Helmholtz's *Ueber die Erhaltung der Kraft*:
The Emergence of a Theoretical Physicist*

1. Introduction

During the 1840s and early 1850s numerous formulations of the perennial attempt to show the "conversion" among natural natural phenomena and the "conservation" of something underlying them appeared. Despite a certain persistence of the term "force" ("*Kraft*") among a few German physicists, by the 1860s the term "energy" was generally adopted, although it did not assume an unequivocal meaning. Before the century closed, several histories of energy conservation were written which raised controversies about priority and about the scientific and philosophical meaning of various formulations of the principle of conservation of energy.[i]

Among modern scholarship the most influential interpretation by far of the appearance of energy conservation between 1830 and 1850 is Thomas S. Kuhn's classic essay, "Energy Conservation as an Example of Simultaneous Discovery," which in turn has stimulated a number of thoughtful alternative approaches to the problem.[ii] Kuhn's principal interest was not, however, to write another history of the emergence of the principle of conservation of energy; instead, he sought to identify "the sources of the phenomenon called simultaneous discovery." He argued that between 1832 and 1854 twelve scientists—above all, Julius Robert Mayer, James Prescott Joule, Ludwig Colding, and Hermann von Helmholtz—"grasped for themselves" the essential "elements" of the concept of energy and its conservation, and he asked why these "elements" became accessible at that time, thereby seeking to identify not the innumerable "prerequisites" of the principle of energy conservation but rather only what he called the "trigger factors." In particular, he identified three such factors: the "concern with engines," the "availability of the conversion processes," and the "philosophy of nature."[iii] For most modern scholars, not the least result of Kuhn's essay has been to see Helmholtz's essay of 1847, *Ueber die Erhaltung der Kraft.*

Eine physikalische Abhandlung, within Kuhn's analytical framework and according his terms.[iv]

Kuhn's general analysis of the “trigger factors” contains a number of problems, however. His first factor, the “concern with engines,” led him to focus on the concept of work as it related to several traditions, above all that of an older engineering practice.[v] Yet his assertion about the ineffectiveness of the well-known theoretical engineering tradition dating from Lazare Carnot is surprising and, as we shall see, unjustified.[vi] Moreover, Kuhn neglected the tradition of potential theory, which identified the concept of work with that of potential, and so opened the way for a mathematical expression of “energy” conservation.[vii] Thus, if the concept of work is taken as loosely as Kuhn did, it can be derived from figures ranging from Hero of Alexandria to Leibniz, rather than from the engineering tradition of the eighteenth century, which would then make it more a “prerequisite” than a “trigger factor.”[viii] Instead, a more pertinent influence was that derived from the technical concept of work linked closely to the emergence of potential theory and *vis viva* conservation.

Kuhn's second factor, the “availability of conversion processes,” led Peter Heimann to note that the emphasis on the interconversion of the forces of nature was not specific to the 1830s and 1840s, and thus to argue that it cannot be considered a “trigger factor.”[ix] Much the same point is essentially true for Kuhn's third “trigger,” that of “the philosophy of nature,” in particular *Naturphilosophie*. From the Greek atomists onward, numerous metaphysical thinkers posited the unity, uniformity, and homogeneity of natural phenomena, and so contributed to the rise of energy conservation.[x] *Naturphilosophie* has no privileged role in this hoary tradition. To be sure, Kuhn sought to show the relevance of *Naturphilosophie* for Helmholtz's work by referring to a controversial remark of 1882, wherein Helmholtz acknowledged the Kantian influences in his 1847 essay.[xi] Yet to note the Kantian roots of the *Naturphilosophen* hardly means that Kant was a *Naturphilosoph*.[xii] The methodological role of Kantian, as well as Leibnizian, elements in Helmholtz's 1847 essay should not be confused with ontological commitments typical of *Naturphilosophie*. In sum, Kuhn's three factors and his dismissal of various prerequisites—

above all, the dynamical theory of heat and the impossibility of perpetual motion[xiii]—do not account for the appearance of Helmholtz's *Erhaltung* or the other versions of “energy” conservation.

Moreover, with respect to Helmholtz himself, Kuhn makes a number of historical claims that, as this essay argues, are doubtful: that “Helmholtz fails to notice that body heat may be expended in mechanical work”; that the concept of work from the old mechanical engineering tradition was “all that which is required” and “the most decisive contribution to energy conservation made by the nineteenth-century concern with engines”; that “Helmholtz was not, however, aware of the French theoretical engineering tradition”; that Helmholtz “fails completely to identify $\int pdp\ 1$ as work or *Arbeitskraft* and instead calls it the `sum of the tensions'”; that “the dominance of contact theory in Germany” might “account for the rather surprising way in which both Mayer and Helmholtz neglect the battery in their accounts of energy transformations”; and that “Helmholtz was able by 1881 to recognize important Kantian residues” in the *Erhaltung* “that had escaped his earlier censorship” and that this is evidence of the influence of *Naturphilosophie*.[xiv]

The concerns raised about Kuhn's study suggest that a reassessment of Helmholtz's contributions to energy conservation is in order, and this essay seeks to do so by providing an *explication de text* of Helmholtz's *Ueber die Erhaltung der Kraft*. In so doing, it argues that Helmholtz's work is best seen as the first and primary event in his emergence as a theoretical physicist. The *Erhaltung*, as this essay shows, presented no experimental or mathematical achievements by Helmholtz. Rather, Helmholtz used it to outline an explicit and sophisticated theory and methodology. His theoretical effort resulted in a formulation of the principle of force conservation based on the impossibility of perpetual motion and on the Newtonian model of forces depending only on positions. This allowed him to define two sharply distinguished and main forms of “energy”: potential and kinetic. Moreover, his methodology distinguished clearly between theoretical and experimental physics and did so by presenting four hierarchical, interacting levels that articulated and demonstrated force conservation: the physical hypotheses of the impossibility of perpetual motion and of central Newtonian forces (level one); the principle of conservation of force (level two); and various specific empirical laws (level three) and natural phenomena (level four) pertinent to force conservation. With reference to these four levels, Helmholtz drew an explicit distinction between theoretical physics (dealing with deductions from level two to three, that is, with the applications of the principle to empirical laws) and experimental physics (dealing with the inductions from level four to three, that is, from natural phenomena to empirical laws). It is important to note, furthermore, that Helmholtz did not at all identify the use of mathematics as the dividing line between theoretical and experimental physics. Section I of the *Erhaltung* sought to show the equivalence of the two main hypotheses (level one), while section II sought to deduce from them Helmholtz's version of the principle of conservation (level two). The remaining four sections (III-VI) dealt with the interactions between levels two and three, that is, with the application of the principle to the existing empirical laws and with an attempt to derive new empirical laws on theoretical grounds. The *Erhaltung* offered no new experimental data (level four), and it criticized the few available data regarding the mechanical equivalent of heat, not least, as this essay seeks to show, on the basis of a technical mistake in converting units of measurement.

Moreover, the essay argues, *pace* Kuhn, for a series of specific points that have been previously misunderstood or neglected: that prior to 1847 Helmholtz was indeed aware of the interconvertibility of heat and work, although he did lack a numerical equivalent; that the concept of work only became an important contribution to force conservation after it acquired the characteristics of a total differential; that Helmholtz was well aware of the French engineering tradition and that he utilized (without rederiving) the new expression for *vis viva*; that he consciously dropped the new interpretation of "*Arbeit*" in favor of "*Spannkraft*"; that he showed that contact theory was not opposed to force conservation and that, indeed, he dedicated the longest section of the *Erhaltung* to an analysis of batteries; and, finally, that he did not censor but rather reinstated the *Erhaltung*'s philosophical introduction before publication and that Kantian transcendentalism, not *Naturphilosophie*, played the main philosophical role. This essay pays particular attention to the *Erhaltung*'s four-level methodological structure; to the demarcation between theoretical and experimental physics; to the overcoming of both the engineering and the mathematical approaches to the concept of work; to the lack of an experimental determination of a work equivalent of heat and the mistranslation of Joule's values; to the difficult (and at points simply wrong) theory-experiment interplay in applying the principle; and, finally, to the formulation of a lasting methodology and of a subsequently discarded conceptual model of energy.

Much of the immediate background to Helmholtz's essay of 1847 lay in his own experimental work in heat physiology between 1843 and 1846 and in his critical knowledge of the work of his predecessors in this area. Section 2 of this essay briefly treats this topic.[xv] Section 3 presents the *Erhaltung*'s methodological structure and its conceptual foundations. Sections 4 and 5 describe and analyze the *Erhaltung*'s concrete results, with section 4 elaborating the conceptual foundations into the principle of the conservation of force and section 5 applying the principle to a wide variety of physical topics.

2. On the Borderline between Physiology and Physics: The “Bericht” of 1846

Helmholtz had studied physiology with Johannes Müller in Berlin, where he also formed close and long-lasting friendships with some of Müller's best pupils, above all Emil duBois-Reymond, Ernst Brücke, and Carl Ludwig. In 1845, these last three, along with several of Gustav Magnus's students (namely, Gustav Karsten, Wilhelm Beetz, Wilhelm Heintz, and Hermann Knoblauch), founded the Berlin Physikalische Gesellschaft. Two years later, the Gesellschaft issued its first installment of the *Fortschritte der Physik*, which included the first of Helmholtz's many “Berichte,” or reports reviewing recent research. Physiology was the research context in which Helmholtz published his two papers on force conservation in 1847.[xvi] So, too, was it the context in which he published a series of five other papers between 1843 and 1848.[xvii] Although these first works were written in a physiological context,[xviii] Helmholtz declared in 1882 and again in 1891 that his interest in force conservation did *not* arise from empirical problems in physiology but rather from his teenage inclination to favor the principle of the impossibility of perpetual motion.[xix] Moreover, it is well to recall that his professional context was that of an army surgeon in Potsdam.[xx]

Physiology offered the battleground for the fight over explaining animal heat in terms of the principle of the impossibility of perpetual motion and the consequent refusal to admit vital forces. The acceptance or rejection of vital forces in explaining the origins of animal heat constituted one of the fundamental problems of mid-century German physiology.[xxi] One of the principals in this fight was Justus von Liebig, who in 1841 had asserted a principle of correlation of forces, that is, of the conversion of forces with constant coefficients—"No force can be generated from nothing...," he averred[xxii]—and who in 1842 had rejected the idea that vital forces could generate animal heat.[xxiii] Although Helmholtz eventually rejected Liebig's approach to the problem, he was initially much influenced by it.[xxiv] In his first two publications he explicitly followed Liebig by assuming a common origin for mechanical forces and the heat produced by an organism.[xxv] He asked whether this origin might not be entirely attributed to metabolism, thereby obviating vital forces. More to the point, his discussion in 1846 of the origins of animal heat reveals two elements of his methodological strategy in the *Erhaltung*: First, while he accepted Liebig's principle of force correlation and his theory of the chemical origin of heat, he also declared that the appropriate conceptual model of heat and the definition of the heat equivalents utilized in the correlation required clarification before a satisfactory application of the principle could be achieved. Second, he saw that Liebig's theoretical determinations did not in fact agree with experimental results.[xxvi]

Helmholtz argued that, assuming a conceptual model of heat as a substance (caloric), the conservation of matter assured that the amount of (latent) heat ingested equalled that emitted by living bodies, and that Hess's law, which claimed that in chemical transformations the order of the intermediate steps was irrelevant for the final emission of heat, gave further support. Although Helmholtz thought that the caloric model of heat was the more useful for refuting the idea of vital forces, he nonetheless used the idea of heat as motion since it was widely spreading throughout the sciences: the recent identification of thermal radiation with light and the generation of heat not ascribable to the liberation of latent heat (for instance, in electrical processes) forced him to accept the model of heat as motion. Yet this model had disadvantages for eliminating vital forces, for the total amount of heat was no longer, as in the caloric model, considered constant, and the production of heat by the action of forces (including, at least in principle, vitalistic ones) now became a possibility.[xxvii] To deny any role to vital forces and to reassert the principle of the impossibility of perpetual motion demanded a solution to the second problem mentioned above: the redefinition of heat equivalents.

Liebig had predicted an amount of animal heat smaller than that measured by Pierre-Louis Dulong and César-Mansuète Despretz. The difference between prediction and measurement might have been explained as due to vital forces. Helmholtz sought to eliminate the discrepancy between theoretical prediction and experimental result so as to eliminate any potential role for vital forces. He attempted to do so by reformulating the terms on both sides of the equation relating the heat ingested and emitted by living bodies: on the one side, the heat of the ingested matter results not from the oxidation of the elements of the food but rather from that of the compounds themselves; on the other, the heat generated in animals occurs not only in the respiratory organs but also in the blood and tissues. Helmholtz's reformulation of this equation gave him a theory-based prediction that satisfied experimental results on the basis of heat as motion while excluding vital forces. Still, the experimental confirmations remained highly problematic.[xxviii]

Moreover, Helmholtz discussed the mechanical equivalent of heat, even though a determination of that equivalent, and hence a component—the work done by animals—in the theoretical energy balance was lacking.[xxix] Already in his paper of 1845 on “Muskelaction” he had asserted that the problem was “whether or not the mechanical force and the heat produced in an organism could result entirely from its own metabolism,”[xxx] and in his paper of 1846 on “Wärme” he noted that one of the differences between the kinetic and the caloric theory of heat was “the determination of the equivalent of heat that can be produced through a given quantity of mechanical or electric force.”[xxxi] To be sure, he did not provide a mechanical equivalent of heat in these papers; yet that was due to his lack of reliable experimental data, not adequate conceptualization. Nor did he do so in the *Erhaltung*. There he explained that, given that the amount of work produced by the animals is small in comparison to the heat generated, work can be neglected and the problem of conservation of force in physiology reduced to the problem of whether the combustion and transport of food can produce the same amount of heat as produced by the animals. He added that the results of his own work in the “Wärme” and the “Bericht” of 1846 compared with the Dulong and Despretz's measurements, allowed a positive, if approximate answer.[xxxii]

Helmholtz's first “Bericht,” written in October 1846 and published in the *Fortschritte* in 1847, was a key step in his elaboration of a methodological strategy. The paper's outstanding feature was a methodological one: the extension of the correlation principle from physiology to various branches of physics and chemistry. Helmholtz explicitly asserted that heat cannot originate from nothing and he used Liebig's own words to state a principle of correlation of forces.[xxxiii] Yet he also criticized that principle's application to animal heat, since Liebig's solution, as already noted, did not correspond to Dulong and Despretz's calorimetric results: the direct measurement of the heat combustion of the hydrogen and carbon in the food ingested ranged between seventy and ninety percent of the heat generated by the animals.

The use of the correlation principle was not limited to the problem of animal heat. The principle was based on the impossibility of perpetual motion, which, as Helmholtz said, was “logically ... completely justified,” and had already been used in the “mathematical theories” of

Sadi Carnot and Emile Clapeyron (using caloric) and of Franz Neumann (using electrodynamic potential). At the same time, Helmholtz also noted that the principle had yet to receive either full expression or full experimental confirmation. He saw correlation as a more sophisticated expression of the principle of the impossibility of perpetual motion and he immediately used it to provide a series of energy balances based on the model of heat as motion. Using that model and the principle of the "constancy of force-equivalents," he argued that "mechanical, chemical, and electrical forces can always generate a determined equivalent of heat, however complicated the transition from one force to the other." He conceded that empirical evidence for this claim was greatly lacking, yet he thought it nonetheless useful to offer specific theoretical applications of the principle to the heat produced by mechanical, chemical, electrolytic, and electrostatic forces.[xxxiv] The case of animal heat now became only the last of five applications of a principle that was becoming increasingly general.

Yet a new difficulty soon appeared: the lack of a mechanical equivalent of heat meant that the most important balance, that between heat and work, could not be written; in fact, the values offered by the theories of Carnot and Clapeyron and of Karl Holtzmann were unacceptable since they were based on the caloric model and since they referred only to the propagation, not the production, of heat. Helmholtz continued to lack experimental data. In October 1846, he still did not know of either Mayer's or Joule's work: "there exist no experiments which can be taken into account for the mechanical forces," he reported.[xxxv] Hence, all the other balances were written as equivalences based on heat units rather than on work units. In the "Bericht," heat, not work, was the unit of measurement common to all the natural phenomena considered. This is a non-trivial difference with the subsequent *Erhaltung*.

In analyzing chemical transformations, Helmholtz employed Hess's law of the constancy of heat production. For electrolytic currents, the heat developed in the circuit was seen as equivalent to the electrochemical transformations in the galvanic chain (battery), independently of their order. The circuit's heat θ 2 could be calculated using Ohm's and Lenz's laws (Joule was not mentioned): $\theta = J^2 Wt$ 3, where **J** is the current intensity, **W** the total resistance, and **t** the time. On the other hand, Helmholtz knew from Faraday's electrolytic law that $\theta = AC$ 4, where **A** is the electrical “difference” of the metals involved and **C** the quantity of atoms “consumed,” that is, that underwent a process of oxidation and reduction. According to the principle of equivalence, the heat produced in the circuit must be equivalent to that which could be produced through the electrochemical transformations in the cells. For static electricity, Helmholtz easily showed that the production of heat by electric discharge followed from Riess's principles; he thus established a balance between the resultant heat on the one side and the product of the quantity of electricity and electrical density (an extant Voltaian term for what was later to be known as the tension or potential difference), on the other. Finally, in discussing animal heat Helmholtz identified the latent heat of chemical reactions with the thermal equivalent that could be produced in further reactions. The energy balance had to hold between the latent heat of the ingesta, on the one side, and the heat “provided by the animals” plus the latent heat of the egesta, on the other. He saw that the equivalent on the left side of the balance was no longer the “heat of combustion of carbon and hydrogen but instead that of the food.”[xxxvi] He reformulated and modified respiratory theory, and so claimed to have eliminated vital forces while satisfying Dulong's and Despretz's experimental results. Liebig's difficulties, he believed, were overcome.

With the “Bericht” of 1846 Helmholtz had acquired a new methodology and was aware of its great generality. The “Bericht” stands on the borderline between physiology and force conservation. In certain fundamental respects Helmholtz's methodology here adumbrated that of the *Erhaltung*: he enunciated a principle, provided a conceptual model of the quantities involved, expressed an equation between the energy terms, and, finally, compared the equation with empirical laws. There is, however, one central difference: despite the application of the equivalence principle to an analysis of several physico-chemical laws, the “Bericht” is still largely dedicated to physiology. In the much longer *Erhaltung*, by contrast, physiology is confined to a few lines of the final section. In the “Bericht,” moreover, the equivalence principle based on the impossibility of perpetual motion (and on the impossibility of destroying motion) is presented along with a model for many equivalents (the terms of the energy balance); yet the equivalence principle of the “Bericht” is quite different from the mechanical principle of the conservation of force expressed in the *Erhaltung*. Indeed, although Helmholtz did not know of Mayer's and Joule's work, his equivalence principle of 1846 is much closer to their ideas. For it only asserts the numerical equivalence of the effects involved and does not employ the assumption of central Newtonian forces or imply that every effect must have a mechanical interpretation in terms of potential and kinetic energy.[xxxvii] Finally, despite his acceptance of the mechanical theory, in the “Bericht” Helmholtz did not discuss the specific determinations of the mechanical equivalent of heat.

In 1847, while still writing the *Erhaltung*, Helmholtz wrote another paper (“Muskelaction”) dedicated to physiological problems.[xxxviii] There he tried to link the problem of animal heat to that of the mechanical force produced by muscle action. Seeking to demonstrate that heat is produced in the muscle itself, he devised a very sensitive thermocouple which, when linked to an astatic galvanometer and a magnifying coil, could detect differences of temperature in the range of 1000th of a degree centigrade. His thorough experiments on frogs' legs showed that heat is generated directly in the muscle tissue, that its origins are due to chemical processes, and that heat production in the nerves is negligible. He had disposed of vital force on empirical grounds. The role of this experimental research on the sources of animal heat, done simultaneously and immediately after composing of the *Erhaltung*, was particularly germane for Helmholtz's understanding of force conservation: it was, in fact, the only experimental research in this field that he conducted. His understanding and evaluation of the mechanical equivalent of heat is almost certainly connected to this very research.[xxxix] Indeed, as the next section argues, the entire *Erhaltung* only offered a theoretical reinterpretation of known results, but no new experiments. Physiology could not and did not provide a key to Helmholtz's conservation of force: it offered neither theoretical arguments nor experimental evidence for the establishment of the principle of force correlation. Instead, the rejection of vital force was based on the previously accepted notion of the impossibility of perpetual motion.

3. The *Erhaltung*: Methodological Structure and Conceptual Foundations

While experimenting on the heat produced during muscular action, Helmholtz also worked (October 1846 - July 1847) on the *Erhaltung*. He presented the *Erhaltung*'s results to the Physikalische Gesellschaft (apparently with great success) on 23 July 1847. However, Magnus's and, above all, Johann Christian Poggendorff's judgments were less than warm, with the latter refusing to publish it in his *Annalen der Physik* because of its non-experimental character.[xl] Helmholtz was forced to turn to a private publisher, Georg Reimer. The final product consisted of an Introduction (“Einleitung”), which is largely methodological and philosophical in character, and six individual sections, the first two of which—“Das Princip von der Erhaltung der lebendigen Kraft” (“The Principle of the Conservation of Living Force”) and “Das Princip von der Erhaltung der Kraft” (“The Principle of the Conservation of Force”)—are dedicated to formulating the principle, and the following four of which—mechanics, force equivalent of heat, force equivalent of electrostatics and galvanism, and force equivalent of magnetism and electromagnetism—are dedicated to the applications of the principle to their respective fields.

By February 1847, at least, Helmholtz had written a sketch of the *Erhaltung*'s Introduction, which he sent to duBois-Reymond.[xli] It caused him problems: before he presented the *Erhaltung* to the Physikalische Gesellschaft and before he sent it to Magnus, whom he hoped would help him see to its publication in Poggendorff's *Annalen*, he decided to drop the Introduction. Then, following Poggendorff's refusal to publish his essay, Helmholtz, at duBois-Reymond's request, restored the Introduction, though he altered it in “certain parts” before sending it to Reimer for publication.[xlii] The alterations are probably what is now the Introduction's opening paragraph.

The Introduction succinctly summarized the *Erhaltung*'s plan. It reveals that the

Erhaltung's structure is based on four methodological assumptions or considerations: the positing of two “physical assumptions” (the impossibility of perpetual motion and central Newtonian forces) and their equivalence (section I of the essay); the derivation from these assumptions of a theoretical law, viz., the principle of the conservation of force (section II); the comparison of this general principle with various empirical laws, and the connection of the principle and those laws to natural phenomena in various fields of physics (sections III-VI).[xliii] Unlike most other researchers involved with conservation problems, Helmholtz not only proposed to offer a specific functional formulation of the quantities conserved and their interrelations; he also proposed to derive this “principle” from more general physical assumptions.

Helmholtz's outstanding methodological innovation in the *Erhaltung* was to compare not only empirical laws with natural phenomena but also with a general principle. It is not difficult to understand Magnus's and Poggendorff's concerns about Helmholtz's essay: without presenting any new experimental results, the young physiologist sought to combine two major physical assumptions to a series of empirical laws and phenomena stretching across the entire spectrum of physics. In so doing, he provided one of the first conscious criteria for demarcating theoretical and empirical physics: while the experimental physicist searches for empirical generalizations that might fit experimental data (for example, the laws of light refraction and reflection), the theoretical physicist searches for agreement between a principle and extant empirical laws (the principle's justificatory role) and for the discovery of new theoretical laws (heuristic role).[xliv] Helmholtz here explicitly posited a long-term task for theoretical physics: empirical laws must agree with principles as well as with experimental data.

The Introduction clearly and consciously shows the methodological control that Helmholtz had achieved over his own research. His methodology had a four-level structure. Two basic physical hypotheses (the impossibility of perpetual motion and central Newtonian forces) constitute the first level; the principle of the conservation of force the second; empirical laws the third; and natural phenomena the fourth. That Helmholtz felt the need to justify his own version of the conservation principle on higher grounds (on which, see below) clearly indicates his awareness of the possibility of alternative formulations of the principle itself. He wanted not only to express a principle, but also to establish a framework and a set of rules by which the principle could be formulated and used. This distinguished his approach as a major step not only in his own emergence as a theoretical physicist but also in the emergence of theoretical physics as a subdiscipline; at the same time it showed that his version of the principle was not only the application of a (meta)physical assumption but also the application of a sophisticated methodology. The two physical hypotheses (first level) brought together, though not unproblematically, two different but well known traditions in physics (the Newtonian and analytic mechanics),[xlv] and thus offered secure grounding for the whole enterprise. Moreover, Helmholtz sought to justify the first level on more abstract grounds: he connected the principle of the impossibility of perpetual motion with the principle of sufficient reason, a transcendental condition for the intelligibility of nature, and gave a conceptual explanation of the model of central forces in the Kantian style. Finally, he hinted at an "empirical" principle of a cause-effect relationship embedded in the actual formulation of the conservation principle (second level). This principle in turn required comparison with existing empirical laws (third level), sought to predict new ones and (eventually) to make natural phenomena intelligible (fourth level).

Helmholtz explicitly distinguished between theoretical physics, which dealt with the applications of the principle to empirical laws, that is, with deductions from level two to level three, and experimental physics, which dealt with the inductions from natural phenomena to empirical laws, that is, from level four to level three. The dividing line between theoretical and experimental physics was not the use of mathematics; theoretical physics was no more to be identified with mathematical physics than with experimental physics. Helmholtz's chief and most successful novelty was his stress on the interplay of the second and third levels: after 1847, physical laws (level three) had increasingly to satisfy not only experiments and natural phenomena (level four) but also theoretical principles (level two) as well. Helmholtz saw these principles, whose basic characteristic was to unify the different branches of physical knowledge, as heuristic tools for discovering empirical laws. He thereby helped the new subdiscipline of theoretical physics to emerge.

As noted above, Helmholtz also sought to justify his innovative methodological approach philosophically. The Introduction aimed to show the meaning of the two initial assumptions for the “final” and “true” goal of the physical sciences.[xlvi] In particular, he believed that physical science should proceed by searching for the “unknown causes” of phenomena and by seeking to understand these phenomena in terms of the law of causality.[xlvii] While he explicitly identified the unknown causes as constant Newtonian forces, it is less certain what he meant by the law of causality (“Gesetze der Causalität”). It seems likely, however, that he was referring to the theoretical search of the “empirical” link between natural phenomena, in particular to the causal link that he will establish in section II between living and tension forces. Hence, the laws of causality may be here taken in the “regulative-empirical” sense, that is, as a theoretical relation between “empirical” terms.[xlviii] On the other hand, Helmholtz also introduced a different meaning of causality, a “transcendental” one, where causality is a precondition for the possibility of scientific knowledge. In this interpretation, the scientist must assume that nature is intelligible, that “every transformation in nature must have a sufficient cause,” as Helmholtz wrote.[xlix] A natural process is intelligible, he held, if it refers to final causes, which act according to a constant law, and thus, if the external conditions are the same (*ceteris paribus*), produce the same effect. To be sure, Helmholtz also asserted, probably with the debate over vital forces in mind, that perhaps some natural processes are not actually intelligible.[l] Some phenomena may belong to a realm of spontaneity and freedom, though this cannot be decided conclusively. Be that as it may, the scientist must assume nature's intelligibility as the departure point for his investigations. Here his second basic physical assumption—the impossibility of perpetual motion—came into play: the impossibility of perpetually providing work without a corresponding compensation limits nature's spontaneity and freedom and offers a physical version of the principle of sufficient cause.

As many scholars have noted, the Introduction has a Kantian character.[li] However, the specifics are important, for different parts of Kant's work played different roles here. Both the regulative principle of empirical causality and the transcendental principle of causality that allows the possibility of scientific knowledge and the lawlikeness of nature have already been noted. In addition, Helmholtz was also preoccupied in the Introduction with the conceptual explanation of a specific physical model, one which tended to show the *possibility* of Newtonian forces and not, at this stage, their inductive validity.[lii] The model in question is that of Newtonian central forces dependent on distance alone. Helmholtz presented a detailed conceptual explanation based on the mechanical categories of matter and force in order to show that Newtonian forces can be considered as the ultimate causes of natural phenomena. In so doing, he followed Kant's method as given in the *Metaphysische Anfangsgründe der Naturwissenschaft*. Helmholtz's well-known enunciation of the mechanical worldview is based on the assumption that both matter and force are abstractions and that the first cannot be considered more “real” than the second. He asserted that the problem of finding unchanging, fundamental causes can be interpreted as the problem of finding constant forces; causes and forces were identified with one another. One characteristic of the definition of force is that it is constant over time; bodies with constant forces acting on one another allow only spatial movements and, if the forces of extended bodies are decomposed into forces acting between material points, then the intensity of the forces depends only on the distances. Helmholtz saw this as a direct consequence of the principle of sufficient reason.[liii] Thus, if a general application of the principle of force conservation allowed the reduction of all natural phenomena to the effects of attractive and repulsive forces whose intensities depended only on distance, then “empirical” causality would match “transcendental” causality and the goal of physical science would be reached: an “intelligible” nature would be “understood.”[liv] The utopian nature of this conceptual model of forces notwithstanding, it was by no means universally accepted: for example, Wilhelm Weber's electrodynamic law of 1846, based on the alternative assumption of forces dependent on distance, velocity, and acceleration was then gaining widespread recognition. Helmholtz recognized that his (that is, the Newtonian) model was not fully accepted. Moreover, later in his career he began to doubt or at least re-define certain Kantian categories, including those of causality, that he had announced in the Introduction.[lv] By the early 1880s, he abandoned the interpretation of Newtonian forces as final causes, though he still adhered to the regulative and transcendental use of causality.

4. The Two Conceptual Foundations of the *vis viva* Principle and Their Supposed Equivalence

In section I of the *Erhaltung* Helmholtz sought to demonstrate the equivalence of his two basic assumptions—the impossibility of perpetual motion and central Newtonian forces—by analyzing the *vis viva* principle. He began with a statement of the principle of the impossibility of perpetual motion, namely: "that it is impossible, through any combination of natural bodies, to continually produce a motive force from nothing."[lvi] He stated that Carnot and Clapeyron had deduced a number of laws from this principle and that his own aim was to introduce the principle into every branch of physics "in the same way" so as to show both its applicability to all instances where laws based on phenomena have already been established (its justificatory role) and its guiding (heuristic) role for future experimental work. The assertion "in the same way" referred to methodology; it did not indicate that the same expression of the principle (as in Carnot and Claypeyron, with their caloric model) was to be applied. Instead, Helmholtz reformulated the principle by utilizing the term "work" ("*Arbeit*") along with the mechanical terms of "force" and "velocity":

> the quantity of work obtained when a system of bodies moves from one position to another under the action of specific forces must be the same as that needed to return the system to the original position, independent of the way, the trajectory, or the velocity of the change.[lvii]

Hence the term "*Arbeit*" became a function of the system's state (position); it is a total differential: in a closed path work cannot be created or destroyed.

Helmholtz then equated this innovative concept of work as a function of position to another function of position: the *vis viva*. From Galileo's relation $v = \sqrt{2gh}$ 5, where **v** is the final velocity acquired by a body of mass **m** during a fall from height **h** under the acceleration **g**, it follows that the work **mgh** equals the expression $\frac{1}{2}mv^2$ 6, which also is a function of position. Here Helmholtz again used the term "*Arbeit*" for work and, explicitly following the French engineering definition of "*travail*," in the work-*vis viva* equation gave priority to the concept of work. Indeed, he defined $\frac{1}{2}mv^2$ 7, not mv^2 8, as the measure of *vis viva*. In this way, he wrote, it "becomes identical with the quantity of work."[lviii] He did this, he added, in order "to establish a better agreement with today's customary way of measuring the intensity of forces."[lix]

Having equated work and *vis viva*, Helmholtz obtained the "mathematical expression" of the principle of the impossibility of perpetual motion, that is, the law of the conservation of *vis viva*:

> When any arbitrary number of movable point masses moves solely under the influence of forces which they exercise on one another or which are directed vis-à-vis fixed centers, then the sum of the living forces of all the point masses together is the same at every instant of time at which all the points are in the same relative positions with respect to one another and towards the fixed centers at hand, whatever their trajectories and their velocities during the time interval may have been.[lx]

The specific meaning of "conservation" here is that the quantity conserved (*vis viva*) occurs at specific positions and not during the process, a definition that echoes Huygens's results for the compound pendulum and Lagrange's definition of *vis viva* conservation.

Helmholtz further sought to show that the principle held only if the forces can be decomposed into central forces of mass points. From

9

$$d(q^2) = \frac{d(q^2)}{dx}dx + \frac{d(q^2)}{dy}dy + \frac{d(q^2)}{dz}dz,$$

where **q** is the velocity of a mass point **m** moving under the forces exerted by a fixed system **A**, and where **x**, **y**, and **z** are the Cartesian coordinates, and from

10 $$d(q^2) = \frac{2X}{m}dx + \frac{2Y}{m}dy + \frac{2Z}{m}dz,$$

where **X**, **Y**, and **Z** are the components of the acting forces and $dq = \frac{X}{m}dt$ 11, Helmholtz, by equating the corresponding components of the second members, incorrectly derived

12 $$\frac{d(q^2)}{dx} = \frac{2X}{m}, \frac{d(q^2)}{dy} = \frac{2Y}{m}, \frac{d(q^2)}{dz} = \frac{2Z}{m},$$

and from this that the force's magnitude and direction must be a function of the position of **m** and thus of its distance from an attracting point **a**.[lxi]

Three remarks concerning Helmholtz's synthesis in this section of many elements deriving from different traditions and his introduction of several novelties (sometimes only implicitly and sometimes unsuccessfully) merit attention. First, concerning the formulation of the impossibility of perpetual motion, Helmholtz introduced specific mechanical concepts (work, velocity, and force) that did not belong to the Carnot-Clapeyron expression. He attempted to frame the impossibility of perpetual motion in a mechanical worldview:[lxii] this was an (implicit) step in his methodological strategy that tended to show that his two initial assumptions belonged to the same conceptual scheme.

Second, Helmholtz reinterpreted the term work ("*Arbeit*") as a total differential in the (new) expression for the impossibility of perpetual motion. He here united two different traditions in mechanics (analytical mechanics and mechanical engineering) along with the old philosophical principle of "nothing comes out of nothing and nothing is destroyed," which he had already partially used in the "Bericht" of 1846.[lxiii] In the French tradition of mechanical engineering, which as we know was well known to Helmholtz, the term "*travail*" was of cardinal importance: the principle of conservation of *vis viva* became the principle of transmission of work.[lxiv] Yet while French engineers accepted the impossibility of creating work, they did not exclude the possibility that it could be lost.[lxv] They were mostly concerned with impact, and so for them work was not a total differential and the concept of potential, which was then only being developed in analytical mechanics, was not generally admitted.[lxvi] On the other hand, in the analytical tradition the quantity that eventually came to be called potential was not meant to be work stored in the system at a certain position. Notwithstanding its formal equivalence, "potential" was understood only as a mathematical function of the positions from which the forces could be derived. In this tradition force by displacement in the direction of force was a total differential but it did not receive a physical interpretation. Helmholtz subtly and skillfully united the two approaches, the concept of work with the function of positions (though not without problems in defining potential).[lxvii] Work could thus no longer be seen as something created or destroyed; instead, it was a state function (of the positions).

Third and finally, Helmholtz's illicit "demonstration" of the equivalence of the two initial assumptions played a vital role in his research program. The generalization of the principle of conservation of *vis viva* into his principle of the conservation of "force," that is, into a principle where the kinetic and positional terms are sharply split, was only possible if the forces depended on distances alone. While this holds for Newton's, Coulomb's, and Ampère's forces, it did not hold for Weber's electrodynamic forces. This was a non-trivial problem for Helmholtz. In 1847, it was impossible to oppose Weber's law on empirical grounds; hence, it was important for Helmholtz to demonstrate that empirical laws admitting forces other than central ones violated the conservation of *vis viva* and the impossibility of perpetual motion. Yet he could not do so, for his demonstration was based on a false assumption: even if the components of the *vis viva* depend on the positions alone, the same does not necessarily follow for the force components. In fact, it was possible to show that forces (like Weber's) depending on velocities and accelerations do not violate the conservation of *vis viva* or the impossibility of perpetual motion. In 1848, Weber showed that his own force admitted a potential, even if a kinetic one. Nonetheless, during the next two decades Helmholtz's approach was astonishingly successful. In the British literature, the point that Weber's force law denied conservation of *vis viva* and energy conservation was maintained by James Clerk Maxwell in 1865, Peter Guthrie Tait and William Thomson in 1867, and Tait in 1868, until, following Helmholtz's own retreat in 1870, it was finally refuted by Maxwell in 1873. By 1882, Helmholtz himself acknowledged that Rudolf Lipschitz had found a flaw in the 1847 demonstration (he still did not mention Rudolf Clausius's criticisms), and he agreed that he had been unable to demonstrate that central Newtonian forces had a privileged status.[lxviii] Based as it was on insecure, not to say faulty, deductive grounds, the validity of Helmholtz's approach had to rely on the inductive side, or its "empirical" success, that is, on its ability to reassess extant results (justificatory power) and disclose new phenomena (heuristic power).

In section II of the *Erhaltung*, Helmholtz presented his grand generalization: the principle of conservation of *vis viva* became the principle of the conservation of force. For a point mass **m**, moving with velocity **q** along the path **r** under the action of a central force ϕ 13, Helmholtz wrote the principle of the conservation of *vis viva* as:[lxix]

14 $$\frac{1}{2}md(q^2) = -\phi dr,$$

or, for **Q** and **q** as the velocities at distances **R** and **r**:

$$\frac{1}{2}mQ^2-\frac{1}{2}mq^2= -\int_r^R \phi dr. 15 \qquad \mathbf{(1)}$$

This expression is formally identical to the well-known theorem of *vis viva*-work. The left-hand side of equation (1) represents the variation of the *vis viva* while the right-hand side has the dimension of work (force by elementary displacement in the direction of the force integrated along a line). In section I Helmholtz had used the word "*Arbeit*" several times, and so it might be expected that he would mention it again here. Yet he did not. Instead, he boldly reinterpreted equation (1) by redefining the right-hand side not as "*Arbeit*" but rather as "the sum of the tension forces [*Spannkräfte*] between the distances **R** and **r**." The tension force, he explained, was meant explicitly as the conceptual counterpart to the living force ("in contrast to that which mechanics calls living force"), a force that attempts to move the point **m** until motion actually occurs. He interpreted the concept geometrically as "the set of all the force intensities acting in the distances between **R** and **r**." If the intensities of ϕ 16 correspond to the ordinates perpendicular to the line of the abscissae connecting the point **m** and the center of force **a**, then the integral represents an area given by the "sum of the infinite abscissae [read: ordinates] lying on it."[lxx]

His partly unsuccessful effort—the integral is not the sum of the abscissae but rather of the infinitesimal surfaces—to provide a geometrical interpretation of the *Spannkräfte* stressed that the tension forces ϕdr 17 are also quite different from the Newtonian forces ϕ 18: they are represented dimensionally by the product of a force by a displacement; exist only when the material point is not in motion; attempt to put it in motion; are "consumed" by the acquired motion (here they should be compared to the constant relation force-matter described in the Introduction); and, finally, while they are a function of distance (two positions), acquire a proper meaning only when summed over a definite interval.

Helmholtz had introduced new theoretical concepts into an old equation. Both the left- and right-hand sides of equation (1) now had a physical theoretical meaning and were connected by an equality holding during a process: a variation of one side equalled a variation in the other. He interpreted the sum of the two sides physically as the conservation of force:

> In all cases in which free material points move under the influence of attractive and repulsive forces whose intensity depends only on the distance, the loss in the amount of the tension force is always equal to the gain in the living force, and the gain in the first is always equal to the loss in the second. *Therefore the sum of the extant tension and living forces is always constant*. We can define our law in this most general form as *the principle of the conservation of force*.[lxxi]

Helmholtz's meaning of conservation is much different from that of Huygens's. For Huygens, conservation of *vis viva* meant that a system's *vis viva* reacquired the same value when the system reacquired the same positions independently of the trajectories that it followed back to those positions (of course, velocity, and thus *vis viva*, changes during motion). Conservation for Helmholtz meant that force (*Kraft*) is conserved during motion and a variation of *vis viva* corresponded to an opposite variation of tension force.

In addition, Helmholtz also deduced the principle of virtual velocities from the conservation of force: an increase of *vis viva* results only from the consumption of a quantity of tension force. Hence, if there is no consumption of tension forces for every possible direction of motion in the first instant, then a system at rest remains at rest.[lxxii]

Helmholtz had thus achieved three results: the principle of conservation of force implies that the maximum quantity of work obtainable from a system is a determined, finite quantity if the acting forces do not depend on time and velocity; if they do so depend, or if the forces act in directions other than that joining the active material points, the “force” can be gained or lost ad infinitum; and under non-central forces, a system of bodies at rest could be set in motion by the effect of its own internal forces.[lxxiii] The hypothesis of central forces depending only on distances was thus basic to Helmholtz's view. Although these three results, as already noted, are not without their problems, Helmholtz's own summation of them did not do himself justice. For he had imparted a real shift in meaning to the well-known old equation (1). In the tradition of analytical mechanics, the stress had been on the conservation of *vis viva*; in the tradition of mechanical engineering on the transmission of work. Helmholtz, by contrast, stressed the equivalence of the two. It was the introduction of the term *Spannkraft* which brought the real shift in meaning: with tension forces we are very far from the concept of work and very close to that of potential energy. Work, which now meant not work done but rather the capacity to do work, now acquired the role of a unit of measurement for a new theoretical concept. As Helmholtz's student Max Planck said: “[h]owever insignificant this interpretation might at first glance seem to be, the perspective that it opens on all fields of physics is nevertheless extraordinarily wide because now the generalization to every natural phenomenon is evident."[lxxiv] Helmholtz's formulation of the principle of conservation of "force," Planck further explained, became similar to that of conservation of matter: "force" as matter cannot be increased or diminished, it can only manifest itself in different forms. The two basic forms of "force," *vis viva* and tension force, can appear in many ways: for example, *vis viva* as motion, light, or heat; tension force as elevation of a weight, elastic or electric potential, or chemical difference.[lxxv] It is thus most surprising that modern commentators like Kuhn have considered Helmholtz's grand innovation as a failure and have dismissed it.[lxxvi]

Helmholtz's approach to conservation issued from a Leibnizian tradition[lxxvii] and, as a few highlights may suggest, unmistakably shows the difference between a theoretical and mathematical approach to physics. First, the duality *lebendige Kraft-Spannkraft* strongly resembles the older *vis viva-vis mortua* duality, as the very terms themselves suggest. While the *vis viva* has almost the same meaning, the *Spannkraft* is quite close to its older Leibnizian counterpart. Yet there are two main differences between Leibniz's and Helmholtz's ideas on positional "energy" which explain the latter's success: Helmholtz provided a formal quantitative expression and he included the Newtonian forces. By properly using the Newtonian concept of force and by fully accepting the inheritance of Newtonian mechanics, Helmholtz provided a formal expression for the second term of the duality that was absent in Leibniz.

A second Leibnizian element, moreover, is that the equality of the two sides of equation (1) no longer meant an analytical identity. Being two independent physical concepts, the equality now implied a causal relationship: the variation in one implied the variation in the other. The equality holds at every instant during a process. This is a Leibnizian concept of conservation. Helmholtz now brought "empirical" causality into play here: not causality as a condition of the possibility of natural laws, but rather causality as a principle which establishes a specific link between different realms of phenomena. The "empirical" causality indicates a quantitative equivalence between phenomena that are qualitatively different (static and dynamic). Like

Leibniz, what is conserved in Helmholtz during the process is the specific equivalence between qualitatively different phenomena. A static cause can generate, as an effect, the motion of a body. This motion, which in turn becomes a cause, has the power ("motive force") to produce the effect of returning the body to its initial position. The quantitative equivalence of cause and effect is maintained at every instant of the process: the interchangeability of the initial and final stage is only an exemplification of this principle.

Yet how are two qualitatively different phenomena to be measured so as to establish a quantitative equivalence? A common unit of measurement was obviously needed, and Leibniz had suggested work (to use the eventual designation) as the unit of measurement of all natural phenomena.[lxxviii] He recognized the impossibility of continually creating and destroying work (that is, "*ex nihilo*" and "*ad nihilum*") without a corresponding compensation as a necessary condition for guaranteeing the invariability of the chosen unit. Similarly for Helmholtz, work became the common unit of quantitative measurement for different phenomena connected by a causal principle but now mechanically interpreted according to Newton's definition of force and laws of motion. The "*ex nihilo*" and "*ad nihilum*" were now both present: the quantitative aspects of one side of the equation must be the same as those of the other. Work can be neither created nor destroyed. Helmholtz argued that all natural phenomena must be measured by a common unit and interpreted by using only two forms of force (*Kraft*). Here was a grand theoretical unification resulting from a shift in meaning.

A still deeper understanding of Helmholtz's result may emerge by briefly comparing it to Clausius's less philosophically but more mathematically inclined approach. Helmholtz had introduced the concept of *Spannkraft* (potential energy) without discussing that of potential. The peculiarity of his approach lay in his jumping theoretically from the *vis viva* theorem to the conservation of force principle without following the now standard formula of **f x *ds* = work**, where work is a total differential or difference of potential. In 1852, Clausius, by contrast, established a different relation between *vis viva* and potential. He started with the *vis viva* theorem and equated the increase in *vis viva* to the quantity of mechanical work produced in the system during the same time.[lxxix] He rejected Helmholtz's “sum of tension forces” (potential energy) and the corresponding interpretation of the conservation principle. For Clausius, work was in most cases a total differential, and thus its integral depended only on the initial and final positions and so was identical with a difference of potential. He explicitly asserted that the potential is work stored in the system. For Clausius, work as total differential and difference of potential were identical concepts. This important statement was a rather different one from Helmholtz's. In the Gauss-Clausius tradition, energy would never acquire the same importance as in Helmholtz's works. The principle of conservation was often to be called, as in the old tradition, the *vis viva* conservation and the only really important requirement was that work be a total differential. This interpretation left open the possibility that forces other than central Newtonian ones could satisfy the conservation principle, if the work done by these forces was, mathematically speaking, a total differential. But in this case the energy terms could not be easily divided into kinetic and positional parts. For central Newtonian forces, as introduced by Helmholtz for example, work was indeed a total differential, and thus his and Clausius's approaches intersected considerably. In section II of the *Erhaltung*, Helmholtz introduced in a straightforward way the sum of tension forces (potential energy) and the energy (*Kraft*) concept —variation of *vis viva* equals variation of the sum of tension forces, the sum of *vis viva* and tension forces is a constant—as conceptual and physical entities. However, he introduced the physical concept of potential only in section V, and he did so only with a shaky grasp of that concept.[lxxx]

Helmholtz's result may be further appreciated by briefly comparing it to Mayer's. Despite his acquaintance with Liebig's papers, Helmholtz was, as already noted, surprisingly unaware of Mayer's contribution of 1842 to the *Annalen der Chemie und Pharmacie*. He did not quote Mayer in the *Erhaltung*, and, indeed, he later claimed that in 1847 he had no knowledge of Mayer's work. Although both men used a Leibnizian principle of causality, Helmholtz alone adopted the mechanical conception of nature, the mechanical theory of heat, the central-force hypothesis, and the reduction of all qualitatively different forms of “force” to two basic ones. Mayer, by contrast, rejected all these elements and his expression of conservation of energy was closer to a principle of equivalence, that is, to a correlation principle. On the other hand, Mayer worked out a mechanical equivalent of heat, although, to be sure, he did so through original thinking rather than through original experimentation; Helmholtz, by contrast, Helmholtz, as section 5 shows, did not work out such an equivalent.[lxxxi]

By restricting itself to Newtonian forces, section II of Helmholtz's *Ueber die Erhaltung der Kraft* sharply distinguished between kinetic and positional terms and fulfilled the far-reaching plan announced in the first lines of the Introduction: the conservation principle just formulated is the “consequence” (level two) of the two basic physical assumptions (level one: impossibility of perpetual motion and central forces). By the end of section II, Helmholtz had already made impressive achievements: within a few pages he had completely reappraised older traditions in physics by providing an original synthesis of different and previously competing approaches (Newtonian and analytical mechanics, Leibnizian philosophy, mechanical engineering). Nonetheless, he had still only provided a general theoretical framework, one that remained to be filled with specific expressions for the *vis viva* and tension forces that resulted from the interaction of the principle (level two) with the experimental laws (level three) in the various realms of natural phenomena (level four). He demonstrated this interaction between levels two, three, and four in the *Erhaltung*'s remaining four sections through application to an extremely wide variety of empirical laws and natural phenomena. In so doing, the “empty” framework shows the strengths and weaknesses of its justificatory and heuristic power.

5. Applications: Mechanics and the Force-Equivalents of Heat, Electrical Processes, Magnetism, and Electromagnetism

Helmholtz first applied his principle to mechanical theorems (section III), mostly using known applications of *vis viva* conservation, which is to say that in this short, non-mathematical section he did not deal with specific applications of the concept of tension forces. Instead, he briefly considered the motions (of both celestial and terrestrial bodies) caused by gravitation; the transmission of motion through incompressible solids and fluids when *vis viva* is not lost through friction or inelastic collisions; and the motions of perfectly elastic solid and fluid bodies without internal friction.

He explicitly noted Fresnel's use of the principle of *vis viva* conservation to derive the laws of light reflection, refraction, and polarization, as well as the application of the principle to interference, thus displaying a broad and deep knowledge of physical problems. His application of the principle of "*Kraft*" suggested that if there is a loss of *vis viva* due to the absorption of elastic, acoustic, or heat waves, then a different kind of quantitatively equivalent "force" must appear.[lxxxii] He maintained that heat must be produced by the absorption of heat rays, but asserted that it had not yet been proven experimentally that the amount of heat which disappears from the radiating body reappears in the irradiated one. (Here was a first instance of his predilection in the final four sections of the *Erhaltung* to suggest and outline applications of the principle independently of any experimental confirmations.) While asserting that light absorption can produce heat, light (phosphorescence), and chemical effects, Helmholtz identified light with radiations producing thermal and chemical effects. He remarked, too, on the (small) effects of light and chemical rays on the eye, an indication perhaps of a small value for their heat equivalent.[lxxxiii] The quantitative relations of the chemical effects produced by light were not well known, and Helmholtz believed that relevant magnitudes were only involved in the case of light absorbed by the green parts of plants.[lxxxiv]

Even in this short section, one which mostly recalled known results, Helmholtz's method started to reveal its fertility by organizing a very large amount of physical knowledge. Yet its limits also became evident: for example, the difficulty of identifying the "tension forces" here reduced the conservation of "force" to a correlation principle. Moreover, it is evident that the principle can only be heuristically fruitful when its predictions are supported by extant empirical laws in the pertinent fields. To confirm the principle required knowledge of specific coefficients of equivalence, knowledge which Helmholtz lacked. He thus had to confine his efforts to broad theoretical applications based on his surprisingly (for a medical doctor) deep knowledge of the physical literature.

In section IV Helmholtz turned to the problem of the force-equivalent of heat. Although it might easily be supposed that this section is the *Erhaltung*'s centerpiece, it is not; rather, it is here that Helmholtz's different approach from that of Mayer (unknown to Helmholtz) and Joule become most evident. In contrast to the German physician and the English brewer, Helmholtz did *not* accurately establish the mechanical equivalent; indeed, he does not even seem to have been concerned to do so, and the lack of such a determination supports his subsequent claim that the *Erhaltung* sought more to review and synthesize contemporary physical knowledge than to produce original experimental results.[lxxxv] Instead, Helmholtz's principal interest in this section lay in a theoretical interpretation of thermal phenomena through his own framework. His approach was rather qualitative, using mathematical formalism only to discuss Clapeyron's and Holtzmann's laws.

He began by looking for actual compensations (equivalents) to an apparent loss of force. He used his principle of the conservation of force to identify the compensation for the loss of living force in inelastic collision and in friction with a supposed increase of tension forces (namely, internal elastic forces) due to the variation of “the molecular constitution of the bodies” and with acoustical, thermal, and electrical effects.[lxxxvi] He first treated the case of the collision of inelastic bodies, where the loss in *vis viva* reappeared as an increase in the tension forces, as heat and sound. He then treated friction, where there is an increase of the tension forces, heat and electricity. Neglecting molecular effects and electricity, he posed two important questions: does a loss of *vis viva* correspond to an equivalent amount of heat? and how can heat be given a mechanical interpretation?[lxxxvii] The first question was connected to a “correlation” approach; the second was specific to Helmholtz's program.

Helmholtz quickly disposed of the first question, though without obtaining any definite results. Unaware of Mayer's and Colding's research, he briskly asserted that "perhaps" too few efforts had been dedicated to this issue. He cited only a paper by Joule, recalling his attempts at establishing a mechanical equivalent through the heat produced by the friction of water in narrow tubes and in vessels (the famous paddle-wheel experiment).[lxxxviii] He reported that Joule's result in the case of narrow tubes was that the heat needed to raise 1 kg of water by 1 °C raises 452 kg to 1 m, and, in the case of vessels, 521 kg. His judgment on Joule's work, the only original experimental determination of the mechanical equivalent of heat cited in the *Erhaltung*, was severe. He thought Joule's measurements inadequate to the "difficulties of the research" and thus false: "probably the figures are too high," he wrote. His criticism of the only empirical evidence corroborating his own theoretical approach is surprising. As a good experimentalist and as one heavily involved in experimental physiological research on an intimately related subject, his judgment of inaccuracy about Joule's highly accurate results demands discussion.

In his paper of 1845, Joule had given his results from the paddle-wheel experiments (890 ft-lbs) along with those from previous work: 823 ft-lbs in 1843 from magnetoelectrical experiments, 795 ft-lbs in 1845 from the rarefaction of the air, and 774 ft-lbs from unpublished experiments on the friction of water moving in narrow tubes.[lxxxix] Joule averaged the two experiments resulting from the friction of water (890 and 774) to 832 as well the results of all three distinct types of experiments (823, 795, and 832) to 817. Given that the final accepted equivalent value was 778, Joule's averaged results make Helmholtz's criticisms seem excessive, not least since in 1847 Joule's results alone favored Helmholtz's approach.

The explanation for Helmholtz's harsh judgment is threefold. First, the *Erhaltung* was intended as a work in *theoretical* physics. Its origin was largely independent of experimental results. Helmholtz intended it to reinterpret extant knowledge, and so its value was not meant to rest on any doubtful experimental results. As an amateur scientist in 1847, Joule had yet to gain full scientific recognition, and Helmholtz may not have wanted to rely on such a weak ally. Second, although Helmholtz quoted Joule four times in different passages of section IV, he may have only become aware of Joule's papers during the *Erhaltung*'s final preparation.[xc] (Helmholtz did not, for example, quote Joule in the “Bericht,” written in October 1846.) Thus, he may not have mastered Joule's results. Third, and perhaps most importantly, Helmholtz systematically erred in converting British and continental units of measurement (from degrees Fahrenheit, feet, and pounds to degrees Centigrade, kilograms, and meters). In his search for a mechanical equivalent of heat, Joule equated the quantity of heat needed to increase 1 lb of water by 1 °F (latter to be called BTU) with the corresponding work expressed in feet by force-pounds. From his experiments with the friction of water he obtained a mechanical equivalent of 890 ft-lb/BTU for vessels and 774 ft-lb/BTU for narrow tubes.

Considering the conversion factors between British and continental units (1 BTU = 0.252 kcal; 1 ft-lb = 0.1382 kgm) to express Joule's results in kgm/kcal, we must multiply them by a factor of 0.5484 (= 0.1382/0.252). Joule's values of 890 and 774 thus correspond to 488 and 424, respectively. The second figure is very close to 778, the definitely accepted value of the mechanical equivalent of heat (in continental units: 778 x 0.5484 = 427 kgm/kcal).

As noted already, Helmholtz's conversion results are different: instead of 488 and 424, he found 521 and 452 for vessels and narrow tubes, respectively. He maintained that the (converted) values of Joule's experiments were too high. Yet the fault lay not in the accuracy of Joule's experiments but rather in Helmholtz's own faulty conversion. The source of his systematic error seems clear enough: the two sets of results above give 521/488.3 = 1.067; and 452/424.6 = 1.065. Now, Helmholtz must have used the *French* foot, a well-known unit of measurement which equals 12.8 inches or 0.3251 meters.[xci] And in fact, 12.8/12 = 1.067. Helmholtz's mistake became relevant both for his general evaluation of Joule and for the specific comparison of Joule's results with those of Holtzmann (given in metric units).[xcii] (See below, p. 41.)

Having misunderstood Joule's experimental results and quickly dismissing the entire unresolved problem of the mechanical equivalent of heat, Helmholtz turned to the second question, a theoretical one which he viewed as far more important: the extent to which heat corresponds to a force equivalent.[xciii] (Here force equivalents, which are theoretically identifiable energy terms, should not be confused with mechanical equivalents, which are numerical conversion factors.) Helmholtz discussed the caloric theory with explicit reference to the interpretation of Carnot and Clapeyron, for whom the force equivalent was the work produced in the passage of a certain amount of caloric from a higher to a lower temperature. Moreover, he criticized William Henry's and Claude Louis Berthollet's interpretation of the heat produced by friction as a displacement of caloric, and asserted that results from the field of electricity showed that the total amount of a body's heat can actually be increased.[xciv] He cited experimental evidence, based entirely on electrical research, against the caloric theory and in favor of the mechanical. While frictional and voltaic electricity did not give indisputable evidence—since the heat produced could be interpreted as caloric displaced—he nonetheless argued that "we still must explain in a purely mechanical way the production of electrical tensions in two processes [electrical induction and movements of magnets] in which any quantity of heat that can be assumed to be transferred never appears."[xcv] For electrical induction, he cited the example of an electrophorus used to charge a Leyden jar; for the movements of magnets, that of electromagnetic machines where "heat can be developed ad infinitum." It was only here that Helmholtz recalled Joule's experiments of 1843 and asserted that Joule "endeavored to show directly" that the electromagnetic current produced heat and not cold even in that part which is under the actual action of the magnet (no displacement of caloric is thus conceivable in the electrical circuit).[xcvi] Again, Joule's results played a minor role in the exposition of Helmholtz's ideas.

For Helmholtz, the caloric theory had to be rejected and replaced by the mechanical theory, which allowed heat to be produced indefinitely by mechanical and electrical forces. As noted in section 2, Helmholtz had already reached this conclusion in the "Bericht." What was new and specific in the *Erhaltung* was the application of the theoretical framework of tension and living forces to the mechanical theory of heat. Again, this was done in a purely conceptual and qualitative way, without mathematical formulation: free heat was now interpreted as the quantity of living force of thermal movement and latent heat as the quantity of tension forces (namely, the elastic forces of atoms). Yet the whole subject remained highly speculative, and Helmholtz was satisfied with "the possibility that thermal phenomena be conceived as motions."[xcvii] Lack of empirical confirmation, not lack of conceptual clarity, as Georg Helm later supposed, fully justified Helmholtz's cautiousness.[xcviii]

Although qualitative, Helmholtz's conceptual scheme was wide-ranging. To conceive of atoms as possessing not only living but also tension forces was a bold step, and his analogy with free and latent heat seems apt, for it allowed an easy reinterpretation of older ideas. The reinterpretation of the heat produced in chemical processes followed: Hess's law, "also partially verified by experience," had been deduced from the caloric theory. It asserted that "the heat developed in the production of a chemical compound is independent of the order and the intermediate steps of the process."[xcix] As Helmholtz had shown in the "Bericht," Hess's law agreed with the force-equivalent hypothesis (correlation principle); in the *Erhaltung*, he showed that it can be interpreted in terms of the new concepts of living and tension forces: the heat produced was now considered a living force, generated by the chemical forces of attraction that played the role of tension forces. Helmholtz here implicitly applied the mechanical concept of conservation developed in section I: the *vis viva* developed between two definite configurations of the system was independent of the trajectory.

The final problem of this section concerned the disappearance of heat; as Helmholtz noted, it had yet to receive much attention. In discussing it, he again displayed his inclination towards a theoretical approach: both the transformations of work into heat and of heat into work were assumed, but as necessary consequences of the principle of the conservation of force and not on the basis of experimental results. Again quoting Joule, Helmholtz asserted that Joule's results on this topic were the only ones available and that they seem “sufficiently reliable.” He was referring to Joule's experiment in which compressed air expanding against air pressure cooled the air; however, this did not occur when the air expanded in a vacuum. In the former instance, the compressed air has to exert a mechanical force to overcome the air pressure's resistance; in the latter, it does not. Hence, in the former instance the heat which has disappeared can be equated to the work done and thus a mechanical equivalent can be found (although Helmholtz did not mention any).[c]

Finally, and most puzzlingly, Helmholtz discussed the research of Clapeyron and Holtzmann. He knew that both men had conducted their work on the basis of the caloric theory; in fact, in the “Bericht” he asserted that they dealt more with the propagation than the production of heat. But in the *Erhaltung* he spoke of their research as “tending to find out the force equivalent of heat” and compared their work with his own.[ci] He discussed and criticized Clapeyron's approach at length, noting that Clapeyron's law, which assumed the caloric theory, had received empirical support for gases alone, and that for that case it was equivalent to Holtzmann's. The latter, for his part, had assumed that if a certain quantity of heat “enters” into a gas, then it produces either an increase in temperature or an expansion. In expansion, the work done allows, according to Helmholtz's account of Holtzmann's work, the possibility of calculating the mechanical equivalent of heat. Using Dulong's values for the specific heats of gases, Holtzmann's equivalent was 374 kgm.[cii] Helmholtz warned that this could only be accepted within the framework of the conservation of force if all of the transmitted heat's living force was actually given as work, that is, if the sum of the living and tension forces (or, in the old terminology, the quantity of free and latent heat) of the expanded gas was the same as that of the denser gas at the same temperature. This approach agreed with Joule's as given above, and Helmholtz compared Holtzmann's equivalent of 374 kgm with a series of results by Joule, whom he credited with having actually performed the experiments and not merely having reinterpreted older data. He cited five values from Joule: the two already noted (452 and 521, which, as argued above, should be 424 and 488) derived from the friction of water, and three others (481, 464, and 479, which should be 451, 435, 449). These last three, although cited without a specific reference, were almost certainly taken from Joule's 1845 paper. The first (481/451) referred to the 1843 experiments with an electromagnetic engine; the second (464/435) to the 1845 experiments on air referred noted above; and the third (479/449) to the average mentioned in Joule's 1845 paper. Helmholtz's comparison of Holtzmann's and Joule's results was seriously distorted by the systematic error in his conversion of Joule's units of measurement. Helmholtz concluded with a detailed comparison of the laws of Clapeyron and Holtzmann.[ciii] Seven years later, in 1854, Clausius, using a mechanical equivalent of 421 kgm, raised a serious objection, asserting that Helmholtz had misunderstood Holtzmann's law wherein the concept of caloric played a role which cannot be eliminated.[civ] This criticism was one of the very few that Helmholtz accepted during his long controversy with Clausius.[cv]

Helmholtz dedicated the fifth (and longest) section of the *Erhaltung* to applying the law of the conservation of force to static electricity, galvanism, and thermo-electric currents. Here, too, he displayed an extraordinarily detailed knowledge of the empirical laws of physics. His first application was to Coulomb's law, which, being a strictly central force law involving attractive and repulsive forces, offered the best possible example of how to formulate a sum of tension forces and to equate them with an increase in *vis viva*. Yet difficulties soon ensued: unaware of Green's results, Helmholtz introduced the concept of electric potential. He defined the quantity $-\frac{e_i e_{ii}}{r}$ 19, corresponding to the sum of the tension forces consumed and the living forces acquired in the motion of the two charges from an infinite distance to the distance **r**, as the potential of the two electrical elements **e** over the distance **r**.[cvi] He then expressed the principle of conservation of force in a new manner, as "the increase of *vis viva* in whichever movement must be considered equal to the difference of the potential at the end of the trajectory with respect to the potential at the beginning."[cvii] A potential so defined is equivalent (but for the sign) to the modern definition of potential energy. In relating potential and work, for instance in the case of the potential of one body with respect to another, Helmholtz showed a good grasp of their equivalence.[cviii] Yet his definition of the potential of a body on itself (the sum of the potentials of an electric element of a body with respect to all other such elements of the same body) was problematical: it did not correspond to the work done (the potential was supposed to be twice the work done).[cix] Hence, in Helmholtz's approach the two concepts were "independent." To be sure, in the original 1847 edition of the *Erhaltung* there was a final correction (the only one) referring to exactly these problems, which suggests Helmholtz's uncertainties and difficulties with concepts that were then by no means common.[cx] He later maintained that his 1847 approach of relating potential and work was basically correct.[cxi] In any case, he was among the first to interpret and use correctly the "new" mathematical tool of potential.

Although Helmholtz thus explicitly unified the tradition of analytical mechanics (the potential function of Gauss, Hamilton, and Jacobi) and of mechanical engineering (the concept of work), he arrived at the concept of potential not (as did Clausius in his more mathematical approach of 1852)[cxii] through the concept of work as a total differential, but rather directly from the concept of the sum of the tension forces. Theoretical rather than mathematical physics lay at the heart of Helmholtz's approach, as is evident from his attempt to clarify the "mathematical" potential through the introduction of physically sound concepts, and not vice versa: he first introduced the "equilibrium surfaces,"[cxiii] later identified with equipotential surfaces, and then the "free tension," later identified (by Helmholtz himself) with the mathematical potential function.[cxiv] As late as 1847, his idea of electric tension was reminiscent of Volta's influential "density of electricity."

Helmholtz sought to apply his *conceptual* framework of living and tension forces to every realm of nature. That his approach differed markedly from that of mathematical physicists, such as Clausius and Bernhard Riemann, as well as from experimentalists, such as Joule, can again be seen in his discussion of galvanism. For Helmholtz, Volta's contact law did not disagree, as

Kuhn has claimed,[cxv] with the impossibility of perpetual motion. Volta's contact tensions were not equivalent to a definite quantity of “force”: they did not produce an electrical imbalance but rather originated from such an imbalance. Helmholtz restricted his use of the contact law to first-class conductors (metals) and recognized that second-class conductors conduct only electrolytically. Hence, he interpreted contact force in terms of the attractive and repulsive forces of two metals which remove electrical charges in the contact area from one metal to the other. Equilibrium was reached when an electrical particle, in passing from one metal to another, neither acquired nor lost living force, that is, when the variation of living force from one metal to the other was compensated by an identical variation of tension forces independently of the shape and dimension of the contact surfaces and in agreement with the galvanic series of tensions.[cxvi]

If conservation of force based on central forces once again provided a conceptual explanatory framework for the contact law, it failed to do so for Helmholtz's long analysis of galvanic currents. Here conservation of force was applied to batteries not producing polarization; those producing polarization but not chemical decomposition; and those producing both. It was applied, however, in the non-mechanical sense as equivalence of numerical effects without a reinterpretation in terms of living and tension forces.[cxvii] Helmholtz knew that precise, experimentally confirmed laws existed only for batteries not producing polarization. By using Ohm's, Lenz's, and Joule's laws, Helmholtz gave the amount of heat that must be generated in the circuit to achieve conservation of force. This heat had to be equivalent to the chemical heat developed without electrical effects; the result was that the electromotive forces of the two metals were proportional to the difference of the heat developed by oxidation and by combination with acids.[cxviii] By contrast, Helmholtz discussed batteries producing polarization as well as polarization with chemical decomposition in detail but without applying his conceptual scheme and, because he lacked reliable empirical data, non-quantitatively.[cxix] Here, too, Helmholtz again cited Joule—this time for his experiments showing the equivalence of chemical and electrical heat[cxx]—and here, too, he again criticized and judged Joule's results and methods as unreliable, despite their providing evidence for a part of Helmholtz's innovative program.

Once more Helmholtz returned to his main line of thought: a conceptual explanation of electrical movements between metals and fluids through attractive and repulsive forces, in analogy with what he had just done for contact forces. For polarization currents, the two metals attracted (until saturated) positive or negative electrical charges, respectively. For chemical decomposition, there was not a stable equilibrium but instead a continuous process, one whose velocity did not continually increase for the loss of *vis viva* by the heat developed. Helmholtz derived an equivalence between the heat produced (living force) and the consumption of chemical elastic force (tension force). His conservation of force thus helped clarify yet another difficult topic. Finally, Helmholtz discussed thermo-electric currents and the Peltier effect. Without applying the concepts of tension and living forces, he utilized the principle of conservation to derive two consequences (on the heat produced and absorbed at equal [constant] temperatures and on equal currents), yet again complaining that he knew of no experimental measurements.[cxxi]

In section VI, the final section, Helmholtz's approach revealed all its strengths and limitations. In treating the force-equivalents for magnetism and electromagnetism, the intrinsic difficulties connected with the formulation and application of the principle of conservation of force are clear enough.[cxxii] In treating magnetism, Helmholtz followed the pattern that he had used for electrostatics: the inverse square law provided an expression for the tension forces. He defined living forces and potentials, both for two bodies and for a body on itself. An interesting application was that of a non-magnetized steel bar brought close to a magnet, then magnetized, and then separated. Here there occurred an expenditure in mechanical work of $-\frac{1}{2}W$ 20 (again, the potential on itself is twice the work **W**) acquired by the magnetized bar. In treating electromagnetism, Helmholtz for the first time showed his mastery of a subject and outlined a research program that he would pursue intermittently during the next forty years. He used not only the well-known laws of Ampère but also the more recent and less-well-known laws of Weber, Lenz, Neumann, and Grassmann. He characterized precisely the approach of Weber's law (which at variance with Ampère's explained electromagnetic induction), and noted how it stood within a conceptual framework that was at odds with his own, for Weber assumed forces depending on velocities and accelerations. Helmholtz pointed out that "until now" it had not been possible to refer Weber's law to central forces.[cxxiii]

Both Neumann's and Grassmann's laws, he noted, agreed with Weber's for closed currents, the only laws for which experiments were available. He thus restricted his application of the principle of the conservation of force to closed currents and showed that the "same laws" could be deduced by using the principle.[cxxiv] His strategy was clear: lacking a central force law for electromagnetism, he hoped to use the principle to deduce "empirical" laws already deduced on the basis of non-Newtonian hypothetical forces, thereby gaining evidence for the principle's justificatory power and, if new consequences could be successfully predicted, for its heuristic power as well. Yet difficulties soon emerged, as can be seen in the following two cases.

In discussing a system consisting of a magnet moving under the effect of a current, Helmholtz identified the tension forces with those consumed in the current, **aAJdt**, where **a** is the mechanical equivalent of heat, **A** the electromotive force of a single cell, **J** the current, and **dt** an infinitely small amount of time; that is, as with his results on galvanism, he identified the tension forces with the heat generated chemically inside a battery. As for the living force, it consisted of two parts: the heat generated in the circuit by the current, $\mathbf{aJ^2Wdt}$, where **W** is the resistance of the circuit; and the living force acquired by the magnet under the effect of the current, $J\frac{dV}{dt}dt$ 21, where **V** is the potential of the magnet towards the conductor carrying a unit current. Hence:

$$J = \frac{A - \frac{1}{a}\frac{dV}{dt}}{W}.$$

Helmholtz interpreted the term $\frac{1}{a}\frac{dV}{dt}$ 23 as a new electromotive force, namely, that of the induced current. His force was similar to but more precise than Neumann's in that he gave the value **1/a** for what in Neumann was an undetermined constant.[cxxv]

If this "demonstration" became famous as an instance of the principle's heuristic power,[cxxvi] a second case discussed by Helmholtz, that concerning the interactions between two currents, became famous as an instance of a false deduction from the principle. Here Helmholtz simply extended the previous formulation for the tension forces provided by the batteries of the two circuits to $\mathbf{A_1J_1}$ and $\mathbf{A_2J_2}$ and identified the living forces with the heat produced by the current in the two circuits with

$$J_1^2 W_1 + J_2^2 W_2 + \frac{1}{a} J_1 J_2 \frac{dV}{dt}, \qquad 24$$

where the third term was interpreted as the living force of one circuit under the effect of the current circulating in the other circuit. Although he claimed that his results agreed with Weber's, in point of fact he dismissed two kinds of potentials that indeed exist: the mutual potential of the two currents (electrokinetic energy) and the potential of a current on itself (self-induction).[cxxvii]

These two difficulties, whose conflicting results both derived from Helmholtz's principle, raise a question about its heuristic utility. In the first instance, Helmholtz's deduction was really a reinterpretation of extant knowledge. In the second, he provided neither new predictions nor a rationale for the specific application of the concepts of tension and living forces. His efforts thus show that, lacking experimental data, he could not give an a priori, precise, theoretical deduction of the energy equations.

6. Conclusion

Helmholtz devoted the *Erhaltung*'s short conclusion to physiological problems, his research domain proper. Here, too, his principal problem was that of formulating the force equivalents for the energy balance. For the plant world, he declared that, due to insufficient data, precise application of the principle was impossible; all that could be said was that the stored tension forces were chemical in origin and that the only absorbed living forces were the "chemical rays of sunlight."[cxxviii] For the animal world, by contrast, he declared that relatively precise applications of his principle were possible. Summarizing his previous research for the benefit of physicists, he introduced for the first time the concepts of tension and living forces in physiology: animals utilize a certain quantity of chemical tension forces and generate heat and mechanical forces. Yet he thought that the mechanical work done by animals was only a small quantity compared to the heat they produced, and thus that it could be omitted in the equation for force equivalents.[cxxix] On the basis of Dulong and Despretz's experimental work, he believed that the combustion and conversion of nutritive substances generated a quantity of heat equivalent to that produced by animals.[cxxx] Helmholtz's use of the principle in this physiological context shows yet again that his principal aim was to outline, independently of the actual experimental determination of the mechanical equivalent of heat and thus independently of precise experimental corroboration, a general framework in which the principle of the conservation of force would be applicable to the largest possible class of phenomena. If the exact value of the mechanical equivalent was unknown, that scarcely mattered in this context: in asserting that the work done by animals was a small percentage of the heat produced, Helmholtz showed that he could be satisfied with a gross figure.[cxxxi] Indeed, he concluded by making only modest claims for the principle itself, declaring *not* that he had demonstrated the principle but only that it was "not in contradiction to any known fact in natural science but rather that it is confirmed in a remarkable way by a great number of such facts."[cxxxii]

In accord with the sophisticated plan that he had outlined at the start of the *Erhaltung*, Helmholtz had completely united the principle of the conservation of force with the pertinent known laws of natural phenomena. At the same time, he was keenly aware that his own extraordinary theoretical efforts lacked experimental confirmation. His goal had been, he said, "to show physicists (with the greatest possible completeness) the theoretical, practical, and heuristic importance of this law [of the conservation of force], whose complete confirmation must indeed be considered as one of physics's main tasks in the near future.'[cxxxiii] Few pieces of scientific literature have ever expressed their goals and results so clearly as did Helmholtz's *Ueber die Erhaltung der Kraft*. It is the first full expression of Helmholtz's ideas on theoretical physics as well as on force. Both from the physical and the methodological point of view, the *Erhaltung* was a masterpiece. Yet it neither enjoyed an immediate success nor did it go uncriticized. Before Helmholtz's career and the nineteenth century ended, Clausius's criticism would lead Helmholtz to abandon his formulation of the principle of the conservation of force and its associated unifying program for physics in favor of a new regulative principle, that of least action.[cxxxiv]

[*] **Acknowledgements**: Financial support for this research has been provided by the Consiglio Nazionale delle Ricerche. Earlier versions of this paper were presented in Urbino (1989) and Munich (1990), and I thank Enrico Giannetto, Rod Home, my wife Leitha Martin, and Stefan Wolff for their comments on the earlier versions. I should also like to thank the editor, David Cahan, for his continuous encouragement and helpful suggestions.

Notes

[i] .See esp. James Clerk Maxwell, *The Theory of Heat* (London: Longmans, Green, 1871); Ernst Mach, *Die Geschichte und die Würzel des Satzes von der Erhaltung der Arbeit* (Prague: Calve, 1872); Balfour Stewart, *The Conservation of Energy*, 2nd ed. (London: H.S. King, 1874); James Clerk Maxwell, *Matter and Motion* (London: Society for Promoting Christian Knowledge, 1876); Moritz Rühlmann, *Vorträge über Geschichte der technischen Mechanik und der theoretischen Maschinenlehre*, 2 vols. (Leipzig: Baumgärtner, 1881-85); J.B. Stallo, *The Concepts and Theories of Modern Physics* (London: Kegan, Paul, Trench, 1882); Ernst Mach, *Die Mechanik in ihrer Entwickelung historisch-kritisch dargestellt* (Leipzig: F.A. Brockhaus, 1883); Max Planck, *Das Princip der Erhaltung der Energie* (Leipzig: B.G. Teubner, 1887); Georg Helm, *Die Lehre von der Energie, historisch-kritisch entwickelt* (Leipzig: A. Felix, 1887); Ernst Mach, *Die Principien der Wärmelehre, historisch-kritisch entwickelt* (Leipzig: J.A. Barth, 1896); Georg Helm, *Die Energetik nach ihrer geschichtlichen Entwickelung* (Leipzig: Veit, 1898); Henri Poincaré, *La science et l'hypothèse* (Paris: E. Flammarion, 1902); Arthur Erich Haas, *Die Entwicklungsgeschichte des Satzes von der Erhaltung der Kraft* (Vienna: Alfred Hölder, 1909); Wilhelm Ostwald, *Die Energie* (Leipzig: J.A. Barth, 1908); Emile Meyerson, *Identité et réalité* (Paris: F. Alcan, 1908); and Ernst Cassirer, *Substanzbegriff und Funktionsbegriff* (Berlin: Bruno Cassirer, 1910).

[ii] .Thomas Kuhn, “Energy Conservation as an Example of Simultaneous Discovery,” in Marshall Clagett, ed., *Critical Problems in the History of Science* (Madison, Wis.: University of Wisconsin Press, 1959), 321-56, reprinted in Kuhn's *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago and London: University of Chicago Press, 1977), 66-104. Other studies on Helmholtz and the conservation of energy include: Yehuda Elkana, “The Conservation of Energy: A Case of Simultaneous Discovery?,” *Archives internationales d'histoire des sciences 23* (1970):31-60; idem, “Helmholtz's `Kraft': An Illustration of Concepts in Flux,” *HSPS 2* (1970):263-99; idem, *The Discovery of the Conservation of Energy* (Cambridge, Mass.: Harvard University Press, 1974); Peter Heimann, “Conversion of Forces and the Conservation of Energy,” *Centaurus 18* (1974):147-61; P.M. Heimann, “Helmholtz and

Kant: The Metaphysical Foundations of *Über die Erhaltung der Kraft*," *SHPS 5* (1974):205-38; Peter Clark, "Elkana on Helmholtz and the Conservation of Energy," *The British Journal for the Philosophy of Science 27* (1976):165-76; Geoffrey Cantor, "William Robert Grove, the Correlation of Forces and the Conservation of Energy," *Centaurus 19* (1976):273-90; P.M. Harman, "Helmholtz: The Principle of the Conservation of Energy," in his *Metaphysics and Natural Philosophy: The Problem of Substance in Classical Physics* (Brighton: Harvester Press, 1982), 105-26; Fabio Bevilacqua, *The Principle of Conservation of Energy and the History of Classical Electromagnetic Theory* (Pavia: La Goliardica Pavese, 1983); and Stephen M. Winters, "Hermann von Helmholtz's Discovery of Force Conservation," Ph.D. dissertation, The Johns Hopkins University, 1985.

[iii] .Kuhn, "Energy Conservation," 66-70, 73.

[iv] .*Ueber die Erhaltung der Kraft. Eine physikalische Abhandlung* (Berlin: G. Reimer, 1847), in *WA 1*:12-75 (including an appendix [68-75] from 1881). For a facsimile edition see *Über die Erhaltung der Kraft*, transcribed by Christa Kirsten, 2 vols. (Berlin: Akademie Verlag, 1982).

[v]. Kuhn, "Energy Conservation," 83-4, 86-8, 90.

[vi] .The French theoretical engineering tradition, which Kuhn mentioned but dismissed, has long (and rightly) received special attention from a number of scientists, historians, and philosophers: Rühlmann, *Maschinenlehre*; Helm, *Die Energetik*, 14-5; Haas, *Entwicklungsgeschichte*; Edmund Hoppe, *Histoire de la physique*, trans. Henri Besson (Paris: Payot, 1928), 96, and cf. Kuhn, "Energy Conservation," 86, n.44; Felix Auerbach, "Feld, Potential, Arbeit, Energie und Entropie," in "Grundbegriffe," in *Handbuch der Physik*, ed. Adolph A. Winkelmann, 2nd ed., 6 vols. in 7 (Leipzig: Barth, 1908) *1*:68-91; Ernst Cassirer, *The Problem of Knowledge*, trans. William Woglom and Charles Hendel (New Haven, Conn.: Yale University Press, 1950), 49-50, 72; and John Theodore Merz, *A History of European Thought*, 4 vols. (reprint New York: Dover, 1965) *2*:101. See also Ivor Grattan-Guinness, "Work for the Workers: Advances in Engineering Mechanics and Instruction in France, 1800-1830," *Annals of Science 41* (1984):

1-33.

[vii] .M. Norton Wise, “William Thomson's Mathematical Route to Energy Conservation: A Case Study of the Role of Mathematics in Concept Formation,” *HSPS 10* (1979):49-83, on 59, notes the lack of mathematical factors in Kuhn's analysis; M. Norton Wise and Crosbie Smith, “Measurement, Work, and Industry in Lord Kelvin's Britain,” *HSPS 17* (1986):147-73, on 154, expresses a different view.

[viii] .Erwin Hiebert, “Commentary on the Papers of Thomas S. Kuhn and I. Bernard Cohen,” in Marshall Clagett, ed., *Critical Problems in the History of Science* (Madison, Wis.: University of Wisconsin Press, 1959), 391-400, on 394, noted that the concept of work was initially meant to explain the five simple machines in Hero of Alexandria's *Mechanica*. According to Cassirer, it was Leibniz who first connected the concept of work with “energy” conservation; see his *Leibniz' System in seinen wissenschaftlichen Grundlagen* (Marburg: Elwert, 1902); *Substanzbegriff*, 226-48; and *Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit*, 4 vols. (vols. 1-3: Berlin: Bruno Cassirer, 1906-20; vol. 4 [in English translation]: 1950, [in German]: Stuttgart: Kohlhammer, 1957; reprinted Darmstadt: Wissenschaftliche Buchgesellschaft, 1973-74), *2*.

[ix] .Heimann, “Conversion of Forces,” 147, 159.

[x] .Haas, *Entwicklungsgeschichte*, esp. 31-45.

[xi] .“Ueber die Erhaltung der Kraft,” *WA 1*:68.

[xii] .L. Pearce Williams, “Kant, *Naturphilosophie*, and Scientific Method,” in *Foundations of Scientific Method: The Nineteenth Century*, eds. Ronald Giere and Richard Westfall (Bloomington, Ind. and London: Indiana University Press, 1973), 3-22.

[xiii] .Kuhn, “Energy Conservation,” 101-3. On the role of the dynamical theory in the work of four British scientists see Crosbie Smith, “A New Chart for British Natural Philosophy: The

Development of Energy Physics in the Nineteenth Century," *History of Science 16* (1978):231-79; and Donald Moyer, "Energy, Dynamics, Hidden Machinery: Rankine, Thomson and Tait, Maxwell," *SHPS 8* (1977):251-68.

[xiv] .Kuhn, "Energy Conservation," 95 (n.68), 84, 90, 88, 73, 100-1, resp.

[xv] .For a detailed analysis of this topic see Kathryn M. Olesko and Frederic L. Holmes's essay, "Experiment, Quantification, and Discovery: Helmholtz's Early Physiological Researches, 1843-50," in this volume.

[xvi] ."Bericht über `die Theorie der physiologischen Wärmeerscheinungen' betreffende Arbeiten aus dem Jahre 1845," *Fortschritte der Physik im Jahre 1845 1* (1847):346-55, in *WA 1*:3-11; and "Ueber die Erhaltung der Kraft." See also "Erwiderung auf die Bemerkungen von Hrn. Clausius," *AP 91* (1854):241-60, in *WA 1*:76-93. Unless otherwise noted, all references to the "Bericht" are to the article above.

[xvii] ."Ueber das Wesen der Fäulniss und Gährung," *AAPwM* (1843):453-62, in *WA 2*:726-34; "Ueber den Stoffverbrauch bei der Muskelaction," *AAPwM* (1845):72-83, in *WA 2*:735-44; "Wärme, physiologisch," in *Encyklopädisches Handwörterbuch der medicinischen Wissenschaften*, ed. Professoren der medicinischen Facultät zu Berlin, vol.35 (Berlin: Veit, 1846), 523-67, in *WA 2*:680-725; "Ueber die Wärmeentwicklung bei der Muskelaction," *AAPwM* (1848):147-64, in *WA 2*:745-63; and "Bericht über die Theorie der physiologischen Wärmeerscheinungen betreffende Arbeiten aus dem Jahre 1846," *Fortschritte der Physik im Jahre 1846 2* (1848):259-60.

[xviii] .See Olesko and Holmes, "Experiment, Quantification, and Discovery." Whether physiological research, apart from constituting the context, should also be seen as the root or one of the roots of Helmholtz's formulation of force conservation, remains uncertain. Richard L. Kremer, "The Thermodynamics of Life and Experimental Physiology, 1770-1880," Ph.D. diss., Harvard University, 1984, 190-93, perceptively contrasts the "standard" view that research in energy

conservation was motivated by physiological problems.

[xix] ."Ueber die Erhaltung der Kraft," *WA 1*:74, and "Erinnerungen," in *VR*^{4} *1*:7-11. Leo Koenigsberger accepted this approach (Koenigsberger *1*:12, 50-2); by contrast, Kremer ("Thermodynamics of Life," 237-38) denies the relevance of vitalism in physiological debates and dismisses Helmholtz's autobiographical claims on the role of the principle of the impossibility of perpetual motion.

[xx] .See Arleen Tuchman's essay, "Helmholtz and the German Medical Community," in this volume.

[xxi] .Timothy Lenoir, *The Strategy of Life: Teleology and Mechanics in Nineteenth-Century German Biology* (Dordrecht and Boston: D. Reidel, 1982), 195-96, 215-17, 230. For a different interpretation see Kremer, "Thermodynamics of Life," 237-38; and Olesko and Holmes "Experiment, Quantification, and Discovery."

[xxii] .Justus Liebig, *Chemische Briefe*, 3rd ed. (Heidelberg: C.F. Winter, 1851), twelfth letter, pp. 116-18, quoted in Helm, *Die Energetik*, 10; Haas, *Entwicklungsgeschichte*, 57; and Kuhn, "Energy Conservation," 95.

[xxiii] .Kremer, "Thermodynamics of Life," 204-9.

[xxiv] .Ibid., 238. Liebig has often been credited with being a pioneer in the history of energy conservation; see Planck, *Das Princip der Erhaltung der Energie*, 33; Helm, *Die Energetik*, 10; Haas, *Entwicklungsgeschichte*, 57; Kuhn, "Energy Conservation," 68; Lenoir, *Strategy of Life*, 196; and Kremer "Thermodynamics of Life," 198-215.

[xxv] .Koenigsberger and Kremer argue that Helmholtz's first paper, that on fermentation and putrefaction, aimed to support Liebig's antivitalist position but raised confusing conclusions for him (Koenigsberger *1*:53; Kremer, "Thermodynamics of Life," 239-40); by contrast, Lenoir and Yamaguchi argue that Liebig's position stimulated Helmholtz to further research (Lenoir,

Strategy of Life, 197, and Chûhei Yamaguchi, "On the Formation of Helmholtz' View of Life Processes in His Studies of Fermentation and Muscle Action—in Relation to His Discovery of the Law of Conservation of Energy," *Historia Scientiarum 25* [1983]:29-37). In his second paper, that on metabolism during muscular activity, Helmholtz had inconclusive results about metabolism because he lacked an exact relationship between the muscular action and the heat developed. (Koenigsberger *1*:60; Kremer, "Thermodynamics of Life," 243; Lenoir, *Strategy of Life*, 202; and Olesko and Holmes, "Experiment, Quantification, and Discovery.")

[xxvi] ."Wärme, physiologisch," *WA 2*:695-700.

[xxvii] .Ibid., *WA 2*:700.

[xxviii] .Kremer, "Thermodynamics of Life," 251.

[xxix] ."Wärme," *WA 2*:699-700. Kremer argues ("Thermodynamics of Life," 238) that in his physiological research Helmholtz never succeeded in bringing heat and work together operationally.

[xxx] ."Muskelaction," *WA 1*:735.

[xxxi] ."Wärme," *WA 2*:699-700.

[xxxii] .*Ueber die Erhaltung der Kraft*, 70, in *WA 1*:66. See also Olesko and Holmes, "Experiment, Quantification, and Discovery."

[xxxiii] ."Bericht über `die Theorie der physiologischen Wärmeerscheinungen,'" in *WA 1*:4, 6.

[xxxiv] .Ibid., in *WA 1*:6-7.

[xxxv] .Ibid., in *WA 1*:7.

[xxxvi] .Ibid., in *WA 1*:8; Planck, *Princip*, 34.

[xxxvii] .Hence one must disagree with Lenoir's claim (*Strategy of Life*, 211) that "the physiology of

muscle action laid before Helmholtz all the elements of conservation of energy.”

[xxxviii] .“Ueber die Wärmeentwicklung bei der Muskelaction.” For a detailed analysis of this paper see Olesko and Holmes, “Experiment, Quantification, and Discovery.”

[xxxix] .Lenoir, *Strategy of Life*, 211; Kremer, “Thermodynamics of Life,” 244; and Olesko and Holmes, “Experiment, Quantification, and Discovery.”

[xl] .Koenigsberger *1*:68-72. Koenigsberger's claim (ibid., *1*:68) that in the first quarter of 1847 Helmholtz conducted extensive experimentation strictly related to the preparation of the *Erhaltung* is difficult to accept.

[xli]. Ibid., *1*:68.

[xlii] .Ibid., *1*:72.

[xliii] .*Ueber die Erhaltung*, 1, in *WA 1*:12.

[xliv] .*Ueber die Erhaltung* der Kraft, 2, in *WA 1*:13.

[xlv] .Elkana, *Discovery of Conservation of Energy*, 49-51.

[xlvi] .*Ueber die Erhaltung der Kraft*, 1-2, in *WA 1*:12.

[xlvii] .Ibid., 2, in *WA 1*:13.

[xlviii] .For the distinction between “regulative-empirical” and “transcendental” causality in Kant see Gerd Buchdahl, “Reduction-Realization: A Key to the Structure of Kant's Thought,” *Philosophical Topics 12:2* (1981):39-98, esp. 83-4.

[xlix] .*Ueber die Erhaltung der Kraft*, 2, in *WA 1*:13.

[l] .*Ueber die Erhaltung der Kraft*, 2, in *WA 1*:13.

[li] .In addition to Helmholtz himself in “Ueber die Erhaltung der Kraft,” 68, see, for example,

Elkana, “Helmholtz's `Kraft'”; Heimann, “Helmholtz and Kant”; M. Norton Wise, “German Concepts of Force, Energy, and the Electromagnetic Ether: 1845-1880,” in *Conceptions of Ether: Studies in the History of Ether Theories 1740-1900*, eds. G.N. Cantor and M.J.S.Hodge (Cambridge, London, New York: Cambridge University Press, 1981), 269-307; and S.P. Fullwinder, “Hermann Von Helmholtz: The Problem of Kantian Influence,” *SHPS 21:1* (1990): 41-55.

[lii] .Heimann, “Helmholtz and Kant,” 229.

[liii] .*Ueber die Erhaltung der Kraft*, 5, in *WA 1*:15.

[liv] .Heimann, contra Elkana, shows that the meaning of “Kraft” is always unequivocal in a given context; see Heimann, “Helmholtz and Kant,” 207 (n. 10), 209 (n. 14). For a detailed analysis of the philosophical points raised here see Michael Heidelberger's essay, “Force, Law, and Experiment: The Evolution of Helmholtz's Philosophy of Science,” in this volume.

[lv] .“Ueber die Erhaltung der Kraft,” in *WA 1*:68-70.

[lvi] .*Ueber die Erhaltung* der Kraft, 7, in *WA 1*:17.

[lvii] .*Ueber die Erhaltung der Kraft*, 8, in *WA 1*:18.

[lviii] .*Ueber die Erhaltung der Kraft*, 9, in *WA 1*:18. For one indication of Helmholtz's theoretical as opposed to mathematical approach to the concept of work, compare his awareness of the role of that concept to the problems posed in section V in adopting a definition of “potential in itself” as the equivalent of work.

[lix] .*Ueber die Erhaltung der Kraft*, 9, in *WA 1*:18. Kuhn's suggestion notwithstanding (“Energy Conservation,” 88), Helmholtz did not himself rederive the definition of *vis viva*.

[lx] .*Ueber die Erhaltung der Kraft*, 9, in *WA 1*:19.

[lxi] .*Ueber die Erhaltung der Kraft*, 11-2, in *WA 1*:20-1.

[lxii] .On this point, see Helm's sharp criticism (*Die Energetik*, 41).

[lxiii] .“Bericht über `die Theorie der physiologischen Wärmeerscheinungen',” in *WA 1*:6.

[lxiv] .Rühlmann, *Vorträge über Maschinenlehre*; and Haas, *Entwicklungsgeschichte*, 73-83.

[lxv] .Haas, *Entwicklungsgeschichte*, 81.

[lxvi] .Grattan-Guinness, “Work for the Workers,” 32.

[lxvii] .*Ueber die Erhaltung der Kraft*, 38-44, in *WA 1*:41-6.

[lxviii] .“Ueber die Erhaltung der Kraft,” *WA 1*:70. James Clerk Maxwell, “A Dynamical Theory of the Electromagnetic Field,” reprinted in *The Scientific Papers of James Clerk Maxwell*, ed. W.D. Niven, 2 vols. (Cambridge: Cambridge University Press, 1890), *1*:526-37, on 526-27; W. Thomson and P.G. Tait, *Treatise on Natural Philosophy* (Oxford: Clarendon, 1867), 311-12; and P.G. Tait, *Sketch of Thermodynamics* (Edinburgh: Edmonston and Douglas, 1868), 76. See also Carl Neumann's defense of Weber in his *Die Gesetze von Ampère und Weber* (Leipzig: Teubner, 1877), 322-24; and, for a discussion of this, Bevilacqua, *The Principle of Energy Conservation*, 122-23, 133-36. For Clausius' critique see his “Ueber das mechanische Aequivalent einer elektrischen Entladung und die dabei stattfindende Erwärmung des Leitungsdrahtes,” *AP 86* (1852):337-75; idem, “Ueber einige Stellen der Schrift von Helmholtz `über die Erhaltung der Kraft',” *AP 89* (1853):568-79; and idem, “Ueber einige Stellen der Schrift von Helmholtz `über die Erhaltung der Kraft', zweite Notiz,” *AP 91* (1854):601-4.

[lxix] .*Ueber die Erhaltung der Kraft*, 13, in *WA 1*:21-2.

[lxx] .*Ueber die Erhaltung der Kraft*, 14, in *WA 1*:22.

[lxxi] .*Ueber die Erhaltung der Kraft*, 17, in *WA 1*:25.

[lxxii] .*Ueber die Erhaltung der Kraft*, 17-8, in *WA 1*:25.

[lxxiii] .*Ueber die Erhaltung der Kraft*, 19-20, in *WA 1*:26-7.

[lxxiv] .Planck, *Das Princip der Erhaltung der Kraft*, 37.

[lxxv] .Ibid., 37.

[lxxvi] .Kuhn, “Energy Conservation,” 88.

[lxxvii] .Planck, *Das Princip der Erhaltung der Kraft*, 35; Koenigsberger *1*:89; and Elkana, *Discovery of Conservation of Energy*, 20 (n 31).

[lxxviii] .Leibniz, “Brevis Demonstratio erroris memorabilies Cartesii et aliorum circa legem naturalem...,” in *Acta Eruditorum* (1686):161-63 [Leipzig: Christophori Guntheri, 1686], reprinted in *Leibnizens mathematische Schriften*, ed. C.I. Gerhardt, 7 vols. in 4 (vols. 1-2: Berlin: A. Asher; vols. 3-7: Halle, H.W. Schmidt, 1849-63), *2*:117-19; and, more generally, see Ernst Cassirer, *Leibniz' System in seinen wissenschaftlichen Grundlagen* (Marburg: Elwert, 1902); and idem, *Substanzbegriff und Funktionsbegriff* (Berlin: Bruno Cassirer, 1923), 226-48.

[lxxix] .Rudolf Clausius, “Ueber das mechanische Aequivalent einer elektrischen Entladung und die dabei stattfindende Erwärmung des Leitungsdrahtes,” *AP 86* (1852):337-75.

[lxxx] .*Ueber die Erhaltung der Kraft*, 39, in *WA 1*:42.

[lxxxi] .“Ueber die Erhaltung der Kraft,” *WA 1*:71-3; “Anhang zu dem Vortrag `Ueber die Wechselwirkung der Naturkräfte und die darauf bezüglichen neuesten Ermittelungen der Physik': Robert Mayer's Priorität,” in *VR*$^5$ *1*:401-14; “Anhang zu dem Vortrag `Das Denken in der Medicin,'” *VR*^{5} *2*:384-86; Planck, *Das Princip der Erhaltung der Energie*, 21-8; Helm, *Die Energetik*, 16-28; Haas, *Entwicklungsgeschichte*, 61-2; Robert Bruce Lindsay, *Julius Robert Mayer. Prophet of Energy* (Oxford and New York: Pergamon, 1973); and Peter Heimann, “Mayer's Concept of `Force': The `Axis' of a New Science of Physics,”*HSPS 7* (1976):227-96.

[lxxxii] .*Ueber die Erhaltung der Kraft*, 23, in *WA 1*:30-1.

[lxxxiii] .*Ueber die Erhaltung der Kraft*, 24, in *WA 1*:31.

[lxxxiv] .*Ueber die Erhaltung der Kraft*, 25, in *WA 1*:31.

[lxxxv] .“Ueber die Erhaltung der Kraft,” in *WA 1*:74.

[lxxxvi] .*Ueber die Erhaltung der Kraft*, 26, in *WA 1*:32.

[lxxxvii] .*Ueber die Erhaltung der Kraft*, 27, in *WA 1*:32-3.

[lxxxviii] .James P. Joule, “On the Existence of an Equivalent Relation between Heat and the ordinary Forms of Mechanical Power,” *PM*, ser. 3, *27* (1845):205-7.

[lxxxix] .Ibid.; and J.P. Joule, “On the Changes of Temperature produced by the Rarefaction and Condensation of Air,” *PM 26*, ser. 3 (1845):369-83. In 1853 and in 1863, John Tyndall attempted to explain Helmholtz's evaluation of Joule's work by claiming that Helmholtz used Joule's paper of 1843, where the force equivalent varied between 1,040 and 547 ft-lbs. (See J.P. Joule, “On the Caloric Effects of Magneto-Electricity, and on the Mechanical Value of Heat,” *PM 23*, ser. 3 [1843]:263-76, 347-55, 435-43, on 438-42; and John Tyndall, “Remarks on the Dynamical Theory of Heat,” *PM 25*, ser. 4 [1863]:368-87, on 375-76.) In point of fact, in the passage of the *Erhaltung* at issue, Helmholtz did not refer to Joule's 1843 paper but rather to that of 1845.

[xc] .“On the Interaction of Natural Forces and Recent Physical Discoveries bearing on the same,” *PM*, ser. 4, *11*:489-518, on 499; see also Helmholtz, “Erinnerungen,” in *VR*^{4} *1*:11; and Koenigsberger *1*:82.

[xci] .On the French foot see H.G. Jerrard and D.B. McNeill, *Dictionary of Scientific Units: Including Dimensionless Numbers and Scale*, 5th ed. (London: Chapman and Hall, 1986), 45.

[xcii] .*Ueber die Erhaltung der Kraft*, 27, in *WA 1*:33. Thirty-four years later, in a footnote of 1881 (*WA 1*:33), Helmholtz modified his comments and praised Joule's work, yet he did not

acknowledge (or see?) his own conversion mistake of 1847.

[xciii] .*Ueber die Erhaltung der Kraft*, 27, in *WA 1*:33.

[xciv] .*Ueber die Erhaltung der Kraft*, 28-9, in *WA 1*:34.

[xcv] .*Ueber die Erhaltung der Kraft*, 29, in *WA 1*:34.

[xcvi] .*Ueber die Erhaltung der Kraft*, 30, in *WA 1*:35, where Joule's results are misdated as 1844.

[xcvii] .*Ueber die Erhaltung der Kraft*, 31, in *WA 1*:36.

[xcviii] .Helm, *Die Energetik*, 44.

[xcix] .*Ueber die Erhaltung der Kraft*, 32, in *WA 1*:37.

[c] .*Ueber die Erhaltung der Kraft*, 33, in *WA 1*:37; and Joule, “On the Changes of Temperature.”

[ci] .*Ueber die Erhaltung der Kraft*, 33, in *WA 1*:37-8.

[cii] .*Ueber die Erhaltung der Kraft*, 35, in *WA 1*:39.

[ciii] .*Ueber die Erhaltung der Kraft*, 36-7, in *WA 1*:40-1.

[civ] .Rudolf Clausius, “Ueber die bewegende Kraft der Wärme und die Gesetze, welche sich daraus für die Wärmelehre selbst ableiten lassen,” *AP 79* (1850):368-97, 500-24.

[cv] .“Erwiderung auf die Bemerkungen von Hrn. Clausius,” *AP 91* (1854):241-60, in *WA 1*:76-93, on 90; and see also Clifford Truesdell, *The Tragicomic History of Thermodynamics. 1822-1854* (New York: Springer, 1980), 162. In an appendix to this section in 1882, Helmholtz discussed the priority problem: “Ueber die Erhaltung der Kraft,” *WA 1*:71-4.

[cvi] .*Ueber die Erhaltung der Kraft*, 38-9, in *WA 1*:42.

[cvii] .*Ueber die Erhaltung der Kraft*, 39, in *WA 1*:42.

[cviii] .*Ueber die Erhaltung der Kraft*, 39-40, in *WA 1*:42-3.

[cix] .*Ueber die Erhaltung der Kraft*, 42-3, in *WA 1*:44-5.

[cx] .In the 1882 reprint of “Ueber die Erhaltung der Kraft” in *WA 1* the correction is incorporated in the text (ibid., 45); and cf. ibid., 75.

[cxi] .Ibid., *WA 1*:75.

[cxii] .Clausius, “Ueber das mechanische Aequivalent.”

[cxiii] .*Ueber die Erhaltung der Kraft*, 40-2, in *WA 1*:43-5.

[cxiv] .“Ueber einige Gesetze der Vertheilung elektrischer Ströme in körperlichen Leitern mit Anwendung auf die thierisch-elektrischen Versuche,” AP 89 (1853):211-33, 352-77, on 224, in *WA 1*:475-519.

[cxv] .Kuhn, “Energy Conservation,” 73.

[cxvi] .*Ueber die Erhaltung der Kraft*, 45-7, in *WA 1*:47-9.

[cxvii] .Helm, *Die Energetik*, 44.

[cxviii] .Given that most of this section (*Ueber die Erhaltung der Kraft,* 48-58 in *WA 1*:49-57) deals with a careful analysis of batteries, it is difficult to understand Kuhn's claim (“Energy Conservation,” 73) that Helmholtz did not discuss batteries in the *Erhaltung*.

[cxix] .*Ueber die Erhaltung der Kraft*, 51-6, in *WA 1*:51-5.

[cxx] .J.P. Joule, “On the Heat evolved by Metallic Conductors of Electricity, and in the Cells of a Battery during Electrolysis,” *PM 19*, ser. 3 (1841):260-77, on 275; and idem, “On the Electrical Origin of Chemical Heat,” ibid., *22* (1843):204-8.

[cxxi] .*Ueber die Erhaltung der Kraft*, 60, in *WA 1*:58.

[cxxii] .*Ueber die Erhaltung der Kraft*, 60-9, in *WA 1*:58-65.

[cxxiii] .*Ueber die Erhaltung der Kraft*, 62-3 (quote on 63), in *WA 1*:60-1 (quote on 61).

[cxxiv] .*Ueber die Erhaltung der Kraft*, 64, in *WA 1*:61.

[cxxv] .*Ueber die Erhaltung der Kraft*, 64-6, in *WA 1*:62-3.

[cxxvi] .In his *Treatise on Electricity and Magnetism*, Maxwell was still quoting Helmholtz's deduction of the law of induction with praise in 1873, though by the third edition (1891), Maxwell's editor, J.J. Thomson, noted that the law of induction cannot be deduced through the principle of conservation of energy alone. See Maxwell's *A Treatise on Electricity and Magnetism* 2 vols. (Oxford: Clarendon Press, 1873; reprint: New York: Dover, 1954), *2*:190-93, on 192.

[cxxvii] .Planck, *Das Princip der Erhaltung der Energie*, 47.

[cxxviii] .*Ueber die Erhaltung der Kraft*, 69, in *WA 1*:66.

[cxxix] .Cf. “Wärme,” in *WA 2*:699-700.

[cxxx] .*Ueber die Erhaltung der Kraft*, 70, in *WA 1*:66.

[cxxxi] .*Ueber die Erhaltung der Kraft*, 70, in *WA 1*:66; and see Kremer, “Thermodynamics of Life,” 248 (n. 148).

[cxxxii] .*Ueber die Erhaltung der Kraft*, 72, in *WA 1*:67.

[cxxxiii] .*Ueber die Erhaltung der Kraft*, 72, in *WA 1*:68.

[cxxxiv] .See Günter Bierhalter's essay, “Helmholtz's Mechanical Foundation of Thermodynamics,” in this volume.