ROMANTIC EXPERIMENT? THE CASE OF ELECTRICITY

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A striking case

In summer 1820, a printed letter circulated through the learned Europe and caused much noise. It reported a spectacular experimental result: an action of galvanism onto a magnetic needle, an effect that had long been sought for in vain. The result provided experimental evidence of a connexion of forces of nature that had formerly been treated separately. No surprise, the result was much praised by those who harbored the belief that the different forces of nature were somewhat interconnected, or even nothing more than different expressions of one and the same basic force – a belief often characterized as “romantic.” No less surprising, however, was the deep distrust in the report from the side of those who had never had, or eventually given up, any belief of such a type. The two groups (to simplify the picture for a moment) have often been associated with “romantic physics” and “Naturphilosophie” on the one hand, and the mathematizing approach to physical science on the other. While the former group was spread out over German speaking countries, with a certain concentration in Jena, the latter was located exclusively in Paris. Characteristic enough, the first reaction in Paris against the report was the suspicion of another “reverie allemande”.[2]

Hans-Christian Ørsted himself, the author of the letter,[3] and a central figure in Kopenhagen academia, knew both groups well. 18 years earlier, right after his studies, he had taken a three-years tour through the centers of research in Germany and France. Among others, he had visited Jena, and made friends with Johann Wilhelm Ritter, then already widely known by his research in galvanism and electrochemistry. Likewise, he had spent considerable time in Paris and learned about the mathematizing enterprise of the Laplacian School. Now a professor of physics in Kopenhagen, he still shared many of the central beliefs of German idealism and “Naturphilosophie,” but at the same time kept a critical distance to speculative enterprises, and was careful, in his own research, to separate clearly between empirical findings and speculative explanations.[4] However, he is often counted among the “romantic physics,” and his discovery of electromagnetism taken as one of the greatest successes, or even a “Sternstunde” (“magic hour”) of that specific historical strand.[5]

Ørsted ’s discovery was spectacular and caused strong reactions. The topic was taken up immediately all over Europe, and many formulated their impression that a new research field had been opened. Most of the ensuing research involved experimentation. The early period of electromagnetism, with many actors focusing on the same problem, provides a promising scenery for studies of experiment, its varieties and specific forms. In particular, the episode might serve as a key for studying the question of whether there is a specific variety that could be called “romantic,” and how it can be characterized. Those questions form the focus of my paper. After some general considerations and specifications, I shall sketch the experimental

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[1] Max Planck Insitute for the History of Science, Berlin. I thank Jutta Schickore, Gerhard Wiesenfeldt, and the participants of the Gran Canaria Symposium for stimulating and critical comments.

[2] As Dulong reported to Berzelius: (Söderbaum 1912-32), vol. 4, 18.

[3] For Oersted, cf. Kirstine Meyer’s introduction to (Meyer 1920), or (Dibner 1962), (Williams 1974), (Franksen 1981), (Jansen, Snorrason & Lauritz-Jensen 1987). The Latin letter (Oersted 1820A) was quickly translated and printed in French, English, German, Italian and Danish, cf. (Meyer 1920), vol.2, 214, and (Steinle in press), ch. 2.

[4] For a critical account of Oersted as being “Naturphilosoph” cf. (Christensen 1995). A selection of Oersted’s research papers has been translated into English in (Jelved, Jackson & Knudsen 1998).

[5] (Meya & Sibum 1987), 146, (Snelders 1990).

procedures of many researchers of the historical episode. In view of that sample, I shall come back to the general question and argue for a significant switch of perspective.

Romantic physics, romantic experiments?

The concept of “romantic science” or, more specifically, “romantic physics,” though often used, has to be treated with caution. Unlike the talk of “romanticism” in literature, the concept of “romantic science” has been introduced only later for various purposes, mostly as a label to classify ‘bad,’ unsound science or, in fewer cases, to rehabilitate it as being a very fruitful and important episode.[6] The category has never been casted consistently, however. Depending on the work it was supposed to do, it either referred only to a small group of those who actually had contact to the literary movement, or, more often, to a much larger group of researchers, comprising such different actors such as Oken, Seebeck, Steffens and Humboldt, or Goethe, Davy and Faraday within one strand, despite their sometimes explicit dislike of romantic literature and of speculative approaches to science. If we nowadays use the term as analytical category in historiography, a closer analysis of its meaning, its borders, and its functions is demanded.

One specific point to be observed here is that one cannot easily equate “romantic science” and “Naturphilosophie,” as has often been done. Ken Caneva has, in a remarkable summary article, well pointed out that those two concepts, if taken in some precise meaning, definitely have different focuses, notwithstanding their well existing overlap area.[7] But still, they are often put together, by cost of considerable unsharpness. While the period has received increasing attention in the last years,[8] the term “Romantic science” is often used, in a loose mode, as a catchword, drawing attention to a historical period, to a specific type of beliefs or of practices, or to a more or less loose group of researchers. The value of such a vague concept for historiography of science is little, of course. While it cannot be my task here to remedy that unhappy state of affairs, it’s necessary to be aware of it. What I shall do in this paper is to focus on a specific aspect of scientific research – experiment – and ask whether it is possible and meaningful in any sense to speak of a specific type of “romantic” experiment. My result, to anticipate it already, shall be negative, but at the same time point to a significant shift of perspective.

Experimenting is a complex procedure, in which actions and considerations, instruments and theories, spaces and materials, actors and questions are most closely intertwined. In looking for a specific “romantic” experiment, one can pursue most different perspectives. This holds even true if one focuses, as I shall do, on research experiments, leaving out experiments used in other contexts such as demonstration to students, or public performance for entertainment. “Romantic” experiment could simply point to experiments conducted by those who count as “romantics.” The question becomes somewhat trivial then – if we decide to speak of romantics, we have romantic experiment – and co-extensive to the question of who counts as “romantic.” The more interesting question, by contrast, is whether there is something specific in the experimental research of those connected to “romantic physics,” in contrast to other researchers in the field. Romantic experiment could, for example, point to experimental activity done with “romantic” ideas in mind, such as the unity of nature, or polarity and

[6] The tradition of bashing “romantic” science goes at least back to Helmholtz and DuBois-Reymond, but surprisingly continues into modern historiography, as in (Gillispie 1960) who portrayed romantic research as ‘armchair’ meditation. A more positive and sometimes apologetic view is taken by (Poppe 1959), (Herrmann 1967), (Herrmann 1968), (Wetzels 1973), (Wetzels 1990).

[7] (Caneva 1997). See also (Engelhardt 1988) or (Nielsen 1989).

[8] As an indicator, see the collections (Porter & Teich 1988), (Cunningham & Jardine 1990), (Poggi & Bossi 1994), (Uerlings 1997), (Shaffer 1998), or the recent conferences at Harvard University (May 2002) and on Gran Canaria (September 2002).

enhancement, or the connectedness of the material and spiritual world. However, it is not at all clear that those ideas should make the experimental activity differ from the activity of those who didn't share those ideas. Thus, the most promising perspective for pursuing our question seems to me to take a look at the experimental *practice* in all its several aspects: the very type of the experimental activity, the use of and the attitude towards instruments, the character of those instruments, the modes of observation, the use of one's own body, the type of questions pursued, and the ways of evaluating experimental outcomes, turning them into formulated results.

Despite a few attempts,[9] the question of such a specific "romantic" experimental practice is largely open.[10] To treat it comprehensively, however, would require extensive studies, comprising different research fields of the period such as physiology, chemistry, acoustics, optics, colors, etc. – a study that could easily fill a book. What I shall do instead is to analyze a specific episode – early electromagnetism – with those questions in mind. As a result, I shall be able to carve out some characteristic problems, to answer some of the above questions, but essentially to shift the general perspective. I shall, moreover, point out some of the reasons that make me suggest that those results reach beyond the particular historical case.

Early electromagnetism

The question of a possible interaction of electricity and magnetism had been considered for a long time, even back in the 18th century, when reports circulated about events such as iron crosses becoming magnetic by the stroke of lightning. By the appearance of Volta's pile in 1800, research was again stimulated. In 1801, Johann Wilhelm Ritter conducted a series of experiments, looking for specific galvanic properties of opposite magnetic poles, without success, however. The experiments by the Padua amateur Gian Domenico Romagnosi in 1802, and the Genuese chemistry professor Mojon in 1804, remained virtually unknown and undeveloped, though they had shown some effect of galvanism onto magnetism.[11] An experiment by two Paris academicians, by contrast, received wide attention. Jean Hachette and Bernard Desormes made, in 1805, a very large Voltaic pile float on water, in order to check whether it would be directed by the earth's magnetism. Very characteristically for Paris research, they left the pile "open," i.e. they did not connect its ends with each other, and they obtained a negative result.[12]

This sketch, rough as it is, makes clear that experiments in search for an electromagnetic action could well be designed and conducted without beliefs and assumptions in mind that were connected to "Naturphilosophie." Likewise, the lessons drawn from those results differed widely. In France, the result of the Paris experiments definitely furthered the belief that an electromagnetic action did not exist – after all, the experiment was done with scrutiny and under the auspices of the authority in the field, the Paris academy. That belief was implicitly corrobated, moreover, by the mathematical theory of electricity, presented by Poisson in 1812, that did not address the topic explicitly, but just did not offer any space for such an interaction: Much like other theories of such a 'Laplacian' type, it worked on the general supposition that there was no interaction between different imponderables such as

[9] (Schulz 1993), while explicitly raising the question of a specific romantic experimental practice, actually only studied programmatic statements. (Daiber 2001) provides a striking example for what goes amiss when conducting studies of experiment solely from a philological perspective.

[10] (Henderson 1998) claims to find a specific experimental approach in Novalis and Ritter, in which science is seen as "an ongoing process using instrumental symbols in the concrete thought of the imagination" (163). But again, experimental practice is strikingly absent in his account. A closer look would put the claimed specificity of the experimental procedures of Ritter as contrasted to Volta, for example, in a totally different light, even more so if Galvani were included.

[11] (Dibner 1962), (Andrade Martins 2001).

[12] (Mottelay 1922), 376. Had they closed the pile, they would probably have got a posititve response.

light, caloric, electric and magnetic fluid. But not everyone cared about the authority of the Paris academy and of Laplacian physics, and not everyone took the Paris experiments to be the last word in the question of electromagnetic action. Nurtured by general beliefs about the relatedness of all forces of nature, some still kept the belief in the possibility of an electromagnetic interaction.

Ørsted , discovering electromagnetic action

One of those was Ørsted . Based on his general views on electrical and chemical action, he emphasized his expectation that electric and magnetic forces should act on each other already in 1812.[13] But only eight years later, in July 1820, he undertook serious experimental work and indeed obtained a positive result:[14] A magnetic needle, suspended like a bussole, was brought near a connecting wire of the galvanic battery (fig. 1). At the moment when the wire was connected to the battery, the needle deviated from its north-south position and settled back only when the wire was disconnected. The experiment showed clearly an action of galvanism onto magnetism. It was this result which he – well aware of its spectacular news – communicated in the most direct, quick, but also expensive way: with the abovementioned printed letter, written in Latin, and sent to researchers all over Europe.

Ørsted 's general belief in the relatedness of the forces of nature had, of course, been supplemented by more specific assumptions. He held, for example, that the "electric conflict" – this is how he called the otherwise unknown process within the connecting wire – had its effects not only within the wire, but probably even in the surrounding space. That idea made him attentive, in contrast to others, to the space around the connecting wire.[15] In general, the pathway to his discovery led from general ("theoretical") assumptions to specific experiment. Such a pathway is not uncommon in research, of course: Hachette's and Desormes' experiment, for example, had a similar structure, even regarding the way it was carried out and stabilized. What were drastically different, of course, were the specific beliefs and assumptions behind the expectations that were to be tested in the experiment. From the episode, we well learn something about the heuristic power of certain basic assumptions in the specific historical situation. But there is nothing to learn about some special type of experiment possibly involved, neither regarding the epistemic structure, nor the way experiments were actually conducted.

More interesting perspectives open up when we look at the wave of experiments stimulated by Ørsted 's discovery. Many researchers emphasized that Ørsted 's result provided not just another singular effect, but opened a whole new field of research. At the same time, the new effect itself offered only little orientation, quite the contrary: it suggested, from first eyesight, that the traditional concepts by which physical effects were usually treated - attraction and repulsion - could not be applied here. That point, explicitly highlighted, caused widespread puzzlement. In the next sections, I shall treat a sample of various attempts to cope with that peculiar situation.

Ørsted , pursuing electromagnetism

The first researcher to look at is Ørsted himself. After he had made public out the news in extreme hectic, he started to work on the topic more closely.[16] The focus and type of his

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[13] (Oersted 1812)

[14] As to the history and background of his researches, Oersted gave different retrospective accounts: (Oersted 1821), (Oersted 1830). Historical studies are given in (Meyer 1920), LXVII-XCVII, (Dibner 1962), and (Snelders 1990).

[15] (Andrade Martins 1999) gives a closer analysis. Of course, Ørsted's knowledge of the former experiences and failures also played an essential role here.

[16] His results were published in a second paper some months later: (Oersted 1820), among others.

research changed significantly: no longer was it central to establish and stabilize one single effect, but rather to explore a broader field of related phenomena. Among others, he analyzed the effect of the battery: While he had initially used quite a large apparatus, he now systematically varied the parameters of the battery: size and number of plates, level of acid, polarity, etc., in order to find out how they affected the electromagnetic action. Besides formulating those dependencies, he realized that much smaller apparatus was sufficient to obtain the effect, even arrangements with a single pair of plates. That made not only the experiments much easier to handle, but provided the opportunity to "invert" the electromagnetic action: Instead of keeping the wire fixed and the magnet moveable, now the wire (together with the small battery) was made moveable (suspended from a wire) and the magnet brought nearby. When Ørsted found the movements of the wire right as expected from considerations of mechanical reciprocity, he went even further and tried to obtain those movements by terrestrial magnetism. Here he failed, however, and surmised that the arrangement was just not sensitive enough.

Schweigger

Although Ørsted published those results quite quickly, he was preceded by others. Johann C. S. Schweigger, professor of physics and chemistry in Halle, and editor of an important journal, had pursued quite similar questions: the effect of various features of the battery, and the reciprocity of the electromagnetic action. Even his experimental procedure was similar: systematic variation of the parameters of the battery and of the arrangement was much in the forefront. Schweigger, however, extended that procedure also to the connecting wire. He realized that the electromagnetic action was enhanced when the wire was not only put above the needle, but additionally led back below it. The procedure could be iterated, thus leading to winding the wire in the space surrounding the needle, an arrangement that largely enhanced the electromagnetic effect. Schweigger called that arrangement a "multiplier" and presented it at a talk at Halle already in September.[17]

Poggendorff

Roughly at the same time, the Berlin student Johann C. Poggendorff took up, on stimulation of his supervisor, the academician Paul Erman, the topic of electromagnetism and was, by similar procedures as Schweigger, led to the insight that the electromagnetic effect could be enhanced by applying many windings of wire instead of only one. Poggendorff experimented on the arrangement (which he called "condenser," since the electromagnetic force was so much concentrated within the windings) and varied systematically its parameters such as number of windings, their mutual connexion (parallel, serial, and mixed, to use later terms), and the diameter of the wire. Again his results were empirical regularities, about the dependency of the effect of the number of windings, for example.[18]

Davy

In London, the reaction to Ørsted 's report came somewhat delayed. Only in October, the report was printed in English translation in a journal. One of those to immediately realize its importance was Humphry Davy, then widely known by his research on electrochemistry, and just about to become president of the Royal Society of London. Davy rushed to experimental work and focused on various topics: the magnetizing effects of the wire, the dependency of the effect on the properties of the battery, the effect of an whole arrangement of wires in contrast to one single wire, the "reciprocity" of the effect, and, in particular, the puzzling spatial constellations. As the list indicates, there was considerable overlap of these topics with those treated by others. Moreover, even the experimental procedure showed close similarities:

[17] He published his results in (Schweigger 1821).
[18] (Poggendorff 1821).

Again the variation of many parameters was the predominant procedure, and again the goal was to formulate ever more general empirical regularities, or, in Davy's words, establishing a "law of the production of magnetism".[19] It should be kept in mind here that Davy, like most other researchers in that early period, had very little or no knowledge of the activities done by others at the same time. One of the most remarkable results of Davy's efforts towards such a "law" was his proposal to determine the direction of magnetization of steel needles nearby the wire by means of the concept of a sense of rotation. The main function of that unusual means of representation was to enable a short and dense formulation of many experimental results.

Ampère, searching for "general facts"

In striking contrast to London, the Paris reactions to Ørsted 's report were most immediate and fierce. After all, it was here that the effect provided a most serious challenge, both to the doctrine of non-interacting fluids in general, and to the notion of central forces in particular – a notion onto which the mathematizing enterprise of the still powerful Laplacian physics was fundamentally based. Thus Ørsted 's report met disbelief and mistrust. Unhappy constellation for the mathematical tradition, its main proponent, Jean-Baptiste Biot, was on travel when the news arrived. Thus an outsider, with a totally unexpected initiative, could work alone in the field, and gain much ground before Biot's return: André-Marie Ampère, professor of mathematics at the Ecole Polytechnique, inexperienced in electricity and in experimental work, rushed to work immediately and managed, by a three month feverish work, to establish himself as a leading figure in the new field, and as the founder of electrodynamics.

From his early work, of which we virtually knew nothing until recently,[20] I shall sketch two episodes. First, Ampère figured out an instrument in which the effect of terrestrial magnetism was drastically reduced: his "astatic needle." The axis of the needle was put right in the direction of the magnetic dip, so the needle could not react to terrestrial effects. With this instrument, Ampère varied many experimental conditions: the strength and polarity of the battery, the length and material of the magnetic needle and, most extensively, the position of the needle relative to the wire (above, below, right, left, horizontal, vertical). Again his aim was to find out what factors were important here, and to formulate regularities. The most difficult task was to formulate how the effect depended on the relative spatial arrangement of wire and magnet. When he realized that the needle was always deflected into a rectangular position, the central problem was to formulate in which of the two possible directions the north pole moved. Ørsted had referred all motions to the compass directions, which had led to lengthy and complicated descriptions and made any generalization impossible. In order to overcome those obstacles, Ampère introduced new concepts. Among others, he introduced the notions of "left" and "right hand side" of the current and explained them by imagining a person, with a current running through him from feet to head. If that person turned his face towards the magnetic needle, his right hand indicated the 'right side' of the current, the left hand the 'left side.' Ampère labeled the resulting regularity (which later became known as Ampère's "swimmer-rule"), as "directive action" and attributed it a special status as a "primitive fact."

But he went even further. He had noted that the battery itself exerted an action onto the magnetic needle, much alike the action of the wire, but in a somehow opposite direction. In order to subsume the two cases under one common regularity, he assigned the galvanic current within the battery a direction opposite to the current in the wire. Shortly later,

[19] (Davy 1821), 14.
[20] The reason is both his specific publication policy and a particularly bad state of the sources. My present account is based on a new type of reconstituting archival material. For a more detailed account, see (Steinle 2000) and (Steinle in press), for my procedure see (Steinle 2003).

however, he saw an easier way. If the direction of the current was no longer referred to the poles of the battery, but rather taken as a sense of circulation, the regularity could be given a coherent and more general form. For the first time in history, the battery was "conceived as forming *one single circuit* with the conducting wire".[21] That notion of a current circuit was to become most fundamental for all further research and was quickly integrated in the language of electrical research. But the background of its formation was to formulate the regularity of the "directive action" under a most general form.

Ampère, proving a theory

For the sake of contrast, I shall sketch a second, somewhat later episode of Ampère's work. Parallel to his search for regularities, he pursued speculations about the 'causes' of the electromagnetic interaction and formed the hypothesis that all magnetism might be caused by circular electric currents within the magnetic bodies. In looking for empirical support, Ampère considered that circular currents should also interact with each other, without any iron involved. In order to test that expectation, he designed a specific experiment. The central part of the apparatus consisted of two spirals of conducting wire. One of them was suspended like a pendulum and most easily moveable towards or away from the other one that itself was mounted on a fixed stand. Ampère expected that the spirals, when connected to the battery, should attract or repel each other. When the effect did not occur, he surmised the obstacles in too much friction of the apparatus and insufficient power of the battery. His attempts went so far that he, by affording half a month's salary, finally bought the strongest battery available in Paris. And with that apparatus, he obtained the expected effect indeed, right in the instrumentmaker's workshop where the experiment was carried out. Only a few hours later, he proudly announced the new effect in a lecture to the Paris academy, and presented it as a "definite proof" of his hypothesis of circular currents as cause of magnetism.[22]

It is worth noting already here how different the role of experiment was in this second episode as contrasted to the first. In the whole second series, the essential elements of the experiment remained unchanged. There was well directed optimizing, not broad exploring, as it had been in the first. From the first idea to the final evaluation, the experiment was defined by a well-defined expectation to be tested. Accordingly, the result was not an "if – then" regularity, but considered to be an experimental "proof" of the theory.

Biot

In contrast to Ampère, Biot had a sharply defined experimental agenda: "The first thing to discover was the law along which the force exerted by the wire decreased in different distances".[23] That was a straightforward execution of the Laplacian program, and Biot consequently developed, together with his assistant Felix Savart, an arrangement to measure that force. The period of oscillation of a horizontally suspended magnet was measured in various distances from a vertically extending wire, and from here by calculation the force law determined. Such a principle of measurement had already been used by Coulomb three decades earlier, and needed just to be transferred to the new situation. Biot and Savart determined an inverse proportionality between distance and force on the whole magnet, and inferred to an inverse square law for the force between infinitesimal elements. After a second, slightly modified series of measurements, they proposed the law: $F \sim 1/r^2 \cdot \sin \omega$ (where F is the force, r the distance, and ω the angle between the wire and the vertical direction), known as Biot-Savart law up to this day.[24] Of course, those experiments were all but trivial:

[21] Archives de l'Académie des Sciences, Paris, Dossier Ampère, chemise 208bis, cf. (Ampère 1820), 198.
[22] In a letter to his son from the very day of the successful experiment: (Launay 1936-43), vol. 2, 562.
[23] (Biot 1821), 228.
[24] (Biot 1820).

Measurements with the extremely unstable Voltaic piles were delicate, and someone with less experimental experience would most probably have been unsuccessful. On the conceptual level, however, nothing new had happened. The force law, in all its exactness, referred only to very few specific experimental constellations. All the complex spatial dependencies of the electromagnetic action were just left out of consideration, in favor of a mathematical law with one specific arrangement. The main goal required by the Laplacian program was thus fulfilled, and it was just by consequence that Biot stepped out of the field.

Faraday

As a last case, I shall briefly deal with another researcher from England. Michael Faraday, then an unknown "chemical assistant" to Davy at the Royal Institution of London, went into electromagnetism only a year later. He had just finished a detailed survey on the state of the field, in the course of which he had reworked nearly all experiments that had been reported.[25] After all, he had free access to one of the best-equipped laboratories in Europe. He took up the question of how the needle behaved in asymmetric positions to the wire – a problem that Ampère had treated, but left unsolved. Faraday focused on the behavior of a horizontal magnetic needle suspended near a vertical wire. In varying the experimental arrangements systematically and broadly, he found the situation to be more complicated than he himself had initially surmised. The problems of traditional language of attraction and repulsion became insurmountable here. Instead Faraday got aware that the concept of circular motion of a single magnetic pole round the wire, or vice versa, allowed a coherent formulation. What is more, if that rotation was taken as a "simple case," all more complicated effects could be deduced from it. Even attraction and repulsion could be understood as being composed of the more elementary rotations. Faraday eventually succeeded in obtaining those rotations in experiment, hereby not only providing a new and spectacular phenomenon, but also strongly corrobating his new conceptual approach to the whole field.[26] Again, the new concept was brought up right as a means to formulate empirical regularities as general as possible.

Romantic experiment?

Some of the researchers I have treated have been called "romantic," such as Ørsted and Schweigger, some others are never put in that row, such as Poggendorff, Ampère and Biot, again others are debated, such as Davy and Faraday. Those assignments are usually based on whether the basic beliefs held by the actors were similar to those of "Naturphilosophie." Is there something, however, that reflects those groupings (if not the label) in research practice? My sample of cases clearly suggests a negative answer. While, with regard to the above aspects of experimental practice, there are well significant differences visible, I do not see any feature that induces a separation concomitant with what usually is taken as the line between romantic and non-romantic actors. The use and character of instruments, the modes of observation, the type of questions pursued, and the ways of evaluating experimental outcomes vary significantly within the sample of historical actors, but none of those differences can be specifically attributed to the "romantic" camp, however framed. Even the use of one's own body is not at all specific "romantic" (though it has sometimes been taken to be so, as because not mediated by instruments): Mainly due to the total lack of sensitive instruments for the effects of the pile, those body-techniques were most widespread, even among researchers such as Volta and Biot. As a result, it becomes clear that there is nothing like a specific "romantic" research practice visible in early electromagnetism, though many of the supposedly "romantic" actors were involved.

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25 (Faraday 1821-22). For a more detailed account, see (Steinle 1995) and (Steinle in press), ch. 6.
26 (Faraday 1821).

Significantly enough, however, the sample of cases points to a different constellation. Regarding experimental practice, it was indeed Biot and, to a lesser degree, Ampère who differed from all the others in quite a number of aspects. Biot was the only one to conduct measurements, and so in most sophisticated manner (Ampère had attempted to do so, but failed), the only to pursue only one highly specified question through all his experiments (the question of the exponent of the force law), and, together with Ampère, the only one to present the results in form of one formula that was taken to be "proved" by experiment. For his contemporaries, Ampère appeared even nearer to Biot in those respects than for us since in his publications he systematically played down or even left out those of his activities that had a more exploratory character (as I have exemplified in my first Ampère section). From an analysis of experimental practice, those approaches appear special indeed, and differing from most other activities in the field. I shall come back to that significant observation in the next section.

Those observations are corrobated by a view to the historical literature of the very period in question. Quite quickly, there were accounts of the history and state of electromagnetism published – Erman in 1821, Faraday in the same year, and Pfaff in 1824.[27] In none of those, however, there was anything portrayed like a special group that we could identify as "romantic." Those who have later been called "romantics" were just treated as mainstream researchers of the day, together with many others. Not that those authors treated the field as undifferentiated and homogenous. They well highlighted specific approaches as special and as different from most of the others. But such a special status was not ascribed to a group of supposed "romantics," but to Biot with his law and to Ampère with his unfolding mathematical theory. It was the mathematizing approach that appeared special.

While my results are based on a rather limited historical study, they are not restricted to the episode of early electromagnetism, but give a characteristic view on the general scenery of electric research in the late 18th and early 19th centuries. To draw only a very rough sketch, one could say that research field was dominated by two major strands: The major one, iconized by Volta's pile, concerned all galvanism with its branches in physiology and electrochemistry, while the minor, but not less visible strand, iconized by Coulomb's torsion balance, focused on quantification and mathematization. In surveying the field, what indeed stands out as a particular group, equipped with its own, specific agenda, was not the "romantics" in whatever sense, but that circle of mathematizing researchers. Essentially confined to Paris, they had an explicit programmatic agenda, they deliberately distanced themselves from the traditional way of doing electrical research, and their works could be understood more or less only by members of the group. And they indeed pursued a rather specific experimental practice, guided by the idea of precision measurements, with all consequences for the type of questions, of actions, and of the use and character of instruments. By contrast, those who are usually called "romantics" were just mainstream contributors to a research strand that involved most researchers in Europe. Such a perspective is well apt to change our view on the "romantic" research, at least in matters of electricity. Whether and how it can be extended to other fields such as physiology, geology, and mineralogy, is an open, but most interesting question.

Exploratory experimentation, concept formation, and electricity

The case of electricity is rich enough to point to a more general point. While the procedures and aims of experiment in my above cases show no distinction between "romantic" and "non-romantic," they nevertheless point to distinctly different types of experimental procedures

[27] (Erman 1821), (Faraday 1821-22), (Pfaff 1824).

pursued. In many cases, a sort of method of variation of many experimental parameters was in the forefront, with the goal to obtain a set of empirical regularities. Ørsted (in his second series), Schweigger, Poggendorff, Ampère (in his first series), Davy and Faraday, had a quite similar approach here. By contrast, however, Biot's approach was different, and so was Ørsted 's first and Ampère's second episode. Rather than broad variation, there was the optimizing of one experimental arrangement much in the forefront, with the goal to determine one numerical factor, or to test a specific, well-formulated expectation.

The two approaches point to two different types of experimental activity. The second type comes close to what can be taken as the 'standard view' of experiment. There was a theory that led to expecting a certain effect; the expectation led to designing and conducting an experiment; and the outcome of the experiment counted as specification or support for the theory. This is how traditional philosophy of science has conceived experiment in general.[28] The other cases, however, show a different type of experiment that I have labeled "exploratory".[29] Far from being a mindless playing with the apparatus, it has definite guidelines and epistemic goals. The main experimental procedure is the systematic variation of experimental parameters, with the aim to find out which of them do affect the effect in question or are even essentially required. The central goal is to formulate empirical regularities about those dependencies and correlations. In many cases, moreover, the existing concepts and categories come out to be inappropriate for that purpose. Thus the revision of existing concepts and categories becomes a topic, and the formation of new ones that allow a stable and general formulation of the experimental results. It is here, in the realm of concept-formation, that exploratory experimentation has its most unique power and importance. Acting and conceptualizing stabilize or destabilize each other on every step. The two types of experiment differ not only in their epistemic goal and the guidelines for actual experimental work, but also in the character of the instruments and apparatus. Instruments for exploratory work must allow a great range of variations, and likewise be open for a large variety of outcomes, even unexpected ones. In testing well-formulated expectations, by contrast, instruments are specifically designed for a single effect. The possibilities of variations are much restricted, and so is the openness for outcomes that are not in the range of the expectation. As both Ampère's second instrument and Biot's arrangement illustrate, the high specificity of the apparatus has its cost in loss of flexibility and openness for unexpected results.

Exploratory experimentation is much more common in scientific research than hitherto has been realized. Just from the history of electricity, many episodes could be listed here, from Gilbert to Dufay to Galvani and Humboldt, from Faraday to Plücker to Röntgen, up to high-temperature superconductivity in the 1980s.[30] In other fields such as chemistry or the life sciences, cases are legion. That exploratory experimentation has escaped attention so long has mainly to do with that it typically does not appear in the presentation of scientific results, even not in later tales of scientists about their doing. After all, it is often the shifting of basic categories, concepts and representational means that is on stake here. And, as already Ludwik Fleck has pointed out, it is extremely difficult, after such a shift or reformulation observed, to re-imagine the previous and imperfect state of investigation.[31]

Rather than being typical for certain subject fields or traditions, exploratory experimentation is connected to specific epistemic situations: those in which the very conceptual foundations

[28] (Popper 1934), (van Fraassen 1981), to give just two examples.
[29] For a more detailed account, see (Steinle 1997), (Steinle 2002), or (Steinle in press), ch. 7.
[30] I discuss those cases in (Steinle in press), ch. 7. (Burian 1997) presents a significant case from early genetics.
[31] (Fleck 1979), 86.

of a field become questionable and thus offer no guidance for experimental acting. Early electromagnetism offers a typical example. Many researchers were puzzled about the features of the effect, and the most bewildering challenge was the spatial properties of the new effect, since it was here that traditional concepts did not work. Hence it was here that, in the course of exploratory experimentation, new concepts were formed: “left and right of the current” and “current circuit” by Ampère, circular patterns by Davy, and circular motions (first imagined, then realized) by Faraday. Later on, Faraday would form, in a very similar context, even the concept of lines of force. All those concepts were introduced as a means for formulating empirical regularities, with explicit exclusion of consideration of microscopic processes. For Biot, by contrast, the situation was different. Being deeply committed to the Laplacian view of the world, there was no way for him to put in question its basic concepts. Thus he worked, in contrast to the others, in a mode of conceptual stability. His experimental approach was clearly set by the program: to find an arrangement that allowed to determine a force law. Broadly varying many experimental parameters would directly have counteracted this well-set agenda. Similar observations hold for Ampère’s deliberate testing procedure in my second episode.

The difference between exploratory and theory-driven approaches throws a new perspective on electrical research in the early 19th century, particularly on the abovementioned particular status of the Paris approaches. The research on the Voltaic pile and all related fields was heterogeneous, with both exploratory and theory- or speculation-driven approaches being pursued widely, sometimes in one and the same person, as the case of Ørsted illustrates. Those committed to the mathematizing approach, by contrast, nearly exclusively pursued theory-driven experimentation. The conceptual assumptions of the program were strong, and provided so strict constraints as to boil down the goal of most experiments to determining one specific parameter: the exponent in the force law. That such a law existed, and that it had the form of simple power function, were not questions, but presuppositions of the framework; indeed, only those presuppositions made the well-directed experimentation possible. The scenery illustrates nicely how much different types of experimentation are connected to different epistemic situations, to more or less (supposed) stability on the conceptual level.

Epilogue

In their outline of this conference, the organizers raise the question whether there are other ways of scrutinizing Nature than by mathematics. The question might have been rhetorical: it is all too obvious that not only there have always been those approaches, but that they have been playing a fundamental role in research! There is another question, of course, lurking behind the organizer’s point: is it such a non-mathematical way of scrutinizing nature that we call “romantic?” Again, from the perspective of electricity, the answer is very clear: By no means had every non-mathematical approach to do with “romanticism!”[32] One may just think of such names as Galvani, Volta, Arago, de la Rive, van Marum, or Poggendorff. Nearly everyone took non-mathematical approaches, the only exception being the small Paris group of mathematizers. And it is not per chance that only this group pursued a unique type of experiment, distinct from all others. A look at experimental practice does well throw new light both on the so-called “romantic science” and on experimentation in general.

[32] And, one might add, not every romantic approach was unmathematical, as the case of the “romantic physicist” *par excellence*, Johann Wilhelm Ritter shows. With his extremely abstract and powerful scheme for dealing with galvanic arrangements he came much closer to an mathematical formalism than any of his contemporaries: (Ritter 1798).

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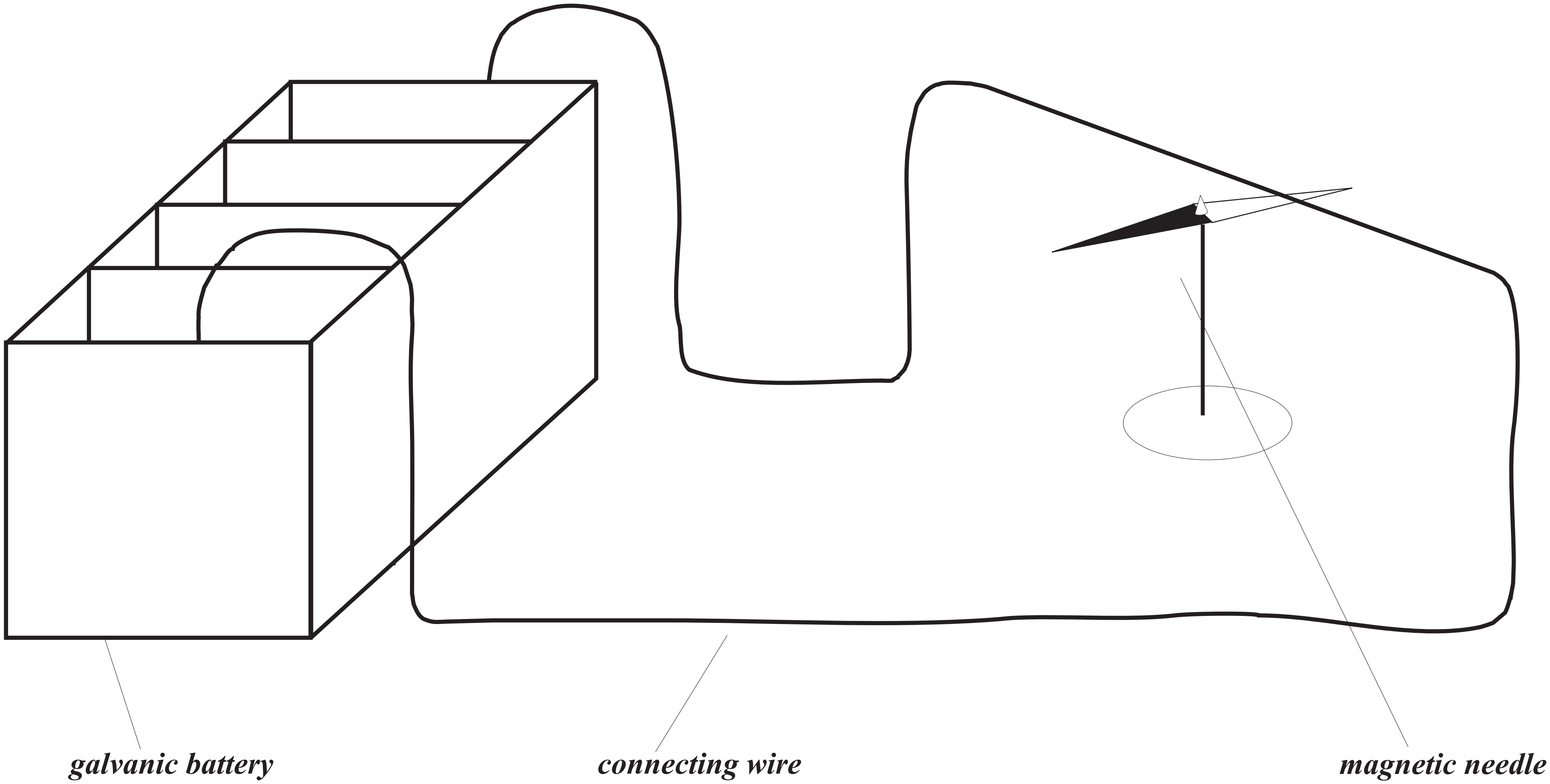

Schematic representation of Oersted's1820 experimental arrangement