The Form and Function of Scientific Discoveries

by

Kenneth L. Caneva

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Hans Christian Ørsted portrait is courtesy of Danmarks Tekniske Museum, Helsingør; Thomas Johann Seebeck and Johann Wilhelm Ritter portraits are courtesy of the Deutsches Museum, Munich, Germany.

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Kenneth L. Caneva

Dibner Library Lecture

November 16, 2000

Smithsonian Institution Libraries

Washington, DC


David Dibner (left) and Kenneth L. Caneva

Kenneth L. Caneva is a Professor in the Department of History, University of North Carolina at Greensboro. His publications include *Robert Mayer and the Conservation of Energy* (Princeton: Princeton University Press, 1993) and more than 30 articles, reviews, essays, and commentaries. Caneva earned his Ph.D. at Princeton University, where his principal work was in nineteenth-century physics. He was a Fulbright Fellow at the Technische Hochschule in Munich, Germany for a year, and has received a number of awards and honors. He was awarded a grant for the Smithsonian Institution Libraries Dibner Library Resident Scholar program in Washington, D.C. and has been a research fellow at the Dibner Institute of the History of Science and Technology in Cambdrige, Massachusetts.

David Dibner is President of The Dibner Fund.

Foreword

The Dibner Library Lectures contribute immeasurably to the Smithsonian Libraries' efforts to acquaint the larger public with the valuable materials in the Dibner Library of the History of Science and Technology, and how they are used by scholars. The creative research done by these lecturers and all users of the Dibner Library speak clearly and unmistakably to the continuing usefulness of the volumes Bern Dibner collected and donated to the nation in his 1976 gift to the Smithsonian. This is the tenth lecture in the Dibner Library Lecture Series, supported by The Dibner Fund, and the second to be published.

Kenneth L. Caneva delivered this lecture on "The Form and Function of Scientific Discoveries" in November 2000 and prepared it for the present publication with notes and bibliography. We first met Professor Caneva in 1995 when he was selected to be a Smithsonian Libraries Dibner Library Resident Scholar in the early years of that program. Caneva is one of twenty-five scholars who have benefited from this program also generously supported by The Dibner Fund since 1992.

The cover image is a well known illustration of science represented as a ship, boldly sailing beyond the Pillars of Hercules, the emblematic limits of the old world. Appearing on the title page of Francis Bacon's *Novum Organum* (1620), the image epitomizes the spirit of scientific inquiry that forms the basis of Professor Caneva's observations about the experiments and discoveries of Hans Christian Ørsted, Johann Wilhelm Ritter, and Thomas Johann Seebeck, the subjects of his lecture. Bacon's eminence as a philosopher of science was recognized by Bern Dibner, the creator of the Dibner Library of the History of Science and Technology, who included Bacon in his galaxy of 200 pioneers in the history of science and technology, now celebrated as the "Heralds of Science."

We thank The Dibner Fund for supporting the lecture series, its publications, and the successful resident scholar program.

Nancy E. Gwinn
Director
Smithsonian Institution Libraries
May 2001

I believe that our observations so far have shown clearly that this incongruence between an idea as experienced retrospectively and the description given by the "originator" himself … can be explained simply by the fact that the true creator of a new idea is not an individual but the thought collective. As has been repeatedly stressed, the collective remodeling of an idea has the effect that, after the change in thought style, the earlier problem is no longer completely comprehensible.

—*Ludwik Fleck*[1]

[D]iscovering a new sort of phenomenon is necessarily a complex event, one which involves recognizing both *that* something is and *what* it is. Note, for example, that if oxygen were dephlogisticated air for us, we should insist without hesitation that Priestley had discovered it, though we would still not know quite when. But if both observation and conceptualization, fact and assimilation to theory, are inseparably linked in discovery, then discovery is a process and must take time.

—*Thomas S. Kuhn*[2]

[1] Fleck 1979, 123.

[2] Kuhn 1962b, 55; *cf.* 1962a, 762 = 1977, 171. It is worth pointing out that Kuhn's notion of extended discovery is still essentially an individualistic process, and does not pay regard to the kind of extended collective restatement that is the subject of this paper. I defer a more extended discussion of this issue to another occasion.

Dramatis Personae

Chorus

Fleck, Ludwik (1896-1961)

Kuhn, Thomas S. (1922-1996)

Main Characters

Ørsted, Hans Christian (1777-1851)

Ritter, Johann Wilhelm (1776-1810)

Seebeck, Thomas Johann (1770-1831)

Minor Players

Ampère, André-Marie (1775-1836)

Becquerel, Antoine-César (1788-1878)

Becquerel, Edmond (1820-1891); son of the foregoing

Brücke, Ernst (1819-1892)

Daguerre, Louis-Jacques-Mandé (1789-1851)

Eisenlohr, Wilhelm Friedrich (1799-1872)

Esselbach, Ernst (1832-1864)

Fraunhofer, Joseph (1787-1826)

Helmholtz, Hermann (1821-1894)

Herschel, John Frederick William (1792-1871)

Herschel, William (1738-1822); father of the foregoing

Keferstein, Christian (1784-1866)

Mascart, Éleuthère-Élie-Nicolas (1837-1908)

Melloni, Macedonio (1798-1854)

Moser, Ludwig Ferdinand (1805-1880)

Mousson, Albert (1805-1903)

Schelling, Friedrich Wilhelm Joseph (1775-1854)

Stokes, George Gabriel (1819-1903)

Thomson, Thomas (1773-1852)

Tilloch, Alexander (1759-1825)

Tyndall, John (1820-1893)

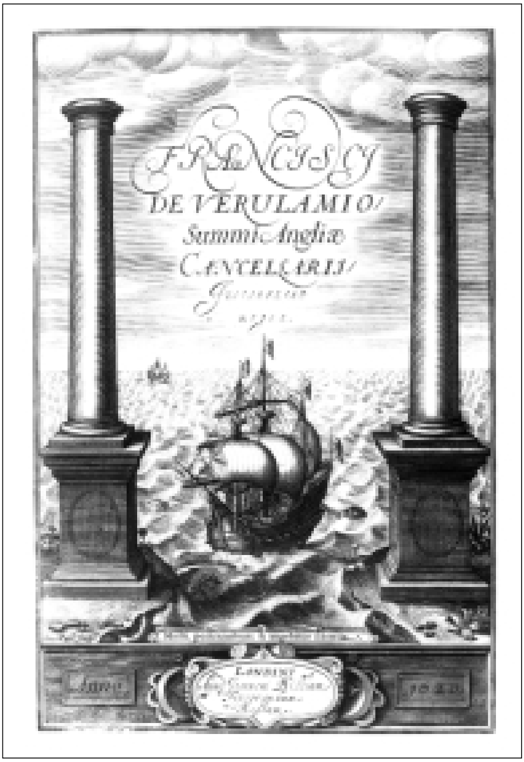
Whewell, William (1794-1866)

Wollaston, William Hyde (1766-1828)

Yelin, Julius Conrad von (1771-1826)

The *Novum Organum* by Francis Bacon (1561-1626) was selected by Bern Dibner as one of his 200 Heralds of Science. The caption for this illustration in Dibner's book, *Heralds of Science* (1955, 1980), reads

> "[Herald] No. 80. The great scientific societies of Italy, England, and France were founded within a few years of each other. Francis Bacon, sensing the bold spirit of the age, pictured science as a ship venturing beyond the Pillars of Hercules, the limits of the old world."



The Form and Function of Scientific Discoveries

Kenneth L. Caneva

Dibner Library Lecture, Smithsonian Institution Libraries
November 16, 2000

In the course of my work over the last twenty-five years, I have repeatedly noticed that what a scientist is typically credited with having discovered often differs significantly from the way in which the scientist himself characterized his work.

For example, everyone knows that Danish physicist Hans Christian Ørsted (1777-1851) discovered electromagnetism in 1820. Understanding electromagnetism to be the interaction between electricity and magnetism, I was startled, a few years ago, to discover that Ørsted had intended the term—which he introduced—to mean something much different, indeed something quite foreign to the way we now view things.[1]

One of the many researchers inspired by Ørsted's work was the Berlin academician, Thomas Johann Seebeck (1770-1831), whom the world of science and scholarship honors as the discoverer of thermoelectricity.[2] Hence in studying German work in electricity during the first half of the nineteenth century many years ago, I was startled to find that Seebeck's well-known discovery of thermoelectricity in 1821 did not correspond to what he himself thought he had found.[3] Failing to pay due attention to the significance of the title of his collective memoir, "On the Magnetic Polarization of Metals and Ores by Means of Temperature Difference," I followed others in mistakenly thinking that what Seebeck had thought he discovered was thermomagnetism. Although there is some truth to that, the full story is more complicated.

In a third and still more complicated case, a study of the work of

[1] Caneva 1998a, 82-90. I intend on giving a much more detailed and extensive treatment of the topic of this lecture in a later paper.

[2] Examples of early and modern canonical characterizations include Breger 1999, 30; Hermann 1971-1972, 2, 346 (by Lothar Suhling); Jungnickel and McCormmach 1986, 44; Ostwald 1896, 379; Siegel 1983, 413; Whewell 1837, 3, 889-90; Whittaker 1951, 88-89. Even Nielsen's excellent account of Seebeck's work was headed "The discovery of thermoelectricity" (Nielsen 1991, 363-395; *cf.* 360). Tyndall (1873, 141) reported that "Thomas Seebeck, of Berlin, discovered that electric currents might be derived from heat," which gave rise to the construction of the "thermo-electric pile." John Herivel wrote that Seebeck showed "that temperature differences could produce electric currents" (in Williams 1982, 411).

[3] Caneva 1974, 125-126; 1978, 92.

German romantic physicist Johann Wilhelm Ritter (1776-1810), I came from the secondary literature with the naive idea that among Ritter's lasting achievements was his discovery of ultraviolet light. Even knowing something of Ritter's indebtedness to Schelling's *Naturphilosophie* I was startled to discover the overwhelming otherness of the terms in which he reported his findings, embedded as they were in a wide-ranging web of polarities.[4]

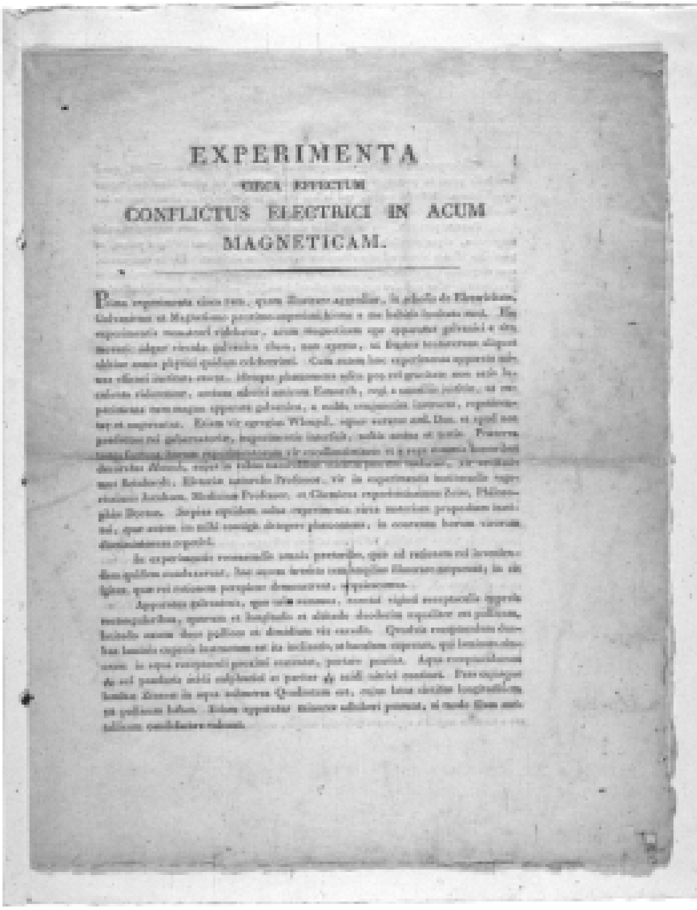
For the most part I tended to see such instances as examples of careless historiography, of the distortions of a story rewritten from a later perspective. My thinking began to change as I noticed that it wasn't just that historians were guilty of retrospective distortion, but that such recastings were an *intrinsic* part of the scientific enterprise. More recently, I have come to see how such instances can be exploited as examples of how what is generally accepted as scientific knowledge is essentially the outcome of a process by which knowledge is reshaped as it passes through the hands of people with different agendas using different language. Such a 'discovery' is not an atomized contribution to knowledge that others need merely recognize and accept, but rather represents a retrospective characterization coming at the end of a complex process of transformative negotiation. That characterization simultaneously formalizes the essential character of the discovery and confers upon it the stamp of objectivity *as* an aspect of the physical world that was there waiting to be 'discovered.' Before developing these interpretive aspects any further, let me pass in review some of the particulars upon which my interpretation rests.

Ørsted and electromagnetism

The electromagnetism that Ørsted is universally credited with having discovered is—to use an older vocabulary—typically understood to consist in the interaction between magnetism and so-called voltaic or galvanic electricity, or, more phenomenologically, in the interaction between the connecting wire of a voltaic pile and a magnetic needle.[5] (*Figure* 1.) That was also the way his discovery was characterized by the commentators in Paris and Geneva who immediately and consequentially followed up on his work. So, too, in England. In Germany the situation was more complicated. For the most part, however, Ørsted himself had a

[4] Caneva 1997, 44-47.

[5] Examples of modern canonical characterizations include Muir 1994, 388; Porter 1994, 520; Root-Bernstein 1989, 136; Taylor 1941, 631; Whittaker 1951, 88; Williams 1982, 396 (by John W. Herivel). *Cf*. Breger 1999, 28: "Ørsted's discovery of the magnetic field of the electric current in 1820 was a convincing great success for the romantic program of research. . . . In the terminology of the time, which did not yet know the field concept, Ørsted had transformed electricity into magnetism." The anachronism of Breger's first sentence is obvious; the inappropriateness of his second claim is evidenced in Caneva 1998a, 54-55, 66, 77, 79-80, 110, 113-114, where the distinction is noted between the *transformation* of forces and the *calling forth* of one force by another typically spoken of by Ørsted.



EXPERIMENTA
CIRCA EFFECTUM
CONFLICTUS ELECTRICI IN ACUM
MAGNETICAM.

Figure 1. First page of the pamphlet in which Ørsted announced his discovery of electromagnetism (Ørsted 1820a). Courtesy Dibner Library, Smithsonian Institution Libraries.

different understanding of what the electromagnetism was that he had discovered and named. Our story is thus one of changing interpretations.

In a phrase quietly recalling his involvement with the conceptual schemes of *Naturphilosophie*, Ørsted had spoken initially of “the effect of the electrical conflict on the magnetic needle.”[6] Ørsted did not thereafter much use the term *conflictus electricus* (in any language), and his contemporaries seem to have paid little attention to his declarations that the electrical conflict is not confined to the connecting wire, but also takes place in the circumjacent space, spiraling around the conductor.[7]

[6] Ørsted 1820a, from the title.

[7] Ørsted 1820a, 2, 4 = 1920, 2, 215, 218. He once spoke of the “flow [*cursus*] of the electrical forces in the connecting wire” (4 *bzw*. 218).

Hans Christian Ørsted in 1822. Portrait by Christopher Wilhelm Eckersberg (1783-1853). Courtesy Danmarks Tekniske Museum, Helsingør. A 1959 copy by Daniel Hvidt hangs in the library of the Dibner Institute in Cambridge, Massachusetts

In a followup paper on his "Recent Electromagnetic Experiments," Ørsted introduced the adjective thenceforth favored to describe the new phenomena—electromagnetic.[8] At this point it is necessary to review some of Ørsted's earlier concerns, which were to impress a peculiar stamp on his interpretation of the new phenomena.

From early on in his career, Ørsted had struggled to attain a systematic understanding of the relationship among magnetism, electricity, the so-called chemical process, heat, and light.[9] Following Schelling, in 1805 he speculated that magnetism, electricity, and the chemical process correspond to the three dimensions of space—magnetism to a line, electricity to a surface, and the chemical process to space.[10] More fruitfully for him, in 1812 Ørsted had begun to speak of the different "forms of action" in which the opposing fundamental forces of nature manifest themselves.[11] It was the pursuit of these speculations that led him to his discovery of the interaction between a magnetic needle and the connecting wire of a galvanic circuit.

In his first Danish-language discussion of that work, he interpreted the new relationships by invoking an echo of his notion of form of action: "What we here a moment ago called electricity is not so in the word's stricter meaning; for the force that in the open galvanic or electric

[8] Ørsted 1820b, 365; 1820c, 78.

[9] See the account in Caneva 1998a, 58-90, for details and references.

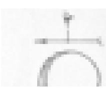
[10] Ørsted 1805, 18-19 = 1920, 3, 103-104.

[11] From among the many occurrences of *Wirkungsform, see* Ørsted 1812, 5, 236, 248, 252, 258 = 1920, 2, 38, 142, 147, 149, 151.

circuit acted in a distinctive manner—under a different form—that we call the electric or galvanic, acts here under an entirely different form that we most appropriately call the magnetic; meanwhile, since magnetism acts under the form of a straight line …[while] the forces here … flow incessantly into each other and form a circular course [*Kredslöb*], the author has called the action dealt with here *electromagnetism*."[12] It thus appears that for Ørsted the principal need for a new term stemmed from the unprecedented circular *form* of the electromagnetic action and not so much from the fact that it represented an interaction *between* electricity and magnetism. But Ørsted early left the development of this new field to others, and his peculiar conceptualization of the phenomena died without issue.

If that was what Ørsted understood by electromagnetism, then we must reconsider what we mean by saying that 'Ørsted discovered electromagnetism.' He did indeed discover that a magnetic needle is deflected by some action present in the connecting wire, but that was not the *meaning* of the discovery for *him*. While *he* applied the term electromagnetism to his particular *theory* of the form of action of spiraling electromagnetic activity, *others* applied it simply to the new *phenomena*. And that, for the most part, is the meaning that has come down to us, sanctioned by longstanding consensus.[13]

Before the dust had settled, however, the issue over whether the phenomena were to be traced to an underlying electric current or an underlying magnetism gave rise to several theories of so-called transversal magnetism, which supposed the existence of small magnets running head-to-tail around the circumference of the conducting wire.[14] Such ideas never gained much currency outside the German-speaking scientific community, and their appeal quickly waned as Ampère's electrodynamic theory gained ground, but for a time they were a serious contender for the explanation of what virtually everyone recognized as electromagnetic phenomena.



Seebeck and thermoelectricity

Although most writers have continued to speak in terms of Seebeck's discovery of thermoelectricity, a minority voice—including my own—has insisted that Seebeck termed the new phenomena thermomagnetism, although without actually citing examples of that

[12] Ørsted 1821, 14 = 1920, 2, 448.

[13] The extended process of redefining just what it was that Ørsted discovered has at length also credited him with the discovery of the magnetic field (Lindsay and Margenau 1936, 302).

[14] On theories of transversal magnetism, see Rosenberger 1887-1890, 205-207; Caneva 1974, 61-65, 68-73, 75-80, 119-120; Caneva 1978, 78-83, 90-91.

usage from Seebeck's own work.[15] In the event, it turned out that the identification of just what Seebeck had discovered was a complex process in which Seebeck himself played only a late and ineffectual role. Because of his nearly four-year delay in publishing his findings, conceptual and terminological possession of the new field fell to others as word of his experimental findings leaked out and then attracted the attention of a spate of investigators throughout Europe.

Thomas Johann Seebeck. Undated engraving, courtesy Deutsches Museum, Munich.

Seebeck reported his early findings at three meetings of the Academy of Sciences in Berlin in August and October of 1821.[16] The earliest published accounts reported his having discovered how to produce magnetic effects in metals other than iron by heating various metals connected together into a closed circuit.[17] The phenomenon was explicitly *magnetic*, sometimes more specifically electromagnetic. By the fall of 1822 it had become clear to the scientific public just what the essential phenomena consisted in experimentally. How they were to be properly characterized remained a topic of discussion through the 1830s, with a clear consensus in favor of a tacitly unproblematical "thermoelectric" not being achieved till after around 1840. The role of Seebeck's Danish colleague in this process was pivotal.[18]

Ørsted, in Berlin for a week or so during November and December of 1822, spent two mornings and an afternoon with Seebeck being shown the new experiments that Ørsted immediately regarded as

15 Streit 1902, xv-xxii; Hermann 1971-1972, 2, 347; Caneva 1974, 125; 1978, 92; Nielsen 1991, 382. *Cf.* Frankel's *DSB* article: "By far Seebeck's most significant discovery … was that of thermoelectricity—or thermomagnetism, as he called it—in 1822. … He did not, however, believe that an electric current was actually set up in the bimetallic rings and preferred to describe his effect as 'thermomagnetism'" (Frankel 1975, 281).Following Frankel's lead are Muir 1994, 464 (implicitly) and Breger 1999, 30 (explicitly). Breger added: "In the terminology of the time Seebeck had thus transformed heat into magnetism." As pointed out with respect to Ørsted in note 5, above, such was *not* the way contemporaries typically spoke.

16 The date of Seebeck's discovery has been variously reported, reflecting the indirect and extended route by which different aspects of his work became known. I will not concern myself with this aspect of the discovery story.

17 Tilloch 1821, 462; Keferstein 1823, 4; Thomson 1822, 318.

18 Keld Nielsen's perceptive and detailed account of Seebeck's work includes a section called "Thermomagnetism becomes thermoelectricity" in which Ørsted's role is emphasized (Nielsen 1991, 391-395).

continuations of his own discovery.[19] Having arrived in Paris in January 1823, Ørsted informed the French Academy in March of "Seebeck's new experiments on electromagnetic actions."[20] Its first sentence continued the subtle process by which Ørsted sought to control the interpretation of those experiments: "Seebeck … has discovered that one can establish an electric circuit in metals without the interposition of any liquid. One establishes the current in this circuit by disturbing the equilibrium of temperature."[21] In noting that "[o]ne can only discover these electric currents by means of the magnetized needle," he was already ignoring the issue of the legitimacy of speaking in terms of an electric current when what one observes is magnetic effects.[22] He went on to suggest several new coinages, which would eventually become standard: "It will from now on doubtless be necessary to distinguish this new class of electric circuits by an appropriate term; and as such I propose the expression *thermoelectric circuits* …; at the same time one would be able to distinguish the galvanic circuit by the name *hydroelectric circuit*."[23] His later Danish account already reported impersonally—if not entirely accurately—that "[o]ne now calls the Seebeckian circuit the *thermoelectric* circuit, and the Galvanic, in opposition thereto, the *hydroelectric* circuit."[24] By the end of the year most researchers in France and England had adopted the language of thermoelectricity and its attendant conceptualization of the phenomena as due to the generation of an electric current in the metallic circuit. In Germany things were a little more complicated.

Among the earliest to pursue Seebeck's lead was Julius Conrad von Yelin in Munich. Yelin had been investigating the relationship between magnetism and heat and light when Ørsted, passing through Munich, informed him of Seebeck's work. As soon as Ørsted left Munich, Yelin undertook his own experiments, reporting his first results to the Bavarian Academy in January 1823.[25] He referred to Seebeck's "thermelectromagnetic experiments" as he went on to claim for himself the discovery "that through unequal heating all bodies acquire

19 Ørsted, letters of 2 December 1822 and 4 April 1823 to his wife, in Ørsted 1870, 2, 31-32 and 59.

20 Ørsted 1823a. Anonymous (1823, 315), reporting on the meeting of 3 March 1823, says Ørsted entertained the Academy "with the work that Seebeck has just done on electromagnetic phenomena. (See the preceding *cahier*.)"—*i.e.*, Ørsted 1823a. The report was quickly translated into German (Ørsted1823c) and English (Ørsted 1823e).

21 Ørsted 1823a, 199 = 1920, 2, 263.

22 Ørsted 1823a, 199 = 1920, 2, 264. Ørsted was aware that Seebeck had another theory about these effects, but it is not clear that he had a very distinct idea of what that theory was (see his letter of 4 April 1823 to his wife, in Ørsted 1870, 2, 59-60).

23 Ørsted 1823a, 199-200 = 1920, 2, 264.

24 Ørsted 1823d, 9 = 1920, 2, 461.

25 Yelin 1823b, 4; 1823a, 419.

magnetomotive properties," a fact he demonstrated by forming circuits composed of a single metal which, when heated at one place, produce a deflection of an appropriately placed magnetic needle.[26] (*Figures* 2 and 3.) Having further detected a magnetic effect—a magnetic polarization—by the appropriate heating of a bar of metal not part of a closed circuit, he noted that "[b]ismuth most strikingly exhibits the polarization occurring in this *thermomagnetism* of metals." A footnote explained the phrase "*Thermo-Magnetismus* der Metalle": "It appears to me that we will thus have to denote in a characteristic fashion this kind of magnetic action, in contradistinction to the already known Ørstedian electromagnetism, on account of its peculiar behavior."[27] Yelin's further discussion made clear that he did not intend thermomagnetism to apply to the full range of phenomena discovered by Seebeck, but only to a new class of actions he had discovered which did not appear to involve a closed electric circuit and in which no trace of free electricity could be detected.[28] That was not, however, the way the term came to be employed by others, who applied it without restriction to the range of phenomena discovered by Seebeck.

As of 1825 there was still no terminological or conceptual consensus. Were the phenomena to be described simply and generically as electromagnetic, in a more phenomenological sense of the word than Ørsted had intended with its coinage, or perhaps, more specifically, as thermelectromagnetic, as Yelin proposed? Or were they rather better characterized as thermoelectric, as Ørsted confidently urged? Others broadened the application of Yelin's more restricted concept of thermomagnetism to cover the same field as thermoelectricity. Behind such terminological matters lay uncertainty over whether the phenomena were fundamentally electric or magnetic, whether magnetism could be reduced to electricity (as Ampère said), and whether and where one was entitled to speak of an electric current. Nor were matters brought closer to settlement when Seebeck's long memoir finally made its appearance toward the end of 1825.

As mentioned earlier, the discovery Seebeck announced in the title of his memoir was the "Magnetic Polarization of Metals and Ores by

[26] Yelin 1823c, 361.

[27] Yelin 1823c, 363.

[28] Yelin 1823b, 11. Yelin's reasoning was correctly captured in the editor's précis of his *Der Thermomagnetismus* published in the *Bibliothèque Universelle*, which may have introduced the term to the French-speaking world: "Yelin, having thus been led to consider the rupture of the equilibrium of temperature as the principal cause of the electromagnetic action of Seebeck's circuit, determined to try the effect of this rupture on a circuit, or on a piece *of a single metal*. Having even then obtained very pronounced effects, he thought it necessary to designate this class of phenomena by the name *thermomagnetism*" (Yelin, 1823d, 256 ≈ Yelin 1824, 159).

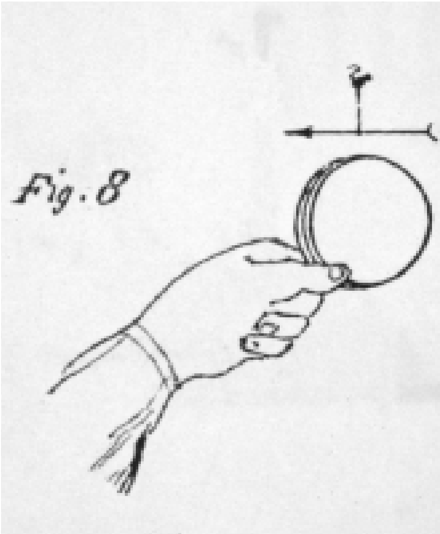
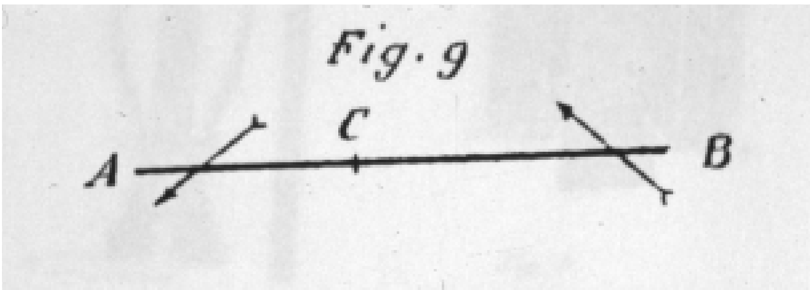


Figure 2. Demonstration of the deflection of a magnetic needle by the "thermelectromagnetic" action of disks of two different metals held between the fingers. From Yelin 1823c.

Figure 3. The "thermomagnetism" exhibited by a rod of a single metal heated along CB and cooled along AC, showing the opposite deflection of magnetic needles along each half. From Yelin 1823c.



Means of Temperature Difference."[29] In repeating and extending Ørsted's experiments, Seebeck came to suspect that *any* inequality of action on the metals used in the galvanic circuit might produce what he persisted in calling a "magnetic polarization" of the circuit, his choice of terms indicating his disinclination to trace Ørsted's electromagnetic phenomena to the generation of an electric current. A series of trials led him to the discovery that heat alone, applied to the junction of two metals, would produce that effect. He thereupon took pairwise combinations of twenty-eight different metals and ores joined together to form a ring, heating one junction and noting the direction and rough strength of the resulting "magnetic polarization" by means of the deflection of a suspended magnetic needle.[30] From these experiments he

29 As Seebeck explained, the published paper was an "extract" from four lectures delivered at the Academy of Sciences in Berlin on 16 August 1821, 18 and 25 October 1821, and 11 February 1822, plus later additions in the form of footnotes and an addendum (Seebeck 1825a, 265 = 1825b, 1). This clarification is omitted from Seebeck 1826.

30 Seebeck 1825a, 266-283 = 1825b, 2-19.

concluded that metals form “a peculiar *magnetic series* that does not correspond to any of the known series derived from other properties of metals”—in particular, the well- established electromotive series of metals governing the production of electricity in the galvanic circuit.[31] This fact underlay his rejection of the hypothesis of the identity of electricity and magnetism and, in particular, Ampère's derivation of magnetism from electricity.[32] Seebeck argued that the magnetic polarization he observed could not be solely due to any free electricity—such as could be detected with an electroscope—and that one was therefore not justified in calling such circuits “electromagnetic.”[33]

What Seebeck thought was going on was the excitation of a “magnetic polarity” or “magnetic polarization” by means of a difference in temperature.[34] Electricity as such played no role. Nor in his original paper did Seebeck employ the language of thermomagnetism, though he did use it five times in subsequently added notes. That was the extent to which Seebeck retrospectively assimilated his discovery of magnetic polarization to the language of thermomagnetism in this, his first and last paper on the subject. To many of his contemporaries, however—especially among German contemporaries of his generation—that was what Seebeck was credited with having discovered until well into the 1830s.

Although time prevents me even from sketching the details of the community-wide reception of Seebeck's and others' work, by the 1840s it appears that, for the most part, the language of thermoelectricity had wholly driven out that of thermomagnetism. Few seem ever to have noticed that Seebeck himself had spoken of the “magnetic polarization” of metals by heat. In the end, *what* Seebeck discovered was decided for him by others.

[31] Seebeck 1825a, 283 = 1825b, 19.

[32] Seebeck 1825a, 292-293 = 1825b, 28-29. Seebeck did not name Ampère explicitly. In a footnote to the German translation of A.-C. Becquerel 1826, Poggendorff noted with satisfaction Seebeck's objection to the notion of this identity (A.-C. Becquerel 1827, 353-354, citing Seebeck 1826, 140-141). He began the note with the comment that “[w]hat Becquerel here calls contact electricity is, as is well known, what we customarily call thermomagnetism” (353).

[33] Seebeck 1825a, 296-297 = 1825b, 32-33 (quote on 297 *bzw*. 33).

[34] For a sampling of such usage, see Seebeck 1825a, 312, 330, 334, 338 = 1825b, 48, 66, 70, 74, among many occurrences.

Ritter and ultraviolet light

An even more striking example of this sort of recasting of meaning has attended what we have learned to see as Ritter's discovery of ultraviolet light, rays, or radiation in 1801.[35] The path from Ritter's work to our conceptualization of it is long and complicated. Our odyssey begins just before the earliest accounts of his discovery.

Prompted by William Herschel's discovery in 1800 of calorific rays beyond the red end of the solar spectrum and his own conviction that polarities underlie the principal phenomena of nature, Ritter thought to see if he could identify invisible solar rays also at the other end of the spectrum. As he wrote hastily to the editor of the *Annalen der Physik*, "On February 22nd [1801] I found solar rays—discovered by means of horn silver—also on the violet side of the spectrum of colors, outside of it. They reduce even more strongly than violet light itself, and the field of these rays is very large… . More on this soon."[36] He announced his discovery more fully in a note in an Erlangen periodical with the revealing title, "Chemical Polarity in Light," in which he summarized his findings under three headings:

> I. There are rays in sunlight that do not illuminate and of which one part is refracted more strongly, the other more weakly, than all those that illuminate.
>
> II. Sunlight in the undivided state is a neutralization of the two ultimate determinants of all chemical activity, oxygeneity and deoxygeneity (= hydrogeneity).
>
> III. By means of the prism the two diverge like poles. The red side of the spectrum and that which borders on it externally become the side of oxygeneity, the violet side, on the contrary, and that which borders on it become the side of hydrogeneity. The maxima of both fall outside the visible

[35] See the works cited in note 4, above, which identifies a number of commentators who have analyzed Ritter's work with greater sophistication. Not being then sensitive to the distinction, my reference there (Caneva 1997, 95, n.60) to authors who cited "Ritter's discovery of ultraviolet light" as evidence for the influence of Schelling's *Naturphilosophie* included six citations (out of eight) that were in fact to Ritter's discovery of ultraviolet rays or radiation. For the "discovery of ultraviolet light," see Hermann (1987, 58) and Eichner (1982, 23). Berg paired Ritter's "discovery of UV radiation" with Herschel's prior "discovery of infrared radiation" even as the schema he drew up nicely displays the complexity of the polarities that underlay Ritter's understanding of the phenomena (Berg 1976b, 33, 32, respectively). Breger, among those who spoke of Ritter's "discovery of ultraviolet rays," noted Ritter's interpretation of the phenomena in terms of the polarity between oxidation and reduction processes without saying that Ritter saw his discovery as the chemical polarity of light (Breger 1999, 28). The body of McRae's account is faithful to Ritter's way of thinking, though in his bibliography he described Ritter 1806 as "his paper on his thought processes in the discovery of ultraviolet radiation" (McRae 1975, 474, 475). For Ritter's discovery of invisible chemically active rays beyond the violet end of the spectrum see Rosenberger 1887-1890, 67, and Whittaker 1951, 100. Three recent biographical dictionaries employ what appears to have become canonical language: "he discovered the ultraviolet rays in the spectrum by means of its darkening effect on silver chloride" (Muir 1994, 436); "from the darkening of silver chloride in light he discovered ultraviolet radiation" (Porter 1994, 583); "he discovered ultraviolet radiation by its darkening effects on silver chloride" (Williams 1982, 445; article by Colin Russell).

[36] Ritter 1801b, 527.

spectrum; their indifference, however [falls] inside it in the region of green.[37]

What most excited Ritter was the evidence his discovery provided for the thoroughgoing polarity of all the activities of nature and their complex interconnections. In his final paper on the subject Ritter emphasized the fundamental distinctiveness of the invisible chemical and thermal rays he and Herschel had discovered vis-à-vis the light of the visible spectrum.[38] There seemed to be three distinct spectra, which an appropriate arrangement of prisms could separate out from the solar rays.

Johann Wilhelm Ritter. Undated woodcut, courtesy Deutsches Museum, Munich. Reproduced in Ritter 1986.

The light under which Ritter's work was seen was soon colored by the contemporaneous work of William Hyde Wollaston. Like Ritter, Wollaston was inspired by Herschel's discovery in 1800 of calorific rays less refrangible than red light and guided by Scheele's experiments with muriate of silver. Wollaston reported in June 1802 that "on the other [side of the solar spectrum] I have myself observed, (and the same remark has been made by Mr. Ritter,) that there are likewise invisible rays of another kind, that are more refracted than the violet. It is by their chemical effects alone that the existence of these can be discovered."[39] Finding that the blackening of the silver chloride extended far beyond the violet end of the spectrum and that "by narrowing the pencil of light received by the prism, the discoloration may be made to fall almost entirely beyond the violet," Wollaston concluded "that this and other effects usually attributed to light, are not in fact owing to any of the rays usually perceived, but to invisible rays that accompany them."[40] Although there were to be a few dissenters, Wollaston's insistence on the essential distinctiveness of chemically active and luminous rays was generally subscribed to until the 1840s. Indeed, commentators from the

[37] Ritter 1801a, col. 123. Prompted by the translation of this passage in Caneva 1997, p. 45, Andreas Kleinert informed me in a personal communication that the obscure words "immer dasselbe" as printed in the original (in the last phrase quoted here) are a misprint for the (grammatically irregular) "inner dasselbe," as he confirmed by examining the corrected copy of the text in the Erlangen University Library.

[38] Ritter 1802, 410-411, 414.

[39] Wollaston 1802, 379 = 1803, 100, in a footnote.

[40] Wollaston 1802, 380 = 1803, 100.

early 1840s through the early 1860s regularly remarked on the generality of the belief in the qualitative distinctiveness of the chemical, calorific, and luminous rays, a belief they were concerned to oppose.

Things began to change decisively in 1842 with the combined advocacy of Macedonio Melloni and Edmond Becquerel for the fundamental identity of all the variously named rays of the solar spectrum, and with Ludwig Moser's more limited advocacy of the identity of the chemical and luminous rays.[41] Their significantly different arguments all combined an appeal to the identity of the physical laws those rays obey, an acceptance of the wave theory of light—whereby the only relevant variable seemed to be the refrangibility or frequency of the ray—and a distinction between the latter as the "essential properties" of the rays versus the "accidental qualities" those rays exhibited in their various thermal, luminous, and chemical effects.[42] For a long time it had been known that different observers place the ends of the visible spectrum at different places; now the conclusion was *decisively* drawn that that phenomenon illustrates the fundamentally subjective nature of color and visibility. Relinquished now was the deep-seated conviction since Newton's day that color is an intrinsic property of light. Against earlier experimenters who had failed to detect any heat towards and beyond the violet end of the spectrum, Melloni claimed that it was now possible to measure the heat developed by all types of rays falling on a black surface. Where visibility had once been the defining character of light, Moser now spoke of "invisible light," implying that some criterion other than visibility is necessary to define what constitutes light.[43]

It was Becquerel, however, who provided what was to prove to be perhaps the most decisive bit of evidence in favor of the identity thesis as he exploited the recent development of photography for scientific ends. Exposing to sunlight a plate prepared "according to M. Daguerre's method," he obtained what he called "this curious result, that the chemical spectrum has the same lines as the luminous spectrum, provided

[41] To the canonical luminous, thermal, and chemical rays Becquerel added phosphorescent (or phosphorogenic) rays, *i.e.*, those that excite phosphorescence in certain substances (E. Becquerel 1842, 341, 342). On the basis of certain distinctive phenomena Moser argued explicitly against the identity of the rays of light and heat (Moser 1842d = 1843a).

[42] The quoted words are from Melloni 1842, 136. Moser did not pronounce himself in favor of any theory as to the nature of light, though his mild support of the wave theory is suggested by his favorable citation of J. F. W. Herschel, who "considers it possible that some animals, viz. insects, do not receive an impression from any of those colours which are visible to us, but are affected by a species of oscillations which lie beyond the limits of our senses" (Moser 1842a, 199; quoted from 1843, 437).

[43] Moser 1842b, 569; *cf*. 1842c, 2 ("invisible light rays"). Hermann Helmholtz echoed these views in an address of 1852: "[P]hysics teaches us that there is also light that we do not sense, invisible light, *i.e.* radiations that proceed from luminous bodies, notably the sun, have entirely the same laws of motion as light, are subject to exactly the same phenomena of interference, reflection, refraction, diffraction, polarization, and absorption, and, with regard to their whole physical behavior, differ from visible light only by a somewhat different magnitude of period of oscillation and refrangibility" (Helmholtz 1852, 13; quoted from 1882-1895, 2 [1883], 603).

we only consider the parts of the same degree of refrangibility of these two radiations."[44] Incorporating the results of several such exposures into a single diagram, he illustrated these spectral lines in a large and beautiful plate, pairing them line for line with the corresponding lines that Fraunhofer had first identified and named for the visible spectrum. (*Figure* 4.) It was hard, now, to avoid the conclusion that it is the same rays—as defined by their refrangibility—which produce either luminous or chemical effects, according to circumstances, and which make up a visually continuous spectrum indicating no qualitative difference as one passes from the visible into the invisible regions of the solar spectrum.

If, to be sure, the argument for the ontological identity of the various species of radiation depended greatly on the accumulation of good, hard evidence, it also derived much of its plausibility from the increasingly taken-for-granted implications of the wave theory of radiation. Thus Ernst Brücke, after a tortuous consideration of the experimental pros and cons of the still- disputed identity of the rays of light and heat, concluded as follows:

> If on the other hand one recognizes that the rays of light and heat are both polarizable (thus both are composed of transverse waves), [and] that both pass through empty space (thus both, if one does not wish to assume a second unknown medium besides the aether, must consist of oscillations of one and the same medium), then one perceives that there no longer exists any mechanical understanding for a difference between the two radiations. If one further recognizes that the invisibility of the rays beyond the red and beyond the violet—even if they differ from the luminous rays only in terms of wavelength—has absolutely nothing mysterious about it, then it must appear rash to give up the identity hypothesis completely and, with regard to radiant heat, to sink back into the former perplexity before one has tested in the most precise fashion the probative force of the counterexperiments.[45]

In other words, the *inconceivability* of radiant heat's being in any way *essentially* different from other forms of solar radiation, as judged from the standpoint of the generally accepted undulatory theory, should outweigh any experimental quibbles that might suggest some underlying difference. There seems to be a tipping point with regard to the believability of a theory, beyond which potential counterevidence no longer easily counts as such, but is rather generously reinterpreted so as to be in likely accord with the now-accepted view of things.

The explanatory function of attachment to the wave theory of light is nicely illustrated by the response of George Gabriel Stokes to the

[44] E. Becquerel 1842, 347; quoted from 1843, 542.

[45] Brücke 1845a, 274 = 1845b, 605-606.

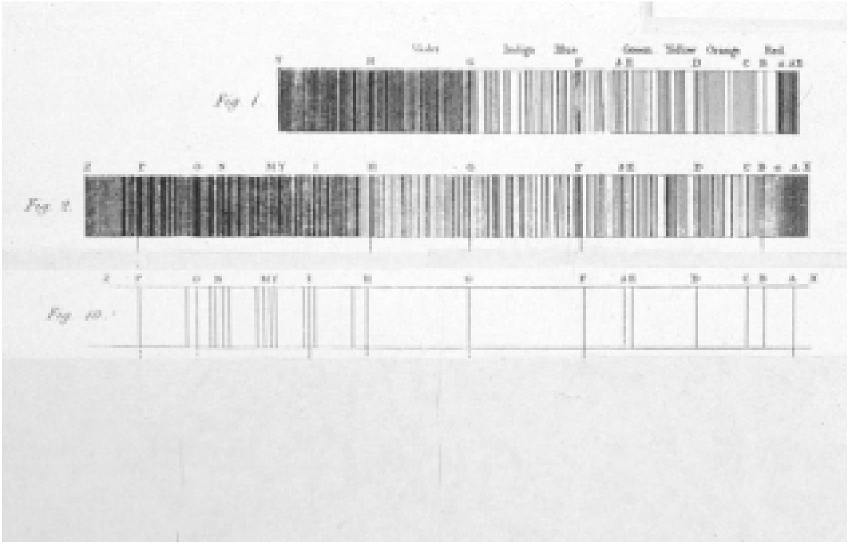


Figure 4. Edmond Becquerel's demonstration of the correspondence between the dark lines in the luminous and chemical solar spectra. Fig. 1 represents the luminous spectrum as drawn by Fraunhofer. Fig. 2 represents a composite chemical spectrum from a number of photographic exposures. Fig. 10 is a schematic representation of the dark lines common to all solar spectra. From E. Becquerel 1843 (edited).

phenomenon of internal dispersion he named "fluorescence" in 1852.[46] It was *obvious* to him that the phenomenon could *only* be explained by one or the other of the *only* two things that define the nature of light, its period of vibration and its state of polarization. To its period of vibration, of course, corresponds its refrangibility. Aware of the long tradition that assigned qualitative differences to the rays responsible for producing different effects, Stokes went out of his way to emphasize that there are not *and cannot be* any such differences: visibility is a function of the eye depending solely on the rays' refrangibility, not a property of some special class of rays. The last of his formal conclusions was of universal scope:

> The phenomena of internal dispersion oppose fresh difficulties to the supposition of a difference of nature in luminous, chemical, and phosphorogenic rays, but are perfectly conformable to the supposition that the production of light, of chemical changes, and of phosphoric excitement, are merely different effects of the same cause. The phosphorogenic rays of an electric spark … appear to be nothing more than invisible rays of excessively

[46] After having termed the phenomenon "dispersive reflexion," he added in a footnote that he was "almost inclined to coin a word, and call the appearance *fluorescence*, from fluor-spar, as the analogous term *opalescence* is derived from the name of a mineral" (Stokes 1852, 479). In the event he mostly used the term "internal dispersion."

high refrangibility, which there is no reason for supposing to be of a different nature from rays of light.[47]

Embracing this new conceptualization of light, Wilhelm Friedrich Eisenlohr soon thereafter introduced the *term* that defined the region beyond the violet end of the spectrum not in terms of its chemical action but simply in terms of its location along a spectrum that was now explicitly all *light*.[48] He opened his 1854 paper, "On the Action of Violet and Ultraviolet Invisible Light," with a reference to Stokes' recent work:

> The phenomenon to which Stokes gave the name *fluorescence* has led me to the conjecture that it is produced by interference of the shorter systems of waves, blue-violet and ultraviolet (thus can one call for brevity's sake the chemically active invisible light alongside the violet in the spectrum). … Light itself consists accordingly of the visible systems of waves and, in addition, of waves that are longer than red and of such that are shorter than violet. … Ultraviolet light … whose existence before the wonderful discovery of Stokes could only be demonstrated by its chemical activity, consists of innumerable systems of waves, whose mutually different lengths all have a shorter period of oscillation than violet light.[49]

For Eisenlohr, the apparent transformation of highly refrangible and invisible ultraviolet light—the erstwhile chemical rays—into less refrangible and thus visible light meant that all are of essentially the same nature. Other Germans quickly adopted the new terminology of "ultraviolet light," "ultraviolet rays," and "the ultraviolet."[50] (*Figure* 5.)

Although the more detailed tracking of this usage into the standard vocabularies of European scientists is still to be done, we are finally at the point where it becomes possible even to *speak* of Ritter's having discovered ultraviolet light, regardless of whether or not that misrepresents what he thought he had done. It should be clear that major conceptual transformations had to take place with regard to the common understanding of light—entailing both general acceptance of the wave theory of light and the concomitant conclusion that solar rays simply *could not have* any qualitatively distinguishing features—before such

[47] Stokes 1852, 557.

[48] J. F. W. Herschel had introduced the term "ultra-violet ray" in 1840, but he did so with such ambivalence that he himself did not further employ it, and no one else—Stokes included—seems to have picked up on his nonce usage (Herschel 1840, 20).

[49] Eisenlohr 1854, 623-624.

[50] Helmholtz 1855, 206 ("ultraviolette Strahlen"), 208 ("überviolettes Licht"); Esselbach 1855, 757 ("ultraviolettes Licht"; "das Ultraviolett"). The first usage I've found in French is Mousson 1861, 239 ("rayons ultra-violets"). Mascart (1863, 789) spoke of the "spectre solaire ultra- violet," and by 1867 Edmond Becquerel was regularly applying "ultra-violet" and "infra-rouge" to the respective spectra and rays (1867-1868, 1 [1867], 138-145). For a few years John Tyndall wavered between "extra-violet" and "extra-red" rays (Tyndall 1864, 329; 1866, 16) and "ultra- violet" and "ultra-red" rays (Tyndall 1865a, 44; 1865b, 5-6) before deciding in favor of the latter pair (Tyndall 1868, 437, 441; 1873, 127-141).

terminological rephrasing was possible. That change in understanding itself very much depended both on the cumulative evidence pointing to the analogous physical behavior of the variously named rays—same laws of refraction, interference, etc.—and, in particular, on the evidence provided by Becquerel's photograph of the broad chemical spectrum, showing the exact correspondence between its dark lines and those Fraunhofer had identified in the luminous spectrum. That and, ten years later, Stokes' forceful advocacy of the undulatory identity of the entire range of solar radiations, coupled with his experimental demonstration of the transformation of invisible into visible rays, seem to have been the most important events in bringing about a general shift in the way scientists thought about light. The changes in vocabulary that followed soon after finally made it possible to speak without circumlocution of

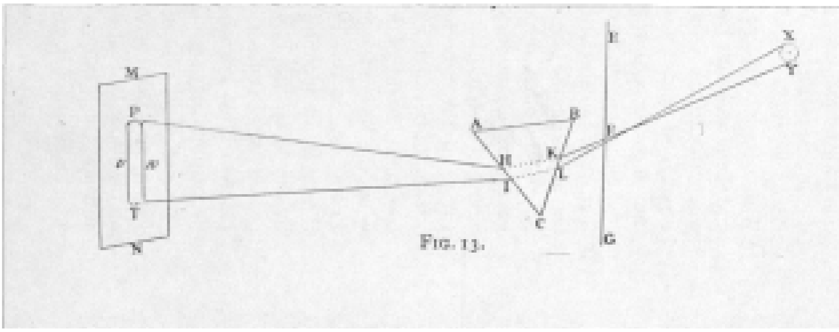
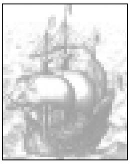


Figure 5. Newton's illustration—from the first edition (1704) of his *Opticks*—of the theorem that "The Light of the Sun consists of Rays differently Refrangible." His arrangement of the prism and the resulting orientation of the solar spectrum—violet above and red below—became canonical and probably encouraged the terminology of "ultraviolet" and "infrared." Courtesy Dibner Library, Smithsonian Institution Libraries.

invisible light rays beyond the violet end of the spectrum. Their *defining* characteristic was now their *location* along a spectrum of varying refrangibilities, not the chemical *means* by which their presence was detected, let alone a presumed qualitative difference between them and other species of solar rays regarded as representing distinct and separable spectra. If, after all this, we still do not want to say that Ritter discovered ultraviolet light, we might nevertheless be happy with saying that Ritter *became* the discoverer of ultraviolet light.[51]

[51] As of when I break off this story, Ritter had not yet become either the inevitably cited or the unique discoverer of ultraviolet rays. All I am confident in asserting is that, sooner or later, Ritter did attain that privileged status, as witnessed by his place in contemporary histories and the complete disappearance of Wollaston as in any way involved in the discovery. My purpose here has been to trace the historical preconditions for his having attained the status of discoverer of ultaviolet light.



Reflections on the form and function of scientific discoveries

The origins of this paper lay with the recognition that the conventional characterization of *what* Ørsted, Seebeck, and Ritter discovered does not correspond to what each man *thought* he had discovered. The bulk of the historical material adduced here has been chosen to demonstrate that fact and to sketch the reasons for and route by which that conventional characterization was attained. That essentially descriptive enterprise must now give way to an analysis of the general significance of that process, a process one might describe as the collective construction of scientific knowledge.[52]

Perhaps the most important point about the characterization of any discovery is that, in order to be intelligible, it must be phrased in language understood by the intended audience, in language that typically implicates the taken-for-granted reality of that audience. It thus verges on the unintelligible to say that Ørsted discovered the circular form of the electromagnetic action of the electrical conflict, or that Seebeck discovered the magnetic polarization of metals by heat, or that Ritter discovered the chemical polarity of light—in his words, that "[s]unlight in the undivided state is a neutralization of the two ultimate determinants of all chemical activity, oxygeneity and deoxygeneity."[53] To be sure, each man's discovery can be rendered in largely phenomenological terms: do this, and you will observe this. Appropriately suspend a magnetized needle near the connecting wire of a closed galvanic circuit, and you will observe the needle to move in a certain way. Indeed, in each case contemporaries' first order of business was precisely to convince themselves that the phenomena were in fact as described, and in every case the establishment of at least phenomenological consensus was quick

[52] The fullest account of certain aspects of the process described here is Augustine Brannigan's *Social Basis of Scientific Discovery* (1981), the more explicit consideration of which I must defer to the longer version of this paper I intend to publish. His very similar goal was to "explain how certain achievements in science are *constituted* as discoveries and not how they occurred to an individual" (11), whereby "discoveries are social events whose statuses as discoveries are retrospectively and prospectively objectified" (133). His answer involved an analysis of "the role of the social recognition in the constitution of a phenomenon's identity," as a result of which process "members of a society in one instance socially construct an event as a discovery only to later orient to it as a natural fact of life" (133).

[53] All three men were inspired by the conceptual resources of *Naturphilosophie* to look for polarities, but that concern has left no traces on the canonized characterizations of their discoveries, in large part, it seems to me, because we do not regard polarities and dichotomies as one of the fundamental regulative *or* constitutive concepts necessary for the comprehension of phenomena. Significant in this regard is the fact that one of the most accurate and perceptive accounts of polarities in Ritter's work—one which described them in terms of the transformation of quantity into quality—came from an East German (Berg 1976a, 73). In a later essay, Berg and Richter commented that "[i]t has been too little noted up to now that Ritter was one of the first people during the time of the development of dialectics by German classical philosophy to recognize objective dialectics in nature—even if still in *naturphilosophisch* disguise—and to work according to the dialectical method" (Berg and Richter 1986, 10). What Ritter discovered looks different from the perspective of dialectical materialism.

and lasting: the reported phenomena are real and repeatable, constituted by generally understood material practices. In that sense, too, these discoveries have retained their legitimacy and integrity *as* discoveries of phenomena that have an objective existence in physical reality even as their characterization has changed.

But, then as now, scientists do not typically content themselves with purely phenomenological descriptions—nor did any of our three principals. Where Ørsted saw the effect of an electrical conflict extending beyond the confines of the connecting wire, others saw the effect of a transversal magnetism within the wire. Where Seebeck saw an unambiguously magnetic effect, others, like Ørsted, saw that same effect as the clear and direct manifestation of an electric current. What Ritter actually observed was the local darkening of silver chloride beyond the violet end of the visible solar spectrum, but he, like everyone else, saw that as resulting from the action of invisible rays. All such characterizations entail the acceptance of the existence of certain theoretical entities—entities which, however, are commonly not seen as theoretical constructs but as unproblematic statements about things in the world. Or at least they sooner or later become that. The discoveries we've been concerned with became 'facts' only by being recast in terms corresponding to accepted theory in congruence with other accepted facts and theories.[54] As people's ideas change about the underlying nature of physical reality, so too must their characterizations change of what it was that someone discovered. The process by which a discovery acquires meaning is thus not effectively over until a locally stable consensus has been attained with regard to its characterization in up-to-date terms.

Which in turn brings us to one of the most important functions of anachronistically recast discovery accounts: they validate as real current views both about the nature of reality and the nature of science.[55] More than that, by saying that so-and-so discovered such-and-such, one implies that such-and-such was there all along waiting to be discovered, an objective part of physical reality whose nature is given by the very structure of the world, not by a labored process of consensus formation.[56]

[54] "[U]ntil the scientist has learned to see nature in a different way—the new fact is not quite a scientific fact at all" (Kuhn 1962b, 53). Kuhn appropriately insisted on the importance of scientists' *seeing* things in a certain way without a conscious moment of interpretation.

[55] One result of redescribing a discovery in a later vocabulary is "the linearized or cumulative histories familiar from science textbooks and from the introductory chapters of specialized monographs" (Kuhn 1984, 248 = 1987, 366).

[56] "The metaphor of scientific discovery, the idea of dis-covery, is precisely that of uncovering and revealing something which had been there all along. … The crucial part is the prior existence of the discovered object. … The rhetoric of this ontology portrays the objects of discovery as fixed, but the agents of discovery as merely transient" (Woolgar 1988, 55).

Acknowledging an historical element in the characterization of basic scientific phenomena threatens their status as objective facts.[57] Indeed, the perceived danger of the so-called social construction of scientific knowledge is that, by historicizing knowledge claims to a contingent context, it threatens to strip them of any claim to objective truth and thus to undercut the authority of science.[58] Recast discovery accounts help avert this danger, especially since conventionality of representation itself seems to imply a kind of objectivity. De facto terminological consensus is necessary before a phenomenon can be taken for granted as simply 'the way the world is.'

The concept of scientific discovery is thus intimately connected with the concept of scientific fact, and the transformations that discovery accounts undergo all reinforce the objectivity, the facticity, of that which was discovered. Although the name of the discoverer survives as an identifying icon, all that was personal, historically contingent, and not in accord with presently employed concepts and language is erased from the public record. Discoveries tell us the way things *are*.

From a sociological perspective, discoveries function as part of the intellectual reward structure of science. Indeed, to some extent—especially in the nineteenth century—making a discovery was what it meant to be an original and productive scientist. Hence canonized discovery accounts reinforce a particular conception of the role of the individual scientist as the originator of the facts, concepts, and phenomena that make up the abiding edifice of science.[59]

A final function of anachronistically simplified attributions of discovery cuts closer to home. It will not have escaped the notice of many historians of science how difficult it can be in the classroom *not* to give in to the simplifying formula, so-and-so discovered such-and-such

57 Kuhn interpreted the desire "to recast past developments in the language of modern concepts" in terms of scientists' resistance to entering into an "alien culture," seen as threatening because it "expos[es] the foundations of a previous life form as contingent" (Kuhn 1984, 250 = 1987, 368).

58 One of my reasons for preferring to speak of the collective—not social—construction of scientific knowledge is because 'social' tends to be understood as 'merely social,' in which case the inference against any kind of objective truth appears unavoidable. Although a concept of objectivity as unconnected to specific (and hence contingent) circumstances appears to me to establish its untenability by definition, I believe a reinterpretation of relativism as (precisely) connected to specific circumstances—which, in the case of scientific knowledge, typically means *relative to* a vast array of repeatedly tested experimental and theoretical claims—can go far to ground the authority of many (most?) scientific claims precisely in the cumulative history of each, which includes the personal experiences and physical experiments of a vast number of observers. One must, however, get past the imputation of 'merely relative' that customarily attaches to the notion of relativism. For a further discussion of these issues, see Caneva 1998b.

59 "[U]nit discoveries are the bricks from which, in the familiar image, the edifice of science is piecemeal built. … The concept of the unit discovery is constitutive of the scientific life as we know it" (Kuhn 1984, 251 = 1987, 369). "These stories are crucial to maintaining the values of the institution of science—the specificity and unique character of the knowledge it produces, for example" (Pestre 1999, 205).

in some particular year. Even if the complex stories one might tell concerning the kinds of 'discoveries' dealt with here would in fact enable us to make other important points about the history of science and the nature of the processes by which scientific knowledge is produced, we usually don't have the time to elucidate the contexts within which the work of—for example—Ørsted, Seebeck, and Ritter must be situated in order to be understood properly. We typically don't even know the full story behind the perhaps decades-long process by which such 'discoveries' acquired their canonical form. And if we did, it would usually be too complicated to tell students. Hence we often collude in the perpetuation of an epistemological fiction as we struggle to achieve order—*i.e.*, certainty, simplicity, and vividness, to use Ludwik Fleck's terms.[60] The only alternative, both pedagogically and professionally, would seem to be to refuse to separate knowledge as a product from the process of its production. In both historical and scientific practice, however, such a solution proves very difficult to sustain both because it is intrinsically difficult and because its denial offers such tangible rewards.

[60] An important part of the process Fleck described involved the roles of what he called "vademecum science" (*Handbuchwissenschaft*)—that is, of the selective restatement and canonization of scientific knowledge in compendia, handbooks, and the like, prepared for the professional—and of the "popular science" prepared for the nonexpert, the interested amateur. Essential to popular knowledge—indeed for Fleck originating there—are certainty, simplicity, and vividness (*Anschaulichkeit*): clear and simple statements shorn of confusion and qualification that convey generally accepted truths about the world (Fleck 1979, 112-115; for the German see Fleck 1980, 146-152).

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