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Employment Before Formulation: Uses of Proto-Energetic Arguments

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ABSTRACT

The historiography of the principle of energy conservation has concentrated on the formulation of the law by a few individual scientists. This paper turns to the *employment* of energetic considerations, examining the uses of related arguments in scientific reasoning before the formulation of a well-defined principle. It shows that conceptual ambiguity and a limited formal realm of validity did not prevent the successful employment of such notions to generate novel scientific results. From the late 1810s to the 1840s, researchers including Fresnel, Ampère, Carnot, Roget, Faraday, and Liebig invoked proto-energetic arguments to address particular problems concerning wave optics, electromagnetism, theory of electric batteries, heat motors, and animal heat. Thereby they extended the realm of applicability of arguments based on the conservation of "power" beyond non-frictional mechanical systems, where the conservation of the living forces (*vis viva*) was accepted in the early nineteenth century; they also furnished scientists a theoretical toolkit with a new powerful method. Their development of proto-energetic arguments as tools for reasoning was an important historical process in itself, which together with other developments led to the emergence of "energy physics." This history, thus, exemplifies the important role played by practices of reasoning in the formation of scientific laws and principles.

KEY WORDS: energy conservation, principles, rational mechanics, electrodynamics, André-Marie Ampère, Augustine Fresnel, Justus Liebig, Michael Faraday

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INTRODUCTION

The history of energy conservation has attracted interest as one of the most important principles in physics, and science in general, almost since its announcement in the 1840s. Famously, Thomas Kuhn regarded the formulation of energy conservation as a simultaneous discovery. While he identified twelve individuals who "grasped for themselves essential parts of the concept of energy and its conservation,"[1] he acknowledged that their ideas and discoveries significantly diverged, leading him to use the ambivalent term "pioneers" rather than discoverers. For many later historians, the divergence indicated that the term "simultaneous discovery" is inadequate for describing the development of energy physics.[2] Part of the disagreement among historians stems from a failure to distinguish between different kinds of contributions to the field of energy physics, in particular between protagonists who used a notion later associated with "energy conservation" within a specific argument to reach a particular conclusion and those who explored general laws of conservation in nature.[3] Exploring the origins of the principle, historians concentrated more on the *ideas* held by the protagonists and did not pay enough attention to how the ideas were *employed*. In this essay, I examine applications of proto-energetic arguments to generate new physical results, i.e., relations between physical variables and observables. Through this examination, a few of the "pioneers" are no longer regarded as precursors who failed to grasp the full meaning of energy conservation, but as successful employers of "proto-energetic" notions in their scientific reasoning.

I examine here the uses of notions and arguments that resemble those associated with modern "energy conservation" before the formulation of the complete principle.[4] More specifically, I look at arguments based either on conservation of some physical quantity, or on a balance between a few quantities whose sum is conserved, or on the impossibility of creating "power" out of nothing. Those who advanced these arguments did not necessarily possess a clear concept of "energy" (either mathematical or verbal), nor did they always

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[1] Thomas S. Kuhn, "Energy Conservation as an Example of Simultaneous Discovery," in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (1959; Chicago: University of Chicago Press, 1977), 66–104, on 69.

[2] I sketch their central claims in the first section of this article.

[3] Kuhn furthered muddied the water by including scientists who expressed a quantitative equivalence between mechanical work and heat, a distinct, albeit connected, issue.

[4] The attempt has been made to include all such uses, but I assume that a few escaped my survey. I include also later uses, which were done without knowledge of the well-formulated principle.

believe that the quantity conserved is conserved in every (isolated) natural process. Furthermore, they referred to the conserved quantity by different names (i.e., living force, action, motion, motive power, force, power, and energy), and they often defined it only vaguely. Still, their texts clearly did imply notions of conservation or rejection of creating power, even if those notions were vague. Moreover, the scientists discussed here produced novel scientific results by implementing their proto-energetic ideas, in some cases despite semantic and conceptual ambiguity.

The following examination offers a diachronic rather than a synchronic picture of the development of the principle. Instead of a focus on the path of a particular individual and his background, which characterizes many important and instructive studies of the topic,[5] the picture suggested here encompasses quite a few scientists and the dynamics among them, without confining them to the particular analytical framework of our contemporary view of energy. Kenneth Caneva has suggested such a dynamic picture in his excellent book on the evolution of Robert Mayer's ideas. Yet, regarding the subject of his study, Caneva restricted his discussion of the role of proto-energetic considerations to the particular, albeit relatively wide, scientific context of Mayer's work.[6]

Arguably the earliest employment of a proto-energetic argument was in mechanics. A rigorous and well-defined equation of the conservation of the living force was employed already in the eighteenth century for some problems in non-frictional mechanical systems.[7] My interest here, however, is in the

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[5] E.g., Donald S. L. Cardwell, *James Joule: A Biography* (Manchester, UK: Manchester University Press, 1990); Edward Jurkowitz, "Helmholtz's Early Empiricism and the *Erhaltung der Kraft*," *Annals of Science* 67 (2010): 39–78; Fabio Bevilacqua, "Helmholtz's Ueber die Erhaltung der Kraft: The emergence of a theoretical physicist," in *Hermann von Helmholtz and the foundations of nineteenth-century science*, ed. David Cahan (Berkeley: University of California Press, 1993), 291–333; Kenneth L. Caneva, "Colding, Ørsted, and the Meanings of Force," *Historical Studies in the Physical and Biological Sciences* 28, no. 1 (1997): 1–138; Geoffrey N. Cantor, "William Robert Grove, the correlation of forces, and the conservation of energy," *Centaurus 19* (1975): 273–90.

[6] In that physiological discourse, energetic-like considerations were used in some arguments regarding sources of heat (less frequently than one might assume), but were rarely used to infer relationships between different theoretical and observable variables, like the oxygen consumed in respiration and the heat generated by the animal. I refer below to the central example discussed by Caneva, that of Liebig. The case of Mayer himself is beyond the scope of this article, as he attempted also to formulate a complete theory of the conservation of force. Kenneth L. Caneva, *Robert Mayer and the Conservation of Energy* (Princeton, NJ: Princeton University Press, 1993).

[7] I prefer the English term "living force" over the Latin *vis-viva*, since the authors writing in French and German, who form the majority of the scientists discussed here, employed its translations to their own languages (*force vive* and *lebendige Kraft*, respectively).

creative aspects of applying laws in the absence of clear rules for their use, that is, in cases where the law was not rigorously defined, or when researchers employed the law beyond its secured realm of validity, as was the case with the conservation of the living force. Its use, mainly by members of the French analytical tradition, helped to extend the conservation of the living force beyond mechanics to other realms of physics, contributing to the formulation of a universal conservation law. Ivor Grattan-Guinness and Olivier Darrigol have shown that the equations of the living force and their uses underwent significant developments also in mechanics in the late eighteenth and early nineteenth century, turning mechanics, among others, into a fully conservative theory.[8] A précis of these developments is presented here as preliminary to the main subject of this article, namely the use of similar conservation rules and other proto-energetic arguments beyond mechanics from the 1810s. Unsurprisingly, most of these uses were done in new fields of study or in the realm of novel theories. The paper follows these implementations from wave optics, on its mechanical underpinning, to fields for which no mechanical reduction was accepted: the magnetic effect of electric currents (electrodynamics), the dynamic effect of heat (thermodynamics), the theory of the voltaic pile, and chemical theories of animal heat. It begins with a brief discussion of the historiography of energy conservation in order to help situate my argument.

The equation of the living force in mechanical systems was one of two notions invoked by employers of proto-energetic consideration. Following its premise, scientists used this equation in systems governed by central forces (i.e., attractive and repulsive forces between two bodies directed along the line that connects them, whose magnitudes depend only on their relative positions and constants), regardless of the specific force. Further, it was held valid in the realm of every phenomenon assumed to be reducible to mechanics. The second notion that justified proto-energetic consideration was the belief in the inability to create an effect out of nothing, where effect was regarded as equivalent to motion. Those who formulated a clear law of energy conservation invoked the same notions. Hermann Helmholtz in particular relied on both. In *Über die Erhaltung der Kraft* (1847), he showed that energy conservation

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[8] Ivor Grattan-Guinness, "Work for the Workers: Advances in Engineering Mechanics and Instruction in France, 1800–1830," *Annals of Science* 41 (1984): 1–33; Ivor Grattan-Guinness, *Convolution in French Mathematics, 1800–1840: From the Calculus and Mechanics to Mathematical Analysis and Mathematical Physics* (Basel: Birkhäuser, 1990): 1046–1121; Olivier Darrigol, "God, Waterwheels, and Molecules: Saint-Venant's Anticipation of Energy Conservation," *Historical Studies in the Physical and Biological Sciences* 31 (2001): 285–353.

logically follows from each of these assertions. Yet, unlike the natural philosophers discussed below, he employed a sharp definition of the inability to create and to annihilate an effect, namely equating effect with mechanical work, and required a complete reduction of physics to central forces. Helmholtz was well aware of earlier implementations of proto-energetic conservation arguments. Yet, these arguments should not be seen merely as a pre-history, precursors of energy physics, but rather as a significant development in contemporary physics that provided scientists with a powerful theoretical tool. The history of this development is also worth reconstructing regardless of its contribution to the emergence of energy physics.

ON THE HISTORIOGRAPHY OF ENERGY CONSERVATION

A proper discussion of the rich and complex historiography of energy conservation would require at least a full-length article. In this section I can only sketch out central positions relevant to this essay, using the last attempt at a general picture of the emergence of the principle, Kuhn's 1959 article, as a useful starting point. In viewing the case as a simultaneous discovery, Kuhn identified twelve "pioneers." Four among them—Robert Mayer, James Joule, Ludvig Colding, and Helmholtz—combined "generality of formulation with concrete quantitative applications." The other eight presented only "essential parts" of the conservation in their studies, either limited to the special case of the "quantitative interchangeability of heat and work," or lacking a quantitative concept of energy.[9] By examining twelve pioneers rather than a single discoverer, Kuhn was able to identify three factors recurrent in their individual paths to the conservation of energy as the main reasons for its emergence in the 1840s. One factor was the acquaintance with a growing number of conversion processes (current electricity and magnetism, heat and chemistry, light and chemistry, etc.), which manifested itself most clearly in a qualitative discussion of the "correlation of forces" rather than in the quantitative announcement of their conservation.[10] The second factor was the concern with engines. The

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[9] In the second group, Kuhn included Sadi Carnot, Séguin, Holtzmann, and Hirn; in the third, Mohr, Grove, Faraday, and Libieg. Kuhn, "Energy Conservation" (ref. 1), 67, 69.

[10] J. T. Merz had already observed that the idea of interchangeability of forces "seems to have forced itself independently on many minds, through the study of very different groups of natural phenomena." As is shown below, however, some of the assertions to which Merz and Kuhn referred (e.g., of Faraday and Liebig), however, followed previous claims and uses known to their

third was a metaphysical belief in the indestructibility of one unified force, which Kuhn associated with the German school of *Naturphilosophie*.

Later historians contested Kuhn's general framework as well as major parts of the causal explanation that he drew from it. They also contested the central claims regarding particular scientists, including the meaning of their concepts, the character of their work, and the factors that influenced them. However, they mostly retained his emphasis on the ideas held by the proclaimed discoverers. Producing important and impressive conceptual analyses of the scientists' thoughts in their contexts, historians, however, paid less attention to the *uses* of proto-energetic considerations. Yehuda Elkana and Crosbie Smith objected most explicitly, but in different ways, to the view of energy conservation as a simultaneous discovery. According to Elkana, different scientists addressed different problems and attained different results. A group of English natural philosophers "was preoccupied by problems of the efficiency of conversion between the various 'mechanical powers,'" which led them to demonstrate that heat is a kind of motion. A second group of mostly Germans, on the other hand, was concerned with the problem of animal heat and with philosophical questions of conservation in nature.[11] In Helmholtz's work, this led to the mathematical formulation of energy conservation. According to Smith, however, energy conservation should not be seen as a discovery of a truth about nature but as a cultural construction. This proposition formed only a part of a "science of energy"—a new way of formulating physics advanced by northern British scientists led by William Thomson within their particular physical, technological, and religious cultural contexts. The reception and diffusion of the idea was more important than its original formulation. Smith regards the development of energy physics as a local process embedded in a particular culture, within which Helmholtz just provided a useful formulation.[12] I share his view that historians should examine a process rather than isolated formulations, but differently from what his book implies, I show below that the development of the uses of energy-like arguments was a cross-European phenomenon stimulated by scientific and technical problems that contributed to the emergence of energy physics.

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authors. John T. Merz, "On the physical view of nature," in *A History of European Thought in the Nineteenth Century* (Edinburgh: W. Blackwood, 1904), 105.

[11] Yehuda Elkana, "The Conservation of Energy: A Case of Simultaneous Discovery?" *Archives internationale d'histoire des sciences* 90 (1970): 31–60, on 39–40.

[12] Crosbie Smith, *The Science of Energy: A Cultural History of Energy Physics in Victorian Britain* (Chicago: University of Chicago Press, 1998), especially 8–14.

Other historians revised the list of pioneers suggested by Kuhn by reevaluating the contributions of particular scientists, without explicitly engaging in the question of simultaneous discovery. Among others, Caneva showed that Colding held neither a concept of energy nor its conservation,[13] and Darrigol showed that Adhémar Barré de Saint-Venant formulated a principle of energy conservation, if only for mechanical systems, in 1834.[14] In these and other studies of central figures in the formulation of energy conservation (e.g., Mayer, Joule, and Helmholtz), historians suggested alternative and competing views of the process that led different individuals to the idea that a quantity, which we today identify as energy, is always conserved. They also rejected two of the three reasons suggested by Kuhn for the emergence of energy conservation. Heimann showed that already in the late eighteenth century, physicists were aware of many conversion processes and their implications, preempting Kuhn's claim for the importance of the discoveries of further conversion phenomena close to the formulation of the principle. In closer analysis, no important contributor to energy conservation was found to be connected to *Naturphilosophie*. This had been quite clear in the case of Helmholtz, who, newer studies showed, was more concerned with methodology than with metaphysics, and as Caneva showed, this was also true for Colding, Liebig, and Mayer.[15] As discussed below, uses of partial notions of conservation and impossibility to create power stimulated Helmholtz and Liebig to employ similar considerations in their works. The concern with engines, however, remained an important factor for most historians. Although it did not influence some of the main protagonists, e.g., Helmholtz, it was central to others, e.g., Joule. In the latter case, Cardwell shows, the formulation of energy conservation relied also on related empirical findings about the conversion between work and heat.[16]

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[13] Caneva, "Colding" (ref. 5).

[14] Yet, for Saint-Venant all physics was (in principle) reducible to mechanics, Darrigol, "God, Waterwheels, and Molecules" (ref. 8).

[15] P. M. Heimann "Conversion of forces and conservation of energy," *Centaurus* 18 (1974): 147–61; Bevilacqua, "Helmholtz's Ueber Erhaltung der Kraft," (ref. 5); Yehuda Elkana, *The Discovery of the Conservation of Energy* (Cambridge, MA: Harvard University Press, 1974); Jurkowitz, "Helmholtz's Early Empiricism" (ref. 5); Caneva, "Colding" (ref. 5); Caneva, *Mayer* (ref. 6).

[16] Cardwell, *Joule* (ref. 5), especially 49–89 and 273. Based on a bibliometric study of later references, Kipnis claimed that the equivalence between heat and work, deduced in the discussion of engines, was the most important source for energy conservation (Nahum Kipnis, "Thermodynamics and Mechanical Equivalent of Heat," *Science & Education* 23 (2014): 2007–44).

Another important context for the emergence of energy conservation, well examined in recent decades, was the discussion of animal physiology, including the controversies regarding the sources of animal heat and the existence and nature of vital forces, central especially for Mayer, Helmholtz, and Liebig. The current article is concerned with connections between these discussions and the uses of proto-energetic arguments in physics. In general, the postwar historiography undermined the role of rational mechanics and the concept of mechanical work in the emergence of the principle, ascribing to it, at most, a role as background knowledge. Yet, according to Grattan-Guinness and Darrigol, the transformation of mechanics from non-conservative into a conservative theory, and the formulation of the concept of mechanical work, which provided a highly useful tool for the formulation of energy conservation, were major factors in the emergence of energy conservation. Consequently, energy physics resulted from a full mechanical reduction of physics, a claim made also by earlier historians, e.g., Elkana, regarding Helmholtz.[17] Like Caneva in his study of the physiological context for Mayer's work, these studies provide a rich picture of the development of ideas and a few technical tools by a community of scientists. Here I build on their work in recognizing the significance of this tradition of rational mechanics for a few of those who employed proto-energetic considerations to attain new physical results (e.g., Fresnel and Ampère), but I show, further, that it was usually coupled with informal and more general notions of conservation in nature.

PRELIMINARIES: THE CONSERVATION OF THE LIVING FORCES IN MECHANICS

The concept of living force played a central role in the employment of proto-energetic ideas before the formulation of the conservation principle. Originating in Leibniz's work, the conservation of the sum of the multiplication of mass by velocity squared appeared in quite a few formulations of mechanics during the eighteenth century. In *Analytical Mechanics* (1788), Joseph Louis

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[17] Grattan-Guinness, "Work for the Workers" (ref. 8); Grattan-Guinness, *Convolution in French Mathematics* (ref. 8); Darrigol, "God, Waterwheels, and Molecules" (ref. 8); Olivier Darrigol, *Physics and Necessity: Rationalist Pursuits from the Cartesian Past to the Quantum Present* (Oxford: Oxford University Press, 2014): 77–93; Elkana, *Discovery of Conservation of Energy* (ref. 15), e.g., 12.

Lagrange deduced an equation "express[ing] the principle known by the name of *the conservation of the living forces*":

$$\sum\left(\frac{u^2}{2}+\Pi\right)m = F$$

where u is the velocity of the body; m is its mass; Π is the displacement integral on the forces impressed on the body (each force by its own displacement), assuming that its differential "is integrable, [which] is always the case with accelerating forces directed at fixed centres, or with bodies of the same system, that are proportional to some functions of the distance"; and F is an arbitrary constant of integration.[18] This equation became a reference point for some of the mathematical physicists discussed below.

Although he used the term "principle," Lagrange regarded the conservation of the living forces as a deducible theorem in mechanics limited to a particular, although large, realm of phenomena like those related to planetary motions, rather than as a basic axiom. Moreover, the term "conservation" did not imply the existence of an entity that maintains its value. The living force of the system does not maintain a constant value. As he explicated in the second edition of *Analytical Mechanics* (1815), it "has at every instant the same magnitude that the bodies would have gained if, acted upon by the same forces, they were each freely moved on the line they described."[19] For Lagrange, thus, the equation of conservation of the living forces shows that the sum of the living forces and the sum of "moments of force" (the product of force by displacement) together have a constant value.[20] Other authors employed the theorem of conservation of the living forces only when the system returned to the same spatial configuration, or when the forces returned to the same value (usually when the forces could be ignored, as in considering elastic collisions and motions), and thus regained the same value for the living forces per se, as will be seen in its use by Poisson in the next section.

Lagrange's equation of the living force was valid for every interaction among bodies in motion under the influences of forces, provided that the forces could be derived from an integral function, or in modern terms, from a potential. This condition on the forces, however, excludes the very common case of friction, which cannot be expressed as a gradient of a potential function.

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[18] Joseph-Louis Lagrange, *Méchanique analitique* (Paris, 1788), 207–08.

[19] Joseph Louis Lagrange, *Analytical Mechanics*, trans. Auguste Claude Boissonnade and Victor N. Vagliente (Dordrecht: Kluwer, 1997), 213; originally published as *Méchanique analitique* (1815), 290. Grattan-Guinness, *Convolution in French Mathematics* (ref. 8), 280–82.

[20] Lagrange, *Méchanique analitique* (1788) (ref. 18), 195.

Moreover, the equation did not hold for collisions between completely hard bodies, i.e., completely indeformable bodies. Such inelastic bodies could not recoil in case of collisions, since recoiling requires a process of compression and expansion (e.g., two hard equal bodies with equal velocities would halt after collision). Following the tradition of rational mechanics, Lagrange assumed that the basic particles in nature are indeed totally hard (otherwise one should assume that they do have an internal structure, a "spring" that allows their contraction and expansion), and thus assumed the destruction of motion in their collisions. The sum of their living forces decreased in such cases.[21]

Authors on rational mechanics continued to maintain the assumption of perfectly hard bodies well into the 1820s. Two sources, according to Darrigol, contributed to its demise. One was Laplacian physics, which aimed at explaining all phenomena by central forces between "molecules." The other was the development of mechanical theories by scientific professors of engineering, who were more inclined to reject the unobservable assumption of rigid bodies. The same tradition, Grattan-Guinness showed, also introduced the concept of work into mechanics. In 1803, Lazare Carnot, a founding figure in this tradition, equated the change in the living force to the "moment of activity," the integral of the force along the displacement. This integral was formally similar to Lagrange's Π, the integral on the moment of force. Yet, it was not assumed to be an exact integral. Thus, one could use Carnot's equation when moment of activity is provided to the system or is consumed by it. The living force was not a quantitative property of the system itself. Coined "work" by Gustave Coriolis in 1829, Carnot's moment of activity and its equation, as developed in the engineering tradition, were important for the formulation of a full principle of energy conservation.[22] Yet, they did not seem to play a significant role in the employment of proto-energetic considerations.

WAVE OPTICS

The earliest employment of the conservation of the living forces beyond mechanics (including gravitation) that I have found was in optics. This should

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[21] Lagrange referred explicitly to the conservation of the living forces in the elastic case and its diminution in others at the second edition, Lagrange, *Méchanique analitique* (1815) (ref. 19), 292; Darrigol, "God, Waterwheels, and Molecules" (ref. 8), 301–05.

[22] Grattan-Guinness, "Work for the Workers" (ref. 8); Darrigol, "God, Waterwheels, and Molecules" (ref. 8), 308–11.

not be a surprise since the undulatory theory, developed as an alternative to the dominant emission theory in the 1810s, reduced light to a mechanical motion within an elastic body—the ether. Developers of the wave theory could therefore employ laws, like that of the living force, from mechanics in optics. The law was also valid within the emission theory of light, which in accordance with Lagrange's deduction assumed only central forces between light particles and material bodies. Yet, it did not lead the proponents of the emission theory to invoke the conservation of the living forces. Apparently, the developers of that theory did not think in such terms. For example, in his 1805 authoritative exposition of refraction according to the emission theory, Pierre Simon de Laplace calculated the change in the velocity of light in passing from one transparent body to another. He offered a two-page, non-trivial proof based on Newton's second law to conclude that "the quantity $4\rho K$ [a displacement integral on the force] is the increase of the velocity squared of light, where it was affected by [*lorsqu'elle a éprouvé*] the whole action of the transparent bodies," which was an immediate consequence of Lagrange's equation of the living force.[23] Two years later, Étienne Louis Malus employed the specific relation between action and velocity found by Laplace in deriving an expression for the incidence angle of total internal reflection. He did not refer to any general idea of conservation.[24]

Like Laplace and Malus, Thomas Young, the English champion of the wave theory of light, found it useful to argue for the conservation of the living forces in optics, instead of just relying on its known conservation. Unlike Laplace and Malus, Young based his argument on an energetic-like notion of conservation. He employed what is equivalent to the conservation of the living forces to derive the ratio of the intensities of the refracted and reflected light rays in the transition between two media, for an incidence angle normal to the surface

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[23] Similar to Lagrange's derivation of the law of the living force, Laplace integrated Newton's law for this case: $\frac{dds'}{dt^2} = \rho\Pi_1(s')$, where s' is the displacement of a light "molecule," and $\rho\Pi_1(s')$ is "the attraction that the bodies exert on it." Yet, he needed further physical consideration to reach the quoted conclusions. The mass of the light "molecule" does not appear in Laplace's equations. The force should, thus, be regarded as the force per unit mass of light. Pierre-Simon de Laplace, *Traité de mécanique céleste*, vol. 4 (1805), 233–37, on 236.

[24] E. L. Malus, "Mémoire sur la mesure du pouvoir réfringent des corps opaques," *Journal de l'École polytechnique* 8 (1809): 219–28, on 220 (written in 1807). See also Jed Z. Buchwald, *The Rise of the Wave Theory of Light: Optical Theory and Experiment in the Early Nineteenth Century* (Chicago: University of Chicago Press, 1989), 28–31. Notice that Buchwald introduces the concept of *vis viva*, which does not appear in Laplace's and Malus's writing, in his description of their works.

boundary. In his theory, the transition involved no force, but a process analogous to elastic collisions between the particles of the two transparent bodies. Although the conservation of the living forces for such cases had been known since Huygens in the seventeenth century, Young argued for it:

> The intensity of a ray of light must always be considered as proportional to the energy or impetus of the elementary motions of the particles concerned, which varies as the square of the velocity, and not simply as the velocity itself: for if the velocity were made the measure of intensity, there would be an actual gain of joint intensity, whenever a ray is divided by partial reflection: since it follows from the laws of the motion of the centre of inertia, that when a smaller body strikes a larger, not the sum but the difference of [the absolute value of] the separate momenta, will remain unchanged by the collision, while the sum of the energies remains constant in all circumstances.

Notice that Young assumes here the conservation of the intensity of light (or of motion) to infer the conservation of the living forces as the only conserved magnitude in the mechanical process.[25]

A year later, the physicist and mathematician Denis-Simèon Poisson, independently from Young, reached similar results for the analogous problem of determining the intensities of the reflected and the transmitted *sound* waves at the surface of two elastic fluids in a cylindrical pipe. Yet, Poisson followed a different deduction based on the dynamic laws of elasticity. After he had deduced the intensities, however, he verified mathematically that his results agreed with the "conservation of the sum of the living forces before and after the division of the sound waves, [which] followed the general principles of mechanics." Unlike Young, Poisson positioned himself clearly within the tradition of (French) rational mechanics, alluding to works like that of Lagrange.[26] The conservation that Poisson evoked did not refer to any forces, or integral function, but to an elastic case were no forces are assumed. By invoking the law of

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[25] Young provided a numeric example to show that the sum of the absolute values of the momenta are not conserved, whereas the sum of the squares of velocities by mass is. Thomas Young, "Chromatics," *Miscellaneous Works of the Late Thomas Young*, 3 vols. (1855) 1: 279–342, on 336–38; originally appeared in *Encyclopaedia Britannica*, supplement 1817. Olivier Darrigol, *A History of Optics from Greek Antiquity to the Nineteenth Century* (Oxford: Oxford University Press, 2012), 212.

[26] Simèon-Denis Poisson, "Mémoire sur le mouvement des fluides élastiques dans les tuyaux cylindriques, et sur la théorie des instruments á vent," *Mémoires de l'Académie Royale des Sciences* (1817/19), 305–402, quotation on 387; Grattan-Guinness, *Convolution in French Mathematics* (ref. 8), 880–84.

the conservation of the living forces, Poisson arguably triggered Augustin Fresnel to apply it in a similar analysis.[27] In a summary of his work published in François Arago's *Annales de chimie et de physique*, Poisson noted that his analysis was applicable also to the undulatory theory of light, where its intensity is measured "by the sum of the living forces of [vibrating molecules]. . . . The propositions demonstrated in my essay are in agreement with these physicists [who support the wave theory]; but they are appropriate only for perpendicular incidence." Poisson thought that the wave theory was not likely to yield intensity laws for reflection and refraction in agreement with the conservation of the living forces for non-normal incidence, a condition that he regarded as necessary for putting the wave theory on par with the emission theory.[28]

Until Poisson presented his argument, Fresnel, the main proponent of the wave theory, had not employed the concept of living forces. For example, in January 1818, Fresnel elaborated an argument for deriving the relationships between the velocity of the ether molecules of the incidence wave and the velocities of the molecules of the ordinary and extraordinary waves of polarized light, without reference to living forces. Later, however, he added in the margin of his own manuscript, "This equation follows much more naturally from the principle of the conservation of the living forces, which I hadn't considered at the time [*auquel je ne songeais pas alors*]."[29] In a later unpublished note, probably from the summer of the same year, Fresnel claimed that the basic assumptions of the wave theory with the help of the principle of living forces, "which is an immediate consequence of an undulation system," suffice to deduce the rules that account for the colorations of crystals by polarized light. He regarded the economy of this theory an advantage over Jean-Baptiste Biot's.[30] It is likely that Fresnel had encountered Poisson's argument from March at some time between the writing of these two texts.

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[27] Fresnel must have known Poisson's argument. He was strongly invested in the wave theory, was active in the same community of Parisian mathematical physicists as Poisson, and was a protégé of Arago, who published Poisson's summary.

[28] S. D. Poisson, "Extrait d'un Mémoire sur le Mouvement des fluides élastiques dans des tuyaux cylindriques," *Annales de chimie et de physique* 2 (1818): 288–99, on 294.

[29] Augustin Fresnel, "Supplément au mémoire sur les modifications que la réflexion imprime a la lumière polarisée," *Œuvres complètes d'Augustin Fresnel*, Tome 1 publiées par MM. Henri de Senarmont, Emile Verdet et Léonor Fresnel (Paris: Impr. impériale, 1866), 487–508, on 496.

[30] Fresnel, "Note sur la théorie des couleurs que la polarisation développe dans les lames minces cristallisées," in *Œuvres* 1 (ref. 29), 523–32, on 526. Buchwald dates this originally unpublished note to summer 1818, *Rise of the Wave Theory* (ref. 24), 225, 441.

Fresnel addressed Poisson's challenge directly in another unpublished note, probably written in summer 1819. There, he derived the intensities of the refracted and reflected rays at any angle of incidence from the conservation of momentum and Snell's law, and checked its agreement with the conservation of the living force (calculated per the mass of the ethereal molecules in both media). He found, however, that the rays obey the principle only for normal angles of incidence, as Poisson had expected. For Fresnel this conclusion indicated that the basic assumptions of the wave theory of light should be altered. He explained:

> We have seen . . . that the results of this calculation are incompatible with the principle of the conservation of living forces; yet it should be noted that we have considered only the movement of the ethereal molecules in the direction of propagation of the disturbance [*ébranlement*], and that it is possible that they have also transversal movements.
>
> With the help of transverse motions one could satisfy both the general principle of the conservation of the center of gravity and that of the conservation of the living forces, which must be satisfied in all the vibrations of elastic fluids, and one might be able, by determining the transverse motions of waves in this way, to define that singular modification of light to which we have given the name *polarization*.[31]

For Fresnel, as for Poisson, "the principle of the conservation of the living forces, . . . [was] an immediate consequence of a system of undulations." This inference followed his view of light waves as molecular oscillations in a perfectly elastic ether. As such the ether conserved its motion, i.e., its living forces. In this context, Fresnel did not consider cases involving forces (other than the elastic ones, which are translated to the motion of the molecules).[32] The principle seemed to contradict the theory, yet this apparent contradiction turned out to be a potent tool for disqualifying a particular assumption, albeit basic to the theory (light as purely longitudinal vibrations), and replacing it

[31] Augustin Fresnel, "Appendice," in *Œuvres* 1 (ref. 29), 649–53, quotation on 652. See also Buchwald, *Rise of the Wave Theory* (ref. 24), 387–94, part of the quoted text is translated on 389. To my knowledge previous historians did not connect this important note of Fresnel to Poisson's work on sound waves. Grattan-Guinness pointed out the connection between later publications of Fresnel and Poisson's work (Grattan-Guinness, *Convolution in French Mathematics* (ref. 8), 884). See also note 34, below.

[32] See, for example, his discussion of the elastic ether and the living forces regarding reflection in Augustin Fresnel, "Note sur l'application du principe d'Huyghens et de la théorie des interférences aux phénomènes de la réflexion et de la diffraction," *Œuvres* 1 (ref. 29), 201–16, on 211–12.

with another (light as transverse vibrations). Indeed, Fresnel had already assumed, in 1816, that polarized light is a mixture of longitudinal and transverse oscillations. Yet, he had thought that normal light—i.e., unpolarized light—is longitudinal. The principle of the living forces provided a strong argument for also viewing unpolarized light as transverse vibrations, thereby helping Fresnel to formulate his now famous view that light consists of transverse waves.[33]

It was often in this ability to negate laws or assumptions that notions of conservation were most powerful. As a methodological rule, scientists rejected particular assumptions that led to conclusions that contradict proto-energetic rules. Yet, the principle of the living forces gained a constructive role in Fresnel's elaboration of the question of light intensity. In 1821, he mentioned a new "mechanical solution" for the relative intensities of the reflected and refracted rays, and presented it in detail in 1823. Since he now analyzed transverse rather than longitudinal oscillations, he needed another equation, in addition to those of conservation of momentum and Snell's law. To that end, he explicitly employed the conservation of the living forces.[34] It had here, thus, a constructive role in forming particular mathematical relationships in the theory.

The extension of the principle of conservation of the living forces to the elastic medium that carries light waves did not seem obvious to all those who employed the wave theory of light. In 1837, Franz Neumann elaborated Fresnel's theory for optical phenomena in crystals, including double refraction. He set forth the postulates or axioms (*Voraussetzungen* or *Grundsätze*) of the theory. The conservation of the living forces is one of these postulates, and Neumann made use of it in his theory. Yet, he raised doubts about the validity of the principle for real bodies. The question of whether there are really transparent bodies that do not absorb light should be tested empirically, he claimed. He suggested that part of the living force vanishes (*verschwinden*), for example, due to longitudinal waves. The basic argument (even if not the reference to longitudinal waves) is valid also in the realm of energetic physics.

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[33] Buchwald, *Rise of the Wave Theory* (ref. 24), 205–13, 225–31.

[34] Fresnel, "Extrait d'un Mémoire sur la Loi des Modifications imprimées à la Lumière polarisée par sa réflexion totale dans l'intérieur des corps transparents," *Oeuvres* 1 (ref. 29), 753–62, read on 1 Jul 1823, and first published in *Bulletin de la Société philomathique pour 1823*. Buchwald, *Rise of Wave Theory* (ref. 24), 389–93. In these publications, Fresnel referred to Poisson's and Young's mentioned works, Grattan-Guinness, *Convolution in French Mathematics* (ref. 8), 884 (see also note 31, above). Fresnel did not read English, and it is much more plausible that he was influenced by Poisson's mathematical treatment rather than by Young's elementary derivation.

Yet, it is interesting that Neumann did not ask what happened to the vanishing living force. A physicist thinking in terms of the conservation of an energy-like magnitude, would have probably said something about the absorption of the living force and its transformation into heat. Apparently, Neumann did not think of physics in general in conservation terms. This does not, however, diminish the role that the conservation of the living forces played in his theory. The case shows that the significance of the conservation rule could have been independent of the conviction in its validity.[35]

ELECTRODYNAMICS

About the same time that Fresnel used the conservation of the living forces in optics, André-Marie Ampère, employed a similar notion in the new field of electromagnetism, a field in which he led the research following Hans Christian Ørsted's 1820 discovery of the magnetic action of electric current. Even earlier, Ampère had been supporting and consulting with Fresnel in elaborating the wave theory. He suggested to Fresnel the hypothesis of transverse waves to explain polarization.[36] In 1822, Ampère applied the proto-energetic argument to disqualify the assumption that electromagnetic rotation could be accounted for by forces between static magnets, as assumed by followers of Laplace. In this case the proto-energetic notion did not lead him to a new conclusion; instead, combined with Faraday's 1821 discovery of continuous electro-magnetic rotation, it provided an additional forceful argument to support his view that the source of the magnetic action of electric currents is in the motion of the current, and that it should be understood in dynamic terms. Ampère wrote:

> A motion that continues always in the same direction, despite friction, despite resistance from the environment, and [that] this motion is produced by the mutual action of two bodies that remain constantly in the same state, is a fact unparalleled [*un fait sans example*] in all that we know about the

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35. Franz E. Neumann, *Über den Einfluss der Krystallflächen bei der Reflexion des Lichtes und über die Intensität des gewöhnlichen und ungewöhnlichen Strahls* (Berlin: Druckerei der Königlichen Akademie der Wissenschaften, 1837), especially 11. Neumann referred a few times to "the principle of the conservation of the living forces" (*Princip der Erhaltung der lebendigen Kräfte*) on pp. 25, 66, 100, and 147 (among others), and in other cases to the principle, or to the conservation. For the use of *Voraussetzungen* and *Grundsätze*, see pp. iii, 11, 5, and 8.

36. Buchwald, *Rise of the Wave Theory* (ref. 24), 205–06 and *passim*.

> properties that inorganic matter can present; it proves that the action which emanates from voltaic conductors cannot be due to a specific distribution of certain fluids at rest within these conductors, as is the case for ordinary electric attractions and repulsions. This action can only be attributed to fluids in motion within the conductors.[37]

In the context of contemporary physics, Ampère employed the proto-energetic argument to defend a strong conclusion, which implied the rejection of the Laplacian general program of explaining all physical phenomena by central (static) forces. Ampère was well versed in rational mechanics and knew Lagrange's equation of the living force, which should be valid for any system of central forces. It is, I believe, significant that he did not invoke this exact equation, but preferred to base his argument on a less formal claim, namely that in the inorganic realm no motion is conserved against resistance. This choice suggests that he regarded the claim as more general than the mechanical law of the living forces.[38] Ampère's claim is not merely an empirical observation. It originated rather in common *knowledge*, that is, a general notion of conservation of motion in nature, or more precisely in the impossibility to produce motion out of nothing (Ampère did not refer in this context to the possibility of annihilating motion). Motion comes from motion (in this case, of the electric fluids).

A year later, Ampère presented the claim more formally, referring to the derivation of the equation of the living force, albeit in a form that differs from the modern one and without using the term. He relied on "a rigorously demonstrated theorem . . . that when the elementary forces depend only on the distances between the material points that exert them, [then] the material

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[37] André-Marie Ampère, "Notice sur les nouvelles expériences électro-magnétiques faites par différents physiciens, depuis le mois de mars 1821," *Journal de physique, de chimie, d'histoire naturelle et des arts* 94 (1822): 65–66. Ampère republished the text many times, for example, in the 1823 collection of his papers (André-Marie Ampère, *Recueil d'observations électro-dynamiques: contenant divers mémoires, notices, extraits de lettres ou d'ouvrages périodiques sur les sciences relatifs à l'action mutuelle de deux courants électriques, à celle qui existe entre un courant électrique et un aimant ou le globe terrestre, et à celle de deux aimants l'un sur l'autre*, 1823) and in a footnote to his synthetic 1826 *Théorie mathématique des phénomènes électro-dynamiques, uniquement déduite de l'expérience*. The second part of the quote is translated by James R. Hofmann, *André-Marie Ampère* (Cambridge: Cambridge University Press, 1996), 297.

[38] I agree with Hofmann and Darrigol that Ampère probably thought about the specific impossibility of a continuous production of living force from a system of central forces. Yet, one should notice that he refers neither to the term nor to the formal equation. Hofmann, *Ampère* (ref. 37), 297–99; Olivier Darrigol, *Electrodynamics from Ampère to Einstein* (Oxford: Oxford University Press, 2000), 24–25.

points of a system that these forces put in motion cannot all return to the same situation with velocities higher than they had on leaving." Here he referred to the view of the conservation of the living forces when the system returns to the same conditions. Since the sum of the velocities of the material points in electric rotation increases, he concluded that the forces between the infinitesimal current elements depend also on the direction of the electric current. Repeating the claim in 1824, he added that the force can depend also on velocity and time.[39]

Despite his staunch rejection of the accounts of Ørsted's findings by static magnetic forces (theories of "transverse magnetism"), in late 1821 shortly after the publication of Faraday's discovery of electromagnetic rotation, Ampère had experimentally verified that different combinations of static magnets cannot induce continuous motion.[40] This experiment reveals a methodological caution on his side, since a positive result (i.e., finding an effect) would have contradicted his fundamental assumption that the effects observed by Ørsted are due to electric currents. It is difficult to tell if this is a sign of a genuine doubt in his convictions at the time that static magnets cannot produce the same phenomena. It is also unclear whether he had already conceived his proto-energetic arguments for the necessity of inner electric motion (i.e., current) in order to produce the rotation observed by Faraday. These experimental findings and the proto-energetic denial of creating motion strengthened Ampère's belief; both indicated that although currents can behave like magnets, magnets cannot replace currents.[41] In Ampère's view (and in the view of many of his readers, as exemplify below), this was a strong claim against theories of transverse magnetism. As often happens, the arguments did not impress the main opponents.

In 1823, Biot, an ardent supporter of Laplacian physics, reformulated his theory of the magnetic action of electric currents. He agreed with Ampère that the force between an element of current and a magnet depends not only on the

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[39] A.-M. Ampère, "Extrait d'un mémoire sur les phénomènes électrodynamiques lu à l'Académie des sciences le 22 décembre 1823," *Collection de Mémoires* (Paris, 1885) tome 2, 406–7, quote on 406; "Extrait, fait par M. Ampère, de son Mémoire sur les phénomènes électrodynamiques" *Annales de chimie et de physique*, 26 (1824), 134–62, 246–58, on 255–57. See also Robert Locqueneux, *Ampère, encyclopédiste et métaphysicien* (Les Ulis: EDP Sciences, 2008), 592–95.

[40] Hofmann, *Ampère* (ref. 37), 299–300.

[41] With his reductionist approach (which he regarded as methodological), Ampère concluded that all magnets are due to small electric currents. Christine Blondel, *A.-M. Ampère et la création de l'électrodynamique (1820–1827)* (Paris: Bibliothèque Nationale, 1982).

distance but also on the angle between the line that connects the two and the current. However, Biot did not regard this as a contradiction to his belief that the electromagnetic force is ultimately the result of central forces due to an unknown distribution of microscopic magnets. Biot failed to explain how this is possible, a failure that contributed to the demise of his theory. Yet, for my discussion here it is important to notice that he could disregard the argument that his theory—that the magnetic effect of currents is reducible to central forces between magnets—would lead to the creation of motion out of nothing, an argument that was put forward by Ampère.[42]

Apparently other scientists were more impressed by Ampère's proto-energetic argument. For example, the medical doctor and polymath Peter Mark Roget, in his 1832 treatise on electro-magnetism, shows the strong influence of Ampère's work. Roget found that Ampère's theory "satisfies every condition that is required of a true theory." Roget claimed that no combination of magnets could produce electro-magnetic rotation. Echoing Ampère, he argued that "nothing but an agent in motion could produce" a rotary motion that "may be maintained with uniform velocity notwithstanding the retardation from friction." He further added that Ampère's theory leads to the same conclusion.[43] As I show below, Roget employed similar reasoning in explaining the source of power in batteries.

THERMODYNAMICS AND HEAT

A decade later, Ampère applied the conservation of the living force in the theory of heat, which he regarded as an expression of mechanical vibrations

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[42] Jean-Baptiste Biot, "Sur l'aimantation imprimee aux metaux par l'electricite en mouvement'," in *Précis élémentaire de physique expérimentale*, 3rd ed., vol. 2 (Paris: Déterville, 1824), 704–71, on 766–72; Hofmann, *Ampère* (ref. 37), 278–82; Darrigol, *Electrodynamics from Ampère to Einstein* (ref. 38), 15.

[43] P. M. Roget, "Electro-Magnetism," in *Treatises on Electricity, Galvanism, Magnetism, and Electro-Magnetism* (London, 1832), 92, 80, 85. Roget dedicated twelve of 99 pages of the treatise to Ampère's theory. The other parts were concerned mostly with experiment. Roget relied, among others, on Ampère's 1823 collection of papers and his 1826 synthetic book, which both included the above-quoted argument (ref. 37). T. Jock Murray, "Roget, Peter Mark (1779–1869), Physician and Philologist," in *Oxford Dictionary of National Biography* (Oxford University Press). Due to the acceptance of the dynamic source of electromagnetic action, it is quite difficult to follow the weight of the particular conservation arguments. Yet, the fact that Ampère gave them a central role supports the assumption that they were influential. Its confirmation, however, requires a systematic examination of early presentations of the theory that is beyond the scope of this paper.

and radiation. He explained how this view can lead to Fourier's equation for the distribution of heat, in an argument based on an analogy between vibrating molecules and tuning forks. As the motion of a few tuning forks would be transmitted among a system of tuning forks, so would the heat vibrations of molecules.

> Consider a tuning fork put in vibration, and define it by the *living force* of its vibratory motion. This living force is the sum of the products of all the masses of the molecules by the squares of their velocities at a given instant, plus twice the integral of the sum of the products of the forces by the distance transverse by each molecule in the direction of these forces; this integral, which depends only on the relative position of the molecules, being taken such that it is zero in the equilibrium position around which the vibration occurs.

Ampère thus extended the meaning of "living force" to include also the integral of the force by the distance (Lagrange's Π). Thus, he could regard its value as constant, as he explained in a footnote:

> [T]he total living force remains unchanged [*restant la même*], the sum of the products of the masses by the square of the velocities would be the highest possible when all the molecules passed together their equilibrium position, because then the other part, always positive, of what we call living force, is null.[44]

Ampère considered three different cases of the relationships between the tuning fork and its environment. In all cases the living force of the system (the tuning fork and the fluid around it) maintains its value.[45] Turning to heat, he invoked "the equality of the living force" in order to assume that the living force transmitted to the immediate infinite thin layer of molecules (analogous

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[44] "Idées de M. Ampère sur la chaleur et la lumière," *Bibliothèque universelle des sciences, belles lettres et arts* 49 (1832): 225–35, on 229–30 (emphasis in the original). Although the text referred to Ampère in the third person, Ampère later referred to it as his own publication. Ampère, "Sur la chaleur et la lumière considérées comme résultant de mouvements vibratoires," *Annales de chimie et de physique* 58 (1835): 432–44, on 434.

[45] The cases are: (1) A tuning fork in vacuum would maintain its living force. (2) The living force of a tuning fork in a fluid of lower density would decrease in the amount that the tuning fork provides to the (sound) wave in the fluid ("*a chaque vibration, la force vive du dispason diminuera de toute la force vive qui passe dans lónde*"). (3) When the surrounding fluid is in the same density and "elasticity" as that of the tuning fork, the whole living force would be transferred to the environment, "Idées de M. Ampère" (ref. 44), 230.

to the tuning forks) is approximately proportional to the difference of the living forces between two successive layers. Translated into temperature, this assumption leads to Fourier's equation. One may wonder if the conservation of the living force was needed in order to make this quite obvious linear assumption. It seems that the concept of living force as the expression of molecular motion (i.e., velocity and distance from point of equilibrium) and as proportional to the temperature (rather than taking the momentum, for example) was more important in leading to Ampère's assumption. This identification, it is well known, was later necessary for embracing the mechanical theory of heat within conservation physics. Ampère, however, did not attempt to connect heat with other natural phenomena. In this case, unlike in electromagnetism, Ampère relied on the mechanical conservation of the living force, rather than on a general notion of inability to produce power out of nothing, and he used it only to show the compatibility of his assumptions with known results.[46]

Pre-energetic notions, if not exactly conservation, played an essential and more creative role in the now famous analysis of a heat engine by Sadi Carnot, the son of Lazare, in 1824. As is well known, Carnot's reasoning and conclusions, including the "second law of thermodynamics," relied on two basic assumptions: the conservation of heat (caloric) and the impossibility "of motive power [work] being created in unlimited quantities without the consumption of caloric or of any other agent." He needed both assumptions to prove that "the maximum amount of motive power gained by the use of steam [i.e., in the reversible (ideal) cycle of the heat engine] is also the maximum that can be obtained by any means whatsoever."[47] If that had not been the case, he showed, one would have been able to create motive power in contradiction to the basic assumption. The conclusion allows, among other things, his analogy between the motive power of heat and of fall. In both, one cannot gain more motive power than that determined by the maximal motive power obtainable for a specific fall of either height or temperature, and the amount of material

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[46] In this context, Ampère also named the two kinds as *force vive explicite* (the motions) and *force vive implicite* (twice the integral of the force by the distance). According to Darrigol, he was the first to introduce their sum "as the conserved quantity for special isolated system." Darrigol, "God, Waterwheels, and Molecules" (ref. 8), 322.

[47] Sadi Carnot, *Réflexions sur la puissance motrice du feu et sur les machines propres à développer cette puissance* (Paris: Bachelier, 1824), 21, 22; translations in Sadi Carnot, *Reflexions on the Motive Power of Fire: A Critical Edition with the Surviving Scientific Manuscripts*, tr. and ed. Robert Fox (Manchester, UK: Manchester University Press, 1986), 69, 70.

that falls. Hence, the maximal mechanical work that can be yielded from heat depends only on the amount of heat and on the difference in temperature. Thus, work can be produced by a heat engine only when heat passes from a hot to a cold body, which since the mid-nineteenth century is a popular formulation of the second law of thermodynamics.

Like Ampère in his discussion of electromagnetism two years earlier, Carnot justified his rejection of the creation of motive power, and of perpetual motion, on general notions of physics. "Creation of this kind completely contradicts prevailing ideas, the laws of mechanics, and sound physics; it is inadmissible." Carnot justified the extension of the impossibility of perpetual motion from mechanics (where it was established through the equation of the living force) to all physics on three grounds. First, he suggested that heat and electricity might also be reduced to motion, and thus these phenomena were also governed by the general laws of mechanics. Second, on an empirical basis he pointed at the failure to yield surplus of motive power by non-mechanical means, like the electric pile. Third, in a broad sense perpetual motion means an unlimited production of motive power. "If such creation was possible," Carnot wrote, "we should have no need to seek motive power in air currents, water, or combustible materials. We should have at our disposal a limitless supply on which we could draw indefinitely."[48]

Carnot presented the three arguments only in a footnote to the text, which seems reasonable as they distract from the goal of showing that no engine can be more efficient than the reverse cycle. It does, however, indicate that the impossibility of creating motive power had primarily a heuristic role in the *Réflexions*, as a theoretical tool for the derivations. Had the inability to create motive power been of major interest, Carnot would probably have devoted a special discussion to this principle. This role of the principle in the theory suggests that Carnot formulated the impossibility in order to justify a crucial step in his inference about the production of work from heat. As commentators have pointed out, his arguments show that one cannot gain work from nothing, but do not show that work or its equivalent are conserved. The theory allows the destruction of motive power.[49] Indeed, Carnot did not employ a full argument of conservation

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[48] Ibid., original on 21–22, translation on 69.

[49] In unpublished notes that were probably written at the time of writing the *Réflexions* or shortly after. Carnot adopted the mechanical view of heat. This led him to a full mechanical reduction of physics, which he had not adopted in his analysis of the heat engine. The conservation of motive power in mechanics (i.e., the equation of living force) led him to an argument for its full conservation in nature, reminiscent of his argument for the impossibility of creating

of a magnitude (like that of motive power), but only the denial that motion can be created.[50] That sufficed for his derivations, as it did for Ampère. The particular consequences that they could draw from announcing the impossibility of creating power motivated both to formulate the proposition.

Although highly celebrated today, Carnot's theory was ignored by most contemporaries. The mathematician, engineer, and professor Émile Clapeyron, the first to seriously discuss Carnot's theory, recognized the importance of the impossibility of creating power to the theory, in his 1834 mathematical elaboration of Carnot's ideas.[51] A year later Stéphane Mony-Flachat, a practicing civil engineer, regarded this proposition as a basic principle of heat machine theory. Yet he did not apply it for any inferences beyond its use by Carnot. Other users of Carnot's theory, like Charles Louis Franchot, an inventor of an air engine, did not refer to this impossibility in recapitulating its main claims. Not all readers of the *Réflexions* adopted the argument. For example, in 1841 the mathematician, physicist, and engineer Jean-Victor Poncelet referred to a similarity between his and Carnot's conclusion that the amount of "work" (his term) produced depends only on the amount of heat employed (which in his concept was also a function of the temperature difference). Yet he did not follow Carnot's reasoning, but based his conclusion on a particular view of the caloric as an ideal elastic fluid. As a perfect elastic gas, heat can return to the exact same initial condition, and cannot gain additional motive power. From this mechanical analogy, Poncelet inferred that the reversible cycle (free from resistance) provided the maximum work from a particular amount of heat. With this particular assumption about the mechanism of the mechanical power of heat, Poncelet did not need to rely on a principle that denies the creation of work.[52] Carnot, on the other hand, refrained from hypothesizing

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motive power in the *Réflexions*. "[T]he true creation of motive power is impossible . . . [I]t must also be impossible for power to be destroyed," he wrote. "For otherwise all the motive power in the universe would, in the end, be annihilated. Hence there is no such thing as true impact between bodies." With this rejection of hard collisions, he formulated a full principle of conservation. He might have also employed this principle to calculate the mechanical equivalent of heat, but he did not leave the argument that led to his results. See Carnot's notes in Carnot, *Reflexions on the Motive Power* (ref. 47), 181–212, and Fox's introduction on 30–32.

[50] See, for example, Philip Lervig, "On the Structure of Carnot's Theory of Heat," *Archive for History of Exact Sciences* 9 (1972): 222–39, on 228–29; and Fox's discussion, ibid., on 123.

[51] E. Clapeyron, "Mémoire sur la Puissance Motrice de la Chaleur," *Journal de l'École Polytechnique* 14(1834): 153–90; Fox, "Introduction" (ref. 47), 22–37.

[52] Pietro Redondi, *L'accueil des idées de Sadi Carnot et la technologie française de 1820 à 1860: de la légende à l'histoire* (Paris: Vrin, 1980), 126–28, 134–36. Redondi reproduced Franchot's longer

about the nature of heat and thus needed a general argument regarding heat to reach similar conclusions. The impossibility argument found attentive ears toward the end of the decade. Ludvig Colding, famous for his formulation of ideas close to energy conservation, adopted the view that "it is an absurdity to assume that one can produce motive force [*Kraft*] or heat from nothing." Two of the main protagonists of energy physics, Thomson and Helmholtz, embraced Carnot's theory and its use of the impossibility argument. All three relied on Clapeyron's account.[53]

Those who did find the proto-energetic argument convincing did not necessarily accept either Carnot's assumption that heat is conserved or his theory. In 1825, Marc Seguin (called Seguin aîné, "the elder") employed the impossibility to admit "the creation of force" to rebut the substantial view of heat in support of his own theory. According to the caloric theory, he claimed one would be able to reuse the same caloric in many cycles of a steam engine because the caloric could be regained by condensation. "We may, by means of a small quantity of caloric, produce an indefinite number of oscillations." To prevent such a perpetual motion, he suggested that caloric is consumed "in the dilatation of the aeriform fluid."[54] This conclusion agreed with his particular theory of heat as motion presented three years earlier. According to his theory, molecules of all bodies are in constant elliptical motion around each other, like small celestial systems. Heat is a manifestation of these motions.[55] One may question the force of the argument against the caloric view. Did not Carnot use the same reasoning in the caloric theory? Clearly, an argument against the material view of heat did not establish Seguin's particular hypotheses about its nature. Still, Seguin advanced it as a support for his theory.

He had suggested the theory "in order to obtain an explanation" of "the principle . . . that the *vis viva* could neither be created nor annihilated, and consequently, that the quantity of motion on the earth [sic] had a real and

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manuscript submitted to the *Académie des science* on pp. 205–26. Jean-Victor Poncelet, *Introduction à la mécanique industrielle, physique ou expérimentale*, 2nd ed., 1841, 203–4, see also 86–91.

[53] Caneva, "Colding" (ref. 5), 18–19; Fox, "Introduction" (ref. 47), 38–39.

[54] [Marc] Seguin aîné, "On the Effects of Heat and Motion," in a Letter to Dr. Brewster, *Edinburgh Journal of Science* 3 (1825): 276–81, on 280; Redondi, *L'accueil de Carnot* (ref. 52), 140–47; Charles C. Gillispie, *The Montgolfier Brothers and the Invention of Aviation 1783–1784: With a Word on the Importance of Ballooning for the Science of Heat and the Art of Building Railroads* (Princeton, NJ: Princeton University Press, 1983).

[55] Seguin aîné, "'Observations on the effects of heat and of motion', letter to Sir J. F. W. Herschel, 12 Sept. 1822," *Edinburgh Philosophical Journal* 10 (1824): 280–83.

finite existence."[56] Thus, this was not a case of formulating and employing a conservation law to advance a particular theory, as examined in the present essay, but the formulation of a speculative theory in order to justify a conservation law. Seguin credited the principle to his late grand-uncle and teacher of science and engineering Joseph Montgolfier, who was famous for the development of hot air balloons and a member of the revolution era's *Institut des sciences et des arts*. Seguin, however, failed to provide a rigorous basis in either physics, mechanics, or philosophy for his announcement of an absolute conservation of the living force. Therefore, his argument was not accepted at the time. This failure can explain his change of strategy in 1825, when he attempted to support the theory on a more acceptable conviction, namely, on the impossibility to produce endless power. Yet, the inference to the particularities of his 1822 theory remained unclear.

A decade later he used reasoning similar to those suggested in 1825, in estimating the amount of mechanical work produced by heat in a steam engine.[57] The impossibility of "obtaining unlimited quantity of motion, which can be accepted neither by common sense [*bon sens*] nor by sound reasoning [*saine logique*]" justified his assumption that heat is transformed into mechanical effect (the product of weight by height). In his calculation, he translated this general idea into the more specific assumption that the decrease of heat can be measured by the mechanical work made by gas when it is expanded, as is the case of expansion of steam against a piston. Thus, the impossibility argument helped Seguin to justify his calculation. Yet since he believed that heat is motion, he could infer for himself the equality between heat and the mechanical motion of the piston from his understanding of the conservation of the living force.[58]

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[56] Ibid., 280.

[57] Seguin obtained results for particular temperature differences between 80 and 180°C. In addition, he added a list of the "corresponded temperature differences," from which one can infer a value of 365 kilogram-meters per 1 degree of kilogram water for the mechanical equivalent of heat. It is unclear whether he himself calculated the value (and produced the table from it), and what was his procedure if he did so. Seguin aîné, *De l'influence des chemins de fer et de l'art de les tracer et de les construire* (Paris: Carilian-Goeury, 1839), 378–89. Nahum Kipnis, "Thermodynamics and Mechanical Equivalent" (ref. 16); Gillispie, *The Montgolfier Brothers* (ref. 54), 175–76.

[58] Seguin (ref. 57), 282. The case of Holtzmann shows that the assumption that heat is convertible to work was not necessary for calculating the maximal amount of work produced by the transfer of heat from a hot to a cold body by equating it with the mechanical work of expansion. Carl Holtzmann, *Ueber die Wärme und Elasticität der Gase und Dämpfe* (Manheim: Tobias Loeffler, 1845).

THEORY OF THE VOLTAIC PILE

An additional issue in which pre-energetic notions were useful was the explanation of the voltaic pile. Like the electromagnetic rotation, it presented a continuous action capable of producing motion. Historians of the energy principle had long ago referred to Roget's and Faraday's use of an energetic argument to reject the "contact" theory for the source of power of the electric pile. According to that theory, the current produced by the pile originates in a contact force between the two metals that formed it (e.g., zinc and copper). Its rival, "the chemical theory," regarded the origin of the electric flow to be in chemical reactions among the metals and the solution between them. Despite contrary claims by both sides, the question of the source of the pile's effect haunted scientists for many decades after its invention in 1800.[59]

In 1829, Roget claimed that the evidence was conclusive for the chemical theory, still he added "a forcible argument" against the contact theory:

> If there could exist a power having the property . . . of giving continual impulse to a fluid in one constant direction, without being exhausted by its own action, it would differ essentially from all the other known powers in nature. All the powers and sources of motion, with the operation of which we are acquainted, when producing their peculiar effects, are expended in the same proportion as those effects are produced; and hence arises the impossibility of obtaining by their agency a perpetual effect; or in other words a perpetual motion. But the electro motive force ascribed by Volta to the metals when in contact, is a force which as long as a free course is allowed to the electricity it sets in motion, is never expended, and continues to be exerted with undiminished power, in the production of a never ceasing effect. Against the truth of such a supposition the probabilities are all but infinite.[60]

Roget's claim resembles Ampère's in its reference to a motion in the *same direction* and in its claim that it would be different from all other known effects in nature. As mentioned above, Roget had read closely Ampère's publications

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[59] Helge Kragh, "Confusion and Controversy: Nineteenth-Century Theories of the Voltaic Pile," in *Nuova Voltiana, Studies on Volta and His Times*, ed. Fabio Bevilacqua and Lucio Fregonese, vol. 1 (Pavia, IT: Università degli studi di Pavia, 2000), 133–57.

[60] P. M. Roget, "Treatise on Galvanism," in *Treatises on Electricity, Galvanism, Magnetism, and Electro-Magnetism* (this part originally appeared in 1829), 32. This is the full argument by Roget.

and adopted his electrodynamic theory and the claim that rotation must originate in a moving agent.[61] Here he applied the same reasoning for the contact theory. As Ampère rejected the central force explanation of electromagnetism on the basis of its failure to account for the continuous source of motion against friction, so Roget rejected the contact theory on the basis of its failure to account for the steady flow of current.[62] Like Ampère, Roget's justification for this proto-energetic claim did not rely on the equation of the living force. Since his discussion was qualitative rather than quantitative, unlike his French predecessor, the English polymath did not even hint at the equation. Both arguments were used to invalidate certain assumptions, and that did not require a full symmetric conservation principle, but only the rejection of creating motion out of nothing. In his argument for the impossibility of a perpetual motion, Roget assumed that the effect is proportional to the cause, but he did not declare their equality. In other words, it does not follow from the text that the cause is reproducible from the effect. Something might get lost in the process.

A decade later, Faraday advanced a very similar argument as a conclusive proof of the chemical theory, on top of what he regarded as its empirical demonstration.

> The contact theory assumes, in fact, that a force which is able to overcome powerful resistance . . . can arise out of nothing. . . . This would indeed be a creation of power, and is like no other force in nature. We have 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 case . . . is there a pure creation of force: a production of power without a corresponding exhaustion of something to supply it.[63]

Like Ampère, Faraday argued that the contact theory should be *a priori* rejected since it assumes an impossible force that could produce an

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[61] Roget published *Treatise on Galvanism* three years before his *Treatise on Electro-magnetism*, in which he referred to Ampère (see ref. 43). Yet, it is highly likely that he had adopted Ampère's views soon after their publication in the early to mid-1820s, when they were at the center of scientific attention in London. There is no sign of a strong conversion of Roget's ideas.

[62] Unlike in his discussion of electromagnetism, here Roget failed to mention that the impulse acts against friction, but it was probably implicit in his reference to an impulse rather than to motion.

[63] Michael Faraday, "Seventeenth Series: On the Source of Power in the Voltaic Pile—continued," *Experimental Researches in Electricity*, vol. 2 (1840; London: Taylor, 1844), 103 (section 2071).

inexhaustible effect, which is unparalleled in nature. His words even echo the literal translation of Ampère's "un fait sans exemple." Faraday most probably read the original argument, which appeared in a paper that Ampère sent him in 1822 and again in a reprint a year or so later. The two corresponded and exchanged papers during that decade, and they showed much interest in each other's work.[64] In a later letter to Faraday, Ampère also specifically mentioned that producing indefinite motion against friction is "absolutely impossible."[65] Faraday might have also read Roget, as the latter's publication dealt with a subject of his interest. If Faraday had read these texts, that was probably quite a few years before formulating his argument. In a note added to the manuscript he claimed that he "was not before aware" that Roget made a similar argument.[66] Whether inspired by others or not, Faraday based his argument not only on the empirical inability to find a similar phenomenon in physics (as Roget and Ampère claimed) but also explicitly on the metaphysical equality of cause and effect: "[W]ere the contact theory true, then . . . the equality of cause and effect must be denied. Then would the perpetual motion also be true."[67] Unlike Roget, Faraday claimed that the cause and the power that it produces are equal, or at least that the creation of the latter would correspond to an exhaustion in the former. He found such relations in known physical processes of conversion among electricity, chemistry, heat, and magnetism. Although announcing a full conservation of power or force, Faraday employed only one side of it, namely the inability to create power in rejecting the contact theory. As is well known, when Faraday elaborated his ideas about the conservation of force in 1857, it became clear that they were in disaccord

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[64] In early 1822, Ampère bound a few of his papers with some further notes and a few contributions of others, and sent them to a handful of people including Faraday. Michael Faraday, *The Correspondence of Michael Faraday*, ed. Frank A. J. L. James, vol. 1 (London: Institution of electrical engineers, 1991), letter from 10 Jul 1822 on 268, editor's note with the content of the volume on 280. He further sent his *Recueil d'observations électro-dynamiquesi*, a collection of earlier publications including the "notice" that included the argument, as mentioned in a letter from 27 Apr 1824 (ibid., 349); the editor suggests that this is the same collection sent earlier, but Ampère referred in the letter to the title of his collection and to the recent completion of its printing, which indicates that this was the published *Recueil* rather than the earlier special collection sent to a handful of colleagues. Faraday had a special interest in these parts of the article that related to his own discovery of rotation, the explanation of which was a subject of disagreement between him and Ampère. Faraday's correspondence includes about twenty letters with Ampère, and there are references to others that were probably lost.

[65] Ampère to Faraday, 12 Jun 1826, ibid., 414–15.

[66] Faraday, "Source of power" (ref. 63), 103–4.

[67] Ibid., 104.

with the modern concept of conservation of energy,[68] a fact that did not harm his argument against the contact theory.

"The unphilosophical nature of the assumed contact force" seemed to Faraday "to remove the foundation itself of the contact theory." Yet, this was not the original reason that he preferred the chemical theory. He had supported the theory already six years earlier. As is often the case, the argument initially convinced those who were already convinced. Faraday scorned the proponents of the chemical theory for their initial neglect of his proto-energetic argument.[69] The argument was first raised by supporters of the chemical view. Reporting on a recent development in the field in 1843, the Heidelberger chemist Leopold Gmelin agreed with Faraday's argument, emphasizing its physical rather than metaphysical justification: "One activity calls forth the other; frictional electricity comes into being as a result of mechanical motion; thermoelectricity is connected with the motion of heat, but contact electricity comes into being out of nothing, is a creation of force."[70]

Two years later one of the ardent advocates of the contact theory, Christoff Pfaff, tried to rebut Faraday's and Gmelin's claim. He accepted that motion is neither created from nothing nor destroyed by itself. Yet he assumed that motion can be produced by an inexhaustible source (*Grund*), and regarded the force of gravitation as such a source. The contact force could be analogous to gravity. This was clearly foreign to the mathematical tradition of the conservation of the living force and unconvincing for Faraday. Caneva identified Pfaff's discussion as the first serious treatment of Robert Mayer's ideas about the conservation of force. Pfaff connected the general statement of Mayer with Faraday's employment of a similar though less general and qualitative claim (and as Caneva shows, Mayer's 1842 formulation was also not free from

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[68] Michael Faraday, "On the conservation of force," *Experimental Researches in Chemistry and Physics* (originally 1859): 443–63 (from 1857, with an appendix from 1858); David Gooding, "Metaphysics versus Measurement: The Conversion and Conservation of Force in Faraday's Physics," *Annals of Science* 37 (1980): 1–29. Gooding claims that Faraday held a non-quantitative principle of conservation from the 1830s.

[69] Faraday, "Reply to Hare's critical remarks" (Mar 1843), in *Experimental Researches in Electricity* 2 (ref. 6), 276. On his earlier support of the contact theory, Kragh, "Confusion and Controversy" (ref. 59), 146.

[70] Leopold Gmelin, *Handbuch der Organischen Chemie, Bd. 1: Cohäsion, Adhäsion, Affinität, unwägbare Stoffe und unorganische Verbindungen der Nichtmetallischen wägbaren Stoffe*, 4, umgearb. u. verm. Aufl. (Heidelberg: Winter, 1843), 454–55; translated and discussed by Caneva, *Mayer* (ref. 6), 183.

contradictions).[71] For us this is a reminder (a) of the connection between the use and the formulation of the idea, and (b) that scientists often referred to the general and abstract ideas, like the impossibility of producing power, when they were relevant for the particular scientific questions that occupied them, like the source of the action of batteries. Apparently, Pfaff's position was unpopular. For the physical chemist Christian Schönbein, the fact that the contact theory allows the production of unlimited power was one of the three reasons to reject it, despite its empirical support. A former advocate of the chemical theory, in 1849, he used this argument to suggest a compromise between the two theories, hoping to end the controversy.[72] Although for a decade the controversy calmed down, the hope for consensus was premature. Energetic arguments were far from conclusive. In 1862, William Thomson, one of the champions of energetic physics, advocated the contact theory, which he showed can be formulated to be compatible with energy conservation.[73]

Thermodynamic and Heat Again

Roget's and Faraday's argument also inspired discussion unconnected to batteries. In 1844, James Joule adopted the impossibility argument but turned it on its head. Where Faraday and Roget asserted the impossibility of *producing* power to support their view of the electric pile, Joule raise the claim "that the power to *destroy* belongs to the Creator alone" to support his view on the convertibility of work and heat. Creatively misinterpreting their claim, he "entirely coincide[d] with Roget and Faraday in the opinion that any theory which, when carried out, demands the annihilation of force, is necessarily erroneous." Structurally, however, he employed the argument like his senior colleagues; he showed that the rival theory leads to the absurd conclusion and should therefore be rejected. In this case, he rejected the view "that the mechanical power of the steam-engine arises simply from the passage of heat

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[71] Caneva, *Mayer* (ref. 6), especially, 183–84; C. H. Pfaff, *Parallele der chemischen Theorie und der Volta'schen Contacttheorie der galvanischen Kette* (Kiel: Universitäts-Buchhandlung, 1845), 105–6 (there are several version of Pfaff's given name).

[72] C. F. Schönbein, "Ueber die chemische Theorie der Volta'schen Säule," *Annalen der Physik* 78 (1849): 289–306. Although Schönbein wrote this paper after the formulation of the energy principle, e.g., by Helmholtz, the paper did not disclose any knowledge of these publications, which were not well known at the time. Instead, Schönbein argued on the basis of the impossibility of creating power. I therefore regard this publication as done without knowledge of the full principle.

[73] Kragh, "Confusion and Controversy" (ref. 59), 148–50.

from a hot to a cold body, no heat being necessarily lost during the transfer." The supporters of that view, like Clapeyron, admitted their theory assumed "loss of *vis viva* in the passage of the heat from the furnace into the boiler." In his work, on the contrary, Joule advanced the view that heat and mechanical power are convertible into each other. According to his theory, "the steam, while expanding in the cylinder, loses heat in quantity exactly proportional to the mechanical force which it communicates by means of the piston."[74] The power of the heat engine is explained by the conversion of heat into mechanical power. No power, force, or living force is lost.

Joule used the impossibility argument to invalidate the rival theory, raising thereby the reliability of his own view. As with Ampère, Roget, and Faraday, his own view did not originate in the proto-energetic consideration. In the previous few years he had accumulated experimental evidence for the transformation of mechanical power into heat by different means and developed a view of heat as motion compatible with its transformation to mechanical motion. The experiments on condensed air presented in this paper indicated also the conversion of heat into mechanical power and corroborated the assumption of a constant mechanical equivalent of heat, i.e., a constant proportion between heat and mechanical motion or power. Still, asserting the impossibility of destroying power justified his claim of an exact equivalence between heat and mechanical force. A year earlier he had asserted that "the grand agents of nature are, by the Creator's fiat, *indestructible*; and that wherever mechanical force is expended, an exact equivalent of heat is *always* obtained." Yet, there he employed the view only to dismiss the need of repeating experiments that showed "that heat is evolved by the passage of water through narrow tubes."[75] Despite Donald Cardwell's contrary claim, it is difficult to regard either of Joule's statements as an expression of energy conservation. He failed to define the magnitude of the mechanical force or power; although he referred in 1844 to "*vis viva*," he also referred to "power" and "force." In addition, he did not explicitly deny the creation of power. He

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[74] James Joule, "On the Changes of Temperature Produced by the Rarefaction and Condensation of Air," in *The Scientific Papers of James Prescott Joule*, vol. 1 (London: Taylor and Francis, 1884), 188–89 (emphases in the original); Cardwell, *Joule* (ref. 5), 62–69. Strangely, Cardwell claims that "it is not clear what opinions of Roget or Faraday Joule had in mind" even as he referred to their above-quoted texts.

[75] James Joule, "On the Calorific Effects of Magneto-Electricity, and on the Mechanical Value of Heat," in *Scientific Papers* (ref. 74), 123–59, on 158–59 (emphases in the original); Cardwell, *Joule* (ref. 5), 52–60.

advanced the main tents of energy conservation in a lecture before a more general audience three years later.[76]

BATTERIES, CHEMISTRY, AND HEAT

Unlike Joule, the celebrated German chemist Justus Liebig employed Faraday's argument in a connected controversy over the economy of the electric battery. In 1841, he claimed that the voltaic pile would continue to be a considerably more expensive source of power than coal. His argument was based on the view that the chemical affinity between the metals and solutions in the battery is the source of its power, and thus the amount of ingredients poses limitations on its total power. To argue that the power is limited by its source, Liebig invoked the metaphysical claim that "no power [*Kraft*] can be generated out of nothing."[77] In batteries the electric power originated in the oxidation of zinc in the solution of sulphuric acid. It can be produced also by other means like direct oxidation, i.e., burning in the air, which is easily comparable to the burning of coal in the steam engine. Resorting to chemical equivalences, Liebig showed that carbon produces considerably higher chemical effect per mass than zinc. Even allowing, for the sake of argument, that the mechanical effect gained through the pile is a few times greater than the power gained through heat (due to waste), coal would still produce more power per weight, and would be much cheaper to use.

To support the claim that the chemical affinity is the source of the effect of the battery, Liebig advanced an idea of conservation of these powers: "Heat, electricity and magnetism stand in a similar relationship to each other as that between the chemical equivalents of carbon, zinc and oxygen. With a certain quantity of electricity we produce a corresponding proportion of heat or magnetic power [*Kraft*], which are mutually equivalent." They are further equivalent to chemical affinity. Moreover, he distinguished between the ability of a magnet to hold a large mass and its ability to produce the effect of lifting it. In modern mathematical terms that would be the difference between work and static force (no discussion of its dynamic effect was suggested), but Liebig did

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[76] James Joule, "On Matter, Living Force and Heat (Apr 1847)," in *Scientific Papers* (ref. 74), 265–75. For Cardwell claims, see *Joule* (ref. 5), 58, 68.

[77] "Aus nichts kann keine Kraft entstehen"; Justus Liebig, "Zehnter Brief," in *Chemische Briefe* (Heidelberg: C. F. Winter, 1844), 117; originally published as letter 4, in *Allgemeine Zeitung* (Augsburg) 30 Sep 1841, 2177–79.

not use such terms. His concept of *Kraft*, power or force, was not well defined, and he did not formulate a clear law of conservation. He did not need a well formulated law for his inference about the relative economy of batteries.[78]

Still, consideration of the power of batteries induced Liebig to formulate clearer ideas about the inability to produce power and about the conservation of power than he had hitherto done. Liebig's reliance on the proto-energetic argument seems to draw upon his reading of Faraday. Caneva pointed at Liebig's new use of the electrochemical equivalents, employed by Faraday, and to the striking change from a letter that appeared on September 16, 1841, two weeks before the one discussed above. In the earlier letter, Liebig suggested that in some chemical reactions mediated by platinum, the process is "a *perpetuum mobile*, a clock that, having run down, winds itself up again—a power [*Kraft*] that never exhausts itself."[79] In between he probably read the above-mentioned paper of Faraday that includes the principal rejection of the creating power, which appeared in the August issue of *Annalen der Physik*. There both "power" and "force" were translated into *Kraft*, the term used by Liebig.[80]

A few months later, Liebig employed the impossibility of creating power (*Kraft*) and action (*Thätigkeit*) out of nothing to support his view about the source of animal heat and motion. This assertion contradicted the view that the nervous system produces action in the body without any kind of chemical reaction. It shows that one should look for the origins of the forces observed in animal process (like the electric currents in the nerves) in other forces like those of chemical affinity. Ultimately, Liebig claimed, all animal action originates in the chemical reactions of the ingredients of food with oxygen. In particular, he invoked the impossibility of creating power to support the claim that respiration is the only source of animal heat. Despite his productive use of the principle, as Caneva showed, Liebig's concepts of force and motion continued to be incoherent in this essay and even more so in subsequent ones that dealt with the role of the vital force (*Lebenskraft*), which characterizes living

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[78] Ibid., quotation on 118, translation follows Caneva, *Mayer* (ref. 6). 181–82. For Liebig's concepts, see also "Zehnter Brief" (ref. 77), 109–19, 173–83.

[79] Liebig, "Zehnter Brief" (ref. 77), quotation on 175; 180–82, 175–78, [J. Liebig], "Chemische Briefe II," *Allgemeine Zeitung* (Augsburg), 16 Sep 1841, 2069–71, on 2070 (Liebig omitted this letter from the book that collected the letters).

[80] Faraday, "Siebzehnte Reihe von Experimental-Untersuchungen über Elektricität" (Fortsetzung), *Annalen der Physik* 53 (1841): 548–71, on 568–69.

organism (not to be confused with living force). He republished these essays in his widely read book, *Animal Chemistry*, which first appeared in 1842.[81]

Liebig's principal rejection of the creation of motion in analyzing animal heat stimulated two of the famous "discoverers" of energy conservation, Mayer and Helmholtz, to elaborate and articulate their related ideas.[82] It prompted Mayer, who had been thinking about related questions for some time, to compose his first article on the conservation of force, which appeared in the *Annalen der Chemie*, edited by Liebig, in 1842. Although Mayer's view of "force" and its alleged conservation was far from consistent, it led him to the conclusion that "heat must also naturally be equivalent to motion and fall-force." This assumption allowed him to employ the relationship between the heat capacity of gases under constant pressure and under constant volume to calculate this so-called mechanical equivalent of heat. He continued developing and refining his ideas on conservation in subsequent papers in the following years.[83]

In 1846, Helmholtz wrote a report about Liebig's 1845 re-analysis of the most reliable quantitative experiments on the heat produced by a few animals carried out by Dulong and Desprez from the early 1820s. Employing newer determinations of the heat produced in the direct oxidation of hydrogen and carbon, Liebig showed that, contrary to earlier analysis, Dulong's and Desprez's results agree with the assumption that oxidation of carbon and hydrogen is the sole source of animal heat.[84] Helmholtz endorsed Liebig's

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[81] Justus Liebig, "Der Lebensprocess im Thiere, und die Atmosphäre," *Annalen der Chemie und Pharmacie* 41 (Dec 1841): 189–219, especially 213–17 (see Caneva, *Mayer* (ref. 6), for the date); reappeared as the first part of Justus Liebig, *Die organische Chemie in ihrer Anwendung auf Physiologie und Pathologie* (Braunschweig: Vieweg, 1842), and *Die Thier-Chemie: oder, Die organische Chemie in ihrer Anwendung auf Physiologie und Pathologie*, 2nd ed. (Braunschweig: Vieweg, 1843).

[82] Liebig's earlier discussion of the power of batteries might also have influenced Colding, who quoted it at length in a paper submitted in 1848. Colding was not interested in the question of animal heat. Ludvig Colding, "Investigation Concerning the Universal Forces of Nature and Their Mutual Dependence and Especially Concerning the Heat Evolved from the Friction of Certain Solid Bodies," in *Ludvig Colding and the Conservation of Energy Principle*, ed. and trans. Per F. Dah (New York: Johnson Reprint, 172), 19–45, on 21–22; Caneva, "Colding" (ref. 5), 19–20.

[83] Caneva points out that in Mayer's inference, Mayer relied among others on an inappropriate analogy between fall and compression of gases. Yet, it does not invalidate the logical connection that Mayer perceived between the idea of conservation of force and the quantitative equivalent between heat and motion (Mayer did not regard heat as motion). Caneva, *Mayer* (ref. 6), especially 257–78; Robert Mayer, "Bemerkungen über die Kräfte der unbelebten Natur" *Annalen der Chemie und Pharmacie* 42 (1842), 233–40, quotation on 239.

[84] Justus Liebig, "Ueber die thierische Wärme," *Annalen der Chemie und Pharmacie* 53 (1845): 63–77.

analysis, which he regarded as an addition to the author's earlier theoretical argument based on the impossibility of creating *Kraft*. The fruitful employment of the principal to the question of animal heat suggested for Helmholtz that it should be better formulated and more securely established:

> Although the principle of the constancy of the equivalents of force [*Kraftäquivalents*] when one natural force is excited by another is justified entirely from a logical point of view, and has already been used as a foundation for mathematical theories (e.g., by Carnot and Clapeyron for determining the work that a certain quantity of heat can produce, and by Neumann in the theory of induced currents by the motion of magnets or currents), it has neither been pronounced and recognized [*anerkannt*] theoretically in its full extent nor has it been undertaken empirically, even though the experiments performed so far are entirely consistent with it.[85]

Helmholtz took on the task of formulating, proving, and elaborating empirical consequences of the principle, and immediately began to work on *Über die Erhaltung der Kraft*.[86] In writing this essay, he was clearly aware of early employments of the principle. He might have even exaggerated the role of such ideas, since it is doubtful that Neumann relied on a principle of equivalence, beyond the equality between the work of an external force (in his case of electromagnetic induction) and the loss of the living force of the system, as was common in mechanics. Neumann also introduced a potential function for describing the phenomenon.[87] Between these two features, Helmholtz could read into Neumann's work his own ideas, which he would soon elaborate in his famous essay. There, Helmholtz also mentioned the earlier use of the conservation rule by Fresnel.[88]

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[85] Hermann Helmholtz, "Bericht über die Theorie der physiologischen Wärmeerscheinungen für 1845," in *Wissenschaftliche Abhandlungen*, vol. 1, 3 vols. (1847; Leipzig: Barth, 1882), 3–11, on 6.

[86] Bevilacqua, "Helmholtz's Ueber die Erhaltung der Kraft" (ref. 5), 300–04.

[87] Franz E. Neumann, "Die mathematischen Gesetze der inducirten elektrischen Ströme," in *Franz Neumanns gesammelte Werke*, vol. 3 (1845; Leipzig: Teubner, 1912), 257–344, on 275–78. It should be noted that Neumann's potential is not equal to the modern potential energy; Ole Knudsen, "Electromagnetic Energy and the Early History of the Energy Principle," in *No Truth Except in the Details: Essays in Honor of Martin J. Klein*, ed. A. J. Kox and Daniel M. Siegel, (Dordrecht: Kluwer, 1995), 55–78.

[88] Hermann Helmholtz, "Über die Erhaltung der Kraft," *Wissenschaftliche Abhandlungen* 1 (Leipzig: Barth, 1882), 30.

DISCUSSION AND CONCLUSIONS

My survey revealed a chain that links implementations of energetic notions from Poisson and Fresnel to Helmholtz's realization that he could formulate a full and general conservation principle that would cover all previous uses and related notions. Poisson relied on the conservation of the living force in rational mechanics, clearly valid for sound, to challenge the wave theory of light. Fresnel showed that the undulatory theory could agree with this rule and derived its implications for that theory. Well acquainted with Fresnel's work, Ampère surely knew the latter's exchange with Poisson when he combined similar ideas regarding the conservation of the living force with a general belief in the impossibility of creating motion to show that electromagnetic rotation must originate in motion. Ampère's argument impressed Roget and was surely known to Faraday. Both employed similar reasoning to refute the contact theory of batteries, an argument adopted by Gmelin and Schönbein. Joule transformed Roget's and Faraday's argument by postulating the impossibility of destroying power, and use it to support his claim that heat is converted into mechanical power. Liebig extended Faraday's argument to the action of the electric pile by comparing it to the steam engine, and further employed it in the theory of animal heat, a discussion taken up by Helmholtz and Mayer. Still, the cases of Sadi Carnot and Marc Seguin seem unrelated to this chain. These independent employments indicate that one did not have to know prior uses of the denial of creating power in order to apply it. Yet the links between most implementers of the idea suggest that earlier examples encouraged scientists to apply the same kind of arguments to further topics, extending, thereby, the realm of uses and validity of the energetic-like conservation principle. Moreover, Carnot's uses of the impossibility of creating motive power encouraged further uses by Clapeyron, Colding, Helmholtz, and Thomson.

Most users of proto-energetic claims before the formulation of the principle took care to justify their employment. Unlike the later employment of energy conservation, and the use of well-established principles in general, the earlier uses of connected notion were tentative and not obvious. Even employing the equation of the living force in optics did not seem self-evident to Laplace and Young. Although they understood light in mechanical terms, they argued for the immediate consequences of the equation. This was not the case with Poisson and Fresnel shortly thereafter.

The advancement of proto-energetic reasoning usually relied on the general notion that endless power is outside the order of nature. This notion itself was based either on a metaphysical claim, i.e., the inability to produce power out of nothing, or on a seemingly empirical reasoning, i.e., claiming that such a creation would be "like no other force in nature," or on both.[89] The recurrent appearance of this general notion suggests that participants deemed it more important and appealing to their audience than formal reasoning or mechanical reduction. Well-defined formulation of a conservation principle and its proof were important for scientists occupied with questions of conservation in nature and its laws like Helmholtz, Mayer, and Colding,[90] but seems secondary for the protagonists of this history, who implemented the ideas for their needs. They were concerned first and foremost with addressing particular scientific problems, and employed proto-energetic arguments heuristically and opportunistically.

The successful implementation of an energetic argument required neither admission of its universal validity (i.e., in every kind of interaction) nor an exact and coherent formulation. Fresnel, Ampère, Neumann, and Carnot, for example, did not commit themselves to the view that a magnitude equivalent to the living force is conserved in all natural interactions. A general rejection of the creation of "power" without a well-defined quantitative concept of "power" sufficed for Ampère to reject static magnetism as the source of electrodynamic rotation (although he resorted also to more formal reasoning in explaining why static forces could not account for such an effect), for Roget and Faraday to reject the contact theory of the voltaic pile, for Liebig to reject the possibility of gaining cheap effect from the electric piles, and the production of heat from the nervous system, and for Seguin to reject the conservation of heat. Carnot succeeded to infer from the impossibility of creating power that the motive power producible in a heat engine is limited, a cornerstone of his theory.

Yet, the impossibility of creating power did not suffice to show that unpolarized light consists of transverse waves. For that, Fresnel had to rely on conservation of a well-defined magnitude, the living force of the vibrating ether molecules. The equation of the living force provided the required explicit relation between well-defined variables needed for deducing mathematical

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[89] Faraday, "Source of power" (ref. 63), 103. In an effort to bypass the problem of induction from the inevitably limited number of experiments performed, Carnot invoked the failure of all the attempts to gain a surplus of work from electricity; Carnot, *Réflexions sur la puissance motrice* (ref. 47), 21–22.

[90] Cf. Jurkowitz, "Helmholtz's Early Empiricism" (ref. 5); Bevilacqua, "Helmholtz's Ueber Erhaltung der Kraft" (ref. 5), 300–04; Elkana, *Discovery of the Conservation of Energy* (ref. 15); Caneva, *Mayer* (ref. 6); Caneva, "Colding" (ref. 5).

relations and properties, formally applicable to systems regarded as mechanical and free of friction, as Fresnel viewed light waves. It seems that the equation of the living force had not been used beyond such systems, its proven realm of applicability. That the equation did not serve as the basis of other inferences suggests that the development of the living force and its connection to work in machine theory, discussed by Grattan-Guinness and Darrigol, had limited influence on the early employment of the conservation notion beyond mechanics. Nevertheless, the French tradition of rational mechanics, which occupies only a minor role in the common histories of energy conservation, turned out to have played a major role in the implementation of proto-energetic notions in physics discussed here.[91]

The scientists discussed here extended the realm of applicability of conservation and proto-energetic arguments from non-frictional mechanics to wave optics, magnetism of electric current, electric battery, heat motors, and animal heat. Most of these questions were new. The connection to novel fields is probably not accidental since, in exploring novel fields, scientists often lacked formal rules for inference, and were thereby more open to adopting informal guidance and analogies from more established fields like mechanics. Thought in terms of principles, even if partial or vague, is especially useful at this stage due to their selective power in forming assumptions. The ability to use principles to select among different assumptions and possibilities attracted scientists to their use. Nineteenth-century students of crystal physics employed and widened the uses of symmetry consideration, which were later regarded as principles. Niels Bohr suggested the correspondence principle of old quantum mechanics as a tool to attain particular theoretical results about atomic properties, when one could not rigorously deduce them from the constructive laws of current physical theory.[92]

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[91] Among the contributors discussed here, the tradition was central in the thoughts of Poisson, Fresnel, Ampère, and Carnot. Two reasons might explain the differences in the roles of the tradition. First, important parts of the history discussed here occurred in the last decades of French pre-eminence in physics (the 1810s–20s), whereas common histories of energy conservation examine the following decades. Second, one may claim that members of this tradition tended toward a more "pragmatic" attitude, in which they were interested in reaching physical results rather than in formulating general principles of nature. Thus, they employed proto-energetic ideas when useful, but were less interested in their general formulation.

[92] Shaul Katzir, "The Emergence of the Principle of Symmetry in Physics," *Historical Studies in the Physical and Biological Sciences* 35 (2004): 35–66; Helge Kragh, *Niels Bohr and the Quantum Atom: The Bohr Model of Atomic Structure, 1913–1925* (Oxford: Oxford University Press, 2012), 196–203.

Novel phenomena and theories discovered and suggested during the late eighteenth and early nineteenth century were constructive for the development of energetic arguments. In this sense, the significant extension of quantitative empirical physics beyond mechanics was highly important for the emergence of energetic physics. This conclusion corroborates Kuhn's view that empirical findings were important for the process. Moreover, as Kuhn suggested, many of these findings involved interactions between different powers (electricity and magnetism, chemical affinity and electric current, chemical affinity and heat, heat and motion). Yet the reliance on energy considerations was not due to an increase in the number of known conversion processes, assumed by Kuhn and dismissed by Heimann,[93] but due to the elaborations of theoretical, often mathematical, accounts for a growing number of observed phenomena (e.g., the intensities of reflected rays) in the first half of the nineteenth century.

The story presented here shows the prehistory of energy physics as a process of widening the realm of its applications. In this process, implementation preceded the formulation of the conservation principle. Moreover, the partial formulations of the principle were articulated to address particular questions, rather than to answer a general abstract question like the existence of a conserved quantity in nature. These examples of uses and earlier formulations stimulated the general formulation, often viewed as a discovery, of the energy principle, and its further uses.[94] The history of the principle of symmetry in physics shows a similar pattern of elaboration through uses that preceded a general formulation. These principles were not isolated propositions about a property of the physical world, but part of a more general approach that included their employment as tools for inference.[95] The early employment of energetic-like conservation discussed here formed one of the tributaries that led to "energy physics," which not only proclaimed its conservation but also used it as a potent theoretical tool. In this perspective, the early and unpublished formulations of energy conservation by Carnot and Saint-Venant are seen as formulations (or if one prefers, discoveries), which, although they relied on resources used by some of the protagonists of this history, and later elaborators of energy

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[93] Kuhn, "Energy Conservation" (ref. 1); Heimann, "Conversion of forces" (ref. 15).

[94] As mentioned, Helmholtz and Thomson explicitly referred to the earlier uses of the conservation, Mayer was stimulated from the connected uses by Liebig, and Joule was inspired by Roget and Faraday.

[95] This statement is probably valid for all or at least the majority of the principles in the natural sciences, but the discussion of such a general claim is beyond the scope of this article.

conservation, did not contribute to the main flow of energy physics.[96] More generally, this history demonstrates that scientific laws and the rules of their application are sometimes developed in specific contexts in order to answer problems concerning particular phenomena. Formulations follow use.

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96. Darrigol, "God, Waterwheels, and Molecules" (ref. 8), 332–33; Carnot, *Reflexions on the Motive Power* (ref. 47), 181–206.