CHALLENGING ESTABLISHED CONCEPTS: AMPÈRE AND EXPLORATORY EXPERIMENTATION†

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ABSTRACT: Despite rich historical studies on experiment, a philosophical understanding of the epistemic function and relevance of various types of experiment is still missing. The first first part of this paper deals with Ampère's early research in electromagnetism, a period which hitherto has escaped notice. Focussing on research practice as contrasted to published accounts, I show that Ampère pursued a particular type of experimenting which I call "exploratory". This led him to develop new basic concepts such as the notion of a current circuit. In the second part, I switch to general considerations on exploratory experimentation, its significance in concept formation, and the reasons why it escaped notice so long.

Keywords: experiment, electromagnetism, Ampère, research practice.

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> The more unknowns there are and the newer a field of research is, the less well defined are the experiments. Once a field has been sufficiently worked over so that the possible conclusions are more or less limited to existence or nonexistence, and perhaps to quantitative determination, the experiments will become increasingly better defined. But they will no longer be independent, *because they are carried along by a system of earlier experiments and decisions*, which is generally the situation in physics and chemistry today. Such a system will then become self-evident know-how itself. We will no longer be aware of its application and effect (Fleck 1935 (1980), p. 114, translation slightly altered from Fleck 1979, p. 86, original emphasis).

As Ludwik Fleck pointed out already in 1935, experimentation has many faces. Nevertheless, philosophy of science has for long been concentrated on only one of them. The standard view, according to which the main epistemic function of experiment lies in testing hypotheses and theories, is still held by many, despite the dissenting claims of New Experimentalism. This has partly to do with the lack of a systematic account of the variety of experiment and a detailed understanding of its epistemic functions. In particular, processes of forming and stabilizing concepts and theories have to be taken much more seriously than was suggested by the distinction betweeen discovery and justification. In this paper, I shall concentrate on processes of such a type. First, I shall present a historical case: Ampère's early research in electromagnetism, a period which was formative for all his later research, but which, for several reasons, has not been analyzed in detail. In analyzing that period, I identify different types of experimental work, with different backgrounds and functions. In the ensuing sections of the paper, I shall discuss those types in more general terms. In particular, I shall focus on what I call exploratory experimentation, contrast it to the standard view, and explicate its characteristics and its epistemic significance.

1. Raising a challenge: Electromagnetism in 1820

In July 1820, the Danish researcher and influential president of the Royal Danish Academy of Sciences and Letters, Hans Christian Oersted, announced his finding of the action of a galvanic current onto a nearby magnetic needle. The announcement, distributed in an uncommon way by a printed letter to researchers in all parts of Europe, was immediately received as sensational and caused a wave of research.[1] The effect could be replicated everywhere without difficulty: all the equipment used was standard, though in a combination previously unthought of. The experimental arrangement (fig. 1) consisted in a galvanic battery with its "closing wire," and a magnetic needle suspended as bussole.[2] When the wire was brought near the needle, and connected to the battery, the needle was deviated from its normal north-south position, and returned to it as soon as the wire was disconnected. The effect showed most clearly an action of galvanism onto magnetism, an action which had in vain been sought for since decades (an account of the earlier work is given by Dibner 1962). Oersted's discovery opened, as many of his contemporaries expressed, a new and totally unknown field of research: electromagnetism.

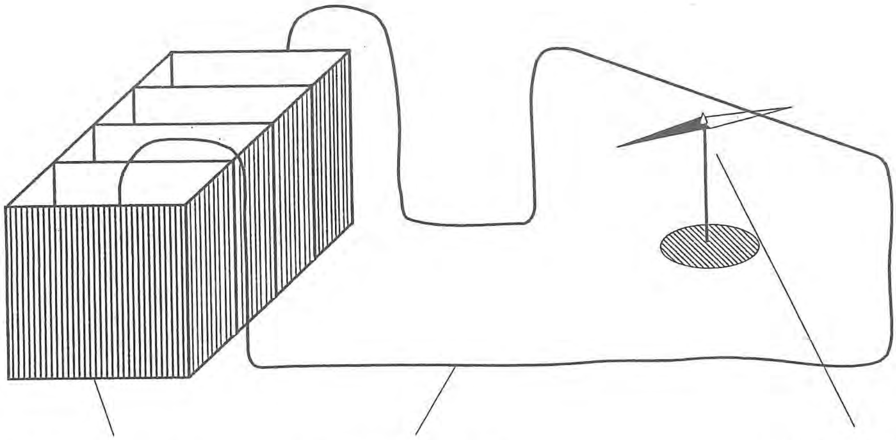

Fig. 1: sketch of Oersteds experimental arrangement

The vehement reaction caused by the effect was partly because some researchers had, after the continuous failure of the search of an interaction between electricity and magnetism, not only given up that search, but turned it into the more or less explicit belief of the impossibility of such an interaction. In the most elaborate electric theory of the period, presented by Poisson in 1812, there was just no space left for such an interaction. Oersted's discovery came here as a serious challenge which was much intensified by some properties of the effect. First, the needle was not attracted to or repelled from the wire, but set itself somewhat across, in a manner which was difficult to grasp in detail. Second, and even more mysterious, the deflection of the needle changed when it was put over instead of below the wire, even while the distance remained the same. Such a behaviour appeared as strikingly incompatible with the notion of attractive and repulsive forces, a notion on which all reasoning on physical processes was based. Oersted himself carried many experiments in order to cast the action of the needle into a regularity, but only with limited success. He finally proposed the idea that the "electric conflict" -a name he invented to point to whatever happened in and around the wire while discharging the battery- "performed circles" around the wire. He did not, however, explicate further that idea and indeed weakened it himself by adding that, to be exact, the conflict had to be considered as a *spiral* movement, which did not fit, however, to any experimental results (Oersted 1820a, p. 276). Probably due to those unclarities, and to the strange character of such an idea for most of Oersted's contemporaries, his proposal was taken up only much later.

In Paris, the reaction to Oersted's discovery was particularly controversial, and all the ensuing work was done in a nervous and hectic spirit. After all, it was the basic notion of the still powerful Laplacian physics -central forces, depending only on the distance- to which Oersted's result so visibly provided a challenge (for Laplacian physics, see Fox 1974). Not surprisingly, the main proponent of that school in the physical sciences, Jean-Baptiste Biot, threw himself into most intense studies. But he could do so only with some delay since he was travelling at the time when the news arrived. At his return to Paris six weeks later, he did not only get the news about Oersted's result, but had to learn that there had already been much activity from a rather unexpected side: from André-Marie Ampère, professor of mathematics at the Ecole Polytechnique, who had not been known before for any interest in electricity, let alone in experimental work (for a general account of Ampère, see Hofmann 1995). Ampère's unexpected interest was partly kindled by that he saw the chance to follow up earlier speculations about a unified account of electricity, magnetism and heat, partly by that he was not sympathetic to Laplacian orthodoxy and now saw a door open to develop an alternative program, and partly by that he was unhappy with his professional situation and hoped to make himself widely known by entering such a spectacular field.[3] Ampère rushed to work immediately after the news of Oersted's effect arrived in Paris. Only two weeks later he presented a first lecture to the academy, and that was just the offbeat to a three-month and nearly unbroken series of weekly lectures in which he continually provided new experimental and theoretical results. Parallel to his lecturing activities, Ampère successfully pushed out a number of publications, both in journals and separate leaflets. At the end of the year, he had established himself as a leading figure in the new field, and presented the outlines of a totally new research field: the effects of interacting electrical currents, later to be named electrodynamics.

2. Ampère's entry: exploring a new field

In this and the next section, I shall discuss an episode out of Ampère's research of the first three weeks after the news had become known, a period about which he himself told nearly nothing Only by recent historical research, based on extensive reconstitution of original material, insight into that phase of Ampère's work was obtained.[4] The results challenge the common picture in which Ampère is often portrayed as so much theoretically oriented that, at the very outset of his research in electromagnetism, he already had a strict conceptual framework in mind which led him to do his experiments. Though Ampère's own presentations well give rise to such a view, a detailed analysis of his research practice leads to a totally different picture. Far from being well-directed towards a certain theory, Ampère's research of the first weeks followed tortuous pathways, pursued various goals in parallel and had an uncertain and open-ended character. To be sure, there was at some point the pronounced emergence of a well-directed research program. But that was not the starting point of his research, but came only after three weeks, as the result of most intense work of a different kind. In what follows, I concentrate on that preceding work.

Ampère's experimental work had a totally different character in that early period than later on. His first activities were directed to get a better idea of the new effect itself. He started with Oersted's arrangement and varied many experimental conditions: the strength and polarity of the battery, the length and metal of the magnetic needle and, most extensively, the position of the needle relative to the wire (above, below, right, left, horizontal, vertical) and to terrestrial magnetism. His aim in those variations was to find out the different factors which contributed to the deflection of the needle and to formulate regularities. He quickly suceeded in designing an instrument by which the effect of the wire onto the needle could be studied separate from the interferring effect of the earth: his "astatic needle" (fig. 2).

The most difficult task was to formulate how the effect depended on position. Since the peculiar behaviour of the needle could not be grasped by traditional notions of attraction and repulsion, and since distance seemed to play a much lesser role than (relative) spatial orientation, a new reference frame or at least a terminology was required, capable of expressing those constellations. The problem had been obvious already in Oersted's report: every single experiment was described with reference to the compass directions. This had not only led to lengthy and complicated descriptions of the positions of wire, needle and battery, but at the same time made any generalization impossible. Exactly such a general regularity, however, comprising more than one experimental arrangement, was Ampère's goal. By working with his "astatic needle" he realized that the needle, by the action of the wire, was always deflected into a rectangular positioin. But to formulate that results independently from the compass directions, he had to introduce new concepts:

> Directive action: If a magnet and a galvanic conductor act on each other, one of them being fixed, the other being allowed to rotate in a plane perpendicular to the line of shortest distance, then the moveable tends to move in such a way that the directions of the conductor and the axis of the magnet will form a right angle, and the north pole of the magnet will be on the left, the contrary pole on the right of the so-called galvanic current (Dossier Ampère, folder 208bis, § 10, my numeration)[5].

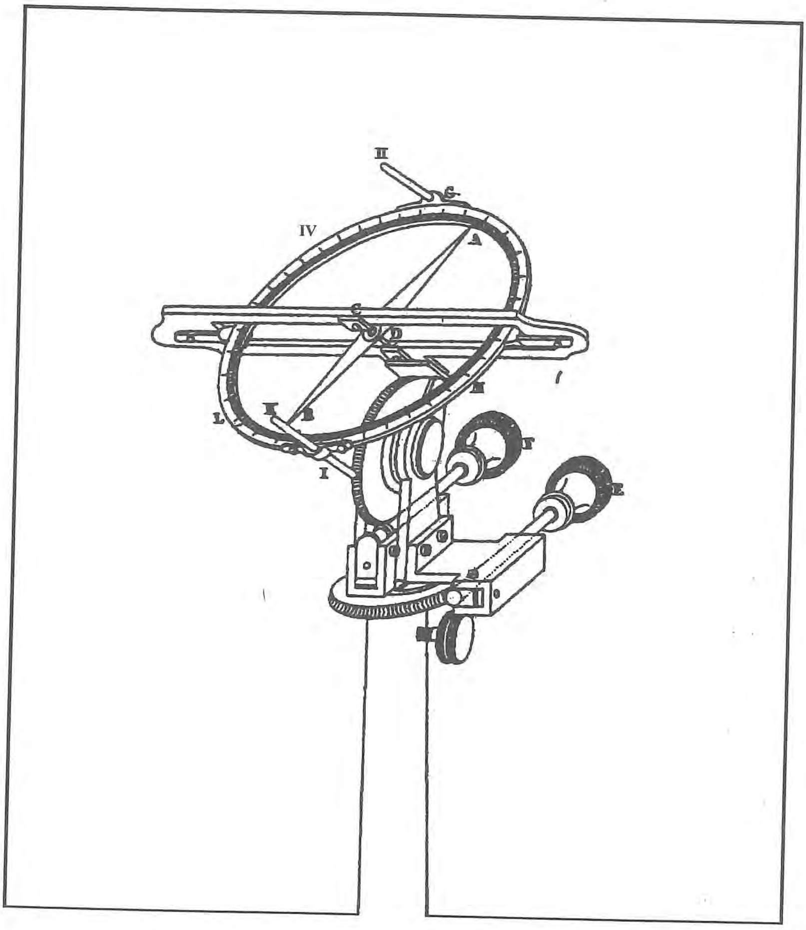

Fig. 2: Ampère's 'astatic needle' (Ampère 1820b)

The "if - then" form of the passage is significant: it states the conditions under which the electromagnetic effect takes place, and describes how exactly the effect looks like under those conditions. In that context, Ampère's talk of current (a notion which had been used before in various contexts) was explicitly instrumental as a shorthand device for facilitating reference to the polarity of the battery:

> It should be noted, moreover, that I use the word "galvanic current" only in a conventional sense in order to define the directions involved. That expression can be applied as in common usage, even if one does not admit the real existence of such a current (*Ibid.*, § 12).

But the notion of current with a certain (conventionally introduced) direction was not sufficient to express the sideways deflection of the needle unequivocally. In the above passage, Ampère introduced the notions of "left" and "right hand side" of the current and explained them thus:

> In order to understand what I mean by the words right and left of the galvanic current, one has to form an idea analogous to how one defines the right and left of a river, by imagining a person moving downstream and looking towards the river mouth. Imagine, in a similar way, a person placed within the galvanic current, the current running from the feet to the head, and the face turned towards the magnetic needle. Then the person's right hand indicates what I call the right of the current, the left hand the left of the current (*Ibid.*, 208bis, § 13 and 19, later cancelled).

In a somewhat abridged version, that proposal later became known as Ampère's "swimmer-rule." It illustrates strikingly how difficult it was to form concepts which allowed at least to express the experimental results in some generality.

3. Forming a new concept: the current circuit

In an appendix to the above regularity of the "directive action," Ampère distinguished two cases: one for the magnetic action of the wire, the other for the magnetic action of the battery itself, an action which had shortly before been discovered by Ampère himself. The experimental results in these two cases could only be grasped under one and the same regularity if the current within the battery was assigned the direction opposite to the current in the wire (*Ibid.*, 208bis § 11). One should keep in mind here that the action of the closed voltaic pile had traditionally been referred to the poles of the battery. Whatever went along in the wire, be it called galvanic current, electric conflict or something else, was considered to run from one pole of the battery to the other. The naming of the poles varied: some gave them the names of the last metal plates at the respective end of the pile (copper or zinc), while some others like Ampère preferred to name them according to whether hydrogen or oxygen developed at their side of an interposed electrolytic cell. The battery itself, however, had always been treated separately from the wire, and in totally different terms. In particular there had been no idea of a current running within the battery. The case of Oersted who, in discovering the magnetic action of the wire, did not in the least consider to try whether the battery itself exposed a similar effect, is most illustrative for a common understanding of the period.[6] Ampère himself had only by chance become aware of the magnetic action of the battery. That result led him to assume that also within the battery such a current was running, a current whose directions he referred, as usual, to the poles of the battery. And since he tried to make his regularity of the "directive action" to hold both for the wire and the battery, he had to assign the currents opposite directions. That step highlights that it was the general expression of the regularity which he aimed at, not a physical theory of galvanic currents. After all, such a concept of opposite currents did not offer itself easily to a physical interpretation of the galvanic current.

A few days later, probably in revising his text for an Academy lecture, Ampère saw a way to overcome that separate treatment and to give his regularity an even more general form by redefining the concept of current:

> One can comprise the two cases [of the opposite currents in the battery and the wire; FS] in one single definition by understanding as galvanic current just the direction into which hydrogen and the bases of salts are transported by the action of the battery. Thus the battery is conceived as forming one single circuit with the conducting wire, a circuit which can be interrupted everywhere by interposing water or a salty solution which is decomposed by the action of the battery (*Ibid.,* (footnote 12), § 10a; cf. the printed version in Ampère 1820b, p. 198).

It was in this passage that the notion of a current circuit, comprising likewise the battery and its connecting wire, was introduced for the very first time. Since that notion became most fundamental for all further research, the characteristics of its introduction may well be emphasized.

The new concept indicates a change of perspective: the focus of attention switched from the poles of the battery to the direction of rotation within a closed circuit (fig. 3). In other words, a change of the reference system was effected here. That change was clearly reflected in Ampère's terminology: whereas he had formerly spoken of the "*pole* at which hydrogen was developed," he now emphasized the "*direction* in which hydrogen was transported."

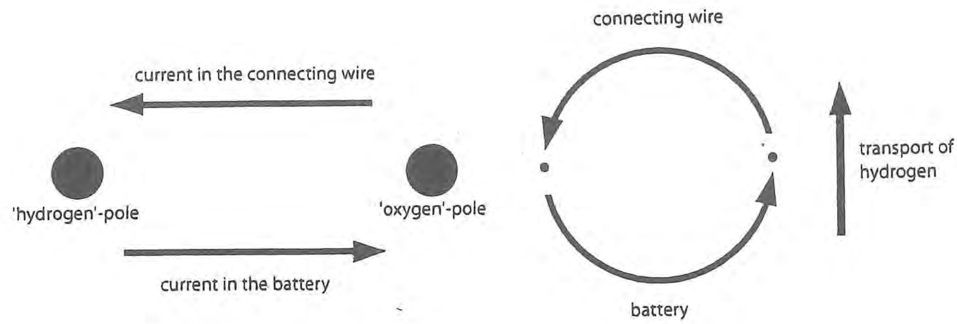

Fig. 3: Transition from poles to the current circuit

The main function of the concept of current circuit, and the reason for its introduction, was to facilitate formulating the regularity of the "directive action" under a most general form. It was here that the new concept did its work: the regularity could now be formulated much more coherently than before. The instrumentalist understanding of current as a "common way of talking" without reference to its real existence, was still maintained, as Ampère again emphasized explicitly (*Ibid.*). After all, he defined the current not with reference to physical processes within the wire, but as a "direction."

The concept of current circuit was well compatible with other empirical results. Defining current as the direction in which hydrogen was transported throughout the whole circuit, only made sense if such a direction could be spoken of even within the battery. Knowledge about the chemical processes in the battery made clear that the hydrogen was indeed transported in the required direction,[7] thus supporting the idea of a uniform direction within the circuit.

It is remarkable that such a fundamental concept was formed and developed right within the context of formulating empirical regularities, as a means to render those formulations ever more general. Microscopic theories about what went on in the wire or the magnetic needle did not play a role. The episode provides a characteristic case for the general features of the type of experimental work which dominated Ampère's early research and which I will discuss more generally below.

Still, however, the regularity of the directive action comprised only those effects in which the wire was parallel to the plane of rotation of the magnetic needle, and the needle was centrally placed against the wire, i.e. the line of shortest distance went through the center of the needle. In other, noncentral and nonparallel arrangements, the experimental results were much more complicated and could not be grasped by that regularity. Already Oersted had reported on some of those cases (Oersted 1820a, p. 275), and Ampère set off to develop a more general regularity. In the course of many experiments, he realized another important effect: The needle as a whole was attracted to the wire if suspended perpendicular to it in the position which it acquired by the directive action. Ampère expressed that effect again in a general "if-then" form, like the "directive action", and called it the "attractive or repulsive action." From those two actions, he claimed, *all* deflections of the needle could be readily understood, or "reduced" to them.[8] The central status which Ampère ascribed to these two actions was highlighted by that he called them "general facts". Such a program of "reducing" a whole field of effects to some central regularities is remarkable and quite specific.[9] I shall come back to it below.

4. Supporting microscopic currents

Ampère, however, though he announced such a program, did not really work it out. He knew well that there were a great many of experiments to be done before his claim could be regarded as fulfilled. But at a certain point he recognized a different possible pathway of research, focused on considerations and speculations about the 'causes' of the electromagnetic interaction. By tortuous pathways which I cannot lay out here, he arrived at the hypothesis that all magnetism might be caused by circular electric currents within the magnetic bodies.[10] Since he quickly realized that those currents were not detectable by ordinary means (after all, magnetic bodies had been analyzed for ages!), Ampère surmised that they might be of microscopic dimensions. Pursuing that speculation further, he got aware that it finally led to dealing with the interaction of galvanic currents among each other. That was breathtaking perspectives indeed, perspectives which appeared more attractive than reasoning about regularities and "general facts." Not only was an incomparably wider scope envisaged, but there appeared even the chance at the horizon of treating such a theory mathematically. That latter point was, of course, of utmost importance in the Paris academic scene in which Ampère wanted to succeed in the race against Biot.

It was for those reasons that, after a first, merely exploratory phase, and an ensuing short phase in which the two research agendas ran parallel, Ampère deliberately took a harsh turn in his research direction. He left his work on the general facts behind, well aware that it was unfinished, and concentrated on the question of hypothetical circular currents, a topic which eventually would render him most famous.

His work on that topic went along with a quite different type of experimenting. After all, his goal was no longer to formulate regularities about effects on the phenomenal level, but to know more about hypothetical entities, considered to lay beyond the realm of phenomena. I shall pick out just one episode. From the moment when he had formed the hypothesis of circular currents as possible cause of magnetism, Ampère sought for experiments which could provide empirical support. As a possible means of indirect support he developed the idea to "imitate" the supposed arrangement of circular currents in macroscopic scale with wires, and to examine whether they would behave like magnets. Thus he formed spirals and helices of wire and studied their interaction with magnets. In many cases those experiments came out to be successful in that the spirals and helices, when connected to a battery, acted on magnets just like other magnets would have done. There was one failure, however, which worried him particularly. If circular currents, so he considered, interacted with magnets and behaved like magnets, they 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 (fig. 4) consisted of two spirals A and B. B was suspended like a pendulum and very easily moveable towards or away from A, while A was mounted on a fixed stand. Ampère expected that the spirals, when connected to the battery, should attract or repel each other, depending on the polarity of the connexion. But he could not obtain that effect for a long time. He surmised the reasons of the failure 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 instrumentmakers workshop where the experiment was carried out, for want of time to carry the apparatus to Ampère's own laboratory. 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 (cf. Ampère's letter of 19 September 1820 to his son: Launay 1936-43, II, p. 562).

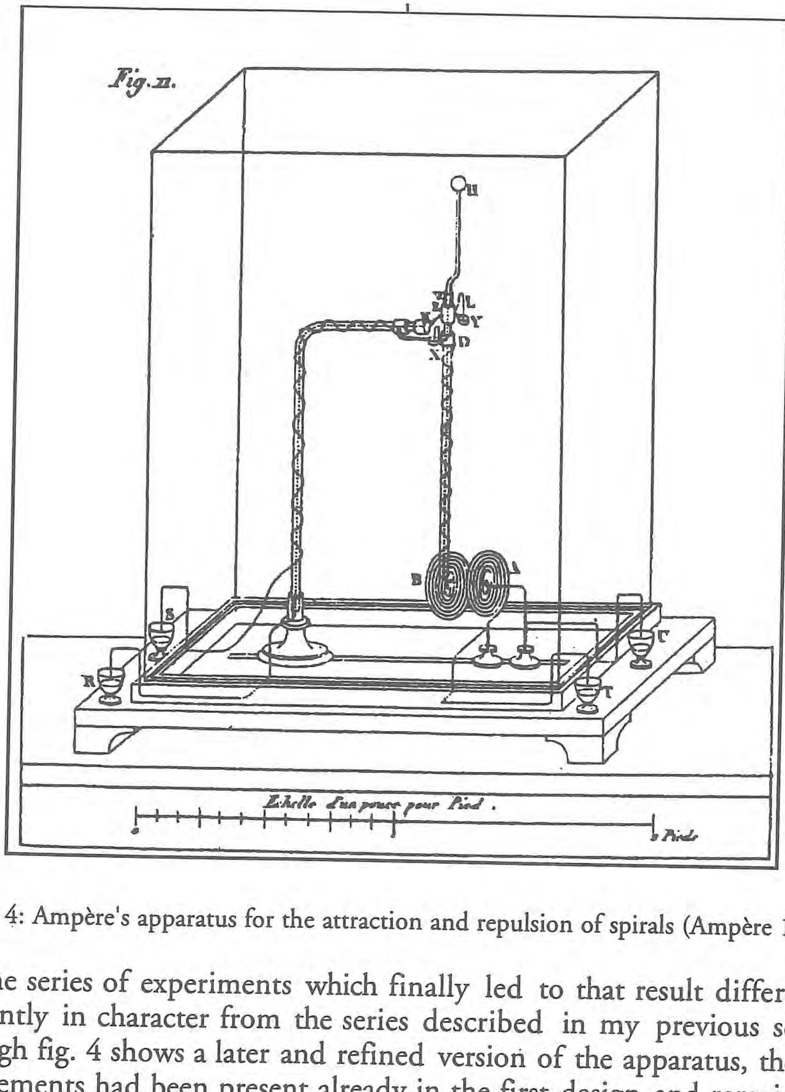

Fig. 4: Ampère's apparatus for the attraction and repulsion of spirals (Ampère 1820b)

The series of experiments which finally led to that result differed significantly in character from the series described in my previous sections. Though fig. 4 shows a later and refined version of the apparatus, the essential elements had been present already in the first design and remained unchanged. The very conception of the experiment had been guided by a clear idea of the expected effect. It gave rise to a definite design which, in its central parts, remained unchanged throughout the whole series. What actually changed was rather those parts which Ampère considered as possibly affecting the effect, such as the suspension of the moveable spiral, the inertia of the swinging arm, the distance of the windings of the spirals, the dimension of the spirals and the power of the battery. Those changes aimed at optimizing, not at exploring. In contrast to the experiments described earlier, the experimental series did not consist in a broad and systematic variation of as many as possible parameters, but in an optimization of one specific arrangement with regard to an expected effect. From the first idea to the final evaluation, the experimental series was defined by the expectation to be tested. Accordingly, the result was not an "if - then" regularity, but considered to be an experimental "proof" of the theory which had given rise to the expectation. The experimental activity leading to such a proof was clearly of a different type than the one described earlier.

5. *Exploratory experimentation*

The type of experimental activity in my second case comes close to what in most of 20th century philosophy of science was the common view of experiment. There was a theory which 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. Otherwise much differing philosophers such as Duhem, Popper, van Fraassen, and many others would agree: yes, this is how experimenting works (Duhem 1906, ch. 10; Popper 1934, § 30; van Fraassen 1981). 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 exactly by focussing on research practice, more becomes visible. There are experiments which cannot be grasped in any reasonable way by such a view. And indeed, the more differentiated views on experiment brought forward in the last two decades are based on studies of research practice. It has repeatedly been pointed out that there is a variety of epistemic goals involved in experimenting: I just mention Hacking's early pointing to a "Baconian" variety of roles of experiment (Hacking 1983, ch. 9). 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 evershifting area right between stability and openness (Hacking 1992, Gooding 1986, Gooding 1990, Rheinberger 1992, Rheinberger 1997).

Not much attention has been paid, however, to how those different epistemic goals may result in different types of experimental work. That such a connexion exists is suggested by the first of the above historical episodes. Ampère's goal in his early research was to find a way to express what he saw as a most puzzling experimental result, and to formulate regularities about the electromagnetic effect. It's worth noting that Ampère was far from singular in that respect. Without knowing from each others' work in the period right after Oersted's announcement, Pictet and de la Rive in Geneva, Oersted in Kopenhagen, Schweigger in Halle, Davy in London, Seebeck in Berlin, and Configliachi in Florence (to name but a few) had a similar goal and pursued a similar type of experimental activity (Pictet 1820, Oersted 1820b, Schweigger 1821, Davy 1821, Seebeck 1821, Configliachi 1821). As to theoretical opinions on the nature of electric currents, magnetism and electromagnetic interaction, there were most profound differences among these researchers. But this was exactly what did *not* matter. All of them faced a peculiar epistemic situation of puzzledness. They realized that their traditional notions, let alone their theories, did not suffice even to express the strange behaviour of the magnetic needle nearby the wire. It was that background which led all of them to conduct quite a similar type of experimental research.

Just to have a shorthand device, I have labelled that type of research exploratory experimentation (Steinle 1994, Steinle 1995, Steinle 1997). Far from being a mindless playing around with apparatus, it 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 do affect the effect in question, and which of them are essentially required. Closely connected, there is the central goal to formulate empirical regularities about those 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, moreover, 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 which 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".[11]

None of those points, of course, is 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, e.g., nobody started with varying the colour of the wires, since it was well known, from numerous experiments in the mid 18th century, that colour 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 of generality, it is mainly a matter of personal ambition, and of the resources and time available, how far the procedure will be driven. Ampère, in the above case, well saw a pathway to extend the generality of his regularities further, but dropped the enterprise in favour of a different one. A year later, Faraday would take up exactly that question at exactly that point, would pursue the variation business much further, and obtain essentially new results (Faraday 1821, cf. Gooding 1985b and Steinle 1995). 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, which affect the choice of certain factors to be varied.

Talking of empirical regularities and contrasting them to theories in a narrow sense which involve reference to "theoretical entities" has its own problems. As common such a distinction was in the 19th century, as much debated it became in the 20th. Though it has become clear that the very categories of empirical, observable and hidden are not entirely sharp (and historically shifting, moreover), it seems likewise obvious that it makes good sense to keep some distinction like that.[12] 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".[13] An aspect which has not found much attention is that stable empirical 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 it is here where my approach goes beyond naive 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 it in terms of search for causal relations (Mill 1843, book III, chs. 7 and 8). An essential shortcoming of Mill's account, however, is that in his view the categorization of the field had to be achieved and fixed in advance by a rather unspecified process, mainly a sort of contemplation. Only within an already given conceptual system could his experimental procedures lead to insights into causality.[14] In research practice, by contrast, it is often the case that the categories which researchers had initially in mind come out to be inappropriate in that they do not allow them to formulate 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, experimenting and forming of concepts are most closely intertwined. Acting and conceptualizing stabilize or destabilize each other on every step.[15]

It is not just a single regularity which comes out as the result of exploratory work, or a disconnnected 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,[16] 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, and thus be explained by, or "reduced" to the general laws. Thus the system of regularities well gains explanatory power.

The contrast of exploratory experimentation to the rather theory-driven type, grasped by 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 must allow a great range of variations, and likewise be open for a large variety of outcomes, even unexpected ones. The restrictions posed by the instrumental arrangement must not be very strict. 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 openess for outcomes which are not in the range of the expectation. Ampère's different experimental arrangements well illustrate the point. Whereas the instrument for the directive action (fig. 2) allowed many variations of position and many different outcomes, the apparatus for the attraction of spirals (fig. 4) 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 high specifity of the apparatus, at a considerable cost of flexibility and openness for unexpected results.

6. Why is exploring significant?

'Well,' a proponent of the standard view might say, 'of course there are more functions of experiment than the testing of expectations and theories. But they are not epistemically relevant: they belong to the context of discovery. And, as we know since Reichenbach, there is neither a need nor an open door to analyze that context philosophically. The epistemically relevant things happen only in the context of (afterwards) justification.' -In what follows, I shall not discuss the merits and failures of the context-distinction (for recent discussions of that distinction, see Nickles 1980, Chmielecka 1982, Hoyningen-Huene 1987). Rather I will try to point out the epistemic relevance of exploratory experimentation and the realm in which it is located. To elaborate what that means for the context distinction would well form the subject of a separate paper.

The central point is that the epistemic goal of exploratory experimentation is located on the level of categories and concepts, as elements of language. Exploratory experimententation typically starts when those categories have been destabilized, i.e. been revealed as being inappropriate to deal with the effects in question. Experimenting goes hand in hand with revising, reforming and re-stabilizing those categories. The main criterion for stabilizing 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 phenomonological level, and thus enabling a stable acting on the field.[17] After such a process has come to a successful end, a new view has been achieved, and the new concepts are quickly included in common talk about the field. They form part of the language guiding even the everyday acting, get out of sight 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 word: a new language, has been formed, it gets difficult to put oneself back in the previous state in which that language did not exist. It is those very points which give the new categorizations and concepts lasting effectivity and influence. Structuring a field on the level of categories and language means to shape all further research most deeply, to give it certain directions and rule out many others in rendering them literally unspeakable and unthinkable. Whereas that immense epistemic significance of categorizations and language has often been realized in the life sciences -one may just remember 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. This holds even more for the type of experimental activity in which the revising and forming of those concepts forms the very core: exploratory experimentation.

In order to stuff out those abstract considerations with some historical flesh, I first take the above case. Ampère's concept of a current circuit became most rapidly an unproblematic part of the common talk on galvanism. The older view according to which current was defined with reference to the poles quickly disappeared, and Ampère himself used, in all his later reasoning on microscopic circular currents, the concept of a current circuit as an unproblematic foundation. It appeared as most natural, and the very fact that it had been created in hard labour went out of view. So did the specific context within which that labour had been done. My general point is even better illustrated by taking a broader look. Exploratory experimentation is far more common in the development of the sciences than hitherto realized. Many basic notions of scientific language have been formed in the context of such activity. Here are some examples from the field of electricity.

The first clear and systematic division between electric and magnetic effects by William Gilbert in 1600 was based on broad exploratory experimentation (Gilbert 1600).

The distinction between two types of electricity which attract each other but repel within themselves, was first introduced by Charles Dufay in the 1730s in the context of broad experimentation. It provided a most effective means by which Dufay could formulate stable and general regularities in a formerly vastly complex field of electric effects (Dufay 1733; Heilbron 1979, ch. 9).

After Galvanis's spectacular announcement, in the 1790s, of the effects which quickly were named after him, most broad exploratory research started in many parts of Europe. Within a short period, new and abstract means of representation of galvanic arrrangements became developed by Humboldt and Ritter (Trumpler 1997, Trumpler 1999).

In the field of electromagnetism in the 1820s and 1830s, most important research was conducted by Michael Faraday in London. Long and essential periods of his experimental research were exploratory, and it was exactly in the course of those periods that he proposed, developed and stabilized the concept of lines of force as an appropriate means for formulating stable and comprehensive regularities (Gooding 1985b, Gooding 1985a, Steinle 1994, Steinle 1995, Steinle 1996).

.Examples can also be found in other fields:

The concept of chemical reaction was formed and developed in the 17th century on the background of broad experimentation, not by single prominent individual researchers, but rather within a community with a close communication structure (Klein 1994, Klein 1996).

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 (Klein 1998).

Biochemical research was revolutionized in the 1930s by the introduction of the idea of circular reaction patterns by Hans Krebs. The experimental research which led Krebs, in studying urea biosynthesis, to develop that idea for the first time, had long and essential exploratory phases (Holmes 1993; Graßhoff, Casties et al. 2000).

In again another field, finally, Jean Brachet's research on protein biosynthesis in the 1930s and 1940s led to introducing new central concepts. His experimental work was mostly of exploratory type. The fact that the philosopher of science Richard Burian, in analyzing Brachet's work, introduced himself the category of "exploratory" experimentation, strongly supported my independent proposal. Burian's emphasis that "the style of exploratory experimentation (...) *should* be of great historical and philosophical interest" significantly underlines my claim of the high epistemic importance of that activity (Burian 1997, p. 27, original emphasis).

In all those cases, selected rather randomly, there was intense exploratory work conducted, categories revised and new concepts developed. Though 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 those developments is well visible, and thus their epistemic significance. Conceiving electromagnetic effects in terms of lines of force, for example, has become basic likewise to school teaching, technical design, and research, while it had been totally inconceivable for most researchers even in the 1840s. As the case of the concept of chemical reaction illustrates even more strikingly, many of the categories and notions thus developed have 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 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-elaborate 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.

7. Epilogue

As I indicated above, I am not the first to emphasize the variety of epistemic functions of experiment. Not only philosophers, but also reflective practicioners have given more or less explicit hints, from Claude Bernard and Michael Polany to François Jacob (Bernard 1865, Polanyi 1958 (1994), Jacob 1998). One of those who made the point quite explicitly was the experimental physiologist Ludwik Fleck. In the passage quoted at the outset of this paper, he pointed to a whole spectrum of different types of experiments which vary along a scale of greater or less definiteness. In fields which have "been sufficiently worked over," experiments are sharp, well defined, and their outcomes "limited to existence or nonexistence, and perhaps to quantitative determination".[18] My above case of Ampère's attraction and repulsion of spirals nicely illustrates that point, and also Fleck's further observation that those experiments are much dependent on foregoing "experiments and decisions," even on a whole system of those. With his most significant remark that such a system quickly becomes part of self-evident know-how which guides research in an unreflected way, Fleck clearly pointed to the level of concepts, categories and language.

About the other end of the spectrum, however, where experiments are less well defined, Fleck was less explicit. Inferring from the contrast, one may surmise that he regarded the system of previous experiments and decisions as being not as immense as in the first case. Though Fleck left much room here for a variety of types of experimental research, exploratory experimentation as described above fits perfectly into his characterization. Even the very point that it is concept formation which is at stake here was pointed out by Fleck when he emphasized that

> 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 (Fleck 1935 (1980), p. 114, English translation from Fleck 1979, p. 86).

Fleck definitely hit an essential point with his characterization of experiments, a point, however, for which it took a long time to be taken up by philosophers of science. Fleck found those different types of experimentation typically represented in different disciplines. In physics and chemistry, as he saw it, most experiments were of a well defined type, whereas the rather undefined, open experiments, belonged to the field of the life sciences, among others. But the situation is more complicated than suggested by such a common cliché. The above cases well illustrate that exploratory experimentation plays a highly important role even in physics, past and present. Revising basic categories and concepts within experimental research, and creating new ones, is not less important in physics than in other fields. Situations which make those processes necessary constantly come up anew. A main reason for playing down or even ignoring that fact in accounts of physical experiments is that, in the self-representation of physical research, those episodes traditionally get even less space than in the life sciences. Again Ampère provides a significant example: in his publications nearly nothing of his exploratory phase ever showed up though it had formed the largest part of his early research. The case just illustrates a much more common tendency. Traditional philosophy of science, both shaped by and itself shaping standard accounts of physical research, has not paid much attention to that point. No surprise that attempts to develop a more differentiated understanding of experiment are often based on studies of the life sciences. It is only by deliberately focussing on research practice (as contrasted to self-representation) that the variety of experiments comes to the fore even in the physical sciences.

Notes

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1 A brief overview of the reaction is given by Meyer (1920, I, pp. ci-cxii).

2 Snelders (1990) discusses some details. Oersted used, as was common in the period, a trough apparatus. There was no common terminology for those devices. Oersted spoke of "galvanic apparatus", the French researchers of "pile", and many others of "battery." My uniform, but sometimes anachronistic talk of "battery" is to avoid the nowadays common connotation of "pile" to Volta's upright arrangement.

3 For a discussion of the background of his interest, see Caneva (1980) and Blondel (1982).

4 Due to an extremely difficult state of the sources, most of the previous accounts of Ampère's work start with his academy lectures after the second and third weeks; cf. Blondel (1982) or Hofmann (1995). Only Williams (1983) tried to focus on the early period. Since he concentrated on Ampère's publications he was, by Ampère's somewhat chaotic publication strategy, led to misguided conclusions. Based on a thorough and detective-like analysis and reconstitution of archival material, I presented in my (Steinle 2000) for the first time a detailed account of that period. The following sketch summarizes some aspects of that research.

5 The Dossier Ampère is at the Archive of the Paris Academy of Science. All translations are mine.

6 That point has largely escaped historical attention, even when focussed on the concept of current, cf. Brown (1969).

7 There are indications, but no clear evidence that Ampère actually conducted experiments on that question: Dossier Ampère, folder 206bis, and some hints in his first paper (Ampère 1820a, p. 65).

8 "reduire à deux faits généraux:" (Ampère 1820b, p. 210), for example. Ampère also used the term "ramener;" (*ibid.*, p. 197).

9 In his early research, Ampère pursued some other topics in a similar type of research. One of them was the great many experiments he undertook to demonstrate the reciprocity of the electromagnetic effects, a topic which he nearly completely supressed in his publications and which I shall not treat in detail here. Again, those researches have been largely ignored in the historical literature. For a detailed discussion, see my (Steinle 2000).

10 It may only be noted that the concept of a current circuit which he had developed before played a crucial role in his reasoning: the idea of a circular current developed out of the beforehand concept of a current circuit. For details, see again my (Steinle 2000).

11 It is this systematic character of exploratory experimentation which has largely been overlooked, even by those who otherwise emphasize the need for differentiating experimental activity. It is significant, for example, that David Gooding who based his very stimulating account on studies of Faraday's research practice, nearly totally overlooked the importance and meaning of Faraday's enterprise of "reducing" certain effects to a "simple case;" cf. his summarizing book (Gooding 1990).

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

13 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).

14 A related shortcoming of Mill's account is the missing insight that the scope of experimental factors to be varied is unlimited in principle.

15 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 et al. (2000).

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

17 Such stabilization is often interpreted as a sign of approaching the "real" state of affairs. Thus the analysis of exploratory experimentation may open a fresh approach even to questions of what "reality" can possibly mean in such a context, and by what means researchers try to get hold on it. Issues of realism, however, are not my topic here.

18 Fleck goes even so far as to say that an experiment, if totally well defined, would not need to be carried out at all, since also the outcome would be known in advance. I find here a promising new perspective on thought experiments, a perspective which has still to be elaborated.

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