboring universities during the first third of the nineteenth century, gave similar emphasis to the new conversion phenomena. Stauffer has shown that Oersted—a *Naturphilosoph* as well as a scientist—persisted in his long search for a relation between electricity and magnetism largely because of his prior philosophical conviction that one must exist. Once the interaction was discovered, electro-magnetism played a major role in Herbart's further elaboration of the scientific substructure of *Naturphilosophie*.[82] In short, many *Naturphilosophen* drew from their philosophy a view of physical processes very close to that which Faraday and Grove seem to have drawn from the new discoveries of the nineteenth century.[83]

78. Quoted by R. C. Stauffer, "Speculation and Experiment in the Background of Oersted's Discovery of Electromagnetism," *Isis* 48 (1957): 37, from Schelling's *Einleitung zu seinem Entwurf eines Systems der Naturphilosophie* (1799).

79. Quoted by Haas, *Erhaltung*, p. 45, n. 61, from Schelling's *Erster Entwurf eines Systems der Naturphilosophie* (1799).

80. Émile Bréhier, *Schelling* (Paris, 1912). This is the most helpful discussion I have found and should certainly be added to Stauffer's list of useful aids for studying the complex relations of science and *Naturphilosophie* (*Isis* 48 [1957]: 37, n. 21).

81. Stauffer, "Speculation and Experiment," p. 36, from Schelling's "Allgemeiner Deduktion des dynamischen Processes oder der Kategorien der Physik" (1800).

82. Haas, *Erhaltung*, p. 41.

83. It is, of course, impossible to distinguish sharply between the influence of *Naturphilosophie* and that of conversion processes. Bréhier (*Schelling*, pp. 23–24) and Windelband (*History of Philosophy*, trans. J. H. Tufts, 2d ed. [New York, 1901], pp. 597–98) both emphasize that conversion processes were themselves a significant source of *Naturphilosophie*, so that the two were often grasped together. This fact must qualify some of the dichotomies set up in the first part of this paper, for the distinction between the two sources of the conservation concept is often equally hard to apply to individual pioneers. I have already pointed out the difficulty in Colding's case (note 32). With Mohr and Liebig I am still inclined to give *Naturphilosophie* the psychological priority, because neither had dealt much with the new conversion processes in their own research and because both make such large leaps. Their cases appear in sharp contrast to those of Grove and Faraday, who seem to proceed by a continuous path from conversion processes to conservation. But this continuity may be deceptive. Grove (*Physical Forces*, pp. 25–27) mentions Coleridge, and Coleridge was the principal British exponent of *Naturphilosophie*. Since the problem presented by these examples seems to me both real and unresolved, I had better point out that it affects only the organization, not the main thesis, of this paper. Perhaps conversion processes and *Naturphilosophie* should be considered in the same section. Nevertheless, they would both have to be considered.

Naturphilosophie could, therefore, have provided an appropriate philosophical background for the discovery of energy conservation. Furthermore, several of the pioneers were acquainted with at least its essentials. Colding was a protegé of Oersted's.[84] Liebig studied for two years with Schelling, and though he afterwards described these years as a waste, he never surrendered the vitalism he had then imbibed.[85] Hirn cited both Oken and Kant.[86] Mayer did not study *Naturphilosophie*, but he had close student friends who did.[87] Helmholtz's father, an intimate of the younger Fichte's and a minor *Naturphilosoph* in his own right, constantly exhorted his son to desert strict mechanism.[88] Though Helmholtz himself felt forced to excise all philosophical discussion from his classic mem-

84. Povl Vinding, "Colding, Ludwig August," *Dansk Biografisk Leksikon* (Copenhagen, 1933–44), pp. 377–82. I am grateful to Roy and Ann Lawrence for providing me with a précis of this useful biographical sketch.

85. E. von Meyer, *A History of Chemistry*, trans. G. McGowan, 3d ed. (London, 1906), p. 274. J. T. Merz, *European Thought in the Nineteenth Century* (London, 1923–50), 1:178–218, particularly the last page.

86. G. A. Hirn, "Etudes sur les lois et sur les principes constituants de l'univers," *Revue d' Alsace* 1 (1850): 24–41, 127–42, 183–201; ibid., 2 (1851): 24–45. References to writings related to *Naturphilosophie* occur relatively often, though they are not very favorable. On the other hand, the very title of this piece suggests *Naturphilosophie*, and the title is appropriate to the contents.

87. B. Hell, "Robert Mayer," *Kantstudien* 19 (1914): 222–48.

88. Koenigsberger, *Helmholtz*, pp. 3–5, 30.

oir, he was able by 1881 to recognize important Kantian residues that had escaped his earlier censorship.[89]

Biographical fragments of this sort do not, of course, prove intellectual indebtedness. They may, however, justify strong suspicion, and they surely provide leads for further research. At the moment I shall only insist that this research should be done and that there are excellent reasons to suppose it will be fruitful. Most of those reasons are given above, but the strongest has not yet been noticed. Though Germany in the 1840s had not yet achieved the scientific eminence of either Britain or France, five of our twelve pioneers were Germans, a sixth, Colding, was a Danish disciple of Oersted's, and a seventh, Hirn, was a self-educated Alsatian who read the *Naturphilosophen*.[90] Unless the *Naturphilosophie* indigenous to the educational environment of these seven men had a productive role in the researches of some, it is hard to see why more than half of the pioneers should have been drawn from an area barely through its first generation of significant scientific productivity. Nor is this quite all. If proved, the influence of *Naturphilosophie* may also help to explain why this particular group of five Germans, a Dane, and an Alsatian includes five of the six pioneers in whose approaches to energy conservation we have previously noted such marked conceptual lacunae.[91]

89. Helmholtz, *Abhandlungen*, 1:68.

90. Much biographical and bibliographical material for the study of Hirn's life and work can be found in the *Bulletin de la société d'histoire naturelle de Colmar* 1 (1899): 183–335.

91. Séguin is the sixth, and the source of his idea remains a complete riddle. He attributes it (*Chemins de fer*, p. xvi) to his uncle Montgolfier about whom I have been able to get no relevant information.

The statistics above are not meant to imply that those exposed to *Naturphilosophie* were invariably affected by it; nor do I mean to argue that those whose work shows no conceptual lacunae were *ipso facto* not influenced by *Naturphilosophie* (see remarks on Grove in note 83). It is the *predominance* rather than the presence of pioneers from the area dominated by German intellectual traditions that constitutes the puzzle.

[The following paragraph was added to the original manuscript in response to points raised during the discussion.]

Professor Gillispie, in his paper, calls attention to a little-known movement in eighteenth-century France that shows striking parallels to *Naturphilosophie*. If this movement had still been prevalent in nineteenth-century France, my contrast between the German scientific tradition and that prevalent elsewhere in Europe would be questionable. But I find nothing resembling *Naturphilosophie* in any of the nineteenth-century French sources I have examined, and Professor Gillispie assures me that, to the best of his knowledge, the movement to which his paper draws attention had disappeared (except perhaps from parts of biology) by the turn of the century. Notice, in addition, that this eighteenth-century movement, which was particularly prevalent among craftsmen and inventors, may provide a clue to the puzzle of Montgolfier (see above).

This preliminary discussion of simultaneous discovery must end here. Comparing it with the sources, primary and secondary, from which it derives, makes apparent its incompleteness. Almost nothing has been said, for example, about either the dynamical theory of heat or the conception of the impossibility of perpetual motion. Both bulk large in standard histories, and both would require discussion in a more extended treatment. But if I am right, these neglected factors and others like them would not enter a fuller discussion of simultaneous discovery with the urgency of the three discussed here. The impossibility of perpetual motion, for example, was an essential intellectual tool for most of the pioneers. The ways in which many of them arrived at the conservation of energy cannot be understood without it. Yet recognizing the intellectual tool scarcely contributes to an understanding of simultaneous discovery because the impossibility of perpetual motion had been endemic in scientific thought since antiquity.[92] Knowing the tool was there, our question has been: Why did it suddenly acquire a new significance and a new range of application? For us, that is the more significant question.

The same argument applies in part to my second example of neglected factors. Despite Rumford's deserved fame, the dynami-

92. E. Mach, *History and Root of the Principle of the Conservation of Energy*, trans. Philip E. B. Jourdain (Chicago, 1911), pp. 19–41; and Haas, *Erhaltung*, chap. 4. Remember also that in 1775 the French academy formally resolved to consider no more purported designs of perpetual motion machines. Almost all of our pioneers make use of the impossibility of perpetual motion, and none feels the slightest necessity of arguing about its validity. In contrast, they do find it necessary to argue at length about the validity of the concept of universal conversions. Grove, for example, opens his *Physical Forces* (pp. 1–3) with a plea for a fair hearing of a radical idea. The idea turns out to be the concept of universal conversions developed at great length in the text (pp. 4–44). The impossibility of perpetual motion is casually applied to this idea without argument in the last seven pages (pp. 45–52). Facts like these have led me to call the steps from universal conversions to an unquantified version of conservation "rather obvious."

cal theory of heat had been close to the surface of scientific consciousness almost since the days of Francis Bacon.[93] Even at the end of the eighteenth century, when temporarily eclipsed by the work of Black and Lavoisier, the dynamical theory was often described in scientific discussions of heat, if only for the sake of refutation.[94] To the extent that the conception of heat as motion figured in the work of the pioneers, we must principally understand why that conception gained a significance after 1830 that it had seldom possessed before.[95] Besides, the dynamical theory did not

93. For seventeenth-century theories of heat, see M. Boas, "The Establishment of the Mechanical Philosophy," *Osiris* 10 (1952): 412–541. Much information about eighteenth-century theories is scattered through: D. McKie and N. H. de V. Heathcote, *The Discovery of Specific and Latent Heat* (London, 1935), and H. Metzger, *Newton, Stahl, Boerhaave et la doctrine chimique* (Paris, 1930). Much other useful information will be found in G. Berthold, *Rumford und die Mechanische Wärmetheorie* (Heidelberg, 1875), though Berthold skips too rapidly from the seventeenth to the nineteenth century.

94. Since the caloric theory was scarcely presented in a developed form before the publication of Lavoisier's *Traité élémentaire de chimie* in 1789, it could hardly have eradicated the dynamical theory in the decade remaining before the publication of Rumford's work. For evidence that even the most pronounced caloricists continued to discuss it, see Armand Séguin, "Observations générales sur le calorique . . . reflexions sur la théorie de MM. Black, Crawford, Lavoisier, et Laplace," *Ann. de Chim.* 3 (1789): 148–242, and 5 (1790): 191–271, particularly, 3:182–90. The material theory of heat has, of course, roots far older than Lavoisier, but Rumford, Davy et al., are really opposing a new theory, not an old one. Their work, particularly Rumford's, may have kept the dynamical theory alive after 1800, but Rumford did not create the theory. It had not died.

95. It is too seldom recognized that until almost the mid-nineteenth century, brilliant scientists could apply the dynamical conservation of *vis viva* to the theory that heat is motion without at all recognizing that heat and work should then be convertible. Consider the following three examples. Daniel Bernoulli, in the often quoted paragraphs from Section X of his *Hydrodynamica* equates heat with particulate *vis viva* and derives the gas laws. Then, in paragraph 40, he applies this theory in computing the height from which a given weight must fall to compress a gas to a given fraction of its initial volume. His solution gives the energy of motion abstracted from the falling weight in order to compress the gas, but fails entirely to notice that this energy must be transferred to the gas particles and must therefore raise the gas's temperature. Lavoisier and Laplace, on pp. 357–59 of their classic memoir (note 72), apply the conservation of energy to the dynamical theory in order to show that for all experimental purposes the caloric and dynamical theories are precisely equivalent. J. B. Biot repeats the same argument, in his *Traité de physique expérimentale et mathématique* (Paris, 1816), 1:66–67, and elsewhere in the same chapter. Grove's mistake about heat (note 34) indicates that even the conception of conversion processes was sometimes insufficient to guide scientists away from this virtually universal mistake.

figure very large. Only Carnot used it as an essential stepping stone. Mohr leaped from the dynamical theory to conservation, but his paper indicates that other stimuli might have served as well. Grove and Joule adhered to the theory but show substantially no dependence on it.[96] Holtzmann, Mayer, and Séguin opposed it—Mayer vehemently and to the end of his life.[97] The apparently close connections between energy conservation and the dynamical theory are largely retrospective.[98]

Compare these two neglected factors with the three we have discussed. The rash of conversion discoveries dates from 1800. Technical discussions of dynamical engines were scarcely a recurrent ingredient of scientific literature before 1760 and their density increased steadily from that date.[99] *Naturphilosophie* reached its

96. Grove, *Physical Forces*, pp. 7–8. Joule, *Papers*, pp. 121–23. Perhaps these two would not have developed their theories if they had not tended to regard heat as motion, but their published works indicate no such decisive connections.

97. Holtzmann's memoir is based on the caloric theory. For Mayer see Weyrauch, I, pp. 265–72, and II, p. 320, n. 2. For Séguin see *Chemins de fer*, p. xvi.

98. The ease and immediacy with which the dynamical theory was identified with energy conservation is indicated by the contemporary misinterpretations of Mayer quoted in Weyrauch, II, pp. 320 and 428. The classic case, however, is Lord Kelvin's. Having employed the caloric theory in his research and writing until 1850, he opens his famous paper "On the Dynamical Theory of Heat" (*Mathematical and Physical Papers* [Cambridge, 1882], 1:174–75) with a series of remarks on Davy's having "established" the dynamical theory fifty-three years before. Then he continues, "The recent discoveries made by Mayer and Joule . . . afford, *if required*, a perfect confirmation of Sir Humphry Davy's views" (italics mine). But if Davy established the dynamical theory in 1799 and if the rest of conservation follows from it, as Kelvin implies, what had Kelvin himself been doing before 1852?

99. The abstract theories of dynamical engines have no beginning in time. I pick 1760 because of its relation to the important and widely cited works of Smeaton and Borda (notes 50 and 51).

peak in the first two decades of the nineteenth century.[100] Furthermore, all three of these ingredients, except possibly the last, played important roles in the research of at least half the pioneers. That does not mean that these factors explain either the individual or collective discoveries of energy conservation. Many old discoveries and concepts were essential to the work of all the pioneers; many new ones played significant roles in the work of individuals. We have not and shall not reconstruct the causes of all that occurred. But the three factors discussed above may still provide the fundamental constellation, given the question from which we began: Why, in the years 1830–50, did so many of the experiments and concepts required for a full statement of energy conservation lie so close to the surface of scientific consciousness?

100. Merz, *European Thought*, 1:178, n. 1.

5 The History of Science

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As an independent professional discipline, the history of science is a new field still emerging from a long and varied prehistory. Only since 1950, and initially only in the United States, has the majority of even its youngest practitioners been trained for, or committed to, a full-time scholarly career in the field. From their predecessors, most of whom were historians only by avocation and thus derived their goals and values principally from some other field, this younger generation inherits a constellation of sometimes irreconcilable objectives. The resulting tensions, though they have relaxed with the increasing maturation of the profession, are still perceptible, particularly in the varied primary audiences to which the literature of the history of science continues to be addressed. Under the circumstances any brief report on development and current state is inevitably more personal and prognostic than for a longer-established profession.

Development of the Field

Until very recently most of those who wrote the history of science were practicing scientists, sometimes eminent ones. Usually history was for them a by-product of pedagogy. They saw in it, besides intrinsic appeal, a means to elucidate the concepts of their specialty, to establish its tradition, and to attract students. The histori-