Experiments in History and Philosophy of Science

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The increasing attention on experiment in the last two decades has led to important insights into its material, cultural and social dimensions. However, the role of experiment as a tool for generating knowledge has been comparatively poorly studied. What questions are asked in experimental research? How are they treated and eventually resolved? And how do questions, epistemic situations, and experimental activity cohere and shape each other? In my paper, I treat these problems on the basis of detailed studies of research practice. After presenting several cases from the history of electricity—Dufay, Ampère, and Faraday—I discuss a specific type of experiment—the "exploratory experiment"—and analyze how it works in concept formation. I argue that a fuller understanding of experiment can only be achieved by intertwining historical and philosophical perspectives in such a way that the very separation of the two become questioable.

Experiment, as concept and process, has interested scholars in the history and philosophy of science for a long time. Francis Bacon, often cited as the champion of modern experiment, pointed out a variety of epistemic functions of experiment, including such different activities as the production of new phenomena, the classification of these phenomena, and deciding between competing theories and hypotheses through the presentation of "crucial" experiments. In the nineteenth century, ideas about the function of experiment developed further, with Mill's discussion of his four "experimental methods" as a prominent case, in which experiment was essentially regarded as means to search for causal relations.[1] Throughout the twentieth century, however, philosophy of science narrowed its perspective on

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1. Mill 1843, book III, chs. 7 and 8.

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experiment significantly. In response to all too naïve inductivist views, Duhem rejected any idea that the role of experiment was to *generate* theories, leaving the *testing* of theories as the only role of experiment.[2] Such a restricted view, much corroborated by Reichenbach's explicit distinction between the contexts of discovery and justification (at least as a popular, though misguided interpretation), eventually became the philosophical "standard view" of experiment in the twentieth century. Whether called the "handmaiden of theory" or "theorizing with different means," it was believed that experiment dealt only with well-defined questions posed by theoreticians, and *only* with those.[3] To be sure, it was only in rare cases that the role of experiment was discussed at all. This standard view of experiment was accepted without much explicit discussion. This held even for Thomas Kuhn's revolutionary view of scientific development and the ensuing discussions: All seemed to focus on theories and their dynamics, while experiment received little attention, let alone new accounts regarding its function in scientific inquiry.

Only in the 1980s, did philosophy of science again take up the question of experiment. The "New Experimentalism" in philosophy of science arose, stimulated by Hacking's emphasis on a "Baconian variety" of experiment,[4] clearly emphasizing the insufficiency of the older accounts. At the same time, historians of science began to focus on experiment, with all its local, cultural, material, rhetorical, and social aspects. Far from being marginal, such aspects were clearly shown, in many cases, to have a marked influence on the questions pursued by experimental activity and the interpretation of "facts." While many new perspectives on experiments have been presented, it is significant that one central area has remained peculiarly unanalyzed, that is, how experiment and *knowledge* relate. Experiment, in all its material, cultural and social dimensions, serves as a central tool in generating knowledge. The shortcomings of recent literature concerning experiment point clearly to how necessary it is to bring questions of knowledge and epistemology back into the picture.[5] How do, for example, certain types of experimentation relate to specific epistemic situations and goals, and how should such situations be characterized? Despite important attempts to address these issues,[6] this field of questioning still remains largely unexplored. This is the focus of my paper.

An important point that has become obvious in recent decades is

2. Duhem 1906.

3. van Fraassen 1981, p. 673; Popper 1959, § 30.

4. Hacking 1983, ch. 9.

5. Hacking (1992) emphasized that need explicitly.

6. Gooding (1990); Burian (1997); Heidelberger (1998); Graßhoff, Casties and Nickelsen (2000), for example.

that the generally understood contrast between theory and experiment is much too unspecific to allow further insights. Both New Experimentalists and some of their opponents agree on the need to differentiate further discussion of "theory" by re-introducing the concept of empirical regularities, for example, or by differentiating between observation theories, etc.[7] In this essay, I shall expand on these previous discussions by systematically taking into account the types of *questions* pursued by experimenters, and the characteristic features of various epistemic situations. What different types of questions are asked in experimental research, how are they pursued, changed, and perhaps resolved, in experiment, and how do the features of experiment themselves shape those questions? What are the specific epistemic situations in which certain experiments are deemed necessary, designed, conducted, and then evaluated? How do questions, epistemic situations, and experimental activity cohere and shape each other? I will argue that pursuing such an analysis opens up important new perspectives on experiment. Furthermore, such an approach will necessarily involve both historical and philosophical analysis, thus pointing to a type of study that deserves much more attention than it has received so far.

In what follows, I will rely on historical cases. First, I sketch out four episodes from the history of electricity. A particular approach to experiment emerges here, one that is distinctly different from the standard account. I call it "exploratory experimentation" and shall, in the ensuing sections of my paper, discuss its specific details. On the base of that discussion I shall return to the general question of this special issue and develop new perspectives on the relationship between history and philosophy of science. I contend that a better understanding of experiment can only be achieved by intertwining historical and philosophical perspectives in such a way as the very separation of the two become questionable.

Charles Dufay on two electricities

My first case deals with Charles Dufay, an early eighteenth century, brilliant academician and director of the Paris botanic garden. When he started his research in electricity in the 1730s, the field was in an unstable and incoherent state.[8] More than a hundred years of research throughout

7. Hacking 1983, pp. 159, 165; Cartwright 1989, p. 352; Carrier 1998, p. 182. Hacking (1992) was the first to give an explicit "taxonomy."

8. Heilbron (1979), ch. 9, gives a brief account of Dufay's research. Dufay's eight Mémoires at the Paris Académie between 1733 and 1737 provide my main source; cf. also his own English summary (Dufay 1734) and archival material from the Paris Academy. Electricity here always means, of course, what we nowadays call static electricity.

Europe had produced a multitude of different and puzzling phenomenal effects, such as:

- Some materials could be electrified by rubbing, others sometimes, again others not at all.
- Sometimes bodies could be made electric by contact with others, sometimes not.
- Sometimes electricity acted as attraction, sometimes as repulsion.
- Sometimes sudden changes regarding attractive and repulsive effects occurred.

Dealing with those questions proved difficult, even more so since the experiments were delicate, the effects tiny, and repeatability difficult. Dufay conducted extensive experiments, varying the procedure in many ways: he might introduce a vast number of different materials, single or in combination, but also shape, temperature, color, moisture, air pressure, and the experimental setting: two bodies in touch; in close neighborhood; in large distance; being connected by a third; and so on. His work led to remarkable results and bold claims such as *all* materials except metals could be electrified by rubbing, and *all* bodies except a flame could receive electricity by communication. But still he was left with serious questions as to when attraction and repulsion occurred and then when it sometimes suddenly switched.

Dufay continued to conduct many experiments, with electrified and/or unelectrified bodies attracting or repelling each other, before and after touching each other. From those experiments, he extracted a regularity, which held for every two bodies: When an unelectrified body was attracted by one that was electrified, and touched it, it would suddenly repel after the contact. The sequence of attraction—contact—repulsion formed a regularity that comprised a lot of previously puzzling phenomena. But it came into difficulties when more than two bodies were applied. When, e.g., the repelled bodies acted *on a third one,* they sometimes repelled each other, in other cases, however, there was attraction. With more bodies, the situation became even more complicated. The regularity was not sufficient to account for those effects. After additional experiments, Dufay finally made what he called a "bold hypothesis": If one did not speak of electricity in general, but of two electricities, with similarly electrified bodies repelling each other, dissimilarly electrified ones attracting each other, then his experimental results suddenly started to make sense. The two electricities, which were produced by rubbing certain materials, had to be conceived as corresponding to two classes of materials. Since glass and wax were the most prominent representatives of these classes, Dufay spoke of "vitreous" and "resinous" electricities. With such a concept, he could immediately account for hundreds of experiments, i.e., he could subsume them under general regularities! It was that success that convinced Dufay that he had hit upon a crucial point, and this is how the two electricities entered our scientific thinking.

Some observations concerning this episode (of which my presentation gives not more than a rough sketch) should be added here:

- Thinking in terms of two opposite electricities was accepted within a few decades and has been common talk ever since. We take such a concept as unproblematic, self-evident, or even "natural." As I have indicated, however, the concept was created, shaped, and stabilized by a huge effort, and in the context of most intense experimental work.
- Dufay is often said to have "discovered" the two electricities. That talk is in many ways misleading. Dufay did not find a ready-made thing in the world; indeed, he never claimed that such a thing as two electricities really existed. What he actually did, was to realize that the use of such a concept enabled him to coherently express a great number of experimental results as regularity.
- There was a specific type of experiment involved. Rather than testing of a theory, the epistemic goal was to formulate regularities and create appropriate notions to express them. I shall discuss the specificities of the experimental procedure later.

Ampère on electromagnetism, looking for "general facts"

In July 1820, the Danish researcher Hans Christian Ørsted announced his finding of the action of a galvanic current on a magnetic needle. The experimental arrangement consisted of a galvanic battery with its "closing wire" and a magnetic needle suspended as compass needle. When the wire was brought near to the needle and connected to the battery, the needle immediately deviated from its normal North-South position and returned to it as soon as the wire was disconnected. Ørsted's discovery opened, as his contemporaries expressed repeatedly, a new and totally unknown field of research: electromagnetism. There was, moreover, fundamental puzzlement over how to grasp or even just formulate the experimental results. The needle was not attracted to or repelled from the wire, but set itself somewhat across. Even more mysterious, the deflection of the needle changed when it was placed over the wire rather than below it. Such a behavior was incompatible with the notion of attractive and repulsive forces, a notion on which all reasoning on physical processes was based. Thus, there was no language, and thus no conceptualization available even to formulate the experimental results in somewhat general terms. Ørsted himself—and many others after him—referred all motions of the needle to the compass directions which led to lengthy and complicated descriptions and made any generalization impossible. The needle set itself not only across the wire, but also across established thinking.

It was in Paris that the challenge was most sharply felt. Besides Jean-Baptiste Biot, the main proponent of the established Laplacian physics, it was an outsider who threw himself into intense studies: André-Marie Ampère, professor of mathematics at the Ecole Polytechnique, and totally inexperienced in electricity and in experimental work. Ampère rushed to work immediately and managed in only three months of feverish work to establish himself as a leading figure in the new field.[9] I shall sketch an episode of his very first steps, a period about which we knew little until recently.[10] Far from being drawn towards a certain theory, as the received view of Ampère suggests, his research of that period followed tortuous pathways, pursued various goals in parallel, and had an uncertain and open-ended character. First, he 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 needle; and, most extensively, the position of the needle relative to the wire—above, below, right, left, horizontal, vertical, etc. His aim in those variations was to find out which of those factors contributed to the deflection of the needle and to formulate regularities.

The most difficult task was to formulate how the effect depended on position. Ampère realized that the needle always tended to take a right angle towards the wire when the two were in a symmetric position (i.e., when the line of shortest distance went through the middle of the needle).

9. My account on Ampère in this and the next sections is based on my more extended, Steinle (in press), chs. 2–4. For an overview of Ampère's research, and considerations about the reasons of his unexpected initiative, see also Blondel (1982) and Hofmann (1995).

10. Ampère's early research has, due to a particularly bad state of the sources, not been well studied, even not in the excellent account by Blondel (1982). Williams (1983) has tried to make sense of Ampère's own few and incoherent historical remarks in Ampère (1820a), though with unconvincing result. Through an extended study of diverse archival material, I have been able to reconstruct his activities; cf. Steinle (2000) and (in press), chs. 2 and 3, and appendix.

But to which of the two possible positions did the North Pole move? It was here that the lack of language became particularly pressing. Ampère, in order to formulate a regularity, had to introduce new concepts. In order to facilitate reference to the polarity of the battery, for example, he spoke of the "so-called galvanic current." The notion of "current" had been used before in various contexts, mostly to refer to some (speculative) processes within the wire, but Ampère's explicit instrumental use was essentially new. Even more so, Ampère introduced the notions of "left" and "right hand side" of the current and explained them by imagining a man with a current running through him from head to toe. If that man turned his face towards the magnetic needle, his right hand indicated the "right hand side" of the current, and the "left hand" accordingly. Only with such a concept, later known as Ampère's "swimmer-rule," was Ampère able to formulate one coherent regularity that he called "directive action" and that comprised a great deal of experimental results.[11] The notions of "left" and "right" of the wire illustrate strikingly how difficult it was to form concepts which allowed researchers to express experimental results in some generality.

Yet, that was not yet sufficient. Ampère had realized by chance that the battery itself exerted an action onto the magnetic needle, much like the action of the wire. In order to subsume the two cases under one regularity, he had to assign the galvanic current within the battery a direction opposite to the current in the wire: not from the copper- to the zinc-pole as in the wire, but in reverse direction. A few days later, however, he saw an easier way. If the direction of the current no longer referred to the poles of the battery, but was rather taken as a sense of rotation, the regularity could be given a coherent and more general form. The concept of current as mere direction was much sharpened here and, this is the essential point, the battery was "conceived as forming *one single circuit* with the conducting wire."[12] The notion of a current circuit, comprising likewise the battery and its connecting wire, was introduced here for the first time.[13] Again, it is worth noting that that concept was to become most fundamental for all further research, in particular for Ampère's own later theorizing about circular currents as the cause of magnetism, and it quickly became part of the language of electrical research. But the point is that it was created in the context of intense experimental work of a specific type. Its original func-

11. Ampère 1820b, p. 197.

12. *ibid.,* p. 198; my emphasis.

13. Again the talk of "circuit" had been much older, but Ampère gave it an essentially new meaning by treating the battery and the connecting wire in exactly the same manner.

tion, and the reason for its introduction, was to facilitate formulating the regularity of the "directive action" under a most general form.

Ampère on electromagnetism: proving a theory

To offer a contrast, I sketch a somewhat later episode. Parallel to his search for regularities, Ampère pursued speculations about the *causes* of the electromagnetic interaction. By tortuous pathways, he arrived at the hypothesis that all magnetism might be caused by circular electric currents within the magnetic bodies. That was a breathtaking perspective indeed. Not only was an exceedingly wide scope envisaged, but the possibility appeared that one could treat such a theory mathematically, a point of utmost importance to Parisian academicians.

In looking for empirical support, Ampère considered that if circular currents interacted with magnets and behaved like magnets, they should also interact with each other, without any iron involved. In order to test this hypothesis, he designed a specific experiment. The central part of the apparatus consisted of two spirals of wire, placed face to face in two parallel vertical planes. One of them was mounted on a fixed stand, while the other one was suspended like a pendulum and moved easily towards the first spiral. Ampère expected that the spirals, when connected to the battery, should either attract or repel each other. But at first, and for a long time, he could not obtain that effect. He suspected the failure was caused by too much friction within the apparatus due to inappropriate suspension techniques (a particularly delicate point that involved the totally new technical challenge of combining the best electrical contact with the least mechanical friction) or by insufficient battery power. His attempts to optimize any of these components went so far that he spent a half of a month's salary on the strongest battery available in Paris. And with that apparatus, he succeeded in obtaining the expected effect, right there in the instrument maker's workshop. Only a few hours later, he proudly announced the new effect in a lecture to the Paris academy, presenting it as a "definite proof" of his hypothesis of circular currents as cause of magnetism.

The experiments, which finally led to that result, differed significantly in character from those described above. Throughout the series, the central elements of the experiment remained unchanged—the arrangement was designed, after all, to test a clearly formulated expected effect. What actually changed in the course of the experimentation were parts, which Ampère considered as possibly hindering the effect. He conducted well-directed optimizing, not broad exploration. From the first idea to the final evaluation, the experiment was defined by the expectation of a hypothesis. Accordingly, the result was not an "if—then" regularity, but considered to be an experimental "proof" of the theory.

Michael Faraday on induction and magnetic curves

Electromagnetism developed rapidly in the decade after Ørsted's initial discovery of 1820. New effects became known, such as rotatory motions, and new devices developed, such as electromagnets. Ampère worked out his theory, soon called "electrodynamics," that comprised the interaction of currents and aimed at explaining all electromagnetic and magnetic effects. One major unsolved problem remained. Despite repeated attempts, the reversion of Ørsted's effect could not be obtained: an action of magnetism onto galvanism or electricity.[14] When Michael Faraday, then director of the laboratory of the Royal Institution of London, was for the first time free to choose his own research agenda in the summer of 1831, he chose electromagnetism and took up this question.[15] Probably stimulated by recent improvements in electromagnets, he designed a special arrangement: a soft iron ring with two separate sets of coils wound around it, one of which was connected to a battery, the other to a galvanometer. And indeed he succeeded: As soon as the first coil was connected to the battery, there was a detection of a current in the second coil. Since the two were separated, Faraday realized that he had succeeded in obtaining the long-sought induction effect.

But the effect raised many questions. The induction ring was a complicated arrangement, and it was not at all clear whether the induced current in the second coil was caused by a magnetism of the ring which itself originated in the current of the first coil, or by some direct influence between the two coils. Even more puzzling, the effect occurred only when the current was switched on and off, and not when the current was steady. These questions kept Faraday from publishing the effect immediately. Instead he undertook several months of intense experimental work. His general idea was to simplify the arrangement and to separate the diverse circumstances. In doing so, he varied a great many of the experimental parameters such as the geometry of the arrangement, the setting of the coils, the material of the core and the use of the battery. At first, he distinguished between two types of induction: induction of currents by currents, without any magnets involved (this he called "Volta-electric induction"), and induction of currents by magnets, without any other current (such as by a battery) in-

14. For the various attempts to obtain an induction effect, see Ross (1965) or Devons (1978).

15. The following sketch is based on my account in (Steinle 1996) as a result of studying both Faraday's laboratory Diary (Martin 1932–6) and his published account (Faraday 1832a). For an extended study of the episode, see also Romo and Doncel (1994). For Faraday's situation around 1830, see the sketch in James (1991a), pp. xxxv-xxxvi, or else in James (1991b).

volved "magneto-electric induction." For both types, he attempted to formulate a regularity.

I shall only deal with his work on magneto-electric induction, a case that turned out to be much more complicated than Volta-electric induction. Again much of this work concerned the variation of experimental parameters, such as the magnet's shape and strength, the shape and thickness of the wires, and the overall configuration. Once it became clear that the relative motions of the wire and magnet were an essential factor, he varied both direction and speed. It was here that the most serious difficulties arose. In particular, it was not clear what features of the apparatus provided an appropriate reference system for expressing those motions: Faraday tried the magnetic poles, the direction of the wire, the direction of the magnet's axis, the compass directions, and even Ampère's hypothetical circular currents within the magnet, but in no case could he formulate a regularity consistent with the experimental results. Finally, he tried the set of "magnetic curves," i.e., the patterns formed by iron filings around a magnet. Those curves had long been known but never considered more than a curiosity. Success was immediate. All the experimental results could now be comprehended under a single principle, the "law of electromagnetic induction." In whichever way the wire was moved, a current was induced in it, as long as it literally "cut" the magnetic curves. With this law, Faraday not only could comprise most of the effects of magneto-electric induction, but also deduce other induction effects.[16] Most important, the transient character of the effect was no longer mysterious, but built into the law: the effect lasted only as long as the motion continued.

It was with this episode that "magnetic curves" obtained, for the first time, an important status in magnetic and electromagnetic research. Later to be renamed "lines of force," they would eventually form the fundamental concept of electrodynamic field theory. However, when Faraday introduced it for the first time, he hurried to emphasize that it was nothing but a means to formulate regularity: the curves should be conceived only as "mere expressions for arranged magnetic forces."[17] Both the experimental work and the problem constellation in which he made his unusual proposal show striking analogies to the Dufay and first Ampère case.

Exploratory experimentation

The above cases show different uses of experiment. On the one hand, the type of experimental activity in my second Ampère case closely reflects the

16. Shortly later, he even widened the concept so far as to include all effects of volta-electric induction, (Steinle 1996).

17. Faraday 1832b, § 238.

common 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 success of the experiment counted as support for the theory. Philosophers, such as Duhem, Popper, van Fraassen, and many others, who in other ways hold differing views, would agree: yes, this is how experiment works.[18] Indeed, the case shows that there *are* experiments of such type in scientific research, even if one goes beyond the scientists' own rhetoric and concentrates on practice. But by focusing on research practice, a richer perspective on the possible role of experiment opens up. And indeed, the more differentiated views on experiment brought forward in the last two decades are based on studies of research practice. Hacking's notion of "topical hypotheses" as contrasted to background knowledge and systematic theories, has some similarities to what David Gooding introduced as "construals," meaning provisional interpretations near to the experimental level. Hans-Jörg Rheinberger, with his division between "epistemic things" and "technical objects," points sharply to the precarious location of experiments in the ever-shifting area between stability and openness.[19]

Not much attention has been paid, however, to how those different epistemic goals may result in different types of experimental work. The historical episodes provide sharp indications here. Dufay was definitely not interested in microscopic theories about the 'hidden nature' of electricity (though he was well aware of the long history of speculations on that question), but rather he intended to establish regularities on the level of phenomena and experiments, in a field that he found in an incoherent and unstable state. His experimental procedure was directed toward that goal, but at some point he realized that the existing vocabulary and concepts were simply insufficient or inappropriate. Likewise, Ampère's goal in his early research was to find a way to express what he and others saw as a most puzzling experimental result, and to formulate regularities about the electromagnetic effect. It's worth noting, as a historical observation, that Ampère was far from alone in that respect. With no knowledge of each others' work, many researchers all over Europe, in the period right after Ørsted's announcement, pursued a similar type of experimental activity.[20] What they had in common, despite their differing theoretical opinions on the nature of electricity and magnetism, was their peculiar puzzlement. They realized that traditional notions, let alone their theo-

18. Duhem (1906), ch. 10; Popper (1934), § 30; van Fraassen (1981)

19. Hacking (1992); Gooding (1986); Gooding (1990); Rheinberger (1992); Rheinberger (1997).

20. Pictet (1820); Ørsted (1820); Schweigger (1821); Davy (1821); Seebeck (1821); Configliachi (1821).

ries, were insufficient for expressing the strange behavior of the magnetic needle when placed near the wire. Likewise, Faraday, in analyzing the puzzling induction effect, focused on formulating the experimental results into some generality, but realized that the traditional conceptual resources did not suffice.

For convenience, I have labeled that type of research "exploratory experimentation."[21] Far from being a mindless playing around with an apparatus, exploratory experimentation may well be characterized by definite guidelines and epistemic goals. The most prominent characteristic of the experimental procedure is the systematic variation of experimental parameters. The first aim here is to find out which of the various parameters affect the effect in question, and which of them are essential. Closely connected, there is the central goal of formulating empirical regularities about these dependencies and correlations. Typically they have the form of "if—then" propositions, where both the if- and the then-clauses refer to the empirical level. In many cases, however, the attempt to formulate regularities requires the revision of existing concepts and categories, and the formation of new ones, which allow a stable and general formulation of the experimental results. It is here, in the realm of concept-formation, where exploratory experimentation has its most unique power and importance. There is, finally, often the attempt to develop experimental arrangements that involve only the necessary conditions for the effect in question and thus represent the general regularity or law in a most obvious way. Those experiments are attributed a particular status in that they serve as core effects to which all other phenomena of the field can be "reduced."[22]

None of those points, of course, are epistemologically innocent:

- In the procedure of systematically varying experimental parameters, the crucial question is, of course, which parameters to vary, and how many. In principle, the number of possible parameters to be varied is unlimited. Neither a starting nor an end-point seem to be discernible. In research practice, however, things are different. First, previous experience in the field or in related ones provides some ideas about where to start, i.e., about what might be promising candidates for being relevant parameters and what not. In the case of electromagnetism, nobody started with varying the color of the wires, since it was well known, from numer-

21. Steinle (1995, 1997), among others.

22. It is this systematic character of exploratory experimentation which has largely been overlooked. David Gooding, for example, in his account of Faraday's work, does not even mention the central enterprise of "reducing" certain effects to a "simple case;" cf. Gooding (1990).

ous experiments in the mid-eighteenth century, that color did not affect electrical effects. Without providing a rigid framework, those aspects enable, in all their variability with individual backgrounds, a pragmatic entry point into the procedure. Second, the question when to end is also pragmatically treated. After all, the procedure of systematic variation has a definite goal: to formulate stable and ever more general empirical regularities. Once a tentatively formulated regularity comes out to be stable, that result is usually taken to indicate that the essential experimental conditions have been grasped, i.e., that the variation procedure has succeeded. Further variations are only needed when there is the intention to widen the scope of that regularity. And in this respect, it is mainly a matter of personal ambition, and of the resources and time available, how far the procedure will be driven. Dufay was quite ambitious here, as was Faraday. Ampère, by contrast, saw a pathway to extend further the generality of his regularities, but dropped the enterprise at some point, in favor of a different one that would more likely resonate in the Paris academy. While stability is an essential requirement for regarding the variation procedure as successful (and hence to stop it), the scope of generality to be achieved is rather a matter of choice. Between the pragmatically shaped points of start and end, there is a wide space for individual preferences, biographical and cultural factors, and not the least for chance, to affect the choice of certain factors to be varied.

- Discussion of empirical regularities and then contrasting them to theories in a narrow sense, which involve reference to "theoretical entities," has its own problems. As common as such a distinction was in the nineteenth century, it became much debated in the twentieth. Though it has become clear that the very categories of empirical, observable, and hidden are not entirely sharp (and, moreover, are historically shifting), it seems likewise obvious that it makes good sense to keep some aspects of these distinctions.[23] This holds in particular for all studies of research practice, and it is not by chance that the distinction has been taken up by philosophers of the "New Experimentalism."[24] An aspect that has not found much attention is that stable empirical

23. The point is clearly exhibited by Nagel (1961), ch. 5.

24. Hacking (1983, p. 159) or Cartwright (1983, p. 352), for example. Even critics of the New Experimentalism concede the need for differentiating the general notion of theory: Carrier (1998).

regularities are required for a reliable handling of the field in question, even though there may be no explanatory theory at hand.

- The point of revising categories and concepts is crucial and opens the perspective beyond naïve empiricist accounts. After all, the procedure of varying parameters in order to get insight into correlations and dependencies is reminiscent of the four experimental methods proposed by John Stuart Mill, who cast them in terms of a search for causal relations. In Mill's view, however, the categorization of the field had to be achieved and fixed in advance by a rather unspecified process, mainly by a sort of contemplation.[25] Only within an already given conceptual system could Mill's experimental procedures lead to insights into causality. In research practice, by contrast, it is often the case that the categories which researchers had initially in mind end up being inappropriate in that they do not allow for the formulation of stable regularities. And in the ensuing process of revising, newly forming, and stabilizing categories and concepts, experimentation often plays a central role. Any new concepts have to prove themselves by enabling the formulation of stable and ever more general regularities of the experimental results. During the whole process, experimentation and the formation of concepts are closely intertwined. Action and conceptualization stabilize or destabilize each other at every step.[26]

It is not just a single regularity that is the result of exploratory work, or a disconnected collection of those, but often a coherent system. At its core, there are the most general regularities or "laws." Named "general facts," "simple" or "pure cases," "elementary experiments" or the like,[27] they refer both to verbal formulations by means of certain categories and concepts, and to experimental arrangements which involve only the necessary experimental conditions and thus represent the regularity in particularly clear form. Any particular effect can be attributed a definite place within the system, connected to the core effects by a chain of intermediate effects,

25. Mill 1843, book III, chs. 7 and 8. A related shortcoming of Mill's account is the missing insight that the scope of experimental factors to be varied is unlimited in principle.

26. The point of the flexibility and revisability of concepts within experimental acting has been overlooked even in the very stimulating account of experimentation presented by Graßhoff, Casties and Nickelsen (2000).

27. Those are terms used by Ampère and Faraday on different occasions; cf. Ampère (1820b); Faraday (1821); Faraday (1832a).

and thus be explained by, or "reduced" to the general laws. Thus the system of regularities gains explanatory power.[28]

The contrast of exploratory experimentation to the theory-driven type, as understood as the standard view, is not only visible in the different epistemic goals (search for regularities vs. test of expectations), but also in the character of the guidelines of the experimental activity. The rather unspecific guidelines of exploratory experimentation bear a methodological character, and give rise to a variety of broadly dispersed experiments. The categories and concepts by which experiments are described and ordered arise typically at the end of experimental series, as their very result. Theory-driven experiments, in contrast, have such an ordering—and much more: a formulated, though perhaps provisional theory—as a precondition from the outset, and are in all essential details determined by that theory. Not a broad variety, but a single, elaborated arrangement is typically dealt with here. A third related difference is visible in the character of the instruments and apparatus used. Instruments for exploratory work have to allow for a great range of variations, and likewise be open to a large variety of outcomes, even unexpected ones. The restrictions posed by the instrumental arrangement must not be too confining. 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 to outcomes that are not in the range of expectation. Ampère's different experimental arrangements are a good illustration of this point. Whereas the instrument for the "directive action" allowed many variations of position and many different outcomes, the apparatus for the attraction of spirals was restricted to proving or disproving the attraction of spirals. The suspension of the moveable spiral, for example, excluded, by its very design for lowest friction, any sidewise or rotatory motions of the spiral. The theory-driven character of the experiment was reflected in the high specificity of the apparatus, at a considerable cost to flexibility and openness to unexpected results.

Experiment and concept formation

The far-reaching epistemic significance of exploratory experimentation has to do with its specific goal. It is directed at ordering and categorizing, at the level of categories and concepts, as elements of language. Exploratory experimentation typically starts when those categories have been destabilized, i.e., been revealed as being inappropriate to deal with the effects in question. Experimentation then goes hand in hand with revising,

28. I have elaborated that point for an earlier Faraday episode: see Steinle (1995).

reforming, and re-stabilizing those categories. The main criterion for stabilization is that the revised or newly formed concepts allow exactly what the former ones failed to do: formulating stable and more or less general regularities on a phenomenological level, and thus enabling a reliable handling of instruments and apparatus. After such a process has come to a successful end, a new view has been achieved, and often the new concepts are included in common talk. They form part of the language guiding even everyday actions, disappear as possible objects of revision, and tend to appear as unproblematic or even "natural." At the same time, the older view disappears. Once a new and successful conceptualization or, to use a great phrase, a new language, has been formed, it becomes difficult to put oneself back in the previous state in which that language did not exist. Ludwik Fleck, one of those reflective practitioners who pointed to an epistemic variety of experiment, highlighted that point sharply:

> if after years we were to look back upon a field we have worked in, we could no longer see or understand the difficulties present in that creative work. The actual course of development becomes rationalized and schematized. We project the results into our intentions; but how could it be any different? We can no longer express the previously incomplete thoughts with these now finished concepts.[29]

It is those very points which give the new categorizations and concepts lasting effect and influence. Structuring a field on the level of categories and language means fundamentally shaping all further research, to give it certain directions and rule out many others in rendering them literally unspeakable and unthinkable. Whereas the immense epistemic significance of categorizations and language have often been realized in the life sciences—one may just recall all the efforts of classification in natural history—it has been much out of focus and underestimated in the physical sciences and in the hereupon based traditional philosophy of science.

The above cases are illustrative here. Dufay's two electricities became unproblematic parts of electrical language within a few decades, and the former approach of conceiving one uniform electricity disappeared entirely. It is indicative, for example, that even in the later debate between one-fluid and two-fluid theories it was never questioned that talking about two electricities made perfect sense when it came to ordering the field and to guiding experimental activity. The debate rather concerned

29. Fleck ([1935] 1980, p. 114); English translation from Fleck (1979), p. 86. Similar observations by practitioners can well be found in Claude Bernard (1865), Michael Polanyi ([1958] 1994), and François Jacob (1998).

the question of how to explain those concepts by a microscopic theory: by assuming two fluids, or rather imbalance (excess and lack) of one fluid. Likewise, Ampère's concept of a current circuit rapidly became an unproblematic part of the common talk on galvanism, replacing the older view according to which current was defined with reference to the poles. Most prominently, Ampère himself considered, in all his later reasoning on microscopic circular currents, the concept of a current circuit as an unproblematic foundation. Similar observations hold, finally, for Faraday's "magnetic curves," though it took, in that case, several decades until the concept appeared acceptable—it was just too intellectually distant from traditional thinking. Nowadays, however, we take it up in school. Since those notions now appear as somewhat natural, the very fact that they have been created out of hard labor easily slips out of view.

The disappearance of exploratory experimentation from scientists' own presentations led also to its disregard and serious under-representation in historical and philosophical accounts. A closer look at research practice, however, shows that it has been far more common in scientific research than hitherto realized. Many basic notions of scientific language have been formed in the context of such activity. A brief look at the history of electricity offers striking cases. I list but a few:

- The first clear and systematic division between electric and magnetic effects by William Gilbert in 1600 was based on broad exploratory experimentation.[30]
- After Luigi Galvanism's spectacular announcement in the 1790s of the effects which quickly were named after him, broad exploratory research started in many parts of Europe. Within a short period, new and abstract means of the representation of galvanic arrangements were developed by Alexander von Humboldt and Johann Wilhelm Ritter.[31]
- In the work of Michael Faraday, there are many more striking examples of long-lasting and successful exploratory experimentation. His research on magnetism, for example, resulted in a complete reorganization of the research field and finally in his self-confident proposal of using "lines of force" as the fundamental concept of electricity, magnetism, and electromagnetism.[32]
- When Julius Plücker undertook, in the 1860s, research on electric discharges in rarefied gases, he developed in long exploratory

30. Gilbert 1600.
31. Trumpler (1997, 1999).
32. See Gooding (1985a, 1985b); Steinle (1994, 1995, 1996).

periods the basic concepts for dealing with that puzzling and endlessly variable field.[33]

Examples can also be found in other fields:

- The concept of chemical reaction was formed and developed in the seventeenth century on the background of broad experimentation, not by single prominent individual researchers, but rather within a community with a structure that allowed for close communication.[34]
- In entering the "jungle" of organic chemistry in the 1830s, most intense and broad exploratory experimentation led to developing new concepts and means of representation, such as formulas.[35]
- Biochemical research was revolutionized in the 1930s by the introduction of the idea of circular reaction patterns by Hans Krebs. The experimental research that led Krebs, in studying urea biosynthesis, to develop that idea for the first time, had long and essential exploratory phases.[36]
- In another field, Jean Brachet's research on protein biosynthesis in the 1930s and 1940s led to introducing new central concepts. His experimental work was mostly of the exploratory type. The fact that Richard Burian introduced the category of "exploratory" experimentation in analyzing Brachet's work, strongly supported my independent proposal. Burian's emphasis that "the style of exploratory experimentation . . . *should* be of great historical and philosophical interest" significantly underscores my claim of the high epistemic importance of that activity.[37]

In all those cases, selected rather randomly, there was intense exploratory work conducted, categories revised, and new concepts developed. Although not all of those revisions were as fundamental as the first clear distinction between electric and magnetic effects or the introduction of circular reaction patterns into biochemistry, the long-lasting formative power of these developments is still highly visible. As the case of the concept of chemical reaction illustrates, many of the categories and notions thus developed have even become parts of everyday language.

As the above list illustrates, exploratory experimentation is not so much bound to certain historical periods, fields of research, or scientific

33. Hiebert (1995).
34. See Klein (1994, 1996).
35. See Klein (1998).
36. See Holmes (1993); Graßhoff, Casties and Nickelsen (2000).
37. Burian 1997, p. 27, original emphasis.

traditions, but first and foremost to specific epistemic situations: those situations namely in which, for reasons whatsoever, the very concepts by which a certain field is treated have been destabilized and become open for revision. Situations in which theories and well-formed expectations are tested, in contrast, require a well-elaborated conceptualization, a stable language by which the expectation can be expressed in the first place. Exploratory and theory-driven experimentation are connected to different constellations and situations of our knowledge, to different regimes of stability on a conceptual level.

History and philosophy of science

The case of exploratory experimentation illustrates my initial claims about the necessity of combining historical and philosophical research. On the one hand, exploratory experimentation, by its intimate connection to the level of categories and language, is of essential (though often underestimated) importance in scientific research. Neither the process of generating knowledge nor the structure and boundaries of the resulting knowledge claims can be truly understood without taking exploratory experimentation into account. Both are, likewise, problems of history and philosophy of science. On the other hand, the only way to specify such a type of experimentation and to study its characteristics is a detailed analysis of research practice in contrast to later presentations of scientific results and, often enough, even to scientists' own later tales about their activities. Using Jacob's metaphor, we have to focus seriously on the "night science," whereas philosophical analysis hitherto has almost exclusively dealt with "day- science." Such work is of a genuine historical type, and has to be informed by modern historiographical methods and standards. At the same time, however, it is plainly clear that such analysis has to be guided by questions about the epistemic process itself, its differentiations, the levels of knowledge aimed at, and the explanatory goals that researchers have in mind— i.e. by questions that have a genuinely epistemological character. Research of such a type can no longer be clearly identified as historical or philosophical in the traditional disciplinary sense.

Additionally, I have emphasized and illustrated that exploratory experimentation is less a matter of particular subject fields, periods or traditions, but rather of specific types of epistemic situations in which it typically occurs. The qualifier "typically," that is to say, "typically, but not necessarily," is deliberate. What actually happened in specific situations was only rarely determined by such an epistemic principle alone, but also by the actor's specific situation—biographic, sociological, material—and sometimes even by mere chance. Studies of research practice that incorporate those different aspects reveal, in a striking manner, how that complexity entails an inherent fragility of the research process at every step.[38] In epistemic situations which (according to principles of empirical research) would have required broad exploring, researchers could chose (and actually did so sometimes) to follow other than exploratory pathways. Sometimes that led them into a cul-de-sac, sometimes to totally unexpected successes that changed drastically the set of interesting questions.[39] Such an *impure* mixture of general epistemic principles and specific historical, material and biographical constellations is, I think, most characteristic not only of experimental research, but of scientific development in general.

Such an observation gives rise to fundamental questions about the status of general epistemic procedures and of specific historical and cultural settings, questions that also arise in the other papers in this special issue. On the one hand, there seem to be concepts, guidelines, principles of reasoning, and experimental procedures that are typically connected to epistemic situations of a specific type, situations that may occur in diverse subject fields and historical periods. Those features seem to be to some degree trans-contextual and trans-historical. On the other hand, it is likewise clear that the specific local, material, historical, social and cultural settings of knowledge generation do affect the questions posed, the modes of reasoning, the guidelines and meta-notions ("error", for example), the experimental procedures, and hence the knowledge generated.

Taking both of these insights seriously, we are placed in a somewhat uncomfortable and most challenging situation. We have to give up the easy generalizing claims, be it the claim that *all* scientific reasoning went and goes along unchangeable epistemic principles, standards and methods (a claim not entirely absent from traditional and even some recent philosophy of science), or the opposed claim that *all* epistemic standards and principles were subject to fundamental historical change in such a way that nothing remained constant and that even the attempt to look for trans-contextual constants could be nothing more than totally anachronistic (again a claim not unheard of in some recent history of science). It has never been too difficult, of course, to make cases for the one claim or the other, even the papers of this volume could provide select material here. But exactly studies like those presented here make clear that none of these general claims leads to an adequate understanding. Instead, a middle, but even more challenging assumption comes to the fore: the claim that there are certainly *some* epistemic principles which can be found throughout the

38. For a discussion of an illustrative Faraday case, see my (1996).

39. The contrast between Ampere, Biot and Faraday, for example, in their simultaneous research on electromagnetism, is most instructive here; cf. chs. 3, 4 and 6 of my (in press).

history of science, in different periods and subject fields, while, at the same time, the actual process of science is driven not only by those, but also by much more specific and changing notions and principles, dependent upon particular historical, practical and cultural settings. As a corollary of such a claim, it comes out that the actual pathway of science can never be appropriately grasped by exclusively focussing on the changing historical settings or on unchanging epistemic principles. Accepting that uncomfortable situation and finding the means to deal with it may well be regarded as the most intriguing challenge to HPS.

Such an enterprise requires definite transgressing of the disciplinary boundaries between history of science and philosophy of science. An appropriate understanding of practice requires scholars to take *seriously* into account the historical, social, and cultural particulars of the case and the variety and differentiations of the epistemic process itself. I do not claim that *all* of history of science or *all* of philosophy of science should be done that way. It seems to be obvious, however, that specific deficiencies of the traditional disciplines can be made up for only by cultivating that intermediate area. One of the major problems here is, of course, the sensible shortage of analytic concepts appropriate to deal with that field. For the most part, traditional philosophical notions such as explanation, reality, theory or experiment, have too sterile a shape to grasp the historical varieties, while genuine historical notions are often not suited to deal with the differentiations of the epistemic process. Those who engage in studies of the intermediate field thus face a situation analogous to that of scientist dealing with a wide range of phenomena and experiments while the very language in which they should be grasped and formulated is in flux. There is the need to revise, adjust, and refine existing concepts, or even to create new ones, all that in face of immense material to be dealt with: a veritable exploratory task, and experimental as well.

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