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Conservation and controversy: Ludvig Colding and the imperishability of "forces"

Helge Kragh

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Centre for Science Studies, University of Aarhus, Denmark
Research group: History and philosophy of science

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HELGE KRAGH♦

The discovery of the principle of energy conservation is a classic case in the history of nineteenth-century physical sciences. Among the several scientists who have a legitimate share in the discovery process was the Danish physicist and engineer Ludvig August Colding, who is sometimes mentioned as a co-discoverer alongside the better known scientists J. R. Mayer and J. P. Joule. The present paper examines Colding's ideas of what he called the imperishability of forces. The focus is not so much on the content of Colding's works, which has been dealt with by other historians, as it is concerned with the national context of these works.

Colding got involved in a complex priority dispute in which he endeavoured, without much success, to establish himself as an independent discoverer and alternative to Mayer. Since Colding was from a scientifically peripheral country, it is relevant to ask to what extent, if any, this national context influenced the priority dispute and his legacy in the history of science. The paper also calls attention to a hitherto little noticed chemical aspect of Colding's work and its connection to the contemporary Danish chemist Julius Thomsen. The thermochemistry developed by Thomsen in the 1850s was not

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♦ Department of Science Studies, University of Aarhus, Denmark. E-mail: helge.kragh@ivs.au.dk.

only among the earliest chemical applications of the law of energy conservation, it is also likely that it influenced Colding's thoughts on the conservation of the forces of nature.

1. Energy conservation and its historiography

It goes without saying that the principle of conservation of energy, also known as the first law of thermodynamics, is one of the most fundamental laws of nature ever formulated. The law was discovered in the 1840s, but not by a single scientist and nor by following a single line of thought. Although Julius Robert Mayer in Germany and James Prescott Joule in England are recognized as the "true discoverers", it is only with hindsight that we recognize that they discovered the same thing; what is more, half a dozen or more scientists were involved in the discovery process, which is often discussed as a prime example of a so-called multiple discovery.

No wonder that this complex and confusing episode in the annals of science has appealed, and continue to appeal, to historians and philosophers of science. In fact, attempts to unravel the episode go back many years before history of science was established as an academic field. To mention but three names from the early period, it was analyzed in conceptual details by Max Planck in a prize essay of 1887, then again by Ernst Mach in 1896, and in 1909 the Austrian physicist Arthur Erich Haas published an even more detailed monograph on the subject.[1]

To jump ahead in time, in 1959 Thomas Kuhn published a most important work in which he considered no less than twelve more or less simultaneous scientists who all had a share, big or small, in the discovery of energy

[1] Planck 1913; Mach 1919; Haas 1909. All three authors mentioned Colding as an independent contributor to the law of energy conservation.

conservation. His main point was that by the early 1840s time was ripe for the discovery, which was, in a sense, due to the intellectual climate prevailing in the scientific thought of the period. Rather than being concerned with priority, the question Kuhn addressed and tried to answer was this: "Why, in the years 1830-50, did so many of the experiments and concepts required for a full statement of energy conservation lie so close to the surface of scientific consciousness?"[2]

The claim that energy conservation constitutes a proper example of simultaneous discovery was critically examined by Yehuda Elkana in a paper of 1970 and subsequently elaborated in his monograph of 1974, *The Discovery of the Conservation of Energy*.[3] Elkana traced the historical roots of the concept of energy, called attention to the confusion between the terms "force" and "energy", and in general emphasized the cross-fertilization of the *a priori* belief in a general conservation principle with the awareness that what is conserved must be expressible in mathematical terms. In accordance with these considerations, the German physicist and medical doctor Hermann von Helmholtz occupies a central position in Elkana's study.

The last historian I shall refer to in this brief review is Kenneth Caneva, who not only has written an authoritative biography of Mayer[4] but also dealt with aspects of the emergence of energy conservation in several articles of unsurpassed detail and sophistication. Among his works is a critical comparison of the concept of "force" in the works of H. C. Ørsted and L. A. Colding.

One point to note, and one which will be of relevance to what follows, is that whereas Kuhn (and to a lesser extent Elkana) considered the romantic ideas of German *Naturphilosophie* to have been important sources for the later law of

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[2] Kuhn 1959, p. 356.

[3] Elkana 1970, reprinted in Elkana 1974, pp. 175-197.

[4] Caneva 1993.

energy conservation, Caneva is much more sceptical. It is indeed possible to construct hypothetical and even plausible routes from the *Naturphilosophie* to energy conservation, but Caneva concludes that these routes do not match historical reality.[5] One thing is the belief of the *Naturphilosophen* in the "unity of forces", another is the concept of energy as it emerged in the early 1840s; the two are different and should not be confused. Rejecting Kuhn's suggestion, Caneva convincingly argues that *Naturphilosophie* cannot be unproblematically associated with the preoccupation with "forces" or their unity or interconvertibility. He finds no compelling evidence that *Naturphilosophie* played a significant role in either Mayer's theory of force or in Colding's somewhat similar ideas.

The aim of this paper is to examine the contribution made by the Danish physicist and engineer Ludvig August Colding to the principle of energy conservation. Colding was from a small country at the periphery of scientific Europe, while Mayer, Joule, and Helmholtz were from the large and central nations. I shall therefore discuss his case from a center-periphery perspective, which includes the priority controversy Colding got involved in. In addition, I briefly refer to a related controversy between Colding's friend, the chemist Julius Thomsen, and Marcellin Berthelot in France. In both cases I am interested in establishing whether the disputes were significantly influenced by the center-periphery asymmetry between the participating scientists.

2. Colding's road to force conservation

When 22-year-old Ludvig Colding enrolled at the Polytechnic Institute in Copenhagen in 1837, he came under the influence of Hans Christian Ørsted, the

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5 Caneva 1997a; Caneva 1997b, pp. 67-71.

famous physicist and discoverer of electromagnetism.[6] Ørsted served at the time as director of the Polytechnic Institute, an institution he had founded in 1829. Even before he graduated as a mechanical engineer in 1841, Colding engaged in speculations concerning the various forces of nature and the interrelationship between them. The starting point for these speculations had no connection to experimental physics and also they seem to have been unrelated to the discussions abroad. It it unclear to which extent, if any, they reflected Ørsted's natural philosophy and his often stated belief in the unity of forces. By Colding's own account, his original ideas were primarily of a metaphysical and religious nature, as he believed that the forces of nature expressed God's essence and consequently could neither be created nor annihilated.[7]

The religious element in the new concept of energy was particular strong in Colding, but it can be found also in other scientists at the time. Thus Joule believed that the conservation of forces was a sign of a divinely created universe, and Mayer considered the law to be a welcome weapon against materialism and atheism. Much the same message came from William Grove, the author of the widely read and influential *Correlation of Physical Forces* from 1846. In a lecture the following year, Joule emphasized how energy conservation maintained order

[6] For English literature on Colding and his work on energy physics, see Caneva 1997, Dahl 1963, and Dahl 1978. Dahl 1972 includes a biographical introduction and translations of Colding's works related to the principle of energy conservation. For reasons of convenience I quote from Dahl's volume rather than Colding's primary writings. See also Kragh et al. 2008, pp. 200-204, where Colding's work is placed within the context of the development of Danish science.

[7] According to Donald Cardwell, "the Danish engineer L. A. Colding had been led to the dynamical theory of heat through his considerations of the working of steam-engines" (Cardwell 1989, p. 229). This is however quite wrong. In 1852 Colding applied the dynamical theory to steam engines, but neither the steam engine nor other technology served as inspiration for his early work.

Ludvig August Colding (1815-1888)

in the universe: “Nothing is deranged, nothing ever lost, but the entire machinery, complicated as it is, works smoothly and harmoniously … the whole being governed by the sovereign will of God.”[8] Colding very much agreed.

On the occasion of being elected a member of the Royal Danish Academy of Sciences and Letters in 1856, Colding said: “My first thought concerning the imperishability of the forces of nature I have … borrowed from the view that the forces of nature must be related to the spiritual in nature, to the eternal reason as well as to the human soul. Thus it was the religious philosophy of life which led me to the concept of the imperishability of forces.” He elaborated by relating his thoughts to a kind of spiritual version of the Kant-Laplace nebular hypothesis. According to this version, what Caneva calls Colding’s teleological cosmogony,

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[8] Joule 1963, p. 273.

spiritual forces could not be separated from the forces operating in inorganic nature:

> We have the significant result that God, from the very beginning of the world, from the time He created the matter out of which the universal globes would form, and conceived the scheme which would govern this evolution, has provided all the forces by means of which the cosmic evolution will reach fulfillment; has provided them in their full generality and grandeur, yet in the highly elementary form of a general mutual attraction between material particles. We may conclude that both force and matter were inseparably linked from the very beginning; yes, so closely linked that one may insist that without force the material would dissolve in a void, just as surely as the reverse is true: that the forces could never have been assembled and evolved into independent forms of activity if matter had not yet existed; indeed, both were necessary for the evolution to take place.

According to Colding, the eternally preserved forces included not only the natural forces such as heat, chemical processes, electricity, and mechanical work, but also intellectual or mental activities. As he explained, this view led to the surprising conclusion that the the totality of natural forces (or what came to be known as energy) was actually in decline rather than staying constant:

> It is my contention not only that life in general demands its nourishment [from forces], but in particular that intellectual activity – the act of thinking – may also be viewed as work demanding nourishment, and I do not believe I am mistaken in expressing the view that it is the forces of nature,

> in their many forms, which serve as support for the intellect, and that intellectual activity evolves at the expense of these. … As the intellectual life evolves at a rapid pace, the abundance of forces of nature must be in a continuous decline, because the sum of all these forces is that invariable quantity originally created by God!

Colding ended his lecture of 1856 by formulating his credo in a manner which clearly reflected the influence of Ørsted: "We must never forget the law of nature which states that only that which is in accord with the soul in nature can persevere, while everything which offers resistance is perishable and must sooner or later be destroyed."[9]

It was presumably ideas along this line that Colding presented before Ørsted in 1840, in the hope that he would be able to announce his idea of the conservation of forces at the second meeting of the Scandinavian Association for the Advancement of Science that took place in Copenhagen this year. However, his admired teacher and mentor received the idea coolly and dissuaded Colding from making an announcement. Vague speculations were not enough, they had to be supported by solid experimental data, and consequently Ørsted advised Colding to conduct experiments on the heat produced by frictional motion. Having made such experiments, in 1843 Colding reported results that confirmed his preconceived conviction that "when we employ a motive force to overcome the resistance which a body experiences in sliding over other bodies of quite different nature, the heat evolved from the friction is strictly proportional to the

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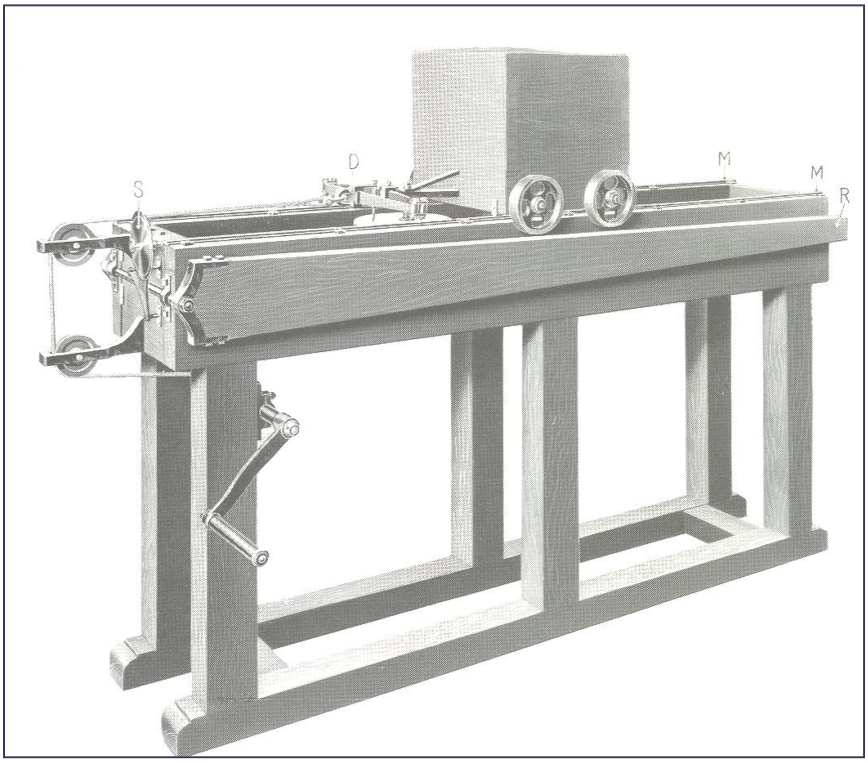
[9] Quotations from Colding, "Naturvidenskabelige Betragtninger over Slægtskabet mellem det aandelige Livs Virksomheder og de almindelige Naturkræfter" (Royal Danish Academy of Sciences and Letters, 1856), here from the translation in Dahl 1972, pp. 118, 125 and 127.

work expended."[10] However, he did not at that time calculate the mechanical equivalent of heat, which he only did in 1847 (with a result corresponding to 3.7 J/cal). Moreover, although he stated his results in a paper submitted to the Royal Danish Academy, it remained unpublished until 1856; and when it was eventually published, it was in Danish language only. In this memoir, originally dating from 1843, he concluded: "When a force seems to disappear, it merely undergoes a transformation and reappears in other forms. … [This thesis] we can assume to be universally valid for all forces."[11]



Colding's apparatus, constructed about 1844, which he used to measure the amount of heat produced by frictional force. The apparatus is on display at the Technical Museum in Elsinore, Denmark.

[10] Dahl 1972, p. 107.

[11] The paper on "Theses concerning force" (1843/1856) is translated in Dahl 1972, pp. 1-18. Quotation on p. 13.

Improved and more elaborate experiments were subsequently carried out by Colding, who reported his new results to the 1847 meeting of the Scandinavian Association in Copenhagen. The apparatus constructed by Colding is shown in the figure. His detailed account was published in two papers in the proceedings of the meeting, which however only appeared in 1850 and because of its language remained unknown to most foreign scientists. His paper "On the universal forces of nature and their mutual dependence" eventually appeared in English translation in *Philosophical Magazine* (vol. 42, pp. 1-20), but this was as late as 1871, at a time when it was of historical interest only. Whereas Colding was unaware of the works of Mayer and Joule in 1843, he briefly alluded to them in footnotes to one of his papers of 1850, which also included a reference to Helmholtz's important memoir "Ueber die Erhaltung der Kraft" from 1847. His reference to Mayer was to a paper of 1845, which probably indicates that at the time he was not yet acquainted with Mayer's earlier paper of 1842, the one upon which his claim of priority rested.

Given that Ørsted had for decades been nearly obsessed with the romantic idea of a fundamental unity of forces, it may appear surprising that he failed to appreciate Colding's somewhat similar ideas of the imperishability of forces. But Ørsted not only realized the weak scientific foundation of Colding's thesis, it is likely that he also considered force conservation in Colding's sense to be opposed to the ideas of *Naturphilosophie* to which he remained committed throughout his life. Ørsted was a firm believer in universal and invariable laws of nature, and he may have found it impossible to harmonize this belief with conservation of forces. If forces were both conserved and governed by law, what room could there be for the freedom and spontaneity of the "Soul in Nature"? What room could there be for free will and personal fulfillment? The consequence might seem to be a causal and deterministic scheme of nature where even the spiritual forces were predetermined. There is no solid documentation that Ørsted thought in this way, but the hypothesis is not unreasonable.[12]

By 1857 Colding had been appointed engineer-in-chief to the city of Copenhagen, a position with heavy responsibilities that left him little time for continuing his scientific interest in the new field of thermodynamics. Yet he managed to do some research and write papers on the theory of the steam engine and the use of thermodynamics in meteorology. In 1864 he wrote his last paper on the discovery of the principle of energy conservation, a summary account that appeared in the *Philosophical Magazine* and which will be considered below. This paper, and also the earlier one of 1856, was to a considerable extent concerned with the priority issue which at that time had become a prominent theme.

3. Priority disputes

As the law of energy conservation became recognized as a cornerstone of scientific theory, interest in its discovery history increased. Who was principally to be praised for this revolutionary insight? Dissatisfied with being ignored in the international literature, in 1856 Colding decided to claim his share of the discovery, which he thought he could do by devaluing the contribution of Mayer. Since Mayer's paper had appeared one year earlier than his own paper of 1843 – and this was only published 1856 – he could not claim priority on reasons of chronology. Instead the strategy of his priority claim was to argue that Mayer had not reached an insight comparable to his own.

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[12] Dahl 1972, p. xxvii; Marstrand 1929, p. 21. However, Caneva denies the hypothesis and argues that it was Ørsted's concept of force that separated him from Colding's ideas. Although Ørsted and Colding both spoke of the different forms that the forces of nature can assume, they attached different meanings to the words (Caneva 1997a, p. 53).

Colding charged that Mayer's paper in the *Annalen der Chemie und Pharmacie* was obscure and without "any evidence or experiments whatsoever." According to Colding, the German physician "has not even conducted experiments of his own to support his basic idea, but confined himself to extremely vague observations."[13] Contrarywise, his own work of 1843 was claimed to be clear and experimentally based, and therefore superior to Mayer's: "My interpretation of the relationship [between forces] is more correct and better founded than the one which Dr. Mayer has elaborated in his first account the year before."[14] It is to be noted that Colding's claim of priority was exclusively directed against Mayer. Although he criticized some of the details in the work of "the excellent experimenter M. Joule", he fully recognized him as an independent discoverer of the law of energy conservation.

Priority also figured prominently in the paper Colding wrote to *Philosophical Magazine* in November 1863 and in which he offered his version of the discovery history. He had finally recognized that "the Danish language is so little understood beyond the Scandinavian countries" that it was pointless to express his priority claim in Danish only. What he needed was to make it known in either German, French or English. At that time few Danish scientists had command of English or close contact with British science; nor did they follow scientific literature in English language. It is telling that Colding introduced his paper by noting that "the libraries of Copenhagen do not contain the Philosophial Magazine and Journal of Science."[15] In his earlier papers he included numerous references to books and articles in German and French, but

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13 Dahl 1972, p. 110 and p. 113.

14 Ibid., p. 109.

15 Dahl 1972, p. 159. Colding 1864. Characteristically, when Colding first came to know about Joule's work it was not through English journals but from Poggendorff's *Annalen der Physik und Chemie* (ibid., p. 64).

not a single one to sources written in English. So why did he choose to communicate his historical account to an English journal? There can be little doubt that he hoped in this way to get a more sympathetic hearing, since some British scientists were at the time interested in raising the status of Joule at the expense of that of Mayer.

After having presented an account of his works of 1843-51, Colding suggested that the *Philosophical Magazine* included an English translation of his first memoir (however, this did not happen). In that case it would be easier to compare his own contribution and the one that Mayer published in 1842. According to Colding, in this work Mayer was unable to support his thesis "by a single experiment, or by anything like a proof of their exactness; whilst I thought it to be my duty, before I wrote, to prove that my suppositions concerning the forces were confirmed by nature itself as a law of nature."[16]

Although Colding did not succeed in replacing Mayer as the discoverer of energy conservation, his efforts were not completely in vain. In an important lecture delivered in 1854 at Königsberg, Helmholtz referred to "a Dane named Colding" who in 1843 had presented a memoir "in which the same law [as the one stated by Mayer] was found, and some experiments were described for its further corroboration."[17] This was the first reference to Colding outside Scandinavia. In a later letter to Peter G. Tait, Helmholtz placed Colding, together with Mayer and the French engineer Marc Séguin, among the predecessors of the law of energy conservation (which he attributed to Joule and, surprisingly, William Thomson).[18] Also the French physicist Marcel Émile Verdet, a specialist

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[16] Dahl 1972, p. 166.
[17] "Ueber die Wechselwirkung der Naturkräfte," translated in Helmholtz 1995, pp. 18-45, on p. 27. It is unknown how Helmholtz got to know of Colding's work, which at the time had not been published.
[18] For Helmholtz's letter, see Tait 1877, p. x.

in optics and thermodynamics, was aware of Colding's work and ready to place him as co-discoverer of the law, in his case alongside Mayer and Joule.

In a lecture before the Société Chimique de Paris in February 1862, Verdet included a historical section in which he presented Mayer, Colding and Joule as the three independent discoverers of the law of energy conservation. "Without knowing each other they arrived at the same time and roughly in the same manner to the same thoughts," he wrote. As to Colding, his works "suffice to secure him a name among the inventors of the new theory." But Verdet also noted that Colding's works were written in Danish and published several years after they had been presented orally, for which reasons "they have exercised almost no influence upon the further developments of science."[19] Colding was aware of Verdet's lecture and in his English paper of 1864 he pointed out that "M. Verdet and M. Helmholtz think it right, according to what is known to them of my investigations, to place me next to M. Mayer in relation to the discovery of the new principle touching the forces of nature."[20] This was a place with which he was not quite satisfied. Having noticed the article in *Philosophical Magazine*, Verdet immediately translated it into French and had it published in the *Annales de chimie et de physique*.[21]

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[19] Verdet did not reveal from where he knew of Colding's work. His lectures, *Exposé de la théorie mécanique de la chaleur*, were published separately by the Société Chimique de Paris in 1862. They are included in Verdet 1868, pp. vii-cxlviii (quotations from pp. xcvi-xcvii). See also Verdet's letter of 6 August 1866 to Tait, quoted in Knott 1911, p. 210. Joule probably became aware of Colding's work through Verdet's lecture (see Lloyd 1970, p. 215).

[20] Dahl 1972, pp. 159-160.

[21] The reference is to L. A. Colding, "Lettre ... sur l'histoire du principe de la conservation de l'énergie," *Annales de chimie et de physique* **1** (1864), 466-477. Verdet left out the final part, in which Colding emphasized the superiority of his own work relative to that of Mayer.

Another Frenchman who mentioned and appreciated Colding's research was the Alsatian engineer Gustave-Adolphe Hirn, who in the 1850s did important work on the dynamical theory of heat and in 1862 published a comprehensive textbook on the subject. He gave credit for formulating the principle of energy conservation to the usual triplet – Joule, Colding, and Mayer – but rated Colding's contributions higher than Mayer's.[22]

Colding found his best ally in the eminent Scottish physicist Peter Guthrie Tait, a close friend of William Thmson, although Tait's interest was not so much to defend the obscure Dane as it was to secure the priority of his compatriot Joule. In the 1860s and 1870s there raged in Great Britain a controversy primarily between Tait and the Irish-born physicist John Tyndall, who in 1862 had praised Mayer's work and defended his claim to be judged as the true originator of the principle of energy conservation.[23] This aroused the chauvinistic feelings of Tait, who in several articles and books argued in favour of Joule's priority. Also Balfour Stewart, professor of physics at Owens College in Manchester and a close friend of Tait, spoke out in Joule's favour. Stewart placed Mayer and William Grove as philosophical predecessors of the law of energy conservation. He also mentioned Séguin, but not Colding. "Nevertheless, to Joule belongs the honour of establishing the theory on an incontrovertible basis: for, indeed, this is preeminently a case where speculation has to be tested by unimpeachable experimental evidence."[24] The controversy between Tyndall and Tait (with the latter supported by Thomson) was concerned with the two principal candidates, Mayer and Joule, but indirectly it left room for Colding as well. Quite simply, Tait could use Colding.

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[22] Hirn 1862, p. 138.
[23] Tyndall 1862. On the controversy, see Lloyd 1970.
[24] Stewart 1873, p. 140.

For example, in a semipopular book on thermodynamics Tait referred to Colding, who after his unfortunate beginning in metaphysics "evidently went to work in the right way, and deserves an amount of credit to which Mohr, Séguin, and Mayer have no claim."[25] In another book, *Recent Advances in Physical Science*, he gave a fuller account of the history of thermodynamical thought, including evaluations of the various contributors to the field. He dismissed Mayer as a dreamer and "speculator", whereas he considered the 1843 papers of Joule and Colding to be based on "sound methods." In Tait's view, "The true modern originators and experimental demonstrators of the conservation of energy in its generality were undoubtedly Colding of Copenhagen and Joule of Manchester."[26] This does not mean that the two were of equal importance, for the Danish scientist Colding was far from comparable to Joule, the eminent British natural philosopher. Still, referring to the Dane, "he stands enormously high in comparison with any of the others who have experimented up to that time upon the conservation of energy."[27] Tait took pleasure is quoting Colding's low appreciation of Mayer, with which he evidently agreed.

Colding got a chance to meet with Tait in 1871, when he attended the forty-first meeting of the British Association for the Advancement of Science in Edinburgh. He was at that time engaged in work in hydro- and aerodynamics, on which subject he had recently published a lengthy paper in the proceedings (*Skrifter*) of the Royal Danish Academy. At the meeting of the British Association he participated in the section on meteorology, offering some remarks on aerial currents. While in Edinburgh, he was awarded an honorary doctorate at the University of Edingburgh (where Tait was a professor). Joule, too, was in

[25] Tait 1877, p. 23.
[26] Tait 1876, p. 56.
[27] Ibid., p. 57.

Edinburgh to receive the same degree, and thus the two pioneers of energy conservation met for the first and last time.

In connection with the Edinburgh meeting, Tait arranged to have one of Colding's papers of 1850 translated in the *Philosophical Magazine*, where it appeared in 1871 as "On the universal powers of nature and their mutual dependence". In a note appended to the paper, Tait commented: "There are other papers by Colding, of at least equal importance, which may also appear in English."[28] Indeed, Tait's high regards of the Danish engineer-physicist made him arrange also for a translation of an abridged version of Colding's recent work on hydrodynamics. Introducing the paper in *Nature*, he praised "the genial Dane" and stated that "everything written by such a man is deserving careful attention."[29]

The hydrodynamical work that Colding published in 1870 was, as usual, in Danish, but this time followed by a detailed résumé in French. It was this résumé that Tait had translated. After much discussion, in 1867 the Royal Danish Academy had agreed that a résumé in French could be appended to the publications in its proceedings. It is noteworthy that the chosen language was French, rather than German, but this was politically motivated: Anti-German feelings ran high after the Danish-Prussian war of 1864 and the painful loss of southern Jutland to the Germans.[30]

The critique that Colding launched against Mayer's work was curious in the sense that it was communicated in Danish and for this reason unknown to the German physician and scientist. If Mayer was aware of the critique – and one may assume that he was, at least after the 1864 paper had appeared in both

[28] *Philosophical Magazine* **42** (1871), 1-20. The paper is reprinted (with a slightly altered title) in Dahl 1972, pp. 47-67.
[29] Colding 1871.
[30] Pedersen 1992, p. 217.

English and French – he chose not to reply. For this reason it is not really appropriate to speak of the incidence as a "Colding-Mayer controversy."[31]

4. Local contexts

Was it a disadvantage for Colding to come from a small and peripheral country? To some extent it was, and especially with regard to the language. As we have seen, until 1864 Colding only communicated his work in Danish, which was a major reason why it remained unnoticed to the larger part of the international scientific community. His target audiences were the Royal Danish Academy of Sciences and Letters and the Scandinavian Association for the Advancement of Science (founded 1742 and 1839, respectively), both of which organizations had as their policy that addresses and publications should be in the local language. There is no doubt that this policy was detrimental to the internationalization of Danish science. On the other hand, Danish physicists and chemists were free to publish in foreign scientific journals, and in fact often did so. German periodicals were by far the most favoured, and in the years about 1850 they received contributions from almost all Danish natural scientists.[32] It is hard to explain why Colding did not communicate his work to the *Annalen der Physik und Chemie* or some other German journal.

One might imagine that Colding's work and priority claim was given both positive attention and support in his own country, but this is not really the case.

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[31] It is generally agreed that a disagreement, in order to count as a controversy, must involve opposing views by at least two scientists and also be an activity taken seriously by substantial parts of the relevant scientific community. See McMullin 1987. The case of Mayer was defended by Tyndall in England and by Eugen Dühring, Karl Friedrich Zöllner, Rudolf Clausius and others in Germany. None of them seem to have responded to the challenge of Colding.

[32] See the table on p. 252 in Knight and Kragh 1998, listing chemical publications of Danish scientists according to language.

Only few Danish scientists commented on his work on the conservation of forces, and no-one unambigously supported his claim of having arrived at a correct understanding of energy conservation prior to Mayer. His younger friend, the chemist and polytechnic candidate Julius Thomsen, was a pioneer in thermochemical studies and the first to incorporate this area of research under the general framework of energy conservation. Interested in the new field of thermodynamics, in 1855 he published a popular survey on the correlation and conservation of forces based on Helmholtz's Königsberg lecture the year before. In this survey he admitted Sadi Carnot as an important precursor of the new science of heat and work. But Thomsen argued that the law of energy conservation owed its existence to four scientists who had reached the insight each in their own way and independently of Carnot's pioneering work: "J. R. Mayer in Heilbronn, A. Colding in Copenhagen, and Joule in England started their works on this subject at about the same time; and some time later, again apparently independently of the others, it was treated by Helmholtz in Königsberg."[33] Although giving Colding credit for his work, Thomsen did not highlight his contribution.

After Ørsted's death in 1851, his pupil Carl Valentin Holten had become professor of physics at the University of Copenhagen, the country's only university. I am not aware of the relationship between Holten and Colding, but

[33] Thomsen 1855, p. 236. This essay is also of interest because it includes the first reference in Danish to the notorious "heat death", the prediction based on the second law of thermodynamics that in the far future the entire universe will decay to a lifeless high-entropic state in which no order or activity exists. There will come a time, Thomsen said, when all the forces of nature have been transformed into a uniformly distributed soup of heat. "Then any source of change will be extinguished and a complete cessation of all natural processes will have occurred. Of course, plants and animals can no longer exist; the Sun will have lost its higher heat and then also its light. And all the constituents of the surfaces of the globes will have entered into those compounds that correspond to their nature"(p. 240).

in 1868 Holten gave an address in which he dealt with the law of energy conservation, including its history. He pointed out the importance of Colding's work, but without ascribing it priority over the earlier work of Mayer and the slightly later by Joule. Without referring explicitly to Colding, he took exception from the idea that material and spiritual forces were equivalent, in the sense that intellectual work takes place at the expense of material forces. This was Colding's view, but not Holten's: "The spirit itself, the soul and its immortality, is completely outside science; it cannot be an object of scientific research." Contrary to Colding, who was an "expansionist", Holten favoured a "restrictionist" view: "Every time we attempt to transgress the domain of science, we end up in insoluble contradictions. It is not up to science to evaluate God and his actions, nor the power of human reason to explore these. The purpose of science is solely to understand nature in its entirety, in its conformity with reason."[34] Holten's attitude was the one of the new science, while Colding's belonged to the past, both in a Danish and an international context.

Colding's religious metaphysics of forces, as expounded in his 1856 memoir to the Royal Academy, also caught the critical attention of Rudolph Varberg, and that in a more polemical way. A journalist and writer, Varberg spoke out in favour of such controversial issues as materialism, atheism and Darwinism. While he found Colding's priority claim to be "personally and uninteresting", he felt provoked by his claim to have demonstrated the immortality of the soul on a scientific basis. This claim Varberg vehemently denied, pointing out various weaknesses and inconsistencies in Colding's arguments. As Varberg saw it, a consistent version of Colding's metaphysics would leave no place for a God interfering in either the material or the spiritual

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[34] Holten 1868. On "expanisonism" and "restrictionism" as attitudes to science, see Graham 1981.

world: “His God becomes throughout a deistic God which in the beginning created everything – the nebula and the universal attraction – and since then has done nothing at all, not even as a clockmaker who has manufactured an artificial clock and from time to time must repair his creation.”[35] Colding did not respond to Varberg’s attack.

As there was no organized attempt in Denmark to upgrade Colding as the true discoverer of energy conservation during his lifetime, so there were no attempts to do it at later occassions. But of course his important contribution remained alive also after 1888, the year of his death. Holten’s successor as professor of physics, Christian Christiansen, was a successful writer of textbooks of physics, including the *Lærebog i Fysik* in two large volumes (1892-94). In his detailed account of energy conservation – now “energy” had finally replaced “force” or “power” – he highlighted the work of Colding and quoted extensively from it.[36] Although he did not enter the priority issue, it is conspicuous that Mayer received much less space than Colding. Mayer was merely mentioned because he was the first to determine the mechanical equivalent of heat. While Christiansen appreciated the value of Colding’s work, he also realized its limited significance. On the occasion of the centenary of Colding’s birth in 1915, he said that “we must regretfully admit that his activity did not have any essential significance for the advance in our understanding of nature.”[37]

The basic reason why Colding’s work was so little known in the period from about 1840 to 1865 was not that that he came from a peripheral country. His unwise decision to publish in his mother tongue was much more important. It should be recalled that most of the contributors to the earliest phase of what

[35] *Dansk Maanedsskrift*, October 1857, reproduced in Varberg 1868, pp. 17-58, on p. 58.
[36] Christiansen 1892, pp. 135-138.
[37] Christiansen 1916, p. 100.

became the law of energy conservation were "peripheral" – not in the sense that they came from countries in Europe's scientific periphery, but in the sense that they were "internally peripheral". With this term I refer to the notion that the center-periphery concept need not relate to different countries, but may also include differences within a single country, either geographically or professionally.

The pioneers of energy physics in the 1830s and 1840s were young and little known, if known at all, to the scientific community, and many of them were amateurs or at least unrecognized as scientists. They were all essentially outside the academic community of scientists and encountered indifference (rather than opposition) from physicists before their ideas won recognition. In this respect, Colding was no exception.

5. A chemical digression

Colding was not a chemist, but of course he realized the importance of chemical phenomena for the thesis of force conservation. Thus, in the second of his papers to the Scandinavian Association in 1847, he referred to galvanic processes and thermochemical measurements, including the so-called Hess' law, so named after the Swiss-Russian researcher Germain Henri Hess. According to this law, also known as the law of constant heat summation, the amount of heat developed in a chemical reaction is constant, regardless of whether the reaction proceeds directly or through a number of intermediate steps.[38] Colding's interest and knowledge of chemistry probably increased as a result of his friendship and collaboration with Julius Thomsen. In 1852, the same year as Thomsen published

[38] In his original formulation of 1840, Hess based his law on the caloric conception of heat. In his classical work on energy conservation (*Über die Erhaltung der Kraft*, 1847), Helmholtz pointed out that Hess' law follows as a direct consequence of the general principle of energy conservation.

his new system of thermochemistry, he and Colding collaborated in a scientific analysis of the probable causes for the cholera epidemic which stroke Copenhagen that summer.[39] I suspect that Thomsen's influence was in part responsible for an interesting but unnoticed section on chemistry that Colding included in his review of 1856.

Concerning thermochemistry and the forces of affinity, Colding wrote that "no one has worked with greater success in this direction than our countryman Polytechnic Candidate Julius Thomsen, as he has not only determined the strength of the chemical power for many elements and their combinations ... but in addition has laid the groundwork for the mathematical treatment of chemistry."[40] The dream of a mathematical or Newtonian chemistry based *a priori* on the laws of nature was an old one, and it would continue to be pursued throughout the century, indeed into the next one as well. In his *Metaphysische Anfangsgründe* of 1786 Immanuel Kant had famously argued that chemistry could never be a genuine science because its subject matter was intractable to the method of mathematization and systematic deduction from higher principles.[41] But Colding believed that with the new theory of force or energy conservation the dream was about to become a reality. We can, he wrote,

> ... anticipate that it will not be long before we, with the aid of mathematics, will probe the smallest constituents of matter with the same clarity and confidence which enabled us to peer out and survey the conditions existing in the boundless universe. Soon the time will come when the chemist, by way of mathematical formulae and computations,

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[39] Colding and Thomsen 1853.
[40] Dahl 1972, p. 116.
[41] Kant 2004, pp. 6-7. See also Gregory 1984.

> will be able to predict in advance the results of experiments in the laboratory; yes, I believe it is not an overstatement to say that we have no idea to what height science will reach in this direction, and all this is based on the law of nature which states that the forces of nature are imperishable.[42]

The belief in a chemistry of the near future based on "mathematical formulae and computations" continued to occupy the minds of later researchers within the tradition of physical chemistry. Some of them believed with Colding that energy conservation and other parts of the general theory of thermodynamics were the methods to build on if chemistry were to turned into a mathematical science. Examples of this aborted genre are provided by Georg Helm's *Grundzüge der mathematischen Chemie* from 1894 and Johannes van Laar's *Lehrbuch der mathematischen Chemie* from 1901. Of course, a proper foundation of a mathematical and computational chemistry had to await the coming of quantum chemistry and its transformation into computational quantum chemistry by means of computers in the period after World War II. But this is a digression.

The optimistic hope expressed by Colding was shared by Thomsen, whose reform of thermochemistry rested on calculations based on the principle of energy conservation. The ambition of his thermochemical theory of 1852-54 was to determine the absolute value of chemical forces by means of thermochemical measurements and thus supply the vague concept of affinity with a quantitative and operational meaning. Thomsen's notion of affinity as a force given by the amount of evolved heat was to be found also in the rival thermochemical system of Marcellin Berthelot in France. The result was an embittered controversy that

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[42] Dahl 1972, p. 116.

lasted for more than two decades. This controversy between a scientist from a peripheral country and one from a central country differed in several respects from the case of Colding versus Mayer. Since it has been thoroughly examined in the literature, I shall not deal further with it.[43]

There is however one point that deserves mention, namely the problem of language in communications related to a scientific controversy. As pointed out, Colding's decision to publish in Danish greatly diminished his international visibility and also his position in the debate concerning the priority of the law of energy conservation. Thomsen's publication strategy was entirely different from Colding's and much more effective. From the very beginning he published parallelly in Danish and German, typically in the proceedings of the Royal Danish Academy and in Poggendorff's *Annalen*. A prolific author, Thomsen published about 270 works, more than half of which were in German. This was a publication strategy that put him, a citizen of a country at the scientific periphery, in a much stronger position in the controversy with the powerful Berthelot.

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[43] Kragh 1984; Dolby 1984.

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University of Aarhus
Ny Munkegade 120, building 1520
DK-8000 Aarhus C
DENMARK
www.css.au.dk

Faculty of Science
Department of Science Studies
AARHUS UNIVERSITET