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Chance in Science: The Discovery of Electromagnetism by H.C. Oersted

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Abstract. Ignoring the role of chance in science distorts the nature of the scientific process. Teachers can address this issue by means of several in-depth historical case studies, such as the discovery of electromagnetism by Oersted. Oersted was led to his lecture experiment by logic (two new hypotheses), but its success from the first trial was largely due to chance. Reproducing Oersted's experiment in the classroom complements the story by allowing students to see for themselves the role of some accidental factors, such as the choice of materials and instruments. The message to students is that chance and logic go together in science.

Key words: history of science, discoveries in science, epistemology, serendipity in science, science teaching, electromagnetism, H.C. Oersted

1. Introduction

'Chance' seldom enters science textbooks, which is not surprising given that most of them are merely compendiums of laws and equations, the origin of which remains unknown. However, when a teacher is willing to address not only the results of discoveries but also the process of discovering, ignoring chance is inadvisable. Indeed, if a discovery is presented as merely a logical process, a curious student may ask of why the discoveries do not follow one another in an orderly way without incomprehensible gaps between them. Why, for instance, was electromagnetism discovered only in 1820, although all technical means and theories necessary for it were available as early as 1801?

Until recently, the history of science had not been of much help in this respect. A curious teacher could have found that chance played a role only in a few discoveries, named 'serendipitous' (Kohn 1989; Roberts 1989)). As a result, when presenting historical materials teachers preferred to use their limited classroom time to study more 'regular' features of discoveries than chance.

A division of discoveries into 'accidental' and 'regular (or theoretical)' implies that if chance is involved, there is no theory behind a discovery; or, conversely, if a scientist follows a certain idea, this precludes any role to chance. In fact, even a superficial examination reveals that real science does not operate according to this scheme. Stories are known of famous scientists discussing accidental moments in their discoveries, which have never been labeled 'serendipitous'. On the other hand, it is difficult to recall a discovery, in which a theory played no role at all.

This implies that when labeling discoveries 'serendipitous' or 'theoretical' people actually mean 'mostly serendipitous' or 'primarily theoretical'. However, such a re-labeling would bring out in the open the 'other' factor, which had previously been silenced or ignored. This would leave a scholar with no choice but to study both factors and *prove* that one played a more important role than the other.

In fact, such a study is feasible if a proper methodology to examine the role of chance in science is available. One such methodology, developed by the author, is applied in this paper. It consists of the following steps:

1. Defining 'accidental' and 'theoretical'.
2. Determining the meaning of the discovery as perceived by the discoverer.
3. Finding various factors, experimental and theoretical, that could have introduced elements of chance.
4. Evaluating, which role – positive or negative – each factor could have played in the process of discovery.

A prior application of this methodology to the discovery of radioactivity by Henri Becquerel proved to be fruitful, because it revealed more details of the involvement of both chance and logic than had been thought before (Kipnis 2000). Here, I intend to use this method with the discovery of electromagnetism by Hans Christian Oersted (1777–1851).

I have chosen this particular discovery for several historical and didactic reasons. First, a number of scholars have claimed that Oersted owed it to certain ideas. Second, it can be easily incorporated into a high-school physics curriculum. Finally, Oersted's experiments are among those that can be repeated by students, which gives them an opportunity to understand the role of chance in a discovery through not only reading and discussion but also through experimentation.

One should bear in mind that this is not a comprehensive account of Oersted's discovery. This paper is concerned only with issues relevant to the role of accident in it, primarily in his lecture experiment. It is necessary to remember that any historical analysis, however abundant the evidence, may lead only to tentative conclusions. This is even more true so in studying the role of chance, where evidence is indirect and the role of reconstructions is greater than usual. In such a situation, a plausibility of an analysis is judged by its results: if they offer new insights and can be applied to other cases, we deem the analysis successful.

2. Background

On July 22, 1820, Oersted mailed a four-page pamphlet in Latin 'Experimenta circa effectum coflictus electrici in acum magneticam' to a number of renowned scientists and institutions (Oersted 1820a, p. 214). He described there a new phenomenon: a magnetic compass placed below a wire deviated from its normal position every time when the wire connected both poles of a voltaic pile. Since

Oersted did not say a word about how he had arrived at this discovery, he was charged with making it by chance.

For instance, Ludwig Wilhelm Gilbert (1769–1824), the editor of the *Annalen der Physik*, remarked that, 'what all inquiries and efforts have not wanted to yield, a *chance* brought to Professor Oersted during his lectures on electricity and magnetism' (Gilbert 1820, p. 292). Oersted rushed to defend himself by asserting that he had been searching for a connection between electricity and magnetism for many years:

> Having for a long time considered the powers which are developed by electricity as the general powers of nature, it necessarily followed that I should derive magnetic effects from them. In order to prove that I admitted this consequence to the utmost extent, I cite the following passage from my *Researches into the Identity of Electrical and Chemical Powers* printed at Paris in 1813: 'It must be determined whether electricity in its most latent state has any action on a magnet as such'. (Oersted 1821a, p. 322)

Oersted also pointed out that he predicted the result of the experiment he carried out in front of his students:

> All my auditors are witnesses that I have *indicated beforehand* the result of the experience. The discovery has not been made *therefore* by accident, as Professor Gilbert has wanted to conclude from the expressions which I have used in my first announcement. (Oersted 1821a, p. 322, italics added)

Later, he emphasized the role of philosophy in the discovery:

> Throughout his literary career, he [Oersted] adhered to the opinion, that the magnetical effects are produced by the same powers as the electrical. *He was not so much led to this, by the reasons commonly alleged for this opinion, as by the philosophical principle, that all phenomena are produced by the same original power*. (Oersted 1827, p. 356, italics added).[1]

Oersted's explanation had satisfied physicists, and the matter appeared to be settled. Then the subject of chance resurfaced in 1870 after a publication of a letter from Christopher Hansteen (1784–1873) to Michael Faraday. Hansteen, a Norwegian astronomer and mathematician and at one time Oersted's pupil and assistant, described Oersted's discovery as follows:

> Oersted tried to place the wire of his galvanic battery perpendicular (at right angles) over the magnetic needle, but remarked no sensible motion. Once, after the end of his lecture, as he had used a strong galvanic battery to other experiments, he said, 'Let us now once, as the battery is in activity, try to place the wire parallel with the needle', as this was made, he was quite struck with perplexity by seeing the needle making a great oscillation (almost at right angles with the magnetic meridian). Then he said, 'let us now invert the direction of the current', and the needle deviated in the contrary direction. Thus the great detection was made; and it has been said, not without reason, that 'he tumbled over it by accident'. *He had not before any more idea that any other person that the force should be transversal*. But as Lagrange has said of Newton in a similar occasion, '*such accidents only meet persons who deserve them*'. (Hansteen 1857, pp. 395–396, italics added)

Although this letter became widely known, it did not reopen the controversy, perhaps because it was obvious that Hansteen did not treat an involvement of chance as diminishing Oersted's merit. And in the next 50 years, historians used

Hansteen's account likewise: if they mentioned the accident factor at all, they treated it the 'Lagrangian way'. (Potamyan 1909, pp. 210–211; Whittaker 1910, pp. 84–85)[2]

The situation changed in 1920 with a publication of Oersted's *Collected Works*, Kirstin Meyer, the editor, found this occasion proper to correct what she perceived as injustice:

> Oersted's merits in the matter and the value of his work gradually became obscured. The point of view which was little by little generally adopted was this: Oersted had by chance discovered the fact that an electric current may deflect a magnetic needle, but all the closer investigation of the matter has been made by others. (Meyer 1920, p. CIII)

Thus, according to Meyer: (1) there had been lack of attention to Oersted (compared to Ampère and Faraday), and (2) the reason for this was a perception of Oersted's discovery as accidental. Actually, while the first statement was true, the second was not. Meyer undertook several steps to refute the charge of accidental discovery. In particular, she argued that Hansteen could not have been an eyewitness to the discovery, because he was not in town at the time (Meyer 1920, p. LXXI). She also pointed out Kant's influence on Oersted and emphasized that the idea of unity in nature had guided Oersted for many years in a search for connections between electricity, chemistry, heat, light, and magnetism.[3]

Until the 1950s, Meyer's work had made no effect whatsoever. Then some historians and philosophers of science took note of it and began to emphasize the crucial role of Oersted's philosophy of science. While they differed on which philosophical school influenced Oersted the most – German 'Romantics', *Naturphilosophie*, or Kant – their general idea was that metaphysical views excluded chance.[4]

For instance, R. Stauffer stated that 'it was *Naturphilosophie*, not chance, that led to the discovery of electromagnetism' (Stauffer 1953, p. 310). At the same time, physicists revived the notion of an accidental discovery. Many physics textbooks reproduced Hansteen's account of the discovery as that of an eyewitness (White 1956, p. 327; Taylor 1959, pp. 631–632; Tricker 1965, p. 22; Dunsheath 1967, p. 81). Others attributed the discovery to chance without offering any evidence (Chalmers 1949, p. 44; Coulson 1950, p. 35; Priestley 1958, p. 234; Taton 1965, p. 285; Riban 1982, p. 282).

It appears that neither the 'accidentalists' nor the 'theorists' have achieved much insight into Oersted's discovery. The 'accidentalists' did not explain how and where chance entered the discovery. The 'theorists' ignored the fact that following a theory does not logically preclude an involvement of accidental factors. Moreover, even if one is willing to accept the belief in unity of nature as a sole cause of Oersted's success, one still needs to understand:

(1) how Oersted's metaphysics led him to the discovery,
(2) why other scientists who had believed in the unity in nature failed to anticipate him, and
(3) why Oersted himself did not make the discovery earlier.

A closer look at the passages cited above suggests two reasons for the differences on the character of Oersted's discovery. First, Oersted and Hansteen differed on the meaning of 'accidental'. Second, they disagreed about the content of the discovery. This implies a necessity in defining the basic terms to be employed in this paper.[5]

3. Definitions

When speaking of 'theoretical', one has to draw a line between Oersted's metaphysical ideas and his physical theories. Although Oersted stated that he owed his discovery to a 'philosophical principle', metaphysics by itself cannot directly lead to any experiment: there must be an intermediary physical theory bridging the two.[6] A physical theory will mean here a set of hypotheses testable by experiment. Unless specified otherwise, 'theory' will be used henceforth in the sense of 'physical theory'.

In this paper, *accidental*, *serendipitous*, and *chance* will be used as synonymous and opposite to *theoretical* or *logical*. I will employ such meanings of 'accidental' as *unexpected*, *irregular*, *unplanned*, and *unpredicted*. They had been used as criteria of a serendipitous discovery. Since it appears that an overall characterization of a discovery is of little practical value, I will apply these criteria to its separable components.

Unexpected discovery will refer only to some of its components. Any discovery must contain new or 'unexpected' results, otherwise, it would not be called a 'discovery'. Other results, however, may be anticipated. For instance, creating a magnetic force by means of electricity was expected by many, but Oersted's method to achieve it was not. Nor was it expected that the magnetic force would be perpendicular to the wire.

Irregular discovery refers to a way a research is prepared and carried out. It means something that scientists normally do not do or do not expect, such as making conclusions on the basis of a single trial, especially in conjunction with untested hypotheses and apparatus. The results obtained in such a way are considered dubious because they involve too much chance. For instance, a negative outcome of a single experiment may mean either absence of an effect or an unsuitable apparatus or procedure.

Unplanned (unintentional) discovery is an unexpected funding that comes in two different cases. In one, while beginning a research, an experimenter plans the first experiment. If its result is unexpected, it gives rise to new experiments that could not have been pre-planned in the beginning. Thus, the term 'planning' should be applied to particular components of a discovery rather than to the discovery as a whole. For instance, what Oersted planned for the lecture was to obtain *a* movement of a magnetic needle by means of a current. According to Hansteen, Oersted did not know in advance that setting the wire parallel to the magnetic needle and in the same vertical plane would produce an almost 90° declination.

Thus, the discovery of transversality of magnetic force was not planned for the lecture. Since the results of the lecture experiment did not suggest anything resembling transversality, neither did Oersted plan to discover it when he resumed experiments three months later.

In the other case, the first step in a discovery cannot be pre-planned, because it comes from researching another topic, perhaps only slightly related to the discovery in question. For instance, some writers believe that Oersted studied a heating effect of electric current when he noticed a declination of a compass left on the table from a different experiment. Important discoveries of a side-effect type are rare (Leyden jar, X-rays), and are usually attributed to chance. Discoverers have a right to resent such a characterization, because the steps following the accidental one are usually logical. Apparently, Oersted understood Gilbert as classifying his discovery as a side-effect type, which did not please him at all.

Unpredicted discovery means an expected results that had not been announced (or recorded) before the experiment. In other words, prediction is an expectation made public. A prediction is important only if the discovery is contested, for instance, about its priority or serendipity. It is an expectation on the part of a discoverer that reduces the degree of serendipity. If an expectation can be uncovered, an absence of prediction does not increase the role of chance. For instance, while Oersted probably predicted only that the needle will move, without specifying the direction of this movement, there are reasons to believe that he had a certain rule in his mind. This means that in determining the role of chance prediction is subordinated to expectation.

To decide whether a discovery was planned or expected, we need to know *what* was planned, etc. In short, we need to know the content of the discovery. In Oersted's case, it was, according to Hansteen, transversality of electromagnetic force. Most authors believe, however, that the discovery consisted of the first 'definite (or 'direct') proof' of a 'connection (or 'relation')' between electricity and magnetism (Thompson 1893; Dunsheath 1967; White 1969). While the former opinion focuses on an experimental result, the latter one is an interpretation of its theoretical role. This study is focused solely on experiment, more exactly on how Oersted's experimental results actually evolved. For this reason, we need to know what these results meant to the author at the moment of discovery rather than to subsequent interpreters.[7]

The next step is to decide on whether the discovery consisted of several separable components. If yes, one should determine the involvement of chance into each of them and then to think whether it is advisable to characterize the discovery as a whole. In this paper, I will distinguish between two parts of Oersted's discovery: the lecture experiment and an investigation he carried out three months later. The main focus will be on the former.

4. Oersted on a Connection between Electricity and Magnetism

As Oersted recalled later, he stated in his book *Ansicht* [1812] that,

> He [Oersted] proved that not only chemical affinities, but also heat and light are produced by the same two powers, which probably might be only two different forms of one primordial power. He stated also, that the magnetical effects were produced by the same powers; but he was well aware, that nothing in the whole work was less satisfactory, than the reasons he alleged for this. (Oersted, 1827, p. 356)

The focus of this treatise was on chemistry; the chapter on magnetism was very small and contained nothing new about possible connections between magnetism and electricity. It ended with a suggestion to investigate 'whether electricity in its very latent state does not have any action on a magnet as a magnet' (Oersted 1812, p. 148). As shown above, in 1821, Oersted referred to this passage as predicting his discovery. Historians have always presumed that Oersted is speaking here of an action of voltaic current on a magnet (Meyer, LI; Stauffer 1953, p. 308). However, the very next sentence casts a doubt on this claim:

> The thing would be not without difficulty, because electricity would act on magnetic bodies the same way as on non-magnetic ones. Still, perhaps it is possible to obtain some information by comparing magnetic and non-magnetic needles (Oersted, 1812, pp. 148–149).

'Comparing magnetic and non-magnetic needles' may mean a voltaic discharge through, for instance, steel and copper needles: the electric effect would be the same but the magnetic one would be different. However, since Oersted is speaking of the electrical action as being identical to all bodies, it must be electrostatic. It was known at the time that even a large voltaic pile does not produce any electrostatic attraction when its circuit is closed. This implies that Oersted intended to compare an attraction of steel and copper needles using an *open* voltaic circuit.

The *Recherches* [1813], the French version of the book, also supports this interpretation:

> This experiment will be not without difficulty, for the electrical action must *always* interfere in it and make the observations very complicated. Perhaps one would obtain some results by a subsequent comparison of the *attractions* of magnetic and non-magnetic objects. (Oersted 1813, 238, italics added)

Indeed, only electrostatic action is the one that cannot be avoided, and the word 'attraction' fully conforms to this idea.

In fact, whatever was Oersted's original interpretation of his 1812 prediction, it did not matter, because no one, including Oersted himself, had followed it up before 1820. This is how Oersted explained his attitude towards the subject at that time:

> I wrote this during a journey, so that I could not easily perform the experiments, besides which, the manner of making them was not at the time at all clear to me, all my attention being directed to the development of a system of chemistry. I still remember that I expected, though somewhat vaguely, the effect in question, and particularly by the discharge of a strong *electrical* battery, and also that I did not hope to obtain more than a weak magnetic effect. (Oersted 1821, p. 322, italics added)

Thus, between 1812 and 1820, Oersted was preoccupied with chemistry, and the only relevant experiment he had been thinking of had nothing to do with the one he did in 1820. Indeed, the term 'electrical battery' meant then a battery of Leyden jars, 'galvanic battery' being reserved for a battery of voltaic cells.[8] Apparently, Oersted was thinking of magnetizing a steel needle by discharging a battery of Leyden jars through it, an experiment well known in the eighteenth century.

In short, we do not see any continuous research on a connection between electricity and magnetism, which, according to some authors, led Oersted to his discovery (Stauffer 1957, p. 48; Bauer 1964, p. 208; Lee 1970, p. 20; Altmann 1992, p. 13).[9] Had he done any experiments prior to 1820, why did he not mention them after July 1820? This would have been the best way to rebuff the charge of an accidental discovery.

5. What Did Oersted Actually Discover and When?

As Oersted himself stated, his interest in this topic rekindled in connection with a course he had to teach at the University of Copenhagen. During the academic year 1819–1820, he lectured once a month to an advanced group of students on new discoveries in physics and chemistry. One of these lectures, possibly scheduled for early April 1820, was on 'Electricity, Galvanism, and Magnetism'.[10] Oersted planned to discuss various connections between electricity, galvanism (which included chemistry), and magnetism, illustrating them with experiments. The subject was not new, for attempts to find connections between static electricity and magnetism, magnetism and chemistry, and other connections between natural phenomena were quite popular since the middle of the 18th century. After the invention of the voltaic pile in 1799, 'galvanism' – the agent produced in the pile – joined the group of 'natural powers' to be studied for their connections with one another. Some scientists firmly believed that certain connections, in particular, between electricity and magnetism, had already been established, while others challenged this claim.

The arguments and experiments Oersted initially planned to use in the lecture were very traditional. While preparing his lecture, however, he came up with two new hypotheses: (1) when electricity produces a great amount of heat and light, it can also create a magnetic action (the 'heat hypothesis'); and (2) this magnetic action streams away from a hot current-carrying wire in all directions, similarly to heat and light (the 'radial emission hypothesis').

These two hypotheses provided a theoretical basis for an experimental arrangement that became known as the 'Oersted experiment'. Two of these arrangements – closing the circuit and using a compass as a magnetic detector – became crucial to its success. Neither of the two was obvious. In the 1800s, several scientists tried to obtain a magnetic effect from a pile using an open circuit but failed. But it was not this failure that made Oersted to close the circuit but a need to heat the wire by electricity. As to detecting a magnetic effect of electricity, the eighteenth-century experiments employed a discharge of a Leyden jar through a steel needle: the effect was present if the needle acquired poles. A compass came into use in this sort of experiments in the early 1800s (see Section 7). Since little was known of the latter experiments, Oersted could not have a prior idea, which of the two devices were more suitable for his purpose.

During the lecture, Oersted conjectured in front of his audience that ‘an electrical discharge might produce some effect upon the magnetic needle placed out of the galvanic circuit’ (Oersted 1821, p. 321). And this was what he observed, as described in the ‘Experiments [1820]’:

> It seemed demonstrated by these experiments [in April] that the magnetic needle was moved from its position by the galvanic apparatus, but that the galvanic circle must be complete, and not open, which last method was tried in vain some years ago by very celebrated philosophers (Oersted 1820b, p. 273).

It follows from these two quotes that what Oersted predicted before the experiment was a movement of the magnetic needle, nothing more. Had he also discovered then, as Hansteen claimed, that the force observed was perpendicular to current? Hansteen based his conclusion on the fact – for which he provides no evidence – that Oersted observed during the lecture an almost 90° magnetic declination.[11] However, Oersted insisted that the effect was ‘*very feeble*’, which probably meant a deviation of merely a few degrees (Oersted 1827, p. 357, italics added). Moreover, the largest angle, mentioned in his published paper ‘Experiments [1820]’, is merely 45°. Now, Hansteen also stated Oersted employed for his magnetic experiment the galvanic battery he ‘had used to other experiments’. However, the only other experiment Oersted carried out during the lecture did not require a battery. This means that Hansteen did not see any experiments, and he described the discovery as he imagined it to happen, that is, resembling a contemporary classroom demonstration (see Section 10).

Thus, we may safely ignore Hansteen’s ‘evidence’ and try instead to recover Oersted’s expectations, which he probably did not reveal to the audience, from the following passages:

> I immediately resolved to make the experiment. Although the effect was *unquestionable*, it appeared to me nevertheless so *confused* that I deferred a minute examination of it to a period at which I hoped for more leisure. (Oersted 1821, p. 321, italics added)
>
> As the effect was *very feeble*, and must, before its law was discovered, seem very *irregular*, the experiment made no strong impression on the audience. (Oersted 1827, p. 357, italics added)
>
> The result *corresponded to expectations*, but only a very weak effect was obtained, and *no particular law* could immediately be observed from it. (Oersted 1828, p. 50, italics added)

Probably the ‘expectations’ referred to a movement of the needle, while the ‘law’ dealt with the direction of the force. An ‘unquestionable’ effect means that Oersted was certain that the needle moved, while ‘irregular’ and ‘confusing’ may mean that the movement did not follow any law. Since it is improbable that Oersted tried to uncover an empirical rule in a few minutes during the lecture, the ‘law’ probably refers to one he had conceived beforehand.

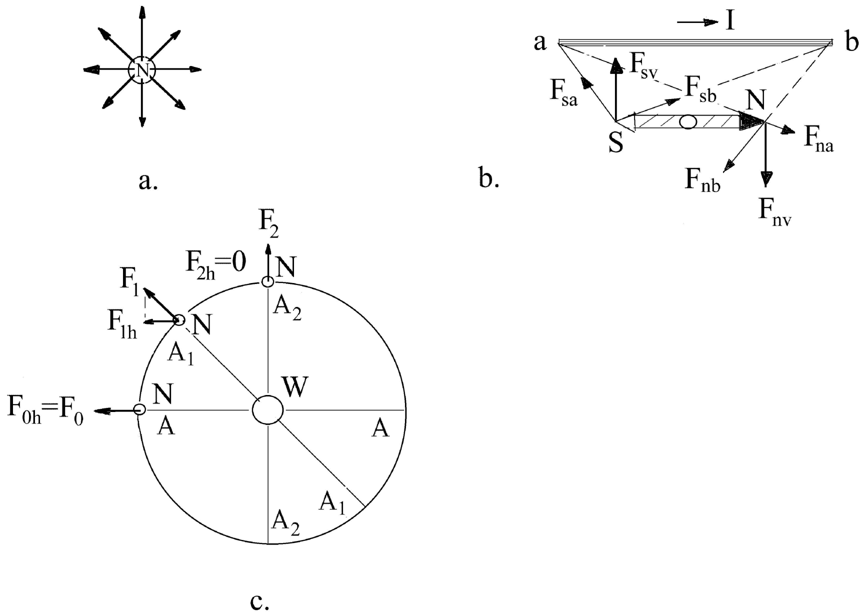

Figure 1. Electromagnetic forces according to Oersted's radial-emission hypothesis (a reconstruction). (a) Forces originating from each point of a current-carrying wire. (b) Forces produced by a horizontal wire *ab* on a magnetic needle NS that is parallel to the wire. (c) Force acting on the north pole of a magnetic needle NS, which is parallel to the wire. The plane made by the needle and the wire is shown at three positions AA, A_1A_1, and A_2A_2 respective the horizontal plane ('h' means a horizontal projection of a force).

We may try to reconstruct this law by supposing that, according to the radial emission hypothesis, magnetism spreads from every point of a hot wire in all directions as in Figure 1a (assuming that each point of the wire is a north-seeking pole N). In this case, if the needle is parallel to the wire and forms a horizontal plane with it (Figure 1b), the south pole should be attracted to the wire and the north pole repelled from it, so that the needle would turn up to 90° (depending on its position). If we turn the plane AA containing the needle and the wire around the latter as an axis (so that the needle and the wire remain at their places), the horizontal component of the force, which is responsible for rotation of the needle, decreases, so that F_{1h} is less than F_{0h} (Figure 1c). This force reaches zero when the needle is exactly above or below the wire ($F_{2h} = 0$), in which case the needle does not turn at all; instead, its poles slightly shift upward and downward.

An experiment shows, however, an opposite pattern: the deviation is maximal when the needle is exactly above or below the wire W (in the plane A_2A_2), and it equals zero when the needle forms a horizontal plane AA with the wire (Figure 2). Moreover, when the needle crosses this horizontal plane, it changes the direction of rotation.[12] Such a difference between theoretical and experimental results would have fully justified labeling them 'confusing'. Naturally, Oersted did not have time

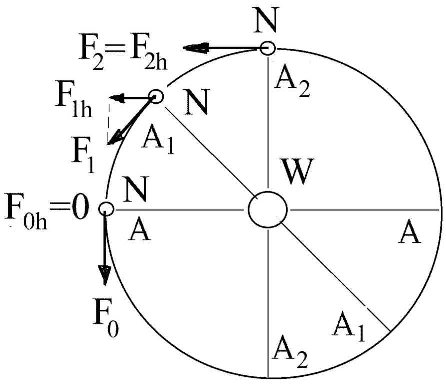

Figure 2. The true magnitude of electromagnetic force in the same positions of the wire and the needle as in Figure 1c.

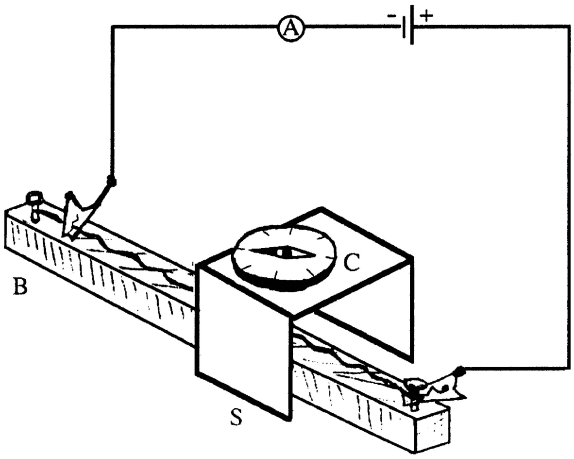

Figure 3. Oersted's lecture experiment as reproduced in the classroom.

during the lecture to correct his hypothesis for the direction of the magnetic force and verify it. He discovered the true law in July and described it as follows:

The electric conflict acts only on the magnetic particles of matter . . . .
The electric conflict is not confined to the conductor, but dispersed pretty widely in the circumjacent space . . . .
This conflict performs circles (Oersted 1820b, p. 276, italics added).

One can distill from this passage the following rule that Oersted called a 'law': 'A wire connected to both poles of a voltaic battery produces around it a magnetic action, which is directed along a circle surrounding the wire'. This is close to what he subsequently formulated as 'the fundamental *law* of electromagnetism, viz. *that the magnetical effect of the electrical current has a circular motion round it*' (Oersted 1827, p. 358, italics added).

The multiple references to the ‘law’ in the ‘Experiments [1820]’ imply that Oersted viewed it at the time of writing (around July 21, 1820) as the core of his discovery. Such a view conforms to the circumstances of its publication, namely, ‘as soon as he discovered it [the law], he *rushed* to publish his work’ (Oersted 1828, p. 50, italics added). ‘Rushed’ is a proper word, for instead of sending his article to a journal, Oersted printed it out and mailed to a number of people and institutions on the same day, July 21.

He even revealed a reason for such haste:

> *Apprehending that others might lay claim to this discovery*, he sent a short Latin description of his experiments to the most distinguished philosophers and learned bodies. (Oersted 1827, p. 358, italics added)

Thus, a few days before 21 July Oersted realized that he discovered something extraordinary – a force different from any known force of attraction – and he did not want to lose priority. But if he had so much to lose, why had he waited for three months with his follow-up experiments?

6. Oersted’s Procrastination

Oersted himself explained his inactivity as follows:

> It may appear strange, that the discoverer made no further experiments upon this subject during three months; he himself finds it difficult enough to conceive it; but the extreme *feebleness* and seeming *confusion* of the phenomena in the first experiment, the remembrance of the numerous *errors* committed upon this subject by earlier philosophers, and particularly by his friend Ritter, the claim such a matter has to be treated with earnest attention, may have determined him to delay his researches to a more convenient time. (Oersted 1827, p. 357, italics added)
>
> So long as the experiments were not more conclusive he feared that he, like Franklin, Wilcke, Ritter, and others, would be deceived by a mere coincidence. (Oersted 1828, p. 50)

What did he mean under ‘numerous errors’, and in particular, those committed by Johann Wilhelm Ritter (1776–1810)? Ritter, a friend of Oersted, claimed to have discovered several new connections between electricity, galvanism, and magnetism, in particular that a zinc-silver needle had magnetic poles. These claims were based on single experiments and probably resulted from spurious phenomena. Apparently, Oersted was not certain that his lecture result was real rather than a spurious one. Indeed, a small movement of a magnetic needle might have resulted, for instance, from an air draft or an attraction to a human body charged with static electricity rather than from a magnetic force.

Oersted noted that ‘the magnetical needle, *though included in a box*, was disturbed’ (Oersted 1827, p. 357, italics added). The implication was that a fully enclosed needle (the box had a glass cover) was protected against an air movement. Did he think of static electricity? There is a sentence – ‘the electric current, like other *magnetic* effects, penetrated glass’ (Oersted 1828, p. 50, italics added) – that may be interpreted so that since magnetic force penetrates glass, the new force might have been magnetic. On the other hand, this does not fully exclude an electrostatic force. In fact, the shielding property of glass to static electricity was a subject of a controversy in the eighteenth century, and we don't know Oersted's view on this subject. Three months later, Oersted found much stronger proofs for the magnetic nature of the new effect, but his references to Franklin and Wilcke hint at his lack of confidence on this point in April. Benjamin Franklin and Johan Carl Wilcke (1732–1796), a member of Swedish Academy of Sciences, were among the physicists who magnetized a steel needle by discharging through it a Leyden jar. The needle acquired opposite poles at its ends. Some of these researchers, including Franklin (initially) and Wilcke, thought the effect to be a proof of a magnetic action of electric current. However, Franz Ulrich Theodore Aepinus (1724–1802), then Professor of Physics and a member of St. Petersburg Academy of Science, disagreed with them. In his theory of magnetism, a stronger magnetization took place when magnetic fluid moved freely between particles of steel, for instance, when the particles vibrated. He thought that electric current merely created vibrations in steel, the same effect that supposedly occurred when steel was struck with a hammer. These vibrations facilitated a movement of magnetic fluid in the steel under an influence of the earth, so that the magnetization was actually accomplished by terrestrial magnetism. Apparently, Oersted sided with Aepinus in questioning this phenomenon as a truly magnetic effect of electricity. And if he was not sure about the conclusion of such a careful experimenter as Wilcke, Oersted might have doubted in the nature of the phenomenon that he observed himself.

One may say that Oersted's uncertainty about his April result explains only why Oersted did not make it public right away. It does not explain, however, why he did not continue his experiments, which would have been the only way to clear up his doubts. There had to be a better reason than lack of leisure time for not trying new experiments in April.

Apparently, the reason was in Oersted's belief that his battery was insufficiently strong and therefore useless for obtaining any definite results. This is clear from his statement that 'as these experiments were made with a *feeble* apparatus, and were not, *therefore*, sufficiently conclusive ... ' (Oersted 1820b, p. 273). Since a proper battery was not available, Oersted had no choice but to build one. Thus, 'I have associated myself with my friend Esmarch, a Royal Counselor, to make together a stronger pile, capable of reproducing and *increasing* the phenomenon' (Oersted 1820d, p. 72, italics added). Making 20 copper troughs required plenty of metal work and soldering, and it might have occupied them for quite a while if not for the whole duration of three months.

Finally,

> In the month of July 1820, he resumed the experiment, making use of a much more considerable galvanical apparatus. The success was now evident, yet the effects were *still feeble* in the first repetitions of the experiment, because he employed only *very thin* wires. (Oersted 1827, pp. 357–358, italics added)

We now understand why Oersted needed a more powerful battery: he wanted to increase the deviation while retaining a red-hot thin platinum wire in the circuit, for early in July he still adhered to the heat hypothesis.

Thus, the main reason for the delay with the follow-up experiments appears to be found: it was lack of a powerful galvanic battery, which would have allowed Oersted to clarify the 'confusion' about the true direction of the force produced by current. Yet, there is one more unanswered question, which casts a doubt on the above given explanation of Oersted's procrastination.

7. Was Oersted's Phenomenon New?

Granted, in April Oersted did not have a definite law for the direction of the new force, nor was he certain of magnetic nature of this force. But was not the mere fact of the needle's movement a wonderful discovery in its own right? If the effect was real, then it had to be new, whatever its nature. Indeed, even assuming that the attraction was electrostatic rather than magnetic – and nothing else were possible – that would have been a totally new phenomenon, because a pile never produced an electrostatic attraction when its circuit was closed. Thus, Oersted had a very good reason to either publish this observation immediately, or to lock himself in the laboratory, as Roentgen did, other duties notwithstanding, until he verified that the new phenomenon was real, that is, the needle's movement was produced by current and not by a mechanical cause. He could have accomplished this, for instance, by watching the needle while connecting and disconnecting the circuit. Fear of losing priority would have prevented Oersted from wasting time on building a better battery and forced him to experiment with those at hand. Having satisfied himself about the reality of the phenomenon, the natural way to proceed would be either to publish immediately or to continue experiments to find more details about the new phenomenon

Since Oersted had done nothing of the sort, it is possible that he had reason to believe that a deviation of a magnetic needle produced by a pile was not a new discovery. In 1802–1804, several authors carried out experiments that involved a voltaic pile and a movable magnetic needle, and Oersted could have known of at least two of them. In particular, he used to read the *Annalen der Physik*, which published a translation of a paper by S. P. Bouvier of Brussels (Bouvier 1803). Bouvier attached an iron support to the top of a pile and set a magnetic needle on its pointed end. When he touched the bottom of the pile with one hand and brought the other hand to the needle, it moved. The circuit was evidently open, and a keen reader could have recognized Bouvier's effect as electrostatic (Martins 2000, p. 89). Thus, Bouvier was not the person Oersted could have treated as his predecessor.

The case is not so clear, though, with an experiment by Gian Domenico Romagnosi (1761–1835), then a jurist in Trento, Northern Italy. Romagnosi, like some other authors, including Oersted, communicated to the Italian physicist Giovanni

Aldini (1762–1834) a paper to be included in Aldini's forthcoming book on galvanism. Aldini published some of these papers at length, and summed up others very briefly. In particular, he described Romagnosi's experiment as follows:

This new property of galvanism was noted by other observers, and lately by Romanesi [sic], physicist from Trente, who found that galvanism made a magnetic needle to *decline*. (Aldini 1804, p. 191, italics added)

This passage does not say whether Romagnosi's circuit was open or closed. Oersted could have learned this detail only from Romagnosi's original article that was published in a local newspaper (Romagnosi 1802). Very few scientists read it, and most of them learned of Romagnosi's experiment only from brief descriptions of it, such as Aldini's, or that of Joseph Izarn, in his textbook:

According to observations of Romagnosi, physicist from Trente, a needle already magnetized and subjected to galvanic current, declines. (Izarn 1804, p. 120)

Had Oersted read Romagnosi's paper before 1820? There is an obscure passage in Oersted's 'Experiments [1820]' suggesting that he might have read it. Oersted emphasized there that 'the galvanic circle must be complete, and not open, which last method was tried in vain some years ago by very celebrated philosophers' (Oersted 1820b, p. 273). The trouble with this interpretation is that neither Bouvier nor Romagnosi were 'celebrated philosophers'. It is more probable that Oersted meant here the experiments by Jean Nicolas Pierre Hachette (1769–1834); Professor at the *Ècole Polytechnique*, and Ritter, and that he did not read Romagnosi's article.

Since Oersted certainly wanted to know whether Aldini published his article, he had to read Aldini's book. Having already been interested in connections between electricity and magnetism, he would have an incentive to read a special three-page section devoted to this topic, which contained, among others, Romagnosi's experiment. Interestingly, Aldini had begun this section with a list of experiments and observations supporting an 'influence of electricity on magnetism' or a connection between the two fluids, and subsequently Oersted used two of these examples in his lecture (Aldini 1804, p. 190). Oersted could have also spotted the description of Romagnosi's experiment by merely browsing the table of contents or the index.[13] While Aldini's term 'galvanism' might have referred to any circuit, closed or open, Izarn clearly spoke of 'current', that is, of a closed circuit. If Oersted read Izarn's book, which is possible, because the textbook was popular, he could have believed that his experimental arrangement was similar to that of Romagnosi. But even if he recalled only Aldini, it would have been prudent for Oersted not to claim a new discovery in April without checking what Romagnosi had actually accomplished. However, in the short time available to Oersted for library research, he would hardly have uncovered the original Romagnosi's article.

One may object that even if Oersted read Aldini in 1804, he could have forgotten the passage about Romagnosi sixteen years later. In fact, he didn't have to remember much: only that Romagnosi had *succeeded* in using a movable needle to detect the magnetism produced by galvanism. The word 'decline' in both Aldini's and Izarn' accounts obviously referred to a movable magnetic needle rather than an immobile steel needle employed in the eighteenth-century experiments.

Oersted's theory did not require a magnetic detector to be movable; it could have been a steel needle placed near a red-hot wire. If the idea to use a movable needle was his own rather than borrowed from Romagnosi, why did he never claim it? Oersted's matter-of-fact description of a compass's role in his experiment implies that such usage was common. In fact, Romagnosi's experiment was the only one, which Oersted could have suspected in using a compass in conjunction with a *closed* voltaic circuit. And, if he did so, he had nothing new to report after the lecture. Uncovering the direction of the new force and proving its magnetic nature might have appeared to be worthy of an investigation, but not of such urgency that it could not wait for a new pile.

Moreover, having been an extension of someone else's discovery, this investigation should have been less appealing than if it originated from Oersted's own finding. Had Oersted encountered additional difficulties in his lecture experiment, such as smaller deviations, he might have decided against the follow-up. Thus, we need to examine to what degree positive results of the lecture experiment were guaranteed, given Oersted's theory, instruments, and procedures.

8. Logic and Chance in the Lecture Experiment

While preparing his lecture, Oersted conceived the following ideas:

> As the luminous and heating effect of the electrical current, goes out *in all directions* from a conductor, which transmits a great quantity of electricity; so he thought it possible that the magnetical effect could *likewise* eradiate. (Oersted 1827, p. 357, italics added)

> Oersted therefore concluded that just as a body charged with a very strong electric current emits light and heat at all times, so it might also *similarly* emit the magnetic effect he assumed to exist. (Oersted 1828, p. 50, italics added)

These statements contain two hypotheses: (1) a current-carrying wire emits magnetism only when it emits heat and light (the heat hypothesis); and (2) the magnetic radiation spreads away from the wire similarly to heat and light (the radial emission hypothesis).

According to these hypotheses, Oersted inserted into a closed circuit a thin platinum wire to be heated by current, and placed a magnetic detector outside the circuit. He also supposed that a movable magnetic needle would be a good detector of magnetism. None of these conjectures had been tested before the lecture.

Three months later, Oersted found both of his hypotheses false. Some scholars believe that a false hypothesis must introduce an element of chance (Martins 1999, p. 24). This is not necessarily true. A hypothesis increases the degree of serendipity of an experiment if it has not been previously tested, regardless of whether it is true or false. Oersted's two false hypotheses contributed to the accidentality of his discovery only because he did not verify them before the lecture. Had he – by a miracle! – had conceived a true hypothesis of circular magnetism without been able to verify it before the lecture, his estimate of the probability of a positive outcome of his experiment would have been the same as with the false hypothesis of a radial magnetism, that is 50%. Of course, we know that if Oersted acted in agreement with the hypothesis of a circular magnetism, he would have succeeded. However, he did not know this, which is crucial, since we evaluate the degree of serendipity of a discovery from the discoverer's perspective.

Within this theoretical framework, Oersted might have considered such variables as:

– sensitivity of a magnetic compass;
– length of a platinum wire;
– distance between the wire and the needle;
– power of a battery to make a wire red-hot;
– position of the wire relative the needle; and
– direction of current.

In fact, he discussed all of them in July, but it is not clear whether he thought of some of them in April.

Let us now see how testing affects the degree of chance that a certain variable brings into an experimental result. A certain apparatus or a procedure is selected for an experiment if it works more often than not. This means that the experiment involving the apparatus succeeds at least six times out of ten (presumably everything else remains the same), or that its success rate is more than 50%. Naturally, this probability is evaluated on the basis of previous experiments. If no such experiments were made, the probability is set at 50%, and we call this variable an 'accidental factor'. If certain adjustments can be made during the experiment in question, some variables may lose their status of 'accidental'.

For instance, during the lecture Oersted employed a battery he had been using in classroom demonstrations to melt thin iron wires. Of course, this was not exactly a test required, because, with Ohm's Law unknown, he could not predict whether this battery would work with a wire made of a different material (platinum) and of different dimensions. Yet, if the platinum wire appeared dark during the experiment, Oersted could have easily make it glow without changing the battery: simply by reducing its length in the circuit. Thus, the power of the battery may be removed from our list of accidental variables.

It is also unlikely that Oersted considered the order of assembling the apparatus as a variable. Since it was meaningless to him to continue the experiment unless the wire is made red-hot, he obviously had to begin the experiment by closing the circuit, checking that the wire was red-hot, and reducing its length if it was not. After that he installed the compass and took the readings. Thus, if the experiment went beyond the first part – he platinum wire being red-hot – Oersted had to consider only such variables as the sensitivity of the magnetic needle, its distance from the wire, the best position of the needle, and the direction of current.

The sensitivity of a compass in this sort of experiment was unknown. Although it would have been easy to replace one compass with another during the experiment, apparently Oersted did not do it, and thus the effectiveness of the compass was an accidental factor.

Oersted most probably assumed that the closer the needle will be to the wire, the greater will be the deviation. Yet, he could not reduce this distance below 1 cm, for the wire was very hot. Having no idea of how the new magnetic force decreases with distance, Oersted could not be certain that the needle will decline at all even as close as 1 cm from the wire. Thus, the distance between the wire and the needle was another accidental factor.

Unlike other variables, Oersted had a prediction for the best position of the wire's relative the magnetic needle. As shown above, according to Oersted's theory, the greatest deviation would have occurred if the needle and the wire were in the same horizontal plane and parallel to one another. We know that the actual declination in this position is zero. A small deviation observed by Oersted could have resulted from an expansion of the hot wire, because an arc made by the wire would be slightly above the horizontal plane. It is important to note here that the position of the wire was an accidental variable not because the prediction turned out false, but because it had not been tested.

Since the probability of success with a randomly chosen setting (or a magnitude) of an accidental variable is 50% or 1/2, a single experiment with four independent variables has the probability to succeed of 1/16. Such a low probability is normal for a first experiment in a totally new field, and that is why researchers do not offer their first experiments for public scrutiny. Apparently, Oersted had doubts about performing the experiment at the lecture:

> The preparations for the experiment were made, but some accident having hindered him from trying it before the lecture, he intended to defer it to another opportunity; yet during the lecture, the *probability* of its success appeared stronger, so that he made the first experiment in the presence of the audience. (Oersted 1827, p. 357, italics added)

> He had set up his apparatus for the experiment before the lecture hour, but did not get around to carrying it out. During the lecture, the *conviction* so grew upon him that he offered his listeners an immediate test. (Oersted 1828, p. 50, italics added)

The words 'probability' and 'conviction' imply that Oersted decided to proceed with the experiment at the last minute. This means that the decision was psychological rather than logical. In other words, it was a gamble.

But, why did he want to risk his reputation by going public with an unprepared experiment, instead of quietly carrying it out after the lecture? In fact, the risk was not great, if Oersted thought of the experiment in question merely as a *lecture demonstration*. He had already believed for many years that electricity, galvanism (which was considered intimately linked with chemistry) and magnetism were closely connected. To convince his students in this he brought to their attention *illustrations* (experiments and stories of some natural phenomena) of various connections between them. Is it more logical to believe that Oersted entered the classroom to make a discovery in view of students?

Unlike the lecture experiment, Oersted's July investigation was very systematic. By repeating experiments with a variety of modifications, he left little room to chance. Yet, one intermediate result was probably obtained by accident. Oersted was so convinced that magnetism could be emitted only together with heat and light that he never tried to check this hypothesis experimentally, even early in July. Most likely, he discovered the truth when he accidentally moved the magnetic needle away from the hot wire and closer to a cold connecting lead and found no difference in the declination (on other possibilities see Section 10).

9. How to Treat Serendipity of a Discovery

At the time of Oersted, and even later, the view of scientists on the role of chance in discovery had been quite primitive. The only aspect of chance found in the literature is a side-effect discovery. This is probably how Oersted understood Gilbert's accusation. To alleviate this charge Oersted emphasized that he had been interested in discovering a connection between electricity and magnetism long before the lecture experiment, and that he predicted the result of this experiment. A positive response to these statements proves that Oersted's understanding of the role of chance was in line with that of scientific community.

Four decades later, transversality of electromagnetic force acquired a great theoretical importance. This prompted a corresponding revision of the meaning of Oersted's discovery, of which Hansteen's account is an example. Having reduced the discovery to a single step – the discovery of transversality – Hansteen had no choice but to place it at the lecture. Since Oersted had not revealed an expectation of a transversal magnetic force, Hansteen called his discovery 'accidental'. Interestingly, the two use the same criterion of expectation for the opposite purposes: Oersted cites his prediction – and, therefore, expectation – to refute accidentality of the discovery, while Hansteen relies on lack of expectation to uphold it. In fact, the two expectations were different: Oersted expected a movement of a magnetic needle, while Hansteen wanted him to expect a *particular* movement. The differences reflect a different understanding of the discovery's meaning, because Oersted emphasized the experimental result obtained in April, while Hansteen focused on the one achieved in July.

One can avoid such confusions by treating a discovery as a process, which consists of separate steps, or findings, and discuss the role of chance and theory for each of them. Usually, a subsequent finding depends on the previous one. For this reason, the result obtained closer to the end of the process cannot be expected in the beginning, for had this been otherwise, this result would have been discovered much earlier. It is reasonable to expect from a scientist to predict only an *initial* result, with the idea that each subsequent step in the discovery will lead to new predictions. Having this in mind, Hansteen's requirement to expect one of the final results – transversality of the force – before the very first experiment, is unreasonable. Hansteen might not have considered the first result – a magnetic deviation – important, but without it Oersted would not have discovered the transversality.

But even if applied properly the criterion of expectation alone would have been insufficient to determine the degree of serendipity. For instance, may we infer from the facts that magnetic deviation was expected while transversality was not, that the former discovery was less accidental than the latter? Bearing in mind that transversality was discovered by a meticulous systematic research, while the movement of the magnetic needle was noticed only due to a lucky combination of instruments and hypotheses in a single experiment, such a pronouncement would defy common sense.

The situation is similar with other single criteria. For instance, R. Martins argues that the lecture experiment 'was not done by chance', because 'Oersted was guided by the second hypothesis, and the position of the wire relative to the magnetic needle was carefully chosen' (Martins 1999, p. 24). In other words, the experiment was pre-planned, and therefore its result was not accidental. As shown above, however, logical factors, such as hypotheses, did not eliminate accidental factors. Moreover, sometimes hypotheses lead to procedures that increase the degree of serendipity of experimental results. Indeed, had Oersted not possessed a particular model of magnetic forces, he would have tried to discover the existence of a new magnetic force and its direction empirically, like he did in July. With the wire kept at the 'best' position relative the needle, Oersted would have estimated the probability of a needle's deviation as 50%, while if he moved the wire around the needle, this probability could have reached even 100% (assuming that other conditions were favorable).

If we need to characterize an overall serendipity of a certain finding, we have to take into account all the factors involved, both logical and accidental. Yet, there is no obvious criterion of how to make this summary. We can try to go by numbers: if the number of accidental factors is greater than that of logical ones, we say that the finding was mostly serendipitous, etc. However, such a cumulative approach may not work in determining whether chance hindered or accelerated the discovery, because different factors do not have the same weight. Apparently, a better strategy would be to list the effect of each factor separately and then to summarize them if possible. It is worth noting that calling the whole discovery mostly logical (or mostly serendipitous) works only for one who knows all the factors involved and their effects. However, sharing this label with others without giving out all the details (the factors) may mislead people. And since labeling a discovery as a whole does not provide any additional insight into its process, it would be more prudent not to use it at all but to give the details instead.

A reader well versed in electromagnetic experiments can imagine experimental modifications created by an involvement of chance. Less experienced readers, however, including students, would greatly benefit from converting the thought experiments into real ones. For this reason, it would be desirable to supplement a lecture, students' reading of excerpts from historical sources, and a discussion, with an experiment.

10. Oersted's Experiment in the Classroom

It is necessary to begin with repeating the lecture experiment. To do this a teacher needs very thin high-resistance wires about 5–10 cm long. If only one platinum wire is available, the experiment can be set up as a demonstration. Since heating a thin wire has its peculiarities important in this case, if platinum is not available, this demonstration can be carried out with a thin copper wire (of a diameter no more than 0.1 mm). To make such a wire red-hot, one needs a current about 4 A that can be provided by four D-size cells. An important part of this experiment is to notice that the magnetic needle moves immediately after the circuit is closed, while it takes some time for the wire to become red-hot. This effect is more obvious with thicker high-resistance wires, but even a thin copper wire creates a time delay about 1 sec.

Nichrome wires are proper for students experiments. To make a nichrome wire of the diameter 0.4–0.5 mm red-hot, they will need a current of 4 A to 7 A, depending on the wire's length and the type of a battery. You can use commercial 12 V electronic power supplies, or a rechargeable battery with the capacity of at least six ampere-hours, or a battery of 8–10 D-size cells. It is better if these cells are rechargeable, because they will be drained out rather quickly.

To use a nichrom wire, stretch it between two non-magnetic screws inserted 10–15 cm apart into a wooden block B about 16 cm long, 2 cm wide, and 1 cm high. Make a Π-shaped support S out of sheet aluminum or brass to hold a compass C about 1 cm above the wire (Figure 3).

To detect the magnetic effect use school compasses of various kinds and magnetic needles made of steel pins or paperclips. Suspend such a needle to a stand by means of a thread. The magnetic needle must be about 1 cm above the high-resistance wire.

Let the first students' experiment imitate as closely as possible Oersted's lecture experiment. This means that each group may use only one set of instruments and materials (the wire, a magnetic needle, and a battery). Also, students close the circuit first and install the magnetic needle only if the wire becomes red-hot. Finally, suggest setting the magnetic needle parallel to the wire in the same horizontal plane. Some groups will succeed with heating their wires until they become red, while others will only be able to make the wires hot but dark. The former groups will continue the experiment, and some of them will report magnetic deviations, probably small. While discussing their results students will conclude that a single experiment carried out by Oersted during the lecture could have easily failed.

It makes sense to devote the second experiment to a verification of Hansteen's account of the initial Oersted's experiment: 'Oersted tried to place the wire of his galvanic battery perpendicular (at right angles) over the magnetic needle, but remarked no sensible motion'. Let students do this experiment without any high-resistance wires (Hansteen does not mention hot wires), using D-size, or C-size, or even AA-size batteries. Some groups will agree with Hansteen, but others will probably disagree. If no one will notice a deviation, suggest to reverse the direction of current. With a proper direction of current, the deviation can be up to almost 180°. The smaller the current, the lesser is the deviation, but it will certainly exceed 90°. If current is less than $2A$, it may take several seconds to complete the movement, thus the circuit should be closed all that time. To succeed, the needle and the wire must be set at an angle slightly different from 90° (for instance, 1° is a sufficient difference), which will be fulfilled without special efforts. The element of chance in this experiment enters with the direction of current: no deviation under one direction of current, and the grand deviation under the other.

The third experiments may imitate Oersted's July research if students may choose the wire, the battery, the magnetic needle, and their own procedure. Those who will choose to close the circuit after setting the magnetic needle in place will notice that the needle moves before the wire becomes hot. This may prompt a hypothesis that the magnetic deviation does not depend on the wire's temperature. Moving the compass over the leads connecting the wire to the battery will prove this hypothesis. This may give some students an idea to compose the whole circuit of copper wires that do not become hot, and they will easily find out that such a circuit produces a large magnetic deviation, which can be, for instance, 60° to 80°, depending on the battery. Likewise, when moving the magnetic needle around the leads students will discover that a declination takes place at various positions of the needle, although its magnitude does depend on the position.

While discussing students' results, the teacher should focus their attention on accidental factors. Those students who failed in the first experiment may note that if Oersted's platinum wire were not too thin, it could have remained insufficiently hot, forcing Oersted to interrupt the experiment without even testing whether his magnetic needle moved. It is likely that a number of students will concur that they discovered the independence of magnetic effect of the wire's temperature by chance. For instance, those who use a power source with a voltage control usually gradually increase the voltage, and they will notice a magnetic deviation of several degrees even at a lower voltage, when the wire is still cold.

Recommend to those who use a battery to take a wire of such a length that when the whole wire is in the circuit it is not very hot. By moving one lead with an alligator clip along the wire students will easily find the length at which the wire becomes red-hot. Warn students to be cautious with the hot wire. High-school students (15 years old or older) should not have a problem in dealing with such a wire if the clips have plastic handles. If they watch the needle all the time, they will notice that it had deviated before the wire became hot.

Thinking about the role of chance while repeating Oersted's experiment will lead students to some insights into the discovery that Oersted never spelled out. One concerns the magnetic needle. Having discovered that longer needles decline less than the shorter ones, students may suppose that an unsuitable needle might have been responsible for weak deviations in the lecture experiment. The other insight is that a proper procedure can make all the difference. Students will see that if they close the circuit first and set the compass in place afterwards, it is very difficult to observe a declination *before* the wire becomes red-hot. On the other hand, reversing the order makes this important observation certain. Since Oersted did not notice this effect during the lecture, students may deduce that he had closed the circuit first. And since it was the heat hypothesis that was responsible for such a procedure, this episode shows that a theoretical bias may have detrimental practical consequences.

Students can also come to a similar conclusion about the radial–emission hypothesis, according to which a magnetic declination should be the strongest (about 90°) when the needle is parallel to the wire and makes a horizontal plane with it. Having observed considerable deviations (15°–60°) at various positions of the needle and at different angles between the needle and the wire, students cannot understand why Oersted's result was much smaller than theirs. But if they set the needle in the position described above (not mentioned in any textbook), they see a deviation close to zero. This may lead them to a plausible conclusion that Oersted employed this very position in his lecture experiment, which could have been the main reason for small declinations he observed.

Thus, students will realize that previously untested hypotheses, materials, and procedures introduced several elements of chance in Oersted's lecture experiment. They will also understand from the second experiment that, unlike a single trial, a systematic investigation eliminates some accidental factors.

11. Conclusions

In July 1820, Oersted discovered that electric current creates a magnetic force directed perpendicularly to the current. This investigation mostly followed logical lines. However, in the initial April experiment, theory was complemented by a considerable amount of chance. In particular, this experiment was based on two new hypotheses that suggested a winning experimental set-up: closing the circuit and employing a compass. However, the hypotheses themselves were incidental and led to some false recommendations, especially about the compass's position relative the wire, which considerably diminished the magnetic deviation.

If this decrease were exacerbated by another accidental factor (a heavier needle, for instance), Oersted could have considered the result negative. There is no certainty that facing a negative result he would have come back to an experiment, which was to him merely another demonstration of a connection between electricity and magnetism. Thus, chance might have ended the story for Oersted, leaving the discovery of electromagnetism to someone else. Oersted's decision to postpone an investigation of the new effect could have led to the same disastrous result, for news about his lecture experiment could have reached – through his audience – other researchers.

Thus, Oersted's discovery involved both theory and chance, with some of its components having a greater degree of serendipity than others. Logical and accidental factors had a positive impact on the discovery in some respects and negative, in others.

While Oersted's two hypotheses originated in his belief in the unity of nature, there was more than one way for this metaphysical view to be transformed into a physical theory. Other researchers who shared the same belief chose other ways and failed. It is impossible to justify Oersted's choice over the others: it was simply a matter of luck. The same is true about Oersted not conceiving the same theory, say, ten years earlier: the timing of this theory was as much a matter of chance as its content.

A proper discussion of this story in the classroom will not only improve students' understanding of the scientific process but also teach them a practical lesson about the art of experimentation. They will realize that some negative effects of accidental factors can be eliminated by repeating an experiment many times with a variety of modifications in the apparatus and the procedure. Given an opportunity to discuss the role of chance in some other historical experiments, students can conclude that perhaps chance is a common component to scientific discoveries. This, in turn will teach them that, unlike mathematics, science cannot be reduced to an exercise in logic and mathematics, which may have a positive impact on students' attitude towards studying science.

Proving a heavy involvement of chance in two discoveries – by Henri Becquerel and Oersted – is insufficient for general conclusions about the role of chance in science, especially because the two had already had a reputation of serendipitous discoveries. However, the results of these studies are quite suggestive. In particular, several accidental factors discovered in these cases, such as untested hypotheses, or an unfortunate choice of materials, instruments, and experimental procedures are found in many scientific researches. This leads to a hypothesis that the currently adopted division of discoveries on serendipitous and regular may be a misnomer, because chance may play a role in all discoveries. This hypothesis will obtain more credence if one will show that even discoveries that have never been called 'accidental' were due to some degree to chance. Apparently, the methodology employed in this paper would be fully applicable to such cases as well.

Notes

[1] T. Shanahan interprets 'the reasons commonly alleged' as an accident (Shanahan 287, p. 304). In fact, Oersted speaks here of how he conceived an *idea* that 'the magnetical effects are produced by the same powers as the electrical', rather than of how he discovered a new *phenomenon*. He held this idea for many years, which excludes accident. What Oersted most probably meant here was various connections between electricity and magnetism, offered since the middle of the eighteenth century.

[2] T. Shanahan's claim that an attribution of the discovery of electromagnetism to an accident was the 'received view' between 1820 and 1920 is a gross exaggeration, and it certainly requires more evidence than the one example he provided (Shanahan 1989, p. 288).

[3] Christensen suggested that Meyer's position – against contemporary positivism and in favor of Kant and *Naturphilosophie* – was due, in part, to Danish nationalism that emerged after WWI (Christensen 1995, p. 154).

[4] R. Stauffer and L. Pierce Williams favored *Naturphilosophie* in general, P. Thullier emphasized the role of Schelling, while T. Shanahan championed Kant (Stauffer 1953, p. 310; Williams 1966, p. 59; Thuiller 1990; pp. 348–350; Shanahan 1989, pp. 289–294).

[5] A neglect of definitions leads to disagreements between not only different people but even different passages from the same paper. For instance, R. Martins says on one page: 'I disagree with Altmann's conclusion that the discovery occurred by chance' but notes on another page that 'in some sense, the discovery of electromagnetism occurred by chance: it was guided by a wrong hypothesis' (Martins 1999, pp. 2, 24).

[6] It has become quite fashionable to attribute Oersted's success to his philosophical views. For instance, in H. Snelders' view, Oersted's discovery was 'a *direct consequence* of his metaphysical belief in the unity of all natural forces' (Snelders 1990, p. 238, italics added). Unfortunately, he did not explain how the '*direct consequence*' was to be realized. Some scholars even made a general claim of a positive influence of German 'Romanticism' on science (Snelders 1970, p. 214). The implied reasoning goes like this: (1) some discoverers of electromagnetism and of energy conservation (Oersted, in particular) held philosophical views similar to those of the 'Romantics', (2) thus, Romanticism led to some very important discoveries, and (3) thus, Romanticism had a positive impact on science. The argument is dubious, because #2 does not compare the role of metaphysics to other factors involved in the discovery, and #3 ignores the fact that the number of scientific failures stimulated by Romantic philosophy was much greater than the number of successes. B. Gower has already criticized this argument as applied to energy conservation (Gower 1973). That the belief in unity in nature did not have to lead to Oersted's experimental arrangement becomes obvious when one studies experimental failures of Oersted's predecessors who had been inspired by the same idea (the details are beyond the scope of this article).

[7] Unlike many other authors, Kenneth Caneva emphasizes the meaning of Oersted's discovery to Oersted himself, and he believes that it is not about the phenomenon of magnetic deviation but about Oersted's theory (Caneva 200, p. 5).

[8] The term 'galvanism' came into being after Galvani's discovery to denote the cause of animal contractions in Galvani's experiments. After the invention of the electric pile, the same term was used in a different meaning, namely to describe processes in all circuits containing a pile, even those devoid of animal parts. In this paper, however, I will call the latter circuits 'voltaic'. On the change of meaning of 'galvanism' see Kipnis: 1987, 'Luigi Galvani and the debate on animal Electricity, 1791–1800', *Annals of Science* **44**, 107–142.

[9] Simon Altmann asserts that 'the perpendicular configuration [magnetic needle is perpendicular to the wire] is the one that Oersted unsuccessfully tried, *probably for some years*' (Altmann 1992, p. 18, italics added). This 'failed experiment of course did not work' (ibid., p. 14) In fact, as shown in Section 10, the perpendicular arrangement produces a very large declination for one of the two directions of current. Having experimented 'for some years' Oersted could have hardly escaped to have, at least once, a favorable direction of current. Altmann also believes that the following passage proves that Oersted did experiments before 1820: 'he conjectured, that if it were possible to produce any magnetical effect by electricity, this could not be in the direction of the current, since this had been *so often* tried in vain' (Oersted 1827, p. 357, quoted in Altmann, p. 39, italics added). In Altmann's interpretation of this passage, 'He [Oersted] creates an impression . that the negative evidence obtained, by other people, persuaded him to try a different line of thought. (Obviously, if he himself had been performing the perpendicular experiment always, it would require some explanation

why he had changed his mind!) I am afraid that I take the view that Oersted is not entirely candid here, since it is almost certain that the only one person who have been trying the experiment at all was Oersted himself' (Ibidem).

One may note that, strictly speaking, the experiments implied in the phrase 'this had been often tried in vain' might have been Oersted's. However, there is another passage from Oersted, which is unambiguous: '*The investigators* had expected to find magnetism in the direction of the electric current .... All investigations had shown that nothing was to be found along this path' (Oersted 1828, p. 50, italics added). While Altmann will probably read this as another example of Oersted's deception, other readers will interpret 'the investigators' as Oersted's predecessors.

[10] Oersted gave several dates for his famous lecture, such as 'last winter', 'the year 1820', 'beginning of 1820', 'the spring of 1820', and 'three months' before July 1820. The exact date is irrelevant for the purpose of this article, and I speak of 'April experiment' merely to distinguish it from 'July experiments' and remind of the time gap between them. Altmann argues that the lecture could not be in April, but, in my view, unconvincingly (Altmann, p. 38).

[11] Were it not for the magnetic field of the earth, the maximal deviation would have been exactly 90°. The stronger the current, the closer is the maximal deviation to 90° Oersted mentioned neither the role of terrestrial magnetism, nor the maximal deviation he expected.

[12] R. Martins makes several points similar to mine: that Hansteen's account is not reliable, that Oersted discovered transversality in July, and that he based his lecture experiment on his 'second' hypothesis, similar to my 'radial emission hypothesis', and compared the results it predicted with possible Oersted's experimental results (Martins 1999, pp. 17–18). We differ, however, on other points. For instance, Martins speculates that Oersted initially (before the lecture) assumed the magnetic force to be directed along the wire (he calls it the 'first' or 'longitudinal' hypothesis). This hypothesis suggests that if a wire is set horizontally and perpendicularly to a magnetic needle, the deviation must be close to 90°. Martins thinks that, although there is no evidence whatsoever that Oersted contemplated the longitudinal hypothesis, it is safe to suppose so, because 'if Oersted attempted such experiments, he could observe no effect' (Ibid., p. 15). However, as shown in Section 10 (see also footnote 11), this conclusion is erroneous. In fact, the probability to observe a movement would be higher when using the longitudinal hypothesis than with the radial emission hypothesis. If Oersted had ever conceived the longitudinal hypothesis, it would have been very easy for him to test it, because neither platinum wire nor a strong battery were required to obtain a movement of the needle. Thus, 'natural' or not, this idea had probably never occurred to him.

I think that both Altmann and Martins came upon the concept of longitudinal magnetization, because they misread the word 'current' in Oersted phrase cited above ('if it were possible to produce any magnetical effect by electricity, this could not be in the direction of the current, since this had been so often tried in vain') as galvanic current, while a more plausible interpretation of it is a current produced by a discharge of a Leyden jar.

Indeed, Oersted might have known of only one experiment, in which a needle was magnetized by galvanic current, while there were at least ten experimenters who managed to achieve the same by means of electrostatic discharge. Thus, Oersted's words 'the experimenters', 'all investigations', and 'so often' are more consistent with electrostatic experiments. As to his expression, 'tried in vain', the negation refers not to experimental results (the needles did acquire poles) but to the interpretation of these results as showing a direct magnetic action by electricity (see Section 6).

[13] One entry is 'Magnet: its connection with electricity. 190.- Means to magnetize a needle by galvanisme. 191'. Another says: 'Romagnesi made experiments with a magnetized needle.191' (Aldini 1804, pp. 381, 396).

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