# Charged with History—
# On the Pluralism of Physics and the Hermeneutics of Learning it

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# Abstract

This thesis discusses ways in which history of physics can be useful in physics education. Examples are mainly chosen from the field of electromagnetism.

The first chapter deals with objections against the use of history in physics teaching. The strongest objections come from critics who have argued that history when used for such purposes needs must be tendentiously written. Others have feared that historical approaches might reduce the effectiveness of physics education. While some of the reservations against using history are very well-founded and some warnings must be heeded, the arguments were generally not found to be compelling.

The second chapter discusses an array of applications of history for physics teaching. History may illustrate lessons on social and ideological contexts for science, and on the nature of physics. Studying the historical development of physical concepts may provide the teacher with insights about problems students may have with learning the same concepts. Many historical experiments have pedagogical advantages—among them the greater transparency of the technology they rely on.

The third, and longest, chapter is concerned with the pluralistic character of physics, and with the nature of understanding. The degree of interpretive freedom in physics is discussed, and the notion of pluralism in physics is clarified. The next issue is what ‘understanding’, and in particular what ‘understanding physics’ can be taken to be, and a lengthy detour into hermeneutics is included here. Hermeneutics as a theory of interpretation and understanding questions the subject/object cut and addresses issues of intelligibility, meaning and significance. The main point of this third chapter as a whole is the claim that understanding physics well involves knowing a plurality of accounts of physical phenomena—and that historical studies can provide such plurality of perspectives.

The fourth and final chapter reports results of a limited focus group study. Three groups of university undergraduates were questioned about their ideas about history in physics courses, and about some basic concepts of electromagnetism. While the study is too limited to permit strong conclusions to be drawn, some hypotheses emerge. One is that students’ electromagnetic concepts are weakly linked with simple physical phenomena of a kind that were central in the early stages of experimental electromagnetic research. Students’ notions appear to be mainly grounded in theory.

The major conclusion, which emerges from the chapter on pluralism and understanding, is that history of physics, judiciously applied, can enhance students’ interest in and understanding of physics, and perhaps also strengthen a sense of personal relevance of that science to the learners.

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# Introduction

## A prehistory of this thesis

When I took my first electromagnetism course at the University of Oslo, taught by Arnt Inge Vistnes, one of the homework assignments called for a short biography of any important physicist in the history of electromagnetism. Finding little material on Wilhelm Weber, my first choice, I settled on Heinrich Hertz. The biographical sketch I handed in some weeks later barely scratched the surface of the physical problems Hertz had been concerned with. This stray encounter with the history of electromagnetism nevertheless left me intrigued.

Through writing this assignment I became aware that my high school notion of Coulomb's law had been one of instantaneously acting distance forces. I had not noticed that this conception was inconsistent with the spirit of field theory, or with the principle that no information may travel faster than light. Another surprise was the fact that our modern notion of electric current seemed to have far more in common with the unknown Weber's rejected account than with the famous Maxwell's idea. Were my other physical conceptions also such a jumble of ideas derived from different historical periods? Was there more unnoticed tension and incoherence in my physical ideas, of a kind similar to the conflict between Coulombian distance forces and the finiteness of any propagation?

The historical debates had been full of bold metaphysical claims and heated controversies about the nature of electricity, matter and forces. With these debates long lost, what seemed to be left was a generally agreed upon formalism. It somehow had a vagueness of meaning that made it easy to overlook the fact, for example, that Coulomb's law should not be thought of as being about instantaneous distance forces. Yet, perhaps this indistinctness was only due to my failure to pay sufficient attention during the course. Did my co-students share any of this confusion?

I kept my eyes open for opportunities to look more closely at questions of this kind, and when Carl Angell of the Physics Education Group agreed to supervise a master's thesis on the value of history for physics teaching I had found one. Arnt Inge Vistnes would co-supervise the work, as I intended to concentrate on electromagnetism.

## History for science teaching

The past twenty years have seen an increased interest in, and advocacy of, the inclusion of history in science curricula. In the late 1980s a number of governmental and educational bodies, notably the American Association for the Advancement of Science and the British National Curriculum Council, recommended the integration of history of science into science education.

At the same time the International History, Philosophy, and Science Teaching Group was formed and began to arrange conferences on this subject. In 1992 *Science & Education*, a journal devoted to this field, was published for the first time.[1] Much material for this thesis was found in this journal.

The arguments for teaching the history of the discipline as a part of physics education are diverse, and not always internally consistent. The form proposed for the history components of science education also varies a lot. On the minimal end there is the use of historical anecdotes for enlivening science texts. A more demanding form is teaching science concepts by following the genealogy of those concepts. The goals aspired to through historically informed science teaching have been even more various. A better understanding of traditional school science, a critical view of the relations between science and society, an appreciation of the greatness and glory of the scientific enterprise, or a knowledge of the fallibility and arbitrariness of scientific knowledge are among the many aims that have been suggested. Clearly, a meaningful answer to the question "should science education include history of science?" can be given only in conjunction with an answer to the questions of what form and scope this history component should take.

While paying some attention to these general debates about history in physics curricula, much of this thesis will concentrate on the how historical studies reveal the *pluralistic* character of physics, and on how this may be valuable in physics education. Examples will preferentially be drawn from the field of electromagnetic theory.

## Pluralism in physics

> The investigations in this text are borne by a delight at the diversity of ideas for synthesis that already are available to us in our century. It is a joy over the opportunity to contemplate, and in a very modest degree to re-experience, brilliantly developed and deeply different philosophies, basic assumptions, views on the world and on human beings ... (Næss 1982 [1969], p. 12)

The quotation is from the work *Which World is the Real One?* in which the philosopher Arne Næss deals with comprehensive, or total, systems of thought, such as those of Spinoza, Kant, or Plato. Næss describes the exhilaration he experiences when immersing himself into such total systems, at discovering their coherence and their imaginative solutions to problems arising within the systems. Quite as fascinating he finds the sheer variety of such systems, the multitude of approaches to the construction of an all-encompassing system of thought. He wanders from system to system, a confidently seeking sceptic, marvelling at each system in turn, but always going further: "It is the joy of a traveller, or rather, that of a vagabond" (Næss 1982 [1969], p. 11).

Would anybody describe the joys of studying physics in a way even remotely similar to this? I do not think high school students doing physics would typically describe their learning experience in these terms. Now physics, unlike the systems Næss is referring to, is hardly a *total* system of thought. It deals with the limited range of objects and phenomena that can be studied with the limited methods available in the discipline, and one might perhaps expect less variation and colour in a field that does not encompass ethics, aesthetics, epistemology and generally the whole range of human experience.

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[1] The book *Science Teaching: the Role of History and Philosophy of Science* by Michael Matthews, editor of this journal, provides an outline of these developments and their history.

However, I think many would feel that the difference between the experience of studying physics and Næss's experience of studying philosophy does not consist merely, or primarily, in these differences in scope or domain. Rather, what is thought to be different about physics is that it gives *one* view of the world, if only of the excerpt of the world that it studies. Physics gives one description of a physical problem, and one answer to a physical question. Knowing this description is what there is to understanding the problem. Progress in physics means describing an increasing range of phenomena, or increasing the precision of the description. Not so with philosophy, where progress may as well mean reinterpreting a problem as solving it. Certainly, not everybody would think about physics in this way, as a unitary system of unambiguously correct answers. However, many would, and, for the purposes of this thesis most importantly, many students do[2], a matter to which I will return.

This thesis will be concerned with the degree of interpretive freedom in physics, with the discipline's multiplicity of perspectives on the physical world. An aim is to show that the physicist need not be entirely deprived of the joy and wonder Arne Næss describes, but can marvel at the explanatory power, imaginativeness, and compelling character of one system at a time, while always expecting there to be other approaches that are quite as reasonable and elegant. In connection with his careful reconstruction of Hertz's experiments with electromagnetic waves, Roland Wittje explains that a significant measure of his motivation had been "an interest in emancipating myself from the claims physics makes to absoluteness (Wittje 1996, p. 107). A kindred motivation informs a good deal of this thesis. Emphasis will be placed on the role of history in highlighting pluralistic characteristics of physics.

## Pluralism and education

If students see physics as a rigid, static system of established facts, a pertinent question might be to what extent this student conception of physics is responsible for the flight from the subject. There is some evidence that the absolutistic nature, or pretensions, of science is one root of the decline in student enrollment in science courses—though such an hypothesis is admittedly not very easy to test. In an address to the Norwegian Physical Society in June 1999, Arnt Inge Vistnes discussed some challenges for current physics education. One issue was the idea of a 'bottom line culture' or 'culture of right answers'[3]. Vistnes raised the question whether physics represented such a 'culture of right answers' and warned that "If so, research in education tells us that the involvement, enthusiasm and interest of those who are to appropriate this culture will suffer" (Vistnes 1999.[4])

In this thesis some attention is given to the question whether students lose interest in science because of its ready-made appearance. Rather more is given to another pedagogical effect of teaching physics as one unified account. I will argue that trying to transmit a monistic kind of physics in schools involves missing an opportunity to develop a deeper understanding of the science. Writing about the relationship between Weber's electrodynamic theory and his own, James Clerk Maxwell writes that:

> In a philosophical point of view, moreover, it is exceedingly important that two methods should be compared, both of which have attempted to explain the principal electromagnetic phenomena, and both of which have attempted to explain

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[2] See section 2.1.2

[3] "Fasitkultur" in the Norwegian

[4] http://www.fys.uio.no/ arntvi/fysmot99.pdf, accessed May 2004

> the propagation of light as an electromagnetic phenomenon and have actually calculated its velocity, while at the same time the fundamental conceptions of what actually takes place, as well as most of the secondary conceptions of the qualities concerned, are radically different (Maxwell 1998 [1891], p. x).

If 'philosophical' were replaced by 'pedagogical' in the first sentence, the statement would be no less appropriate[5]. A primary focus of this thesis is the value of pluralism for deepening understanding. Put commonsensically, learning to view electromagnetism from several perspectives increases understanding. That remains true if some of these perspectives are forgotten historical views that are reconstructed for pedagogical purposes. The history of physics could be mined for differing accounts of the same phenomenon, because of the value these accounts have for teaching.

## Hermeneutics of learning physics

Put less commonsensically, learning a plurality of versions is *necessary* in order to understand electromagnetism, because the multiplicity of accounts of any electromagnetic phenomenon is part of the 'being' of electromagnetic science. At least, this is what certain philosophers of understanding might say. In this thesis much space is devoted to questions about what 'understanding' may be. Assuming (for now without argument, though more will be said about this later) that 'understanding electromagnetism' is not the same as being able to do calculations and to solve problems in electrodynamics, what can 'understanding electrodynamics' mean? There is a great and diverse literature on the nature of understanding, and, unsurprisingly, the roots of such accounts are largely to be found in the humanities.

I have looked more closely at phenomenological hermeneutics, a philosophical approach that accounts for what understanding *is* in such a way that knowing a *spectrum* of accounts of the same phenomenon is an inherent trait of understanding. This approach also suggests some rather surprising analogies between scientific experimenting or interpreting a physics paper on the one hand, and performing a piece of music or enacting a play in a theatre on the other. Such analogies can be found by studying the relationship between the subject doing the interpreting, and the object that is attempted understood. A major idea is that learning consists in a characteristic change in this subject/object relation, as the subject is changed by coming to understand the object, and the object itself—electromagnetic theory in this case—is or can be changed by being interpreted by the subject. Phenomenological hermeneutics suggests that in the process of successful learning, the 'cut' between subject and object is moved, and the ontology of the learning situation recast.

Combining the term 'hermeneutics' with anything physics-related in a title is perhaps a little risky in a thesis to be submitted in a physics department—I am afraid many physicists immediately associate this constellation with Alan Sokal's infamous article[6]... But while many ideas from hermeneutics at first may sound odd, this approach offers so many fascinating and enlightening perspectives on learning, interpreting and understanding that I think it worth the trouble to try to get to grips with it. I hope that it will become clear that hermeneutics

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[5] The quotation from Maxwell is included in the preface to André Koch Torres Assis's textbook on Weber's electrodynamics—a book whose purpose appears to be teaching *physics* at least as much as teaching its history or philosophy.

[6] Sokal, Alan D.: 1996, Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity, *Social Text* **46/47**, pp. 217-252

as applied here is *not* a framework for denouncing science as subjective and relative, a product of power struggles and private ambition, but that rather, a hermeneutic approach can convincingly show

> ...that science is after all about the world, the common world in which we live, not about its own ways. (Eger 1993a, p. 4)

## "The standard Coulomb in Paris"

In the section on the prehistory of this thesis I mentioned certain knowledge blanks that my historical readings had made me aware that I had. I also admitted a curiosity about whether these blanks were typical of modern physics students. Besides, I wanted to know how physics students would react to the idea of using history in physics classes.

The last chapter of this thesis consists of a report from three group interviews with some undergraduate physics students at the University of Oslo. The topics discussed included the use of history in physics education, but most of the interview time was devoted to discussions about fundamental physical concepts like electric charge and current. The students were in part faced with historically significant problems of relating these physics concepts to simple, comprehensible physical phenomena. An example would be the problem of defining a convenient unit of electric charge in a transparent way.

For reasons explained in that chapter, the study became rather smaller than planned, and the conclusions that could be drawn were correspondingly limited. The dialogues still provided material that was interesting from philosophical as well as pedagogical perspectives.

## A note on translations

Finally, wherever quotations from Norwegian or German texts are included, I am responsible for the translations.

# Chapter 1

# Physics and History—Oil and Water?

While the aim of this thesis is to argue for the value of history in physics education, a number of objections can be identified from the outset. Apart from concerns that historical approaches might be too time-consuming to be justified as parts of physics courses, and doubts about the pedagogical value of such approaches, there are some fundamental questions related to the nature of history and physics as disciplines that need to be addressed.

It has been argued that the aims and standards of history and physics are so different as to be contradictory, and that "combining the rich complexity of fact, which the historian strives for, with the sharply defined simple insight that the physicist seeks" (Klein 1972, p. 17), is a "hazardous project", in which "we run a real risk of doing an injustice to the physics or to its history—or to both" (p. 12).

Some consideration of the suitability of historical approaches to our particular target group, the physics learners, concludes this chapter.

## 1.1 Quality of history in physics classes

### 1.1.1 Must history for physics teaching be bad history?

Some important objections to use of history in the teaching of physics, presented by Martin Klein during a conference on this subject at MIT in 1970, are centered on the possibly inevitable distortion of history when used for such purposes. While physicists seek simple and general insights, universal laws and regularities, and in their experimental work strive for reproducibility, the historian's concerns are, according to Klein, quite different. In the words of the historian Herbert Butterfield, quoted in Klein's lecture,

> The value of history . . . lies in the richness of its recovery of the concrete life of the past. . . . There is not an essence of history that can be got be evaporating the human and the personal factors, the incidental or momentary or local things, and the circumstantial elements, as though at the bottom of the well there was something absolute, some truth independent of time and circumstance" (Butterfield 1950 [1931], pp. 68-69), (Klein 1972, p. 16).

For the physics teacher seeking material for physics lessons in the discipline's history, this unstructured wealth of facts is just not relevant—or so Klein thinks. Neither does the solution lie in picking out only significant elements of past events, for the selection itself involves distortion, as "[t]he thing which is unhistorical is to imagine that we can get the essence apart from the accidents" (Butterfield 1950 [1931], p. 69). Trying to fit this historical material into physics lessons is antithetical to attempts at understanding history on its own terms. It involves writing significance and direction into history after the event, and letting hindsight rather than insight into the relevant period dictate the account. In the words of Arthur Lovejoy, also restated in Klein's paper,

> The more a historian has his eye on 'the problems which history has generated in the present' . . . the worse historian he is likely to be. . . . For he may not assume *a priori* that the major problems of the present were the major problems of the past, . . . he may, and often does, need to exercise his mind in thinking in concepts that . . . are alien to his and his contemporaries' habitual modes and moods of thinking. (Lovejoy in Klein 1972, p. 13)

In Klein's opinion, this criterion of what seemed important to earlier men, rather than what seems important to us now, is the only reliable one for the historian, and it is "incompatible with the selection principle invoked in choosing historical illustrations for a physics course" (Klein 1972, p. 13). Hence, the "result is almost inevitably bad history, in the sense that the student gets no idea of the problems that really concerned past physicists, the contexts within which they worked, or the arguments that did or did not convince their contemporaries to accept new ideas" (p. 13). In short, using an historical account for physics teaching means composing an historical fairy tale with a scientific or philosophical moral.

What sorts of things can go wrong in the writing of history when the physics teacher brings her concerns to the field?

Historical narratives where omissions and emphases act together to produce a scheme of history that "converges beautifully on the present—all demonstrating throughout the ages the workings of an obvious principle of progress" (Butterfield 1950 [1931], p. 12) is called *whiggish*.[1] Physics textbook history is often of this kind, and John Ziman (1994) thinks it hardly could be different when students are to acquire an overview of major events in the development of science: "It is almost impossible to present such a chronicle except as a celebration of scientific progress" (Ziman 1994, p. 27). While whiggish history need not be actually *wrong* in the sense of including mistaken statements of fact, Douglas Allchin points out that the resulting account, by ignoring context, casts historical actors as acting for anachronistic reasons, suppresses historical uncertainty, and fails to do justice to possible alternative trajectories of development. By erasing historical contingency, so that the present state of affairs seems inevitable, whiggish history legitimates authority (Allchin 2004, p. 182–83).

Often textbook writers in their historical sections commit not only sins of omission, but present as history accounts which could more accurately be referred to as "rational reconstructions". Andrew Whitaker calls such seemingly historical narratives where events are adapted so as to fit better with a "logical" development *quasi-history* (Whitaker 1979). He sees quasi-history as typically "a result of a rather misguided desire for order and logic, as a convenience in teaching and learning" (Whitaker 1979, p. 239). The bias is due to pedagogical, rather than philosophical or ideological, concerns. Insofar as the kind of history that is usually considered of greatest pedagogical merit is a history in which errors, subjective elements and social aspects are swept under the carpet, the result is often whiggish. Quasi-history "insists that there

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[1] This term is named for the Whig political party in Britain, which Butterfield charged with writing history so that it vindicated their rise to power (Butterfield 1950 [1931]).

is instant understanding and agreement" as soon as a discovery is announced, and can not "accept the idea that even leading scientists can make mistakes" (Whitaker 1979, pp. 239-40). To what extent such accounts where the history is "rewritten ... so that it fits in step by step with the physics" do in fact aid students in learning physics is one question. It may well be that such "rational reconstructions" can be useful pedagogical devices *provided it is pointed out from the outset that it is indeed a reconstruction* (Whitaker 1979, p. 109). Another question is what lessons are implied about the nature of scientific inquiry. Here quasi-history may instill in learners images of physics and physicists that are quite far off the mark.

Writing whiggish history or quasi-history involves some degree of *conceptual shoehorning.*[2] In setting out to write an historical account whose *purpose* is to illustrate a philosophy of science (or to clarify an area of physics, or to show how today's science is the consummation of centuries of scientific labour), there is every danger that the history will be adapted, relevant facts omitted, false or imaginary details added, and emphases misplaced (Allchin 2003, p. 324). Allchin supposes that "conceptual shoehorning may reflect how human brains function unchecked" (p. 324), and refers to psychological research that indicates that we more readily notice and emphasize evidence that confirms our preconceived notions than we do contradictory instances. On this background, the prospects for sound historiography for science teaching may seem bleak.

### 1.1.2 Cases of bad history in physics textbooks

It might be tempting to dismiss these arguments on the inevitability of the distortion of history as unduly purist in an educational context. It would have been easier to do so if confirming instances of "fabrication of history to suit not just pedagogical ends, but the ends of scientific ideology" did not, as a matter of unpleasant fact, "abound in textbooks" (Matthews 1994, p. 73). Klein complains that

> the historical sections in many physics texts at all levels too often make it shockingly clear that their authors do *not* extend a proper respect for their subject matter from physics to its history (Klein 1972, p. 13).

Two well-known instances of textbook-propagated quasi-history teach that "special relativity came from analysis of the Michelson-Morley experiment, and the photon from study of the photoelectric effect. Neither of these historical 'facts' is true, but they may be said to add support to philosophies according to which theories inevitably emerge fairly directly from empirical results" (Whitaker 2000, p. 321). And, contrary to another textbook myth,

> the motivation [for Maxwell's addition of the displacement term to the electromagnetic equations] was not to avoid the incompatibility of Ampère's law with the equation for continuity of charge. ... Neither was it to produce symmetry in the equations ... In fact Maxwell was engaged in the more prosaic task of attempting to calculate the elasticity of the electromagnetic ether. His initial theory was in fact electromechanical rather than electromagnetic. (Whitaker 1979, p. 110)

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[2] Allchin (2003) borrows this metaphor of 'shoehorning' from Stephen Jay Gould's *Wonderful Life* (1989). There Gould "coined the expression 'Walcott's shoehorn' to label a significant bias in the work of paleontologist Charles Walcott", a bias that led Walcott to an unwarranted classification of a trove of fossils (Allchin 2003, p. 315).

Jörg Meya (1990) identifies other instances of quasi-history in modern texts of electromagnetism: in a German undergraduate text Maxwell's ether is portrayed as a polarizable dielectric, that is, built of charges—a representation that renders the introduction of displacement currents plausible and continuous with modern views. However, Maxwell's actual model of the ether was purely mechanical, with 'charge' as a merely apparent phenomenon emerging from the states of mechanical tension of the medium (Meya 1990, p. 52).

Meya also finds whiggish history in the textbooks he has studied. In Halliday and Resnick's textbook of 1966, there is a sketch of the history of electromagnetism by way of introduction to the material.

> The reader gets the sense that Oersted, Faraday, Maxwell, Heaviside, Lorentz and Hertz, together with others whose names it apparently is considered admissible to leave out, contributed to the development of electrodynamics. ... The resulting impression is that since antiquity there has been only one (the classical) version of electrodynamics. ... There is no mention at all of the fact that there were rivaling theories, and only physicists that have contributed to modern conceptions are mentioned (Meya 1990, p. 53).

Thus students will get the impression that Ampère and Maxwell were working within the same paradigm, while historically

> [the law developed by Ampère] describes the centrally acting elecrodynamic force between two current elements; it belongs with the concept of electric distance forces and was one of the pillars of the action-at-a-distance theory that prevailed in the 19th century. ... Only in the case of stationary currents and after integration over a closed circuit does [the law Ampère formulated] describe the relations in agreement with the classical theory. (Meya 1990, p. 50)

In a similar way, Benjamin Franklin is mentioned as the originator of the terms 'positive' and 'negative' for electric charge—but without reference to his theory of the *one* electric fluid, of which an excess or deficiency was given the above labels. Again the effect is to portray Franklin as adding a completed building block to the *current* theory. At the same time the account fails to render intelligible Franklin's choice of labels, which in terms of today's 'two-fluid-theory' appears rather curious (Meya 1990, p. 51).

### 1.1.3 Objections to Klein's empiricist view of history

Before giving up the project of employing history for physics teaching entirely on the basis of the above criticism, it may be worthwile considering how far these warnings also apply to history in general. Is it really the case that the task of the historian is exclusively to "recapture the richness of the moments" and pile up "the concrete, the particular, the personal" (Butterfield 1950 [1931], p. 69)? If principles of selection external to these moments are not to be imposed on the material, is good historical scholarship at all possible?

Michael Matthews (1994) rejects the "account of history as seeking complexity and putting nothing aside" as "simply wrong: all historical writing has to be selective". The issue of empiricism in history has been a debate within the historical discipline itself, with empiricist, fact-finding accounts of history being "roundly criticized" by practitioners in the field (p. 78). Matthews declares that

> it is clearly ludicrous to think that everything that happened can be listed; each event is capable of being described in myriad ways . . . A historian is not an archivist . . . The historian is supposed to select, and further, to make something of the historical record. . . . The scientist does leave aside the color, texture and composition of a falling ball, and replaces all this richness with a simple point mass; historians also have to leave aside some of the richness of historical episodes and seek for some essentials which are pertinent to the story they wish to tell. (Matthews 1994, p. 78)

While selection is necessary, the question *which* selection criteria are legitimate of course is a difficult one.

### 1.1.4 Are the problems for history in physics education so different from those of general school history?

What then about general history—usually political history—as a school subject?

Even if the professional historian dealing with a limited era and area did encounter the unstructured abundance of historical data described by Butterfield, and could immerse herself in the material and understand it in terms of the studied persons' own categories, the *student* of history does little of all that. The high school or college history student confronts history textbooks which contain an extremely limited number of carefully selected historical data, structured along a story line, and sorted into characteristic periods or epochs of which the persons and peoples studied knew nothing. The student is hostage to the textbook writer's selection criteria, and these are often not innocent—and it is a non-trivial question whether they ever can be.

School textbooks are intended to have a formative effect on students' thinking, and the issue of what effect that should be is not easy in the case of history. A history of general histories for education would vividly illustrate the significance of the *goals* of the history lessons. Thus the Nazi and Soviet history textbooks tell us more about the educational policy under which they were written than about the events they purport to describe, and Chinese history textbooks typically have to be rewritten for every change of leadership in the country. One might ponder why history is so readily (ab-)used for indoctrination purposes, and suspect that it is due to an inherent plasticity, ambiguity and corruptibility of historical narrative—in which case one might be particularly reluctant to contaminate physics with it.

I am rather more inclined to explain this application of history to the central importance of history for our self-understanding, a factor that makes it a particularly attractive vehicle for ideological lessons. If we grant (as will be argued later in this text) that very different accounts of physical phenomena can be provided, all of which lead to mathematical formulations consonant with empirical evidence, that still does not, I think, render choice of physical theory nearly as likely to be subjected to political decree as the choice of historical account is. This is, as far as I can see, because scientific beliefs are thought to be less central than historical beliefs for the shaping of identity and opinion—and not because physical theories are all *that* much more absolute. There are facts of history too, facts that can be ascertained through rigorous investigations—and the many cases of shoddy historical presentations does not disprove this statement.

That does not mean that scientific beliefs are irrelevant for our self-understanding, or that they are treated as such. The distortion of history in physics textbooks can to some extent be seen to be due precisely to ideological stakes in physics education. Textbook writers may wish that physics students acquire a sense of participating in a quest for ever-increasing understanding and mastery of nature, perhaps a quest for one complete and completely proven theory of everything, in which all the physicist geniuses from Galileo to Hawking have taken part. The ideology sketched here may be a reason why textbook histories tend to emphasize questions of priority rather than providing plausible contexts for important developments in physics. Issues of the self-understanding of novice scientists are at play when textbook histories ignore mistakes made by great scientists, downplay or omit the role played by scientists other than a few almost divinely talented individuals, and pretend that great scientists whose work is not in a simple sense a part of currently accepted knowledge never existed.

Conveying some sort of romance of the discipline certainly need not be a disadvantage in education, but the dangers of bad history must be acknowledged and addressed. Want of time probably makes it impossible to provide historiographically responsible accounts of all, or most, of the topics treated in a typical physics course. Besides, several years of schooling would have to be added to science teachers' training in order to prepare them sufficiently for teaching good history of physics across the whole syllabus. On the other hand, such a scope of historical lessons may not be necessary in the classroom. I would think that a great deal of the benefits of using historical narratives for physics teaching can be had by treating just one or two topics in adequate detail, while taking traditional, systematic approaches to the majority of the topics. Also, it should be quite realistic , as far as I can see, to incorporate one or two case studies of decent depth and thoroughness into teachers' training, and this may be enough to provide important insights and understanding into the nature of physics—as well, perhaps, as a certain sensitivity to quasi-history and Whiggish history. Besides, it should be noted that historical *narrative*, with the pitfalls discussed above, are not the only form in which history can enrich physics education. More will be said later about the spectrum of applications of history.

### 1.1.5 The value of history's non-linearity for teaching

Klein argued that the circuitous ways in which scientific knowledge has developed is not relevant or interesting for the physics student, and that the detail and non-teleological nature of historians' accounts have no value for the learning of *science*. Several educators disagree, however, insisting that the concern with what seemed important to other men, rather than what seems important to us now (Klein 1972, p 13) is precisely what they want to transmit to their students, and that such learning can make better physicists. In commenting on Klein's lecture, Yehuda Elkana expresses the opinion that

> the "desire for simplicity" has been exaggerated; if you want the students to realize that not everything has been narrowed down to one or two basic problems, then it is just here that good history can provide the needed complexity. (In Brush and King 1972, p. 18)

Matthews makes a similar point in referring to "the mind-expanding purpose" of good history (Matthews 1994, p. 79). Elsewhere he cites Ernst Mach, who in his 1883 history of mechanics maintained that

> The historical investigation of the development of a science is most needful, lest the principles treasured up in it become a system of half-understood precepts,

> or worse, a system of *prejudices*. Historical investigation not only promotes the understanding of that which now is, but also brings new possibilities before us. (Mach 1883, in Matthews 1994, p. 193)

The idea is that authentic history promotes intellectual independence and encourages novel thinking—and precisely because it gives perspectives on physical phenomena that are *different* from those currently accepted.

Allchin advocates that the history in science lessons actually should *focus* on past errors and pseudoscience (Allchin 2004). This would not only counteract the whiggish and hagiographic tendencies of much science textbook history, but would also offer "an ideal opportunity for teaching nature of science" (Allchin 2004, p. 189). "[M]any of today's pseudosciences were yesteryear's sciences," he reminds us, and mentions Kepler's commitedness to astrology ("which indeed fostered many of his discoveries") and the alchemical work of Newton and Boyle. Studying the history of pseudoscience and scientific errors provides opportunity to focus on reasons and arguments rather than on facts to be accepted.

> A historical perspective highlights that many topics typically branded as pseudoscience are not *self-evidently* pseudoscience. ... History is valuable ... for showing students how they might *challenge* "the obvious". ... [T]he very understanding that something may appear reasonable until it is considered more deeply, is a powerful lesson worth offering to anyone. (Allchin 2004, p 191)

### 1.1.6 Lessons from history instead of lessons in history?

Gerald Holton expresses agreement with Klein's disapproval of letting pedagogical devices that help explain problems in physics pass as "history" (Brush and King 1972, p. 23). He does not, however, want to give up using such devices in teaching physics, but makes an appeal "for clear labelling of our product". He suggests that the teacher say "This is not history of science, but it is a *lesson* which I have learned by my study of the history of science", and in this way avoid intellectual dishonesty.

The issue of to what extent such a manner of speaking can render use of quasi-historical narratives defensible in education will not be pursued further here. It should be noted, however, that far from all proposed uses of history for physics teaching involve presenting historical *narrative* to the students. The history of science can be a resource for the teacher in myriad other ways, as a reservoir of experiments and arguments that can be fitted into lectures that do not explicitly include historical accounts. Such applications of history will be discussed in greater detail in chapter 2.

### 1.1.7 Implied history: Can the transmission of history be avoided?

Perhaps some impression of the history of physics is inevitably conveyed through physics teaching, even if the lectures do not address historical issues. Whitaker (1979) and Meya (1990) discuss this possibility. In particular, the nomenclature used may let comments that the textbook author did not intend to be historical *seem* so to the student (Whitaker 1979, p 108). For example,

> [saying that something or other "follows from Huyghens' principle" may] convey to the student completely the wrong impression of the connection between, for example, Huyghens' principle and its consequence. The student may infer that

> the consequence was obvious to Huyghens, or 'immediately obvious' to anybody of intelligence once Huyghens had produced his principle, whereas it was perhas the result of great amounts of labour later (as for the full wave theory of light). (Whitaker 1979, p. 108)

Meya (1990) argues that the names of 'Ampère's law' and 'Faraday's law of induction' similarly imply wrong relations between Ampère, Faraday, and Maxwell's equations (Meya 1990, p 50), as explained for the case of Ampère in section 1.1.2. The impressions gained by the student may be much the same as for explicit whiggish history and quasi-history, but may be "perhaps more insiduous because they are gained subconsciously rather than directly" (Whitaker 1979, p 108).

Perhaps, then, physics teaching by default transmits quasi-history, so that non-historical physics teaching is no way of avoiding the pitfalls of bad history.

## 1.2 Quality of physics when taught historically

The previous section dealt with how the concerns and standards that the physicist brings along to the study of historical material tend to produce bad history. The converse objection, that the historical approach might provide bad physics, will be discussed here.

### 1.2.1 Genesis versus justification

We may start with distinguishing between genesis and justification, between causes and reasons, between psychology and epistemology. It might, rather simplistically, be said that in accounting for developments in the discipline of physics, historians seek explanations in terms of factors that *caused* these developments, while physicists (often together with philosophers of science) may focus exclusively on the *reasons* in the sense of *justifications* for these developments. The physicist may consider a transition in physical theory or practice explained if the arguments given for that transition are sufficiently *good* in an epistemological sense, and concentrate on the reasoning involved. The historian does not overlook the reasoning involved in the transition, but is more concerned with how effective this reasoning just happened to be in shaping the thinking of the persons involved than with evaluating the epistemological merits of the arguments. Thus, the historian may focus not only on the explicit reasoning, but also on available skills and technologies of the time, prejudices and psychological quirks of the persons involved, origins of the relevant ideas (whether in arguments or dreams or mystical revelations), rhetorical skills of the protagonists of the new point of view, geographical spread of the awareness of the new ideas, socioeconomic and ideological factors in society at large—in short, on the whole tapestry of matters that could influence the physicists' scientific beliefs.

Even in a purely *internalist* history, which is a historical reconstruction "based exclusively on published scientific papers" (Miller 1987), the historical, descriptive account of a transition in physics may often not coincide with a good justification of the transition. In an internalist history "the final product is devoid of a discussion of the scientists themselves, that is, of their struggles to create new ideas, how their work was received and interpreted, the philosophical-cultural context in which they worked and lived" (Miller 1987). Yet even in such histories (which are the kind that physicists prefer to write) the reasoning often will not be in terms of what we today would find the most cogent arguments. Hence, of course, the temptation of writing a reconstructed history, in which the reasoning of the characters involved follows more closely what in the light of hindsight looks like a rational development of thought. This is just what Klein's warnings are about. In real history the succession of ideas is a lot more messy than a systematic account of the transition would suggest. "Scientific knowledge, while seemingly a story of success when painted by broad strokes, is, when examined more carefully, a story of many false starts and misdirections" (Duschl, Hamilton and Grandy 1992, p 36).

The differences discussed by Duschl and coworkers (1992), between the concerns of cognitive psychologists on the one hand and epistemologists on the other, are closely analogous to the ones between explanations sought respectively by historians and physicists:

> epistemology is concerned with the rational evaluation of theory choice regardless of the actual historical developments or of human frailties and the causal structures that may produce them. Epistemology is concerned with *knowledge*, which must be true and justified. Cognitive psychology [and, perhaps, history of science], on the other hand, is concerned with understanding the nature, causes, and dynamics of internal representations of conceptual structures and has no concern with rationality, truth, or justification. (Duschl et al. 1992, p 28–29)

### 1.2.2 Does an historical presentation understate the rationality of physics?

Many educators believe that there are important analogies between the conceptual restructuring students undergo in the process of learning science and the restructuring of scientific theory itself in history. More will be said about this later, in section 2.2.1. For now it is enough to mention that such parallelisms are one reason why historical accounts may be thought to facilitate the development of the appropriate concepts in students. However, Duschl and coworkers (1992) see a possible tension between this approach and an approach concentrating on what from a modern point of view constitutes a set of sufficient *reasons* for a conceptual structure or a theory: "The . . . teacher may have to choose whether to emphasize rationality or effectiveness in changing concepts" (Duschl et al. 1992, p 29). Does teaching current theory through historical accounts mean that students appropriate the theory *for the wrong reasons*, on a basis less than rational?

Duschl et al. (1992) do recommend judicious use of history in science teaching, but the question remains whether accounts other than systematic, logically constructed accounts fail to do justice to the certainty and coherence of physical knowledge. Walter Jung's view, as cited in Höttecke (2001), is that the historical process relates to the production of knowledge like "a ladder that one can burn once one has ascended" (p. 155). According to Walter Jung,

> [the physical sciences display] a tendency toward autonomization which is constitutive of these sciences. This applies not only to the reorganisation of the results, that no longer have anything to do with the historical progression, but also to the reorganization of the genesis, that follows the internal logic of the fully developed discipline: The end product organizes itself rationally; it provides its own foundations, phenomena, procedures, inferential connections . . . The now canonical theory constructs its own genesis and through this becomes learnable without recourse to history being necessary (cited in Höttecke (2001), p. 155).

Jung's conclusion may not be a general warning against historical approaches in physics teaching, but this excerpt does provide some arguments against such approaches. The main objection is *not* that the historical approach may happen to be confusing rather than clarifying, as may indeed often be the case. Certainly the history of science can be bewilderingly complex (Ziman 1994, p 26). In the classroom the teacher may be "faced with the difficult task of demonstrating that the growth of knowledge in science, with its periods of consensus and dissention, is, nonetheless, a rational activity" (Duschl et al. 1992, p 37). The detail and empathy required in order to render the process as rational may be beyond what can realistically be achieved in the classroom in many cases. However, the issue here is another, namely whether *successful* historical teaching is ever justifiable as long as current argumentation is better (in an epistemological sense) than the reasoning that originally brought about the physical theories or concepts that are to be taught. The physicist, after all, is to accept Maxwell's equations because of their power to explain and predict phenomena, not because they came out of Maxwell's mechanical model of the aether, and this applies even if knowledge of the latter should clarify the student's ideas about the equations.

### 1.2.3 How historical is physics?

One possible response to the above criticism, that historical approaches emphasize non-rational causes rather than good reasons, is to deny that physics is ahistorical—that is, to deny that it *can* be fully accounted for in terms of "good reasons" in an epistemological sense. In an *historicist* view of physics, the discipline is seen as historically contingent in a strong sense, a product of a specific train of events that might well have been different, and which would likely have resulted in different physical concepts, theories and experimental practices if it had been.

Dietmar Höttecke (2001) disagrees with Jung's description of physics as ahistorical, and sees no reason to believe that physical knowledge can break lose from its past, so that it could be meaningfully understood without reference to its genesis. He points to the wealth of research in history, philosophy and psychology of learning that demonstrates that "learning, thinking and observation invariably take place against an individual, social and historical background *that prestructures these acts*" (Höttecke 2001, p. 151). Historical contingency thus enters the generation of knowledge at many levels, and remains part of the accepted knowledge at any time. Different sequences of events would have been possible that would have resulted in different concepts and theories. Hence physics is not really intelligible without an understanding of its history.

### 1.2.4 How different is a genetic account from a justification? Naturalistic epistemology

Another possible route might be to deny that an account of the genesis of a physical theory is so fundamentally different from a justification of it. This might perhaps harmonize with some version of a "naturalistic" epistemology, attempted developed by the philosopher Willard V. O. Quine, among others. Quine might be understood as proposing that we "simply study how we do go about moving from our data to the formation of belief" (Dancy 1985, p. 235):

> This factual study, squarely within the bounds of psychology, is what [Quine] calls *naturalized epistemology.* It leaves aside questions of justification and considers only the genetic, causal questions. We cease to worry about the gap between evidence and theory, and study instead the causal relations between the two (Dancy 1985, p 235).

This would perhaps seem to allow that an historical account of physical knowledge is as good a justification as any we could demand.

At first glance (and second, and third) it seems absurd to call this "epistemology" at all, as the characteristically *normative* questions of criteria for knowledge and valid argument appear to be just left out. Dancy explains that "Quine's suggestion here only begins to make sense within his more general approach"—but Quine's general approach certainly is beyond the scope of this thesis. An inkling of how naturalistic epistemology could still be an epistemology may perhaps be had from Richard Kitchener's discussion of Piaget's *genetic epistemology*—"a kind of (biological) neo-Kantianism" (Kitchener 1992, p. 122). He introduces the concept of "epistemic competence", which is both a psychological trait *and* has to do with satisfying certain normative criteria. *Knowing-that* is somehow traced back to *knowing-how*, and the latter might conceivably be more amenable to naturalistic treatment, while still having epistemological aspects. However, no attempt will be made here at providing an account of how our questions about rational arguments in physics teaching could be cast within a naturalistic epistemology. The problem of whether historical accounts in physics teaching fail to do justice to the rationality of physical knowledge will hence have to be done in traditional epistemological terms.

### 1.2.5 Should the standards of research be applied to education?

Perhaps this strict emphasis on sufficient reasons, even to the point of compromising intelligibility, is misplaced in the context of education. After all, the students must acquire, or construct, entirely new physics concepts before they can sensibly reason with these concepts. There is good reason to believe that students do *not* manage to get clear ideas of the physical concepts to be learnt from mere definitions of these concepts, for all the logical adequacy of these definitions (Nersessian 1989, p. 179). "[T]he logic of a subject [is] not necessarily the logic of its presentation—a point known to most schoolteachers" (Matthews 1994, p. 99).

Perhaps students' learning of new concepts and relations between them may be considered analogous to scientists' activities in *'the context of discovery'*, which in the philosophy of science is commonly distinguished from *'the context of justification'* by its "depending on subjective factors and chance" (Baune 1991, p 55). By such an account of science, it is the success of an idea or hypothesis in the 'context of justification' alone that allows it to pass as scientific. No rational account of its origins can be given—and no such account is needed. The generation of novel ideas and hypotheses, through whatever obscure processes, is nevertheless necessary for the development of science. Allowing for an analogy between learning science and doing science research may allow us to say something similar about the processes by which students grasp ideas in physics for the first time: Demanding strict, logical reasoning is misplaced in this phase which belongs to what corresponds to the 'context of discovery'. Any kind of cueing that will result in the right representations in the students' minds are acceptable—including historical accounts of change in science. Justification and argument must, of course, follow up on this 'discovery' part.

This argument may suffice to counter the claim that learning physics through history involves downplaying the rationality of physics. However, the whole notion that the 'context of discovery' can be so neatly separated from justification, and that the origination of scientific notions falls outside the domain of rationality, may well be questioned. The following section is concerned with some brief remarks to this effect.

### 1.2.6 Broadening the notion of rationality

Nancy Nersessian (1989) sees the processes of learning science and doing science research as significantly similar (see section 2.2.1.) From examinations of historical cases of scientists' reasoning toward major shifts in scientific theory, she further concludes that rationality and scientific reasoning are more complex and varied than the traditional account would suggest. Arguments to the effect that historical studies are objectionable for physics teaching because genetic accounts may not provide the currently most cogent arguments for the physics rely on unreasonably limited ideas of what rationality is (Nersessian 1992).

Nersessian does not accept the traditional dismissal of the 'context of discovery' as practically external to science as such. Scientific discovery, rather than being more or less instantaneous and resulting from "genius flashes of insight", often follows "periods of intense and often arduous thinking and, in some cases, experiment" (Nersessian 1992, p. 52). Excluding these efforts from the realm of science, and calling them 'non-rational', means applying too narrow a conception of scientific theory and of rationality. Theory is then—erroneously, according to Nersessian—seen as being completely described by a set of equations and definitions, and scientific reasoning as strictly deductive (Nersessian 1995, pp. 221–222). Instead, scientific discovery would be better described as a 'process of invention' (Nersessian 1992, p. 52), and in this process scientists follow strategies and employ techniques that students might benefit from learning about. These strategies are rational even though they involve much more than strictly logical inference:

> Concept formation in science requires such procedures as analogy, idealizations such as limiting case analysis and thought experiment, and the use of imagistic representations. These are heuristic procedures, which while not algorithmic, are systematic and their use can be evaluated (Nersessian 1989, p. 179).

More will be said about these processes and their value in education in section 2.2.1.

## 1.3 Preaching to the wrong audience?

Two final misgivings about history in physics teaching are concerned with psychological, rather than philosophical, issues. One question is whether physics students as a group are selected for a certain impatience with, and lack of interest in, the historical mode of inquiry. Another is whether confidence in the linear growth and certainty of physical knowledge is necessary for maintaining the motivation and persistence required in order to do well in physics—for such faith may conceivably be eroded by historical studies. Harold Burstyn, in a response to Klein's lecture (see section 1.1.1), expresses both concerns when he contends that

> there is a lot of evidence ... that science students and students of other subjects have different outlooks on the world. To phrase it pejoratively, the science students are looking for the "right" answers, they are "convergent" rather than "divergent" thinkers. ... Can you in fact use the historical materials, whose hallmark is their complexity, their diffuseness and their imprecision, in the teaching of people who are interested in getting the right answers, and who, if they are successful, can't be diverted from this quest as we historians might want to divert them? Isn't history therefore somewhat subversive to the aims of physics pedagogy? (In Brush and King 1972, p. 26).

### 1.3.1 Scaring off the last physicists

Also voicing the concern that historical approaches may not speak to physics students' preferences, Walter Jung asks:

> Is it not the case that precisely those who are interested in natural science are not inclined toward the hermeneutical; that they flee the humanities, because they desire unique and definite answers? Would we not thereby drive away the last students interested in physics or chemistry? (Quoted in Höttecke 2001, pp. 158–9).

In response to this worry it may be timely first to note that it should not be answered independently of the question of whether definite and timeless answers are in fact to be had in physics. If historical and other evidence indicates that physical knowledge is subject to rebuttal and change, meeting students' putative demand for certainty might entail that "a mythical understanding of natural science is advanced". This may not be done even if it should satisfy some students' needs (Höttecke 2001, p. 157).

It may also be argued that it is the systematic, disciplinary approach common in physics teaching that "is one-sidedly oriented toward the interests of a minority" (Höttecke 2001, p. 157). At least at the early levels of specialization, taking students' interests and inclinations into consideration would likely mean adopting a broader, more contextual approach in physics education.

Finally, the charge that historical components will repel physics students is an empirical one, and much research suggests that it is wrong (Matthews 1994, p. 80). At any rate, the variety of ways in which history may be employed in the context of physics education speaks against a simple appeal to student interest or absence of such. The multitude of historically inspired approaches (section 2) would have to be addressed one by one before such a broad conclusion could be reached.

### 1.3.2 "Sapping the neophyte scientific spirit"

Then to the issue of confidence in the current paradigm as a prerequisite for successful learning and application of physics. Thomas Kuhn, in his *The Structure of Scientific Revolutions*, has argued that the natural sciences could not have achieved their present state or status if their practitioners were not thoroughly steeped in the orthodox thought of their discipline. Science students are not, and should not be, encouraged to read historical classics of their field, "works in which they might discover other ways of regarding the problems discussed in their textbooks", because such reading might corrode the apprentice scientists' sense of being part of a successful truth-seeking tradition. There is good reason to subject the students to a rigorous training in convergent thinking, and to shield them from alternative approaches (recounted in Matthews 1994, p. 76). Non-orthodox influences "sapped the neophyte scientific spirit" (Matthews 1994, p. 75).

To me this sounds like an unapologetic defence of narrow-minded traditionalism in education, and it appears furthermore unconvincing and contrived. I am inclined to just regard the burden of proof as resting with Kuhn in this case. Why should students lose enthusiasm for, or sense of belonging to, their discipline by learning that it is not monolithic and ahistorical? "The experience of Einstein, when given Mach's *The Science of Mechanics* by his friend Besso, might be more typical: exposure to history enlivened Einstein's commitment to science" (Matthews 1994, p. 80). Apart from the empirical fact of how students do act emotionally to such learning there is, again, the ethical point of 'historical truthfulness' on the part of the teacher, and as Matthews remarks, this "merges very quickly into the issue of indoctrination in education" (Matthews 1994, p. 81).

# Chapter 2

# Applications of History in Physics Courses

There is no consensus as to the purposes of using history for physics teaching, and the suggested ways of utilizing the historical material are likewise diverse. This section is to present an array of means and purposes of using history for physics education, without going into much detail. In a later chapter ways in which history can deepen understanding of physics will be explored more thoroughly.

The aims of education encompass the acquisition of knowledge and skills, and also the development of certain *attitudes and emotional dispositions.* Some attitudes toward science may be valued for making learning *science* more successful, but emotional inclinations may be taken to be goals in their own right (Sjøberg 1998, p. 348 ff). A lasting, critical *interest* in science may, for example, be quite as important an outcome of a science course as knowledge of the specific course content. Educators have argued for the value of history on more than one level, sometimes emphasizing how historical material might awaken students' interest in science, thus making their learning of science more effective, other times hightlighting the historical knowledge itself as important, and still other times stressing ways in which historical approaches may clarify *physics* concepts to be learnt.

As for the possible ways of utilizing historical material, some advocate an 'add-on' approach where one or several units on the history of science are added on to a standard, non-historical science course—while others argue for an integrated approach where a teaching unit will "cover not just equations and practical work, but how these equations were developed and how the concepts embodied in them were formed and changed" (Matthews 1994, p 70). Some do not think historical materials as such should be used in the classroom at all, but that historical knowledge should inform the teacher's approach.

## 2.1 Contexts for physics

### 2.1.1 Physics as cultural heritage

A quite uncontroversial point in favor of teaching some history of physics would follow the arguments given for any history education in schools. A certain acquaintance with our past is an essential part of our identity and a prerequisite for navigation in the present. A number of events in the history of physics, such as Galileo's debates with the Church about his astronomical observations, Newton's synthesis of terrestrial and celestial laws of motion, and the fissioning of the atomic nucleus by Hahn and Meitner, deserve a place in our cultural history in much the same way as the rise and fall of the Roman Empire, the introduction of constitutional rule, or the catastophes of the two world wars would. These events have all contributed to establishing the current distribution of power in the world and have affected our self-perception and ways of thinking. Further, a number of historical issues treated in standard history courses are incomplete without elements of history of physics and technology. This applies, for instance, to the treatment of the origins of Western civilization, the shifting power of the Church, the Industrial Revolution, the Cold War with its balance of nuclear power. In short, the history of physics is the history of one of the greatest achievements of Western civilization and of a powerful factor in shaping the ideas, economics and conditions of life in our world.

While it seems difficult to object to the historical and cultural importance of such events, "the history of science has fallen between academic stools" (Matthews 1994, p. 43). History departments at schools and even universities have usually avoided dealing with it because it has been thought too technical or difficult—and science departments have not dealt with it because it is thought irrelevant. Probably most teachers would agree that it would be nice if students were offered lessons on the history of science—the point of controversy would be whose lessons should be devoted to these issues. While Matthews points out that bringing history and philosophy of science into *science* programs can in part rectify this situation (Matthews 1994, p. 43), many physics teachers would no doubt object to the introduction of further material into already overstuffed physics curricula. Unless the curricula are changed, or the number of hours alotted to physics increased, other arguments would be needed in order to convince physics teachers that spending scarce classroom time on history might be worthwile.

### 2.1.2 'The nature of physics'

A more compelling argument for the inclusion of contextual material—though not necessarily historical material—is that traditional physics courses not only *omit* important aspects of the discipline, but distort and misrepresent them. Any physics course inevitably communicates some notion about what sort of thing or activity physics *is*. A course that exclusively communicates well-established and readily computable results in physics also implies certain assumptions about the scope, certainty and methodology of the discipline. Such a course may transmit an image of physics according to which it is more of an axiomatic, less of an empirical science. It may portray physics as primarily a body of indubitable facts, whose character in no way depends on the persons who developed the science, or on the social, economic and cultural conditions under which it came to be. Such a representation of physics would be in conflict with the results of several decades of research and debate in the history, sociology and philosophy of science.

Effects of the traditional approaches in physics education may be seen in students' notions of the character of physical knowledge and research. Höttecke (2001) recounts results from nine investigations by researchers who have studied students' conceptions of the nature of science. The various reports are not unanimous in their conclusions, reflecting differences in, among other things, age groups selected, countries in which the studies were conducted, and, maybe in particular, formulations of the questions posed to the students. According to Höttecke, "the majority of the research results of different studies of students' preconceptions of natural science show that students show a tendency toward naïve realism" (Höttecke 2001, p. 54) He refers to a comprehensive study of German students by Meyling[1] (1990) which concluded that half of the grade 12 students considered natural laws to be indubitable images ("*Abbilder*") of regularities observed in nature. Along the same lines, Carey et al.[2] are cited as having found that American grade 7 students typically think of scientific knowledge as a "copy" of reality. Although other studies described by Höttecke suggest that a greater proportion of high school students regard science as temporary and subject to change, it is clear that a conception of science as a rigid, timeless structure of correct answers is very common among teenagers taking science classes.

If students are to get an understanding of *physics as a process* (Nersessian 1992) rather than as a system of facts, it may be necessary to study at least some cases of physical knowledge in the process of being made—that is, historical cases of research activity. Myths about physical knowledge as well as about the people doing physics may be countered that way. Students may come to realize the tentative and transitory character of physical knowledge, its susceptibility to re-interpretation or even rejection. Historical case studies may counteract the image of *scientists* furthered by textbooks, with their small boxed histories of geniuses, so that students may appreciate the hard, persistent work behind advances in physical knowledge, and the cooperative efforts frequently required. Aspects of experimentation may become visible that disappear in the caricatured textbook histories emphasizing the progress of ideas—thus the role of practical skills, together with limitations set by available materials, technologies and financial resources can be given attention. Höttecke (2001), in his Ph.D. thesis on understanding the nature of science through history (and understanding science as historical), discusses the issues mentioned in this paragraph with care and in detail.

The 'nature of physics' is not static, however. The differences may perhaps exceed the similarities when we compare, say, the character of Faraday's private researches in his early 19th century laboratory with the activities at CERN, NASA or for that matter the Manhattan Project (Höttecke 2001, p. 216–17, footnote 54). In order to understand today's Big Science, or technoscience, with its frequently multinational organization, thousands of employees, astronomical budgets, and sometimes secrecy and restrictions on publication, it is necessary to choose modern case studies. Kolstø (2001) addresses this issue in stressing the importance of a *balance between historical and contemporary case studies* in courses where the nature of science is an issue:

> One approach could be to use historical studies to gain a perspective on modern industrialised and socialised science. But if case studies on contemporary science are expelled from a curriculum, my advice would be not to include historical studies either. (Kolstø 2001, p. 11)

## 2.2 Clarifying physics concepts

The previous section dealt with how history of physics can place physics in perspective and broaden students' knowledge of their discipline. Another question is whether and how history

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[1] Meyling, H.: 1990, *Wissenschaftstheorie im Physikunterricht der gymnasiale Oberstufe.* PhD thesis, University of Bremen

[2] Carey, S., Evans, R., Honda, M., Jay, E., Unger, C.: 1989, 'An Experiment is When You Try It and See If It Works': a Study of Grade 7 Students' Understanding of the Construction of Scientific Knowledge, *International Journal of Science Education*, 11, 514–529

may aid the learning of the actual *physics* concepts, and may facilitate the attainment of traditional goals of physics education: the ability to remember key ideas in physics, solve problems, and apply physical theory to new situations. First some ways in which historical material may help make traditional course content clearer to the students are discussed. After that two case studies of pedagogical lessons drawn from the history of electromagnetism are recounted. There is a great number of other such case studies, but hopefully these two may exemplify the range and variety of ways in which historical studies may help clarify concepts.

### 2.2.1 Learning physics, discovering physics

A teacher soon finds out that clear presentations, at least if 'clarity' is taken to mean logical sufficiency and coherence, just is not enough to elicit comprehension. There must in addition be some continuity between the material to be learnt on the one hand, and what the learner already knows and masters on the other. This is one statement of a minimal constructivist theory of learning, which is more or less universally accepted among educators.

The insight that learning must start where the learner is, so to speak, directs the teacher's attention to the students' ideas and ways of thinking before instruction, and this raises the question of how students' preconceptions may be identified. Only empirical studies can decide what kind of trouble students are actually having with learning physics. However, as it turns out, an acquaintance with the history of physics aids the generation of useful hypotheses as to what the problems might be.

Once students' preconceptions are identified, the question is how these may be addessed. Again, the movement from these preconceptions to the conceptions and modes of reasoning of physics does not generally follow the shortest path suggested by logic. The route is also determined by quirks of the learner's psychology, whether largely shared by other humans beings or more idiosyncratic. Historical studies may contribute to identifying travelable routes to the desired understanding.

#### 2.2.1.1 "Recapitulation of content"

In many areas of physics, students' ideas of physical phenomena prior to instruction are seen to be remarkably analogous to beliefs held by pioneers in the field—there is a 'recapitulation of content', in Nancy Nersessian's terms (Nersessian 1989, p. 164 ff). For example, when students seem to think of motion as process requiring the continual exertion of force, of heat as a substance to be stored or transferred, of current as the mechanical motion of charged matter, or of vision as a projective activity, they are beginning where physical theory itself did.

This is perhaps not surprising for phenomena with which students and early scientists will have had fairly similar everyday experience, such as falling objects, projectile motion, and heat transfer. Such recapitulation may also to some extent be anticipated where everyday language common to modern students and early physicists conveys assumptions about the nature of the phenomena. Thus, the word 'inertia', which in everyday language refers to some sort of reluctance to move or act, almost predictably carries with it Aristotelian ideas about motion and force. The term 'current', adopted from the description of moving water, suggests a purely convective process. Talk of keeping windows closed to 'keep out the cold' suggests that cold—and heat—are substances that can be transferred or stored. These meanings all identify misconceptions demonstrated by physics students today and physicist pioneers alike.

While knowledge of the historical development of physical concepts and theories can improve the educator's sensitivity to students' initial ideas, the limitations of this approach must certainly be appreciated. "Historical representations are not simply 'generalizations from experience' ... and neither are student representations" (Nersessian 1989, p 164). Hence, different people at different times must be expected to have divergent conceptualizations based on similar experiences. Nersessian emphasizes that

> any analogy [between historical prescientific ideas and modern preinstructional ideas] will break down at the point where metaphysical, sociological, and technological considerations have bearing on the representational content. Just how much recapitulation of content there is can only be discerned by in-depth, domain-by-domain investigation. (Nersessian 1989, p. 165)

Of course, with respect to many physical phenomena, neither the experiences nor the everyday language terms familiar to students today have any counterpart in history. Electric lighting, radio, television and computers are now taken-for-granted parts of novice physicists' experiential world, and terms like 'short-circuit', 'printed circuit boards' and 'electrons' are part of their everyday vocabulary. There is no reason to expect their initial ideas on electricity to be reminiscent of those of the first electricians. Nevertheless, as some of the case studies mentioned below show, the history of electromagnetic theory can in fact shed light on some of the problems students have with learning this material.

#### 2.2.1.2 "Recapitulation of process"

Jenaro Guisasola and coworkers (Guisasola, Zubimendi, Almudí and Ceberio 2002) introduce a research report on students' ideas of capacitance by stating that

> Because the concept of electric capacitance is a school concept framed within the theory of electricity, and therefore quite far from the students' spontaneous ideas, the historical perspective has been decisive for directing our research about the comprehension and use of this concept by students in physics classes. (Guisasola et al. 2002, p. 249)

This may at first seem to contradict what was suggested in the previous section—that a recapitulation of historical ideas was most likely in cases where students' ideas were rooted in their everyday, immediate world, as the early physicists' ideas were. However, this report may be seen as being about what Nersessian calls a 'recapitulation of *process*' (Nersessian 1989, p. 165). The emphasis is on the stages students have to go through in appropriating the concept of capacitance. Like the early electricians, students have to move from discussing the physical situation in terms of charges and charge densities, and learn to analyze it in terms of electric potential energy. In that process misunderstandings arise that are clearly reminiscent of conceptions held by physicists in the 18th and much of the 19th century (Guisasola et al. 2002, p. 250 e.g.)

Nersessian (1989 and 1995) describes many kinds of lessons about the process of learning physics that may be had from studying history. Some of her conclusions may be briefly listed here:

- In all major conceptual changes in science, *whole complexes of concepts* have changed. In teaching a scientific conceptual structure, a number of concepts need to be targeted for revision *at the same time*. In learning Newtonian mechanics, for example, new notions of 'force' and 'motion' must be introduced in a coordinated manner.

- The history of theory change in science is largely the history of changes in ontology, about what kinds of entities the theory claims to be about. Such changes are difficult, as can be evidenced by studying students' problems as well as the historical development.
- The learning of the new conceptual structure is complicated by the fact that often the same word is used in the old and the new structure, but with significantly altered meaning. This makes the conceptual changes required less visible than they might otherwise have been. (Many historical physics texts make no sense until the reader has realized that certain terms just had a different meaning at the time the texts were written.)
- Studying the notes and early theory sketches of historical experts in physics may provide insight into *tacit reasoning processes* leading to the construction of the appropriate conceptions. Making such modes of thinking explicit may have considerable pedagogical value.
- Such historical studies show that the skill of "constructive modelling" is central in conceptual restructuring. In this reasoning process analogical and visual modelling and thought experimentation play a crucial role. Coming to understand physics is not a matter of learning the definitions of physical terms, but of developing models that represent physical problems, and mentally altering, adapting and experimenting with these models.

#### 2.2.1.3 A rhetoric of discovery

Nersessian emphasizes the similarities between the processes students and pioneers in physics go through, and argues that

> conceptual change in science is a learning process for scientists as well. An individual scientist or group must learn how to construct a particular kind of representation of a domain. (Nersessian 1989, p. 165)

The characterization of this process as one of "discovery" is, furthermore, misleading; since "scientific representations are constructed—they are made, not stumbled upon or found" (p 178) it would be more accurate to talk about 'invention'. Students learning a scientific representation must re-construct, or re-invent, the new conceptions, and the accounts given by the inventors themselves may provide useful instructions for this process:

> When the scientists that have constructed the new representations attempt to communicate them to the scientific community, they often employ the very same constructive procedures to help their colleagues learn the new framework. This is, itself, a form of instruction, and we may have much to learn from how it has been—and is—done by scientists. (Nersessian 1992, p. 65)

What distinguishes the original accounts is their use of *discovery argumentation*, in contrast to traditional textbooks' use of *justificatory argumentation* (Nersessian 1989, p 179). The latter "present the student with reconstructed arguments that establish the correctness of the representation. Such arguments are useful when the conceptual structure of a science has been learned and we want to show why, e.g., a particular law holds. But what students need to do initially is to learn the concepts". Nersessian doubts that definitions of scientific concepts are adequate to give students an understanding of what they represent, and advocates a more extensive use of "analogy, idealizations such as limiting case analysis and thought experiment, and the use of imagistic representations. These are heuristic procedures, which while not algorithmic, are systematic and their use can be evaluated" (Nersessian 1989, p 179). An example of the use of 'discovery argumentation' is from the history of mechanics:

> Galileo's endeavours to persuade others of his new theory of motion can be considered attempts at instruction. In instruction he employs some of the same methods he used in construction. He begins by putting forth the position of his opponents, then exposes the difficulties in the position, and finally leads the reader through the construction of a new representation of the situation under discussion. He uses both idealisations and experimental evidence to create conflict with their a priori expectations and then employs idealization techniques and analogical arguments to help the reader to develop the representation. Significantly, he does not simply present the premises and conclusions of his arguments but tries to get others to construct the representation. In so doing, his 'students' are helped to produce the representation in an integrated way that facilitates their use of it. (Nersessian 1989, p. 176)

Apart from what they may reveal about the thinking skills required in order to understand physics, original papers may have value as they are, as good pedagogical texts. To the extent that pioneer physicists effectively use 'discovery rhetoric' in their works, these original texts may well in a number of cases be more accessible than modern textbooks, where the historical path toward the version presented is usually left out.

### 2.2.2 Dissecting misconceptions: two cases

The following sections summarize some results from two applications of history for the study of student preconceptions in electromagnetism. There are very many others, dealing with, for example, electric fields, the nature of current, and the concept of electric potential. The two studies here have in common that application of the found insights in no way requires historical narration in the classroom—the lessons may be for the teacher only.

#### 2.2.2.1 From electrostatics to electrokinetics

Students frequently have a lot of trouble with understanding electric circuits, and Benseghir and Closset (1996) suggest that many of the typical difficulties students have are due to a carry-over of ideas and approaches from electrostatics to electrokinetics. Most modern physics courses deal with electostatics *first*, and students tend to try to understand circuits much in terms of the elecrostatics framework that they have learnt. This leads to misconceptions that are closely analogous to ones entertained by pioneer electricians, who also initially set out exploring electric circuits with ideas and instruments invented for the study of static electricity. Benseghir and Closset first provide an overview of the historical reception of Volta's ideas, emphasizing evidence of 'electrostatic' thinking in the approaches and research reports of then-time electricians. Then they report their findings from a study of the electrical ideas of several groups of students—some in high school, some in the first and second years of university education in science or engineering. Interesting parallelisms emerge.

From about 1800 and until at least 1820, researchers "stubbornly attempted to produce chemical and magnetic effects from the individual ends of batteries in open circuits" (Benseghir and Closset 1996, p. 182). Physicists experimented diligently with isolated batteries, trying to electrolyze water by immersing only one end of a battery into the liquid, or by using opposite poles of two different, isolated batteries. They investigated how electrometers should be applied to galvanic batteries, concluding that a condition of success of such measurements was the complete isolation of the battery. The electricians were familiar with the apparantly instantanous discharges of Leyden jars, and thought of electric currents as successive discharges of this kind (Benseghir and Closset 1996, pp. 182–183). This notion of electric current "makes the distinction between an open circuit and a closed circuit irrelevant and disguises the idea of complete circulation, especially inside generators" (p. 190).

The questionnaire Benseghir and Closset prepared for their student subjects presented several simple setups of batteries and lightbulbs, including arrangements that closely mirrored historical experiments from the early stages of galvanism. In the first problem the students are asked to determine whether a lightbulb connected to opposite poles of two different batteries would light up. The circuit is *not* closed; yet more than a third of the students responded that the bulb would glow. Their explanations, as well as their responses to further problems, echo ideas of early 19th century physics. They emphasize the accumulation of opposite charges on the poles of an isolated battery, as if this electostatic agent were responsible for a steady current through the bulb. They lack a grasp of conceptual distinctions between the sudden discharge of static electricity and a steady current powered by an enduring electromotive mechanism—the interior of the generator is generally ignored (Benseghir and Closset 1996, pp. 183–86).

In another problem the students are presented with a *closed* circuit consisting of two batteries and a light bulb in series, where the light bulb is connected between the *positive* poles of the batteries. Although the batteries have different voltages, a majority of the students do not think any current will flow. They argue that since both battery surfaces in contact with the light bulb carry charge of the same sign, no current can flow. Again the emphasis is on electrostatic attraction and repulsion, not on differences in potential. Also, whether the circuit is open or closed seems to be accorded little importance. Attention is given to whether 'positive' or 'negative' poles are connected, irrespective of whether the circuit is open or closed (Benseghir and Closset 1996, pp. 186–88).

Benseghir and Closset (1996) identify other interesting parallels between the historical development of electrokinetics and student's ideas—the confusion of electric 'tension', or voltage, with charge density is one more example. Their historical study allows them to diagnose a range of student difficulties with understanding electric circuits. In student responses as in historical research reports, there is much evidence of electrostatic ideas in the misconceptions about galvanic phenomena.

#### 2.2.2.2 Electrical resistance

Viard and Khantine-Langlois (2001) discuss how knowledge of the history of the concept of resistance may suggest a more effective approach to teaching that concept. Using the philosopher Cassirer's distinction between 'thing-concepts' and 'relation-concepts', they argue that while the concept of electric resistance *historically* developed as a relational concept, many physics students have a thing-concept of resistance. This, they argue, accounts for a number of misunderstandings students have about circuit theory.

In Cassirer's theory of concepts, as recounted by Viard and Khantine-Langlois, 'thing-concepts' more or less have the character of *sets of properties* of objects, singling out some of the objects from the multitude of things around us. Thus the concept of a tree identifies birches, oaks and beeches as objects having certain common properties, namely those of 'trees'. 'Relation-concepts', in contrast, do not pick out objects by their properties, but rather identify characteristic *relationships between the properties of the object.* Mathematical concepts are typically relational, as are many scientific concepts. The concept of a triangle, for example, can not be constructed by our abstracting the common properties of the drawings of triangles we have seen, so that we can identify further triangles by seeing that they do possess these properties. Rather, "what is specific to a geometrical figure is not the set of its spatial properties themselves, but the relationships between the properties, for example not the absolute magnitude of the parts, but the relationships between these parts" (Viard and Khantine-Langlois 2001, pp. 272–4).

Many physics students even at university level have serious trouble with the concept of electrical resistance (Viard and Khantine-Langlois 2001, p. 269). Apparently, many seem to think of the resistance of a circuit as a simply increasing function of the number of resistive circuit elements. In a study of third-year physics students intending to become physics or technology teachers, as many as 15-20 percent of the students in some of the classes responded in accordance with this idea. Work by other researchers confirms that students commonly have "a difficulty in distinguishing between the equivalent resistance of a network and the resistance of an individual element" (Viard and Khantine-Langlois 2001, p. 269).

The everyday meaning of the term 'resistance', as well as many current textbook expositions of the concept, may be to blame for this state of affairs. The everyday language term 'resistance' denotes an opposition to motion, or an object that opposes motion. Since this concept is "immediately available to the students" they do not construct the concept anew, but rather borrow the "common sense concepts carried by the ordinary language and grounded on everyday experience" (Viard and Khantine-Langlois 2001, pp. 269–70). Resistance in this vague, everyday sense of opposition to motion is additive, so that increasing the number of resistive objects increases the resistance. In agreement with the hypothesis that students bring a commonsensical and object-centered concept of resistance along into the classroom, a number of students are reported to believe that a larger or more massive object offers a larger resistance, and, in particular, that the resistance of a conductor increases with the conductor's cross section (p. 283).

In studying modern French textbooks dealing with circuit theory, Viard and Khantine-Langlois found that many textbook expositions focus on the *resistive objects* of the circuit, promoting, they argue, a 'thing-concept' of resistance.

> The principal task proposed to pupils consists in identifying the constituents of the circuit and their functions. Among them, the [resistor] holds a prominent place. Thus this element plays the active role of an obstacle in the circuit compared with the connection wires which are presented as entirely passive in regard to the electric circuit ... [Graphically], connecting wires are described as a line without dimension and the resistor is represented by a box. (Viard and Khantine-Langlois 2001, pp. 280–281)

Resistance as a physical phenomenon is introduced as a *property* of the resistors, as a "role or ... effect of" these objects (p. 281). Contributing to the development of the thing-concept of resistance, a "confusion between the concepts of resistivity and of resistance" together with an "elimination of the geometrical properties of the conductor" can be identified in a number of textbooks (Viard and Khantine-Langlois 2001, p. 281).

In contrast, in the early development of electric circuit theory, "electric resistance is actually constructed as a relational structure" (p. 274). The term 'resistance' itself was introduced late; the early research into circuit theory was expressed in terms of 'conducting power' or 'conducting faculty'—*of closed circuits* (pp. 274–75). It is important that the research was focused on comparing entire circuits, rather than on sections or components of circuits.

By 1821 Davy had concluded that the ability of a wire to discharge a battery was inversely proportional to the wire's length, and directly proportional to its mass. For further comparisons of circuits, Becquerel introduced the technique of the differential galvanometer: two circuits powered by the same source were adjusted so that their opposing electromagnetic effects on a galvanometer needle exactly cancelled each other. Becquerel investigated different circuit setups whose electromagnetic effects balanced in this way and found, among other things, that two identical wires together (i.e. in parallel) would carry the same current as half a length of such a wire alone.

Pouillet made the idea of 'equivalence of circuits' more explicit. Like Becquerel he used the differential method, a procedure which Viard and Khantine-Langlois acclaim for providing "immediately an operative definition of electrical equivalence" (p. 276). Pouillet established the condition of the equivalence of circuits: If $S$ be the cross-section, $C$ the conductivity and $L$ the wire length of one circuit, and $S'$, $C'$ and $L'$ the corresponding quantities for another circuit, then the circuits are equivalent if

$$S \cdot C \cdot L' = S' \cdot C' \cdot L$$

Viard and Khantine-Langlois emphasize that the concept of resistance still had not been created. Not before 1840 would Lamé single out the fraction $\frac{S \cdot C}{L}$ and give it a name, 'conducting power'. In Pouillet's work,

> the concept of resistance or conductance of a conductor remains implicit. Its formal expression is not yet available because none of the terms of the previous equality refers distinctly to one circuit or another. Variables are not 'semantically' isolated in this expression. (Viard and Khantine-Langlois 2001, p. 277)

'Equivalence of conducting power' is a relational concept. It is a function of a number of properties of the circuit jointly, such as wire length, cross section and conductivity of the metal. It refers to a particular relationship of these properties, and this relational character is, according to Viard and Khantine-Langlois, the "missing part of the students' concept of resistance" (p. 280). An exposition following the historical development more closely might be expected to cause less confusion for the students, and Viard and Khantine-Langlois refer to two (somewhat limited) studies that suggest that this is indeed the case (p. 283). More generally, they warn that

> If the content of a concept is inscribed in the context of its elaboration, forgetting this context may result in a loss of meaning (Viard and Khantine-Langlois 2001, p. 279).

By paying attention to history one might avoid loosing the meaning built into the concept of 'resistance' at the early stages of its development, when the context was one of comparing different circuits and establishing conditions of equivalence.

## 2.3 Uses for historical experiments

### 2.3.1 When mechanisms were mechanical

In his work on Hertz' early experiments with electromagnetic waves, Roland Wittje describes the standard laboratory equipment for studies of electromagnetism in the 1880s. Charge was produced with friction machines and stored in Leyden jars. Current was usually produced by means of chemical batteries. The Rühmkorff spark inductor was relatively novel. For detecting charges there were spark micrometers or electrometers, and currents were detected with galvanometers of various kinds. Freely rotating magnetized needles could point out the direction of electromagnetic forces, while the strength of the forces was gauged by the torque exerted on needles constrained by wires. A common trait of the various measuring instruments was the *transformation of the electric force to be measured into a mechanical force* (the lifting of a weight, or twisting of a wire) by which the electric force is determined (Wittje 1996, p. 29). Even the spark micrometer, that made visible a potential difference when it was equalized through an optical spark, was accepted as a sufficient indicator only after it had been noted that it had the mechanical effect of perforating a layer of paper. This was in accordance with a commonly accepted programme of 19th century physics: the exposition of electric theory in mechanical terms.

> The transformation of electrical forces into mechanical forces had ideological as well as practical reasons. Mechanical effects (or mechanical forces) were considered to be understood. Thus an effect was counted unequivocally demonstrated if it had mechanical manifestations (Wittje 1996, p. 29).

In modern laboratories, in contrast, measurements are generally made with electronic equipment, and the measurement is read from a digital display. Measurements of mechanical entities like mass and velocity are also made with electronic instruments. Even in introductory physics courses the analog ammeter is soon replaced with the digital multimeter, so that measurements of current, voltage and resistance can be read in turn from the same instrument, after flipping a switch. We no longer consider mechanical forces more fundamental than electric forces, and we have no reason to. The word "mechanism" is now used metaphorically.

### 2.3.2 Transparant mechanisms for teaching

In seeking experiments and demonstrations that are clear and understandable to students, the history of physics is worth looking to. Often historical experiments can be found that rely on more transparent processes than current school experiments. The equipment used to introduce students to experimental physics naturally has changed dramatically over the past 100 to 150 years. Of course, the theory to be learnt is also often very different from what it was in the 19th century. In many cases the demonstration equipment used has, however, changed rather much more than the physical ideas that are to be illustrated at the introductory level, and this has not in all cases been a pedagogical advantage, as I will argue.

In this context some pedagogical points may be made for the outdated, mechanical measuring techniques of the 19th century. The electronic instruments of modern school laboratories are and remain impenetrable black boxes to the students. The students do not have—and can hardly have—a clear idea of the physical processes involved in the making of a measurement, of how the current, or voltage, or resistance, causes a certain configuration of digits on the multimeter's display. How does the multimeter "know" the sought quantity? How does the display itself work? Apart from the measuring instruments, the power supply and other circuit elements have also been cast in plastic and wrapped in mystery. There is nothing about a modern capacitor's appearance that suggests anything about its function, and the physics of the power supply must be learned from the textbook.

In contrast, there is something much more intuitive and concrete about the mechanical effects on which electrometers and galvanometers, for instance, rely. Digital multimeters, where a purely electromagnetic sequence of events culminates in a reading on a screen (brought about by altered optical properties of 'liquid crystals' when exposed to weak voltages) are not *in principle* any more mysterious than deflecting magnetic needles. But sticking to measurements done with deflecting needles limits the number of different elements of the setup that can not readily be understood from basic principles. Introducing a new black box for every circuit element or measuring device means a proliferation of epistemic primaries in the classroom, while sticking to simple devices means that just a few physical phenomena (such as electrostatic repulsion and the magnetic effects of a current-carrying conductor) suffice to account for most of what is going on.

A reasonable objection to any recommendation that 19th century experimental setups be reintroduced into classrooms would be that the opaqueness of electronic equipment notwithstanding, the students will be surrounded by such equipment all their lives. If they want to pursue a scientific career, they might as well get used to accepting that they can rely on measurements without knowing how these came about. No pedagogical considerations can simplify away the practical necessity of familiarity with black boxes. Advanced instruments have come to stay, they are an integral part of modern physics, and they constitute necessary and permanent extensions of our senses.

There are other reasons why 'black boxes' are widely used in modern school laboratories. The attraction of such apparatus may lie in the controllable, precise results they give without requiring too much skill on the part of the students. Many teachers would value the predictability of the results, perhaps seeing this as a hallmark of respectable science (but see section 2.4.2). A very important advantage of such equipment is, of course, the built-in safety measures that ensure that students not understanding what they are doing will not get shocks or burn themselves.

However, a possible effect on physics learning needs to be considered. A primary goal of physics education is, I think, to instil in the students an expectation that there are answers to questions of how things work, and to cultivate an inclination to ask that sort of question. What effect does the extensive use of black boxes have on the development of this kind of inquiring attitude? What risk is there that the students are less thoroughly initiated into the physicist's propensity to search for causality, necessity, mechanism? These questions can not be answered without empirical investigations. I would, however, hypothesize that the repeated experience of being able to trace the causal connection, from the phenomenon to be investigated and all the way to the recorded measurement, would be conducive to the development of this inclination.

In a rather different context, Svein Sjøberg argues that being exposed to so much incomprehensible technology may undermine the expectation of naturalistic causation. In his textbook on science education (Sjøberg 1998) he discusses a number of factors that may be responsible for an upsurge of public confidence in pseudoscience and mysticism of various brands, observed in Norway in the 1990s. He proposes that the complexity of modern technology may be one of the causes.

> The new technologies are based on the new and barely intelligible natural science, and like science they acquire an almost magical character. ... Everything seems possible, reality surpasses imagination. We have come to accept that technical reality is completely incomprehensible ... Nothing surprises us any more. We don't understand a thing anyway. (Sjøberg 1998, pp. 131–133)

This leads, on the one hand, to a loss of interest in technology and science. We no longer marvel at technology. A long time has passed since we came to accept remote controls, digital watches, cell phones, satellite television: "We have come to see this as so 'natural' that we do not even bother to let wonder or fascination seize us" (Sjøberg 1998, p. 131). More seriously, the distinction between science and pseudoscience becomes blurred:

> When science and technology both are mysterious and inaccessible, it is perhaps not so strange that mysticism gains ground. It is not easy for people to distinguish between the mystique of 'real science' and the mysticism of many of the occult movements. For people outside of science and technology it may well be that healing, ... pyramid energy or reincarnation do not appear any more mysterious and incomprehensible than all the gadgets they use daily. (Sjøberg 1998, pp. 133–134)

Sjøberg quotes a Norwegian healer who, in a broadcast interview, explained the recently improved conditions for healers in Norway precisely by reference to the opaqueness of modern technology: "[M]ost people after all could not understand their cell phones either, but they used them and relied on them all the same. And healers do not do anything more incomprehensible than that" (Sjøberg 1998, p. 142).

I will not assert any strong or clear connection between use of black boxes in physics education and such mystical tendencies, but restate my hypothesis that using such equipment means missing an opportunity to develop the confident expectation of finding mechanism.

Whatever the truth content of the discussion above, it is tautologically true that more transparent equipment is easier to understand, and intelligibility is generally an advantage in physics education. Peter Heering discusses why electrostatics is "not liked very much either by the teachers or by the students" in German secondary schools, and explains that

> [The electrostatic] experiments are hard for the students to understand. At secondary school level the experiments get more quantitative. The set-ups become more and more complicated and the students are confronted with a lot of black-boxes. This is especially true in electrostatics, and it has a grave consequence: the students cannot understand the experiment and simply get bored watching a person reading some meters or making little changes in the set-up which do not seem to be logical to the students either. (Heering 2000, p. 364)

Pertinent differences between historical and modern experiments are well illustrated by the kinds of ohmic resistors used in the two cases. In Ohm's original experiment, wires of different lengths were used as resistors. As Höttecke (2001) notes, the associated notion of resistance as depending on the distance over which the flow of electricity is impeded makes intuitive sense (Höttecke 2001, p. 249). In contrast, deciphering the code of coloured rings on a standardized resistor hardly encourages a clear conception of electric resistance, and what a digital ohm-meter does is entirely in the dark. "Measuring technique frequently displaces more important things from view" (Falk Rieß in Höttecke 2001, p. 249).

Some modern school experiments cloak the phenomena to the extent that they—as far as I can see—serve no educational purpose at all and might as well be omitted. I remember an experiment from my high school physics course that was supposed to illustrate the photoelectric effect by means of Einstein's experiment. The apparatus was a black box—literally black, and literally a box, all made of plastic—with some dials and displays on it. By means of the dials we could (as far as I remember) adjust the intensity of the light beam and the voltage opposing the ejected electrons, and we could read the resulting current from a display. All this information on what we were actually doing was, however, gathered from the textbook. No understanding of the significance of the input and output was to be had from looking at the black box itself. The experiment served as a passable exercise in drawing a graph and calculating uncertainties, but a table of data taken from a book would probably have been quite as useful a starting point as the results we read from the apparatus. This apparatus probably gave neat, predictable data. But for an experiment whose purpose was to illustrate an important physical effect to beginner science students, I would certainly prefer equipment that gave more erratic data if it provided a glimpse of the cogwheels of the phenomenon under study.

Black boxes and electronic instrumentation of course are valuable and necessary, and must be introduced in physics courses sooner or later. However, I would suggest later is better, leaving time for a stronger expectation of causality to develop first, and limiting the number of incomprehensible elements in the confused early stages of learning physics.

### 2.3.3 Replications of historical experiments

The previous section dealt with *historically inspired* experiments and experimental equipment. The mechanisms or physical principles of historical experiments are actualized with modern materials that are convenient for classroom purposes. Only the 'physical principle' or idea is extracted from the original work—the enactment is modern.

A number of scholars have argued for the value of more historically accurate replications of experiments, using the same materials and techniques as were used originally. In particular, a group of physicists at the Carl von Ossietzky University of Oldenburg, Germany, have specialized in such replications. This 'Arbeidsgruppe für Didaktik und Geschichte der Physik'[3] has rebuilt apparatus for more than 40 historical experiments over the past 20 years, emphasizing faithfulness to the original experiments. Representatives of the group argue that such more authentically historical experiments have great merits in several areas. They are invaluable tools for the historian wishing to understand past experiments. They provide important and surprising insights into the process of experimenting. And they can play a unique role in educational settings.

Some brief remarks about their value for historians are made below, and about the possible value of their aesthetics in education. Some further aspects form part of section 2.4.

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[3] URL http://www.uni-oldenburg.de/histodid/, accessed April 2004

#### 2.3.3.1 Replications for historiography

Höttecke (2000) points out that histories of experiments have traditionally been based on textual sources, on written accounts of the practical work done. Such sources, however, have serious limitations. Laboratory notebooks as well as publications of experimental results are silent about myriad details of the experiment. In part that is because the experimenter takes much knowledge on the side of contemporaneous readers for granted—the target group will be familiar with standard laboratory equipment and procedures of the time, so that going into meticulous detail about such things is unnecessary or even unfortunate. Besides, far from all the knowledge, reasoning, skills and practices involved in performing the experiment *can, even in principle,* be described in explicit language.

Problems arise when a reader from a later period tries to interpret these laboratory notes and publications, without possessing this tacit, taken-for-granted knowledge about then-time standard laboratory inventories and practices. The equipment, and even the materials used, may be completely unknown, and the problems posed unintelligible. Because the knowledge gaps are in the field of practical knowledge and familiarity, trying to find the missing information in further textual sources may be futile. Studying the original apparatus—when it is available—may of course help, but the function of a historical instrument is often not clear without further explanation.

Re-enacting the experiment may then provide clues about what has been left unstated. A replication with emphasis on historical accuracy becomes *an experiment about an experiment* (Höttecke 2001, p. 247), a way of testing possible performances that are consistent with the written and material sources. Extraordinarily careful studies of the sources are required in order to repeat the historical experimenter's work step by step, and so gaps and ambiguities in the instructions are revealed—much is learned in trying to fill out these gaps in various ways. Sometimes it becomes clear that the experimenter cannot possibly have done quite what is reported, and insights can be gained by pondering why these discrepancies are there. The process of replication reveals *historical contingencies* in concrete and material detail, often in surprising ways. The role played by characteristics of the materials used, the available workspaces, financial resources, dexterity and skillfulness, and the amount of time needed, comes into focus. It becomes clear how such factors influence, limit and redirect the experimental activity. If the reception of the experiment in the scientific community is also studied, variations in these factors from one research site to another also become an issue.

These advantages of historically faithful reconstructions are amply exemplified in reports of replications performed at the University of Oldenburg[4].

#### 2.3.3.2 Aesthetic aspects of replicas

A rather surprising advantage of historical experiments for education is pointed out by Höttecke (2001): The apparatus simply looks good.

> The aesthetic dimension of historical experiments can represent an agreeable contrast to the instruments otherwise used in physics education. Materials like wood (often fashioned into rounded shapes), sealing wax or resin can play a role, as can leather, glass, pith and gold leaf. These different, historical materials make other

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[4] A publication list can be found on
http://www.uni-oldenburg.de/histodid/Publikationen.htm (accessed May 18th, 2004).

> optical and tactile qualities possible than do the synthetic materials and metals ordinarily used in school experiments. (Höttecke 2001, p. 249)

A modern physics teacher may well find such considerations irrelevant—it is, after all, *physics* that is to be taught, and the laws of physics surely have nothing to do with the looks of the apparatus? A quick glance at any university's collection of 19th century instruments shows that a change of culture has taken place here. The apparatus once used for teaching and public lectures were frequently handicrafts of considerable beauty, with brass and dark wood polished to shine. What attitudes to natural science were reflected in the labour and artistic competence invested in this equipment? Perhaps some degree of revival of these sentiments might not be altogether useless in the context of physics education.

## 2.4 Sparks, shocks and sociology

While the literature on various applications of history in science teaching is extensive and diverse, there are not that many actual reports and evaluations of integrated historical approaches in classrooms, cases where the teaching of physical concepts has been organized along historical lines. One significant exception is Peter Heering's 2000 report of teaching electrostatics to secondary school students, using original texts and historically accurate replications of experiments. The report beautifully illustrates a number of strengths of this kind of historical approach, of which only a few will be mentioned below.

### 2.4.1 Seeing, tasting and touching electricity

Following the developments in early electric research, Heering initially made the students detect electricity by watching sparks produced during discharge, or by discharging objects through their own bodies and getting shocks:

> [The frictional electrostatic generator] made made it possible to work with charges that could be both felt and distinguished by the students. This had two advantages: the main measuring instrument in the first part of the course was the body instead of any apparatus. This made a sensual registration of electricity possible, something totally uncommon in traditional physics courses but widely used in the history of electrostatics as a first step towards a quantification. Moreover, in a way the students' bodies can be looked upon as a kind of 'black-box', but—contrary to other, traditionally employed black-boxes such as a galvanometer—this was not felt to be strange or deterrent (Heering 2000, p. 365).

Especially for younger students, it is not hard to imagine that these qualitative procedures from the 18th century allow electrical phenomena to be experienced as closer, more vivid, and somehow more *real* than the pale display of a multimeter does. The elements of surprise and wonder are also strengthened. In Heering's class these approaches generated a good deal of enthusiasm.

### 2.4.2 Authenticity of experimental problems

Commenting on the advantages of letting the students *feel* the electric discharges, Heering remarks that

> it made it possible to work without employing instruments that already included the results that were to be produced. (Heering 2000, p. 365)

When relatively advanced equipment is used to illustrate elementary results, as when multimeters are used to demonstrate Ohm's law, the laws and phenomena to be demonstrated are frequently already built into the measuring instruments. This is one sense in which an experiment, here the demonstration of Ohm's law, can be *inauthentic*.

Höttecke (2001) refers to work by several researchers who have studied students' ideas and opinions about the experimental work they do in the classroom (Höttecke 2001, p. 63). A number of students assert that prior to any laboratory exercise, the results to be found are already known. If their results do not correspond to the predetermined outcomes they must manipulate some aspect of the experiment until their results conform. They express awareness that this is not the case in 'real science', and complain that the goals of their practical work are to that extent artificial.

Can work with replicas of historical experiments be more genuinely investigative exercises? Höttecke argues that it can be. Referring to reconstructions of Coulomb's torsion balance experiment, he points out that "the measurement values acquired can neither be unqualifiedly transformed into Coulomb's law, nor is the aim to test that law according to hypothetico-deductive procedure" (Höttecke 2001, p. 245). The efforts at trying to get the apparatus to work provides rich and ample opportunity to exercise practical reasoning and experimental skills. However, here getting the apparatus to work properly and reducing disturbances, sources of inaccuracy and systematic errors *is not the same as forcing the apparatus to give the 'correct' values.* The origination of the experiment in a historical context allows it to retain elements of openness and authenticity, allowing the students "to ask genuine questions and seek genuine solutions" (Höttecke 2001, p. 245).

In Heering's electrostatic unit Coulomb's torsion balance was introduced only after the students through their discussions have come to see the need for more quantitative determinations of amounts of electricity (Heering 2000, pp. 368–9). In that way the apparatus got a place in an authentic inquiry. Introducing new physical ideas and instruments at the pace of students' questions is probably not generally practicable, though.

### 2.4.3 Historical approaches and classroom sociology

Heering's electrostatics unit, that was to an extent to follow the historical development of the subject, caused unanticipated changes in the sociology of the classroom (Heering 2000). Apart from treating a different selection of material compared to what is standard, the aim was to imitate aspects of the historical research situation. In particular, the lessons were not concluded with a summary of correct answers and established physics. Instead, the students were to discuss their way to possible explanations of simple experiments, and unresolved questions would be left hanging in the air at the lesson's conclusion, often over several lessons.

This open situation in a physics lesson was highly unfamiliar to the students, and some of them were not comfortable with it. Discontent with this situation sparked a discussion about "whether what we are doing in physics lessons is actually physics", with some students apparently feeling that since they were not calculating problems they were not doing physics. In this open, ambiguous situation changes in the classroom hierarchy took place:

> The students had to develop a theory that explained all their observations. ... the two dominant students developed theories that initially sounded brilliant. Unfortu-

> nately other students were able to demonstrate that these theories did not in fact explain all the phenomena or that they could be falsified. This led to a change in the hierarchy of the group, Peter and Sven lost part of their authority. On the other hand, two of the female students (Tina and Bianca) in particular gained in self-confidence and began to question the theories of the former authorities. Not only did they criticise the explanations but they—and several other students—started to develop and test their own theories (Heering 2000, p. 366).

These changes in the social structure of the class remained after the historical unit was over. With the exception of one (excellent) student, who refused to participate in discussions afterwards, each student joined in more actively after this historical unit, but without any student taking a dominant role.

Höttecke (2001) thinks learning history of science can motivate students by giving them self-confidence:

> In realizing that even the "great" scientists of history had to change their fundamental views, students are encouraged to reveal their own conceptions, without just feeling small and stupid. The students can appreciate that their own questions often have been those of the history of science (Höttecke 2001, p. 195).

I am unconvinced that learning history per se will have such an effect. Much more would be required to build identification with historical scientists than pointing out similarities in questions asked, I would imagine. On the other hand, emulating aspects of historical research situations may plausibly contribute to such self-confidence and identification. Putting the right answers on hold for a while, and giving the scene to historically informed arguments among the students, may be a promising strategy.

# Chapter 3

# Pluralism for Understanding Physics: The Role of History

In the previous chapter a variety of ways in which historical material may be useful for the physics educator were discussed. Knowledge of the history of physics may help the educator identify possible misconceptions in the students. Historical experiments may be adapted to provide transparent physics demos. Historical texts constitute a library of persuasive presentations of physics topics. Looking at the discipline's past may inspire interest.

Certain arguments for the value of history rely on more specific notions about the nature of understanding, or about the nature of physics, or both. For Dietmar Höttecke (2001), for example, there is no such thing as 'understanding physics' without some understanding of the discipline's historical development. On the one hand he draws on hermeneutic theory of interpretation to argue that genuine understanding must involve knowledge of the history of whatever object is under study. On the other hand he emphasizes the essential historicity of physics: contingencies of the past are part and parcel of today's physics, so that current theories and practices in the field can not be satisfactorily accounted for without reference to history.

The argument in this thesis will center on another way in which material from the history of physics can deepen understanding of physics. An account of 'understanding' will be outlined according to which learning a *plurality* of theories, representations or models of the object under study enhances understanding. This account will also draw on hermeneutic theory, although the emphasis will be somewhat different from Höttecke's. Concomitantly, pluralistic aspects of accepted physical theory and of current textbook accounts will be pointed out. The hypothesis is that historical studies can provide the plurality of perspectives on physical phenomena that is desirable for developing physical comprehension.

The first part of this section will concentrate on clarifying the idea of 'pluralism' in physics, and will address issues of the uniqueness of physical theory and the degree of interpretative freedom in the discipline. The second part will be concerned with what 'understanding' may be. The final part will attempt to show that the multiplicity of perspectives in physics, as laid out in the first part, is valuable for the development of understanding as described in the second part. The role of history as a source of teaching materials in this spirit is emphasized.

## 3.1 How much freedom of interpretation in physics?

I wish to argue that the multiplicity of theories to be gleaned from history is valuable for learning physics. But is it not a hallmark of physics that such diversity of opinion on physical phenomena is overcome as time passes and further experiments and arguments are added? Is not physical knowledge cumulative, so that present theories include the worthwile elements of abandoned theories? Do we not approach unity of theory, so that this dwelling on plurality of theories is unnecessary?

In answering these question, a good deal depends on what we take "differences" between theories to be. Are theories distinguished only by conflicting predictions, or are differences with respect to ontological claims also relevant? What about differing interpretations of the same theory, or differing models? I will start with discussing ways in which physical theories may differ, then remark briefly on the relationship between theories, models, and interpretations, in order to clarify the notion of 'pluralism' in physics. Some contrasting philosophical standpoints with respect to the multiplicity of perspectives in physics are described.

Finally, three historical cases of great reinterpretations, or reformulations, are sketched. They are Dirac's reinterpretation of evacuated negative electron energy states as positrons, Feynman's reformulation of quantum mechanics in terms of paths, and Hertz's recasting of classical mechanics in a form free from the force concept.

This will hopefully suffice to make plausible the claim that, at least given certain notions of what variations between physical theories can be, there is much freedom of interpretation in physics—that, in fact, proliferation and divergence of perspectives are at least as much a mark of progress in physics as unification and parsimony—and that creative interpretative efforts form a central and serious part of the scientific enterprise.

### 3.1.1 Differing predictions versus differing ontologies

Consider the transition from Newtonian to relativistic mechanics. Is Newtonian physics *refuted* by relativistic mechanics, or is it *included* in the new theory as a limiting case? A great proportion of science students need no more than Newtonian mechanics for their careers, and their lessons emphasize the 17th century theory. That is, of course, because the numerical agreement between the two theories is very good in the relevant domain of phenomena. In this sense of numerical correspondence, Newtonian mechanics certainly is included in relativistic mechanics as a limiting case.

Yet, as Thomas Kuhn emphasized, the images of the world underlying classical and relativistic mechanics are quite dramatically divergent. The ontologies implied by the respective theories are different, so that terms refer to different things, and have different meanings, in the two theories. For example, mass in Newton's theory is an intrinsic property of the object, while mass in Einstein's theory is a relational property, changing with the object's speed relative to the observer. In analogous ways, "every single descriptive term in the two theories means something quite different" (recounted in (Couvalis 1997, p. 94)). With respect to ontological claims, then, the Newtonian physics taught in high school is not an approximation to currently accepted theory about what the physical world is like.

Does that make Newtonian physics a fundamentally incorrect theory of matter, space and motion? A host of assumptions will underly any answer to this question. One issue at stake is the question whether physical theories are to tell us about what there is in the world, or whether the knowledge claims of physics should be thought of as restricted to concrete predictions. That issue will be the concern of one section below. Another question is how or why the coexistence of differing theories is a problem in a quest for knowledge. As we will see in a later section (3.3), hermeneutic theory provides a perspective on this point that differs from that of classical philosophy of science. While according to classical philosophy, at most *one* of the competing theories may be true, hermeneutic philosophy does not consider a plurality of theories to be indicative of lack of understanding—perhaps quite the contrary. The discussion of hermeneutic theory is deferred for later, however—at present our concern is with establishing the degree of pluralism in the system of physical knowledge.

### 3.1.2 Theories, models, interpretations...

In the previous section the two differing theories discussed disagreed numerically as well as with respect to their ontological claims. Suppose two theories that appear very different with respect to metaphysical claims in fact prove to be mathematically equivalent. Are the two theories then identical? Or, in other words, are our representations and interpretations of the mathematical relationships part of the theory? This may be a question of nomenclature rather than a philosophical question in its own right; at issue is perhaps just a specification of how the word "theory" is to be used in a given discussion—but such clarification can be important enough.

Heinrich Hertz's dictum, "Maxwell's theory is Maxwell's system of equations," is famous. Certainly Maxwell's equations have survived the physical models Maxwell used to derive them, and are now used in conjunction with theoretical entities whose existence Maxwell might have doubted or even denied, such as elementary charged particles. The electromagnetic equations outlived their mechanical origins. Were they all that was to Maxwell's theory?

To define terms at the start: I will in the following use the word "theory" in a more inclusive sense, as referring to more than the strictly mathematical formulation of the relationships between measurable quantities. Not only systems of equations that give different predictions will be considered different as theories; but also differing metaphysics that are coordinated with equivalent formalisms. Thus, although the mathematics of Newtonian mechanics is a limiting case of the mathematics of relativistic mechanics, "Newton's theory" will here not mean a special case of relativistic theory. With this use of language, Schrödinger's wave theory of quantum phenomena and Heisenberg's matrix mechanics are different theories, although their formalisms agree on all points. A theory will be taken to include an interpretation of the formalism. The word "interpretation" may perhaps misleadingly suggest that the qualitative accounts of the physics came *after* the formalism, something that certainly need not be the case—again, the discussion of this paragraph is merely a specification of how linguistic terms will be used in the present chapter.

Can formalisms and interpretations really be so sharply distinguised? Can one have a formalism without a theory? Any actual determination of what kind of quantities a given formalism is about at all, and what it would mean to measure them, might require a qualitative rendering, a theory, an interpretation, or perhaps a model. Ian Hacking, in part following Nancy Cartwright, argues that between formalistic development and experimentation there is a "wide ranging intermediary activity best called model-building" (Hacking 1983, p. 216), and that these models ("an odd mix of the pictorial and the mathematical") are what allow the mathematics to relate to observables at all, what coordinate the terms of the formalism with entities in the world. Models may further be robust under changes in both formalisms and theories,[1] and models do not converge toward a single picture; rather, in "every single year since 1840, physics alone has used successfully more (incompatible) models of phenomena in its day-to-day business, than it used in the preceding year" (Hacking 1983, p. 218). Such issues will resurface now and then in the course of the following discussions, but much of the time I am going to simplify things by pretending that formalisms, interpretations and models can be taken as independent of each other. While such omission of the interrelationships between them may be distortive, some limited points may perhaps still be made—enough, perhaps, to support the main argument of this thesis, which after all is not primarily about the philosophy of science, but about learning and understanding.

### 3.1.3 Realism, instrumentalism, relativism and pluralism

We can return to the question whether physics teaches us anything about what there is in the world, about the reality of unobservable entities and relations, or whether its truth content is limited to the empirical consequences that it correctly predicts and explains. This debate between *scientific realism* and *instrumentalism* must be as old as science itself. At least, the question whether astronomy taught, or was meant to teach, anything about the constitution of the heavens or was, more modestly, to provide a practical means of getting the calculations of the calendar right, was a matter on which Copernicus and his publisher disagreed, and an issue in the dispute between Galileo and the Church (Matthews 1994, pp. 165–7). In Newton's times feelings ran high about the question whether "occult" entities such as gravitational forces acting at a distance might legitimately be invoked by the physicist in order to explain the motion of the planets. Could one assume more than the empirical adequacy of the equation, its sufficiency to *save the phenomena*? Or should one remain agnostic about "forces" or other purported explanations of what could be observed? (pp. 167–70) Great physicists have been found in both camps, with Planck and Einstein on the realist and Mach and Bohr on the instrumentalist side of the divide.

Returning to the question of Newtonian versus relativistic mechanics, it would seem that the differences between the two systems poses no problems for the instrumentalist's belief in scientific progress. Since the systems agree numerically, and hence give the same empirical predictions for the relevant domain, Newtonian mechanics is not refuted, but included in a more general theory. However, for the scientist hoping to gain insights about the furniture of the world, about unobservable entities, relativistic mechanics appears to constitute a radical break with the previous notions. There does not seem to be much continuity with the past in the metaphysics of the new theory. The realist would seem to be forced to accept that future developments may similarly thoroughly refute present ideas about nature, rather than extending or generalizing them. The realist position seems problematic, opening wide the door to skepticism. Only the more limited, instrumentalist knowledge claim seems tenable.

However, this conclusion presupposes that one unified account be a prerequisite for knowledge of what there is in the world. *Pluralism* denies this presupposition. The pluralist holds that more than one physical theory or conceptualization may be acceptable in the sense of providing valuable physical insight. Pluralism is obviously compatible with instrumentalism, but also with at least some versions of realism. A realist may well, while believing that central terms of physical theory do refer, and that the structure of the theory does in some way reflect the ontology of the world, consider the fit between the mind and the world to be such

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[1] The model of electric current as the mechanical flow of of a liquid may be an example of a model that has survived several changes in the theory of electricity.

that no single account that is intelligible to us also makes exhaustive ontological claims about the world. A spectrum of accounts, that so to speak illuminate the phenomena from different angles, may then be seen as *together* providing knowledge about the world. On a different note, a realist may also, while believing that it is the aim of science to provide a theory whose central terms refer,[2] consider all the current theories to be less than satisfactory, and deem free competition among rivalling theses and methods to be the most rational means toward the end of a true theory. This realist may *temporarily* recommend a pluralist approach. These guarded versions of realism allow, and even imply, pluralism of physical theory. The former brand of realism may seem to hold pluralism an *in principle* necessary aspect of physical knowlege, while the latter may see pluralism as being a contingent aspect of our at present limited knowledge.

These pluralist ways of thinking may at first glance seem to admit relativism, and to remove a driving force for developments in physical theory. For if more than one theory may be accepted, does not much of the point of working out testable differences between competing theories disappear? Is that not one of the central tasks of physics? Would not the pluralist happily accomodate erroneous theories instead of trying to refute them, if no single theory is expected to be sufficient anyway? And what about the motivation to seek more general frameworks for integrating current theories that fit empirical data? What point is there to that if separate theories are accorded value *as* separate theories, and not only as parts of a single, unified truth?

Whatever the psychological effects of embracing pluralism, it must be noted that philosophically, pluralism is by no means equivalent to relativism. Pluralism is different from relativism in "rejecting the relativist view that 'anything goes', that any particular scientific thesis or methodology is as good as any other" (Siegel 1991, p. 54), and it also "differs radically from the relativistic view that there is no evaluating the worth of rival ideas and approaches" (p. 54). Admitting more than one single theory does not rule out rejecting any number of inconsistent, empirically inadequate, or useless theories.

### 3.1.4 Underdetermination of scientific theory

A key idea exploited to various ends by instrumentalists, relativists and pluralists is the *underdetermination of theory by data.* In much the way that a graph is underdetermined by the limited number of points that it is required to fit—there existing any number of graphs that will be consistent with the data points—the number of physical theories that will agree with the available observations, present and future, is seen to be infinite (Baune 1991, pp. 57–8). This section deals with differences between formalisms accounting for the same phenomena, not merely differences between interpretations of the mathematically eqivalent formalisms, which are discussed in the next section.

An illustration due to Nelson Goodman introduces the predicate *grue*, which is taken to apply to "all things examined before $t$ just in case they are green but to other things just in case they are blue" (Goodman 1973 [1955], p. 74). Before the future time $t$, then, the statements "all emeralds are green" and "all emeralds are grue" have the same empirical consequences. There are no observations we can make that disproves the one and not the other, so why should we prefer the one to the other?[3] Little imagination is required to generate hosts of

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[2] A term "refers" if its reference is real; i.e. the term 'electron' (presumably) refers, while 'phlogiston' does not.

[3] To those who might consider such time-dependent hypotheses too contrived to worry about, Goodman

observationally indistinguishable hypotheses following some analogous scheme. Now the issue of dispute is not really whether such strange hypotheses are as admissible as any other in science—Goodman does recognize *grue* and *bleen* as "ill-behaved predicates" (p. 79)—but it is not easy to account for what is wrong with them in a satisfactory way. The point remains that attention to "fact" alone will not uniquely determine a theory. Disagreement enters about what criteria other than observational adequacy may enter into theory choice.

On the other hand, George Couvalis (1997) points to the absence of a proliferation of theories in science, and takes this—together with the predictive power of accepted theories—as "good evidence that an accepted theory is approximately true" (p. 190), in the sense that its central terms refer and that its ontological claims are warranted. He speaks of the failure of intensive efforts by intelligent and knowledgeable people to find credible alternative theories, and concludes that

> we have the same kind of reason for thinking that plausible alternatives ... do not exist as we have for thinking that unicorns do not exist (Couvalis 1997, p. 191).

That may well be true in the case of the "germ theory," which Couvalis refers to in illustration and defense of his position. There may not be a proliferation of theories that are alternatives to the view that (certain) diseases are caused by microbes.[4] However, much of physical theory must be said to relate in more complex ways to palpable evidence than the germ theory mentioned above does. More abstract and elaborate human constructs are required in order to account for the evidence, and conceivably the number of ways in which this can be done is correspondingly greater. The possibility that the density of unicorns be higher in physics than in epidemiology should not be ruled out from the start, and the fact that extraordinary intelligence and originality may be required in order to find them is no refutation of their existence.

### 3.1.5 Underdetermination of interpretation

In the previous section the possibility that available evidence be accounted for by mathematically divergent theories was discussed. Here the issue will be underdetermination of meaning, or of interpretation. For a *given* formalism may still be described in many ways, and its terms taken to refer to different entities.

The range of interpretative freedom is very great. For example, Maxwells equations are consistent with electric current modeled as charged particles in motion, as the flowing of an electric fluid, or as a relaxation of a mechanical tension in an ether. Newton's laws of motion make sense as describing motion in an absolute, flat space—or as describing the low-velocity case of motion in a curved space-time with no preferred frame of reference. The second law of thermodynamics was established long before its statistical interpretation was agreed upon; its current meaning is not derived from the law itself and was a matter of bitter debate. A quantum mechanical system can be characterised by the amplitude, phase and frequency of

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suggests we also introduce *bleen* to describe things blue before $t$ and green after. Then the hypothesis "Emeralds are grue up to the time $t$ and bleen thereafter" (which is observationally equivalent to "emeralds are green") includes a temporal term, while the hypothesis "emeralds are grue" does not.

[4] Couvalis's choice of example is not quite fortunate for his case, though—the history of the germ theory of disease has an ironic twist. The hegemony of the germ theory of disease, following Pasteur's work, led to over two decades of research to isolate deadly beri-beri-bacteria from polished rice. An alternative theory of this fatal disease, that beri-beri is caused by a deficiency in an essential nutrient (vitamin B1, present in unpolished rice) was suggested late and only slowly gained acceptance.

a wave function—or by the length and orientation of a vector in a multidimensional space, to mention only the most elementary representations. The terms of an expansion generated by applying perturbation theory to the interaction between two particles can be thought of as describing 'virtual particles', and speaking about these entities as particles in many ways works fine, for all their being 'virtual'. An so on, for as long as our limited knowledge of physics, or its history, will carry us.

In some of the cases mentioned above the choice of physical interpretation is guided by knowledge from other domains. For example, there is theoretical and experimental evidence independent of Maxwell's equations to suggest that electric current is not a relaxation of a mechanical tension in an ether. Evidence from electromagnetism rules out a flat, absolute space as the world described by Newton's laws. However, the point remains that a given formalism, or a set of mathematically entirely equivalent formalisms, lend themselves to different representations. How, then, is a certain interpretation chosen from the many that are logically consistent and empirically adequate? Of course, some conceptual systems seem more contrived, inelegant and useless than others, but do such criteria tell us more about the world than about ourselves? James Cushing, in discussing why the Copenhagen interpretation is generally preferred over Bohm's (mathematically equivalent) causal interpretation of quantum mechanics, warns that

> Criteria such as fertility, beauty and coherence, while often important, can have a Whiggish aspect to them if they are defined in terms of the successful, victorious or accepted theory and then applied to a competing theory. (Cushing 1995, p. 140)

Given such indeterminacy of interpretation one might want to conclude that we should try to liberate ourselves from the physical images and limit our attention to the formalism. If, as Paul Dirac expressed it, "[Nature's] fundamental laws ... control a substratum of which we cannot form a mental picture without introducing irrelevancies" (D'Agostino 2002, p. 255), we should perhaps not insist on working out the details of what we cannot know anyway. However, whether or not the formalism be 'truer' than the interpretations (whatever that might mean), it is by no means clear that we *could* dispose of the physical ideas if we wanted to. For one thing, could physics ever be taught—and learnt—from the mathematics[5] alone? And, further, could human beings, given the particular kind of conceptual apparatus we happen to have for understanding the world, develop physics without relying on imagery and visual representations?

Section 3.2 below will, on the basis of a discussion of what 'understanding' involves, conclude that it is implausible that physics can be learnt or understood without interpretations, and that therefore interpretations are necessary for physics pedagogy. This section will be concerned with the role of physical images in the enterprise of developing physics itself, and look briefly at three cases of significant inventions of new physical pictures, all by physicists self-conscious and articulate about what they were doing. In the case of Dirac's positron the physical concept followed after the mathematical innovations. In Feynman's path-integral formulation of quantum mechanics, a preference for one physical picture over another was the driving force behind the development of novel mathematical techniques. Hertz's reformulation of classical mechanics, which is better known among philosophers than among physicists,

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[5] I am using the terms "mathematics" and "formalism" interchangably, in a way that perhaps (misleadingly) suggests that doing mathematics is about manipulating symbols according to rules, and not about visualization, geometric interpretation or analogy. My point is not a statement about how mathematics is done or understood, but about how physics (and perhaps also mathematics?) is *not* done or understood.

would seem superfluous from many a practical point of view, but was a source of inspiration to Einstein.

#### 3.1.5.1 A particle for an empty state: Dirac's positron

Salvo D'Agostino (2002)[6] describes how one of Paul Dirac's great achievements was a new assignment of ontological reference to certain mathematical terms, and how Dirac changed his mind on the relationship between mathematics and physical concepts in the context of this work. Initially Dirac held a traditional view of the role of mathematics, according to which, as he wrote in 1930,

> Mathematics is the tool specially suited for dealing with abstract concepts of any kind and there is no limit to its power in this field. For this reason a book on the new physics, if not purely descriptive of experimental work, must be essentially mathematical. *All the same the mathematics is only a tool and one should learn to hold the physical ideas in one's mind without reference to the mathematical form.* (Dirac in D'Agostino 2002, p. 226. My emphasis.)

The following year he would write that

> The most powerful method of advance that can be suggested at present is to employ all the resources of pure mathematics in attempts to perfect and generalize the mathematical formalism that forms the existing basis of theoretical physics, and *after* each success in this direction, to try to interpret the new mathematical features in terms of physical entities ... (D'Agostino 2002, p. 227. Dirac's emphasis.)

Dirac published his relativistic equation for the free electron in 1928. This equation seemed to admit negative kinetic energies for the electrons—a serious problem, as that would mean that electrons could emit infinite amounts of radiation by transiting to ever lower energy states. Dirac suggested that no negative kinetic energy states were available, however, all being already occupied by electrons.[7] The idea of such a 'Dirac sea' of infinitely many electrons filling up the correspondingly infinite number of negative energy states may appear rather contrived—but it would constitute a possible world where Dirac's equation and the known experimental observations both fitted in.

Soon Dirac introduced a further conceptual novelty in order to account for what happens when an electron is excited from such a negative kinetic energy state to a state of energy at least as high as an electron's rest mass. The evacuated negative energy state—a 'hole' in the 'Dirac sea'—was assigned particle status. Such a rare, unoccupied negative state would be indistinguishable from an 'anti-electron', or in other words again, the excitation of an electron from the 'Dirac sea' would correspond to the production of an electron-positron-pair.

> Thus, in Dirac's interpretation, a state, which formerly represented the properties of a physical system, was now elevated to the role of the system itself. (D'Agostino 2002, p. 223)

When Dirac's relativistic equation was published, the mathematical details of his theory were much more developed than its interpretation. In Richard Feynman's words,

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[6] Thanks to Gerald Torgersen for drawing my attention to this article!

[7] Thanks to Jan Ivar Korsbakken for clarifying a number of details on this issue!

> Dirac obtained his equation for the descripion of the electron by an almost purely mathematical propositon. A simple physical view by which all the contents of this equation can be seen is still lacking. (Feynman 1965, p. 177)

#### 3.1.5.2 Feynman's re-expressions of electrodynamics

If Dirac thought it advisable to "perfect and generalize the mathematical formalism that forms the existing basis of theoretical physics, and *after* each success in this direction, to try to interpret the new mathematical features in terms of physical entities" (section 3.1.5.1), Richard Feynman expresses almost the opposite sentiment—a "dislike" of the idea of there "not being a picture possible but we only need to know how to go about calculating" (quoted in (Eger 1993b, p. 314)). He was deeply fascinated with the variety of physical pictures compatible with a given formalism, and in his Nobel Prize lecture repeatedly marveled at the multiplicity of possible approaches:

> The fact that electrodynamics can be written in so many ways—the differential equations of Maxwell, various minimum principles with fields, minimum principles without fields, all different kinds of ways, was something I knew, but I have never understood. It always seems odd to me that the fundamental laws of physics, when discovered, can appear in so many different forms that are not apparently identical at first, but with a little mathematical fiddling you can show the relationship. ... I don't know why this is—it remains a mystery, but it was something I learned from experience. There is always another way to say the same thing that doesn't look at all like the way you said it before. ... I think it is somehow a representation of the simplicity of nature. ... I don't know what it means, that nature chooses these curious forms, but maybe that is a way of defining simplicity. Perhaps a thing is simple if you can describe it fully in several different ways without immediately knowing that you are describing the same thing. (Feynman 1965, pp. 163–64)

Feynman acknowledges the power of mathematics, and reflects on whether the multifarious physical descriptions are not really redundant:

> Originally, Maxwell filled space with idler wheels, and Faraday with field lines, but somehow the Maxwell equations themselves are pristine and independent of the elaboration of words attempting a physical description. The only true physical description is that describing the experimental observations. This being the case perhaps the best way to proceed is to try to guess the equations, and disregard physical models or descriptions. (Feynman 1965, pp. 176–7)

However, he goes on to reject this suggestion, on the basis of how the actual people that scientists are often go about making discoveries:

> I think the problem is not to find the best or most efficient method to proceed to a discovery, but to find any method at all. Physical reasoning does help some people to generate suggestions as to how the unknown may be related to the known. Theories of the known, which are described by different physical ideas may be equivalent in all their predictions and are hence scientifically indistinguishable. However, they are not psychologically identical when trying to move from that base

> into the unknown. For different views suggest different kinds of modifications which might be made and hence are not equivalent in the hypotheses one generates from them in one's attempt to understand what is not yet understood. I, therefore, think that a good theoretical physicist today might find it useful to have wide range of physical viewpoints and mathematical expressions of the same theory (for example, of quantum electrodynamics) available to him. This may be asking too much of one man. Then new students should as a class have this. (Feynman 1965, p. 177)

Feynman's intimate acquaintance with a variety of very different representations of classical electrodynamics, his ability "to express the subject every which way" (Feynman 1965, p. 163), made his own contributions to physics possible. They were, characteristically, reinterpretations that opened up new ways of conceptualizing phenomena and of doing calculations (Eger 1993b, pp. 312–13). In his doctoral work he recast classical electrodynamics into a form that dispensed with fields. Soon after, building on results from that work, he reinterpreted quantum mechanics in terms of paths, translating a picture of particles in motion to a picture of world histories of particles, an over-all space-time point of view. The work for which he was awarded the Nobel Prize was a reinterpretation of quantum electrodynamics. In Eger's words,

> We could, with justification, call this man the 'master reinterpreter' of the physics of our time (Eger 1993b, p. 312).

Eger points to Feynman's work in order to argue that the boundary between reexpressing old knowledge and creating new knowledge can not always be drawn. Feynman's reinterpretation of quantum mechanics "revealed no new laws of nature, predicted no new effects" (Eger 1993b, p. 313), and Feynman himself expressed "a kind of regret for the enormous amount of physical reasoning and mathematical re-expression which ends by merely re-expressing what was previously known, although in a form which is much more efficient for the calculation of specific problems" (Feynman 1965, p. 176). Yet, this work contributed to Feynman's prize-winning reformulation of quantum electrodynamics and, as a technique, to a number of other areas in physics (Eger 1993b, p. 313). Interpretive efforts form a central and serious part of scientific work, and novelty in physics is not restricted to novel predictions.

#### 3.1.5.3 Hertz's new mechanics

Heinrich Hertz spent his last efforts, before dying at the age of 36 years, to reformulate classical mechanics so that the concept of 'force' would be superfluous. His version of the mechanics would, of course, be mathematically equivalent to Newton's mechanics, and so one may wonder at the motivation for these efforts. If he thought that "Maxwell's theory is Maxwell's system of equations," did he not also think that "Newton's theory is Newton's system of equations"?

The slogan about Maxwell's equations is misleading if taken as an assertion by Hertz that the conceptual aspects of the theory simply can be dispensed with. In fact Hertz spent enormous effort trying to clarify the physical pictures of Maxwell's *Treatise*, which were inconsistent, and to work out the conceptual differences between Maxwell's, Weber's and Helmholtz's electrodynamic theories. Working out these conceptual differences proved much more difficult than sorting out the degrees of mathematical disagreement, a fact that made British electrodynamics very difficult for German physicists to comprehend.

At any rate, after having carried out the experiments with electromagnetic waves, for which he is most famous, and having contributed significantly to disentangling electrodynamic theory, Hertz turned his focus to Newtonian mechanics. Not because there were pressing problems or anomalies in this science, but

> solely in order to rid myself of the oppressive feeling that to me its elements were not free from things obscure and unintelligible. What I have sought is not the only image of mechanics, nor yet the best image; I have only sought to find an *intelligible* image (Eger 1993b, p. 311. Eger's emphasis).

Hertz's reformulated mechanics was never used or followed up by physicists. It was complicated, and solved no new problems. However, its value in that it did "demonstrate convincingly the degree of interpretive freedom at the higher theoretical levels" (Eger 1993b, p. 311)was not lost, and Ludwig Wittgenstein lists Hertz's *Principles of Mechanics* among his influences. Another significant reader was Einstein, whose theory of general relativity is indebted to Hertz's work on mechanics.

#### 3.1.5.4 Formalism and interpretation—summary remarks

This section was to consider the role of physical interpretations in the development of physics. No simple conclusion about the interrelationship between formalism and physical picture can follow from all this. Hacking's formulation of the complexity of this relationship may stand instead:

> There are physical models ... There are mathematical structures. Both approaches have led to remarkable insights. According to one misleading cliché about late-nineteenth-century science, German physicists used primarily mathematical approaches while British ones made physical models. Both kinds of work collaborate, and both kinds of worker often uncovered almost the same facts in quite different ways. Moreover, on closer inspection most of the physical modelling, of for example Maxwell, turns out to involve abstract structures. Thus the elements of his statistical mechanics were not hard particles but mathematical differentials with no evident physical meaning. Conversely much of the applied mathematics in Germany hinged on description of plain physical models. These aspects of the human mind are not in general separable, but will continue to be permuted and altered in ways which we cannot foresee (Hacking 1983, p. 213).

### 3.1.6 Excursion: Pluralism and self-reference

Pluralism is a theory about the relationship between theory and truth, and hence must apply to itself. When the pluralist asserts that more than one theory may be valid and provide useful insights, that would imply that other, non-pluralist accounts of knowledge may also be valid or at least enlightening. Does that render the pluralist position incoherent? Is pluralism a self-defeating position?

Such issues of self-reference are important, difficult, and not uncommonly glossed over by educators arguing against the absolutism of school science. But whenever the authoritarianism of school physics is attacked with general assertions about the fleeting, tentative character of human knowledge, a sensitivity to self-referential implications is necessary. For example, historicist accounts that would portray scientific knowledge as very largely a product of socioeconomic or cultural time and circumstance run the risk of becoming paradoxical: the historicist positions themselves are of course also products of their time, so on what terms should they be taken seriously?

These questions will not be dealt with properly here, although they do deserve it. As for pluralism, it may perhaps escape the ironical position of self-referential incoherence. The pluralist does not admit truth status to *any* alternative account of knowledge and truth; it merely asserts that more than one account may be valid and/or enlightening. There will be limits to what theories can be accepted—as there are limits to what theories can be accepted in physics. I imagine that the pluralist can escape the contradiction of pluralistically accepting a theory about knowlege and truth that asserts that one account of the world is uniquely best. It is not, however, easy to get clear about this issue.

## 3.2 What does *understanding physics* mean?

When do we understand physics? What does it mean to understand anything at all? How is understanding different from acquaintance with, or mastery of, a subject—if it is different? What, if anything, is the difference between knowing that something is the case and understanding that it is the case? Or just understanding it? How does the answer to that question depend on what the subject matter is? Is understanding history different from understanding mathematics? If so, how, or why? Can computers understand anything? How could understanding be measured? (And, just by the way—what would it mean to get a grasp of what understanding is, to understand understanding, and thus have a result for the inquiry of this chapter?)

Questions of this kind will lurk near the surface whenever we assert that some or other approach in physics education will or will not tend to foster understanding of physical science in the students. While I by no means aim to answer—or even address—all the queries suggested above, an array of insights, more or less (though usually less) connected, from a variety of authors and philosophical directions, will be sketched. I will provide neither breadth of selection nor depth in presentation, but the topic is complex and encompasses a lot, and one can only start somewhere.

I will begin with a discussion of what understanding physics is *not*, then briefly describe several different contributions toward clarifying the notion of understanding. One approach to the problem of what understanding is, namely that of hermeneutic theory, merits a section of its own (section 3.3), and is deferred for later.

### 3.2.1 Performance without understanding

There is a great amount of research showing that many students who can do physics calculations, who master the formalism of the subject, are unable to analyze relatively simple cases qualitatively (Angell 1996, p. 69). They can correctly work out a numerical problem, but demonstrate misunderstandings when asked to give an account of the same problem in qualitative terms. Andrea diSessa, in interviewing a number of very successful physics undergraduates at MIT, found that they quickly lapsed into intuitive, everyday reasoning when asked to analyze slightly unfamiliar physical situations. Further, these students

> when asked to comment about their high school physics, almost universally declared that they could 'solve all the problems' ... but still felt they 'really didn't understand at all what was going on' (diSessa 1993, p. 206).

In diSessa's words, "They did not understand, even though they could perform" (p. 206).

There is also reason to believe that students frequently come to physics classes without *expecting* to achieve 'understanding' of physics or of the physical world. Prosser, Walker and Millar (1996) investigated a class of first-year university students' perceptions of learning physics, and found that

> Most students adopted a 'surface' approach to learning in terms of attending classes, reviewing notes, learning formulas and doing exercises. Few indicated that they were seeking understanding in terms of how the major principles worked or relating knowledge to real world experiences—a 'deep' approach. (Prosser et al. 1996, p. 47)

To the extent that choosing courses on the basis of 'interest' is indicative of an expectation to achieve 'understanding', further evidence of the above comes from Lie and Angell's (1990) survey of Norwegian high school students' reasons for choosing the courses they did. The survey showed that physics, more than any other subject, was studied for strategic reasons. Students did physics because it was perceived to be useful in relation to their future careers. The proportion of students indicating *interest*—a more personal motif—for their choice of course was considerably lower for physics than for practically any other subject[8] (Lie and Angell 1990, pp. 16–17).

But what is this 'understanding' that the students lack, and in part do not think of seeking? As Angell (1996) remarks, it is easier to say something about what understanding physics is *not*: it is not superficial reproduction and the mere ability to enter numbers into formulae. And while understanding physics certainly involves remembering laws, theories, concepts and experiments, it also has something to do with philosophy, world view, curiosity and wonder at the natural world. It also means being able to relate the physics course content to 'reality' around us (Angell 1996, pp. 67–9).

Here knowledge and attitudes that are—on the face of it—external and contextual with respect to physics are referred to in order to expound the notion of 'understanding physics'. I subscribe to this broader conception of understanding, and as will be seen there are a number of ways of arguing that 'understanding physics' should be understood in such a way.

### 3.2.2 Circling in the concept of 'understanding'

This section will deal with a miscellaneity of insights gleaned more or less haphazardly—or perhaps I could say serendipitously—from a variety of authors.

#### 3.2.2.1 Understanding and knowledge take different objects

Moravcsik (1979) notes that understanding does not take just any object. Thus, for example, while it makes sense to talk of understanding a proof, a person, or a theory, it makes no sense to talk of understanding a mountain, water, or gold. And while saying that we "understand water" violates our intuitions of what understanding is about, saying that we "understand what water is" is perfectly acceptable.

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[8] Only the students doing Oral English cited 'interest' as their major motivation less often than the physics students.

Moravcsik notes that understanding a proof involves a lot of propositional knowledge about the proof, about what its premises are and what rules are drawn on to construct the proof, and also a lot of know-how, or practical knowledge about how to go about setting up the proof (Moravcsik 1979, pp. 206–207). However, he argues in some detail (which I omit here) that the knowing-that and knowing-how involved does not, and cannot, exhaust what goes into *understanding* the proof. Understanding is more than knowledge, and what is required in addition to the knowledge involved—or even in part instead of this knowledge—is a representation, an intuition, of what the proof is about. Exactly what this representation is is hard to nail down, precisely, I suppose, because it is non-propositional in nature. But what is clear is that different persons can have different representations of the proof, different pictures, but that we would not therefore say that they understand the proof in different ways (p. 207). In the case of physics, Moravcsik sees the extensive use of modelling as just such a case of a representation being needed for understanding in addition to the propositional and practical knowledge of the theories.

Speculating further without following Moravcsik, one can muse at the fact that 'electrons', 'sparks' and 'attractive forces' hardly are proper objects of the verb "to understand" (talking of 'understanding electrons' sounds odd in much the same way that 'understanding water' does), but that 'electromagnetic theory', 'Faraday's experiments', and 'Weber's current model' can be understood or misunderstood. Then there seems to be a sort of intermediate category, including perhaps 'fields', 'equations', and 'circuits' that I am unsure of whether we 'understand' or rather 'have knowledge about'. While one may legitimately have reservations about how much insight is to be gained by just studying language usage, one may wonder whether it is a matter of chance that the appropriate objects of 'understanding' are human constructions (theories, models, interpretations), while what we perceive as independent physical entities rather are objects that we have knowledge *about*, knowledge that is integrated in our constructions concerning them. The entities in the intermediate category mentioned above seem also to be ontologically intermediate between 'physically independent' and 'humanly constructed'—or perhaps our decision on whether they can be 'understood' or not depends on our view of how physically independent they are. But this stays at the speculative level, and I do not know what Moravcsik thought, or would have thought, about the matter.

#### 3.2.2.2 Is understanding pictorial?

Moravcsik (section 3.2.2.1 above) suggested that understanding involves having a representation, perhaps a visualization (Moravcsik 1979, p. 207) of that which is understood. Others have also suggested that understanding physics is rather a graphic thing, involving an inner picture—preferably one suitable for inner simulations, or thought experiments. Thus, Nancy Nersessian (1995) emphasizes the role of Maxwell's elaborate mechanical pictures of electromagnetic phenomena, his mental models that he could 'run' to 'see' how the motion of one component would affect another.

How essential are visualizations in 20th century physics? As we saw above in sections 3.1.5.1 and 3.1.5.2, Dirac recommended some sort of emancipation from such inner pictures, arguing that the mental pictures would necessarily "introduce irrelevancies"—while Feynman, on the other hand, insisted that such pictures are indispensable and useful. Different persons probably rely on such images to different degrees. I am not sure that I would therefore conclude that they understand to different degrees. At any rate, in *learning* physics I think it is reasonable to link, if perhaps not equate, the acquisition of a clear and detailed visualization of the theory with coming to understand the theory.

Roland Wittje describes how 20th century physics aims at becoming liberated from the sensible and the picturable, at proceeding toward an abstract meta-level where visualizations are not needed. "Every picture that we attempt to make for ourselves of the entities is from the beginning bound to show itelf as being false" (Wittje 1996, p. 113)—and yet, for the learner, these images are necessary and valuable:

> In my personal experience these *false* pictures and analogies are of central importance in the process of understanding. I do not understand physics through abstract theories, but by means of picturable ["Anschauliche"] analogies and images, that nevertheless in turn must be discarded. (Wittje 1996, p. 113)

#### 3.2.2.3 What if nature is not intelligible?

*Can* nature be understood? Suppose what turns out to be true about nature makes no sense to us?

James Cushing (1991) raises these questions. He argues that scientific theories function on three levels: empirical adequacy, formal explanation, and understanding. A theory is empirically adequate if it provides correct numerical results: an equation providing the position of a planet at any given time would be an example. A theory that in addition gives some reason *why* the equation works also fulfils the second function, of *explanation*. Newton's gravitational distance force, by explaining why the planets move as they do, fulfils this criterion. Finally, a successful explanatory system may or may not be intelligible to us. If we are lucky, we may have "an interpretation of the formalism that allows us to comprehend and to know the character of the phenomena and of the explanation offered" (Cushing 1991, p. 338). But we may well instead have a coherent explanation with empirically confirmed predictions that we are *not* able to understand. Cushing insists that Newton's instantaneous distance forces are an example of a remarkably successful explanatory scheme that is not intelligible ("no one actually ever *understood* Newton's action at a distance" (Cushing 1991, p. 353)):

> Newton himself neither defended nor claimed to understand this concept [of instantanous action at a distance]. Those prior to him and many of his contemporaries found this incomprehensible as a physical mechanism. However, the formalism "explained" the data so well that action at a distance was essentially forgotten as a problem for two hundred years or so. The phrase "action at a distance" continued to be used but was not understood as a physical process. After Einstein's general theory of relativity, defending action at a distance would be considered ridiculous because a causal story replaced it. So, action at a distance had originally been a conceptual problem, was then effectively forgotten (as a worry) for two centuries and has finally been rejected as physically meaningless. However, action at a distance was never *understood.* This was a failed attempt at an intelligible explanatory discourse. General relativity provided a successful one. (Cushing 1991, p. 350)

Whether instantaneous action at a distance is comprehensible to us or not might be a contentious issue, and I will not pursue it further. What is important here is not to misunderstand the discussion as being about epistemology. What is at stake here is *not* the certainty or reliability of physical knowledge, of whether we can, for example, know whether there are instantanous distance forces or not. Cushing is *not* trying to say that a theory we can make sense of is more likely true. A successful explanation is as much as we can hope to get as far as truth is concerned. However, such an explanation just might have the bonus of fitting our psychology in such a way that it seems clear to us—or it just might not. But if we have an effective explanation, can we not come to 'understand' that explanation simply by being exposed to it, gradually adjusting our expectations about what can be in the world, until we achieve a sense of understanding? "It is, of course, possible that the only genuine difference between 'explanation' and 'understanding', as these therms are used above, is mere psychological acclimation", Cushing remarks, but he doubts this is the case: "While we ought not set up a priori criteria of intelligibility ... our own abilities to understand and to comprehend may be inherently limited" (Cushing 1991, p. 346).

What criteria are met by theories that we can 'understand', in Cushing's sense? His intuition is that

> based on experience and on (some) history of physics, ... *under-standing* of physical processes must involve picturable physical mechanisms and processes that can be pictured (Cushing 1991, p. 341).

Further,

> The paradigm of an explanation that *can* (or may) produce understanding ... for physical processes is a causal explanation, consisting either of direct cause-effect between phenomena and events or of a common cause located in the past of the collection of phenomena under consideration(Cushing 1991, p. 338).

Finally, understandable explanations must be susceptible to realist interpretation, in the sense of scientific realism. Understandable theories must be such that when taken literally, as really referring to the entities they invoke, they make sense to us:

> We may need theories that are *able* to be interpreted realistically in order to find them intelligible, even though we may not find such interpretations *justifiable* (or always even possible).

Cushing emphasizes that this is no argument for realism, it is still only an account of our psychology. No theory *requires* a realist rather than an instrumentalist interpretation, but our conceptual apparatus may be such that theories that *can* be taken literally are understandable to us. "That is, the great 'psychological' appeal of scientific realism may be, in large part, accounted for as satisfying our need for understandable explanations of physical phenomena" (Cushing 1991, p. 356, footnote 13)

Whatever the merits of the specific criteria of intelligibility that Cushing sets up, the issue of the fit between our psychology and the world may be worth keeping in mind in a discussion about education and understanding. If certain theories just are not comprehensible to us, while still being successful and at least better than any other we have, we may need to transmit these theories in the classroom anyway. Rather than aiming for impossible understanding, then, in such cases the greatest clarity may perhaps be achieved by *explicitly* addressing the issues of intelligibility, by discussing what aspects of a theory violate our notions about how the world can plausibly be, and why we nevertheless think that the theory is a good or valuable one.

## 3.3 'Understanding' according to ontological hermeneutics

In the humanities much attention has been given to the question about what understanding may be, and this work has developed into the broad, sprawling field of inquiry called 'hermeneutics'. That the issue of understanding has been more pressing in those disciplines than in the natural sciences is perhaps due to the fact that criteria of success *other* than achieved understanding are harder to come by in those disciplines. For example, in physics one may know that there is some correlation between two quantities without understanding how or why this correlation is there, or must be there. In interpreting an ancient text, on the other hand, there are no criteria of success other than—understanding the text. Hence the importance of being clear about what understanding amounts to.

At least, this is one way in which the difference between the natural sciences and the humanities has been construed. I am inclined to believe that both the importance of interpretation in the natural science *and* the occurence of non-interpretive knowledge in the humanities are underrated here, and that there is much more continuity of method and epistemology than this account would suggest. An argument for this position would carry us too far afield at this point, however, though a little of what there is to say both for and against it will surface in the course of the discussion of this section.

Hermeneutics, in any case, arose out of reflection around exegesis of ancient texts ("an unlikely source, if ever there was one, for an exciting philosophical trend" (Eger 1992, p. 338)), in the late 19th century. What is a text's true *meaning*? How can it be grasped? What kind of activity or process is *interpretation*? How can one guard oneself against *mis*interpretation? These are questions of hermeneutics.

In the first half of the 20th century, hermeneutics became coupled with the emerging field of philosphical phenomenology. A chief originator of this trend, Martin Heidegger, wrote about science that

> The existential conception understands science as a way of existence and thus as a mode of Being-in-the-world, which discovers or discloses either entities or Being. Yet a fully adequate existential Interpretation of science cannot be carried out until the *meaning of Being and the 'connection' between Being and truth have been clarified* in terms of the temporality of existence. (Heidegger 1962 [1927], p. 408, emphasis in original)

I do not understand what this means. The Encyclopedia Britannica Online considers Heidegger to be 'almost unreadable',[9] and while that is an exaggeration, he certainly does not make things very easy for the reader. Nor is he particularly concerned with science education. Why, then, look to phenomenology for educationally relevant clarifications of what understanding physics might mean? While Heidegger's style is difficult and his concerns at first glance irrelevant, the works of some scholars who have applied Heideggerian ideas to science and education turn out to be both comprehensible and astonishingly interesting. This section will be devoted to some major ideas in this secondary literature.

Admittedly, much of the literature on hermeneutics and phenomenology, especially that written by Heidegger and Gadamer, is far from clear to me. While some paragraphs seem to make clear sense, others remain opaque. In the light of hermeneutic theory, according to which

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[9] URL http://search.eb.com/eb/article?query=&ct=&eu=40654&tocid=3089#3089.toc, accessed August 3, 2003.

understanding is achieved when cycles of mutual corrections have eliminated tensions between whole and part (see section 3.3.2), I must admit doubts about having achieved a sufficient understanding of the excerpts I have read. For the parts whose meanings I do *not* grasp may conceivably, when understood, influence the meaning of the text as a whole in such ways that the light then shed by the whole on those parts that I now imagine to understand, may reveal my perception of these to be inadequate. Hermeneutics depicts the process of understanding as different from solving a jigsaw puzzle, in that parts can not conclusively be seen as fitting together before the whole is complete.

On the other hand, also according to hermeneutic theory, part of the process of coming to understand a text is the *application* of that text to one's own situation, the finding out what the text may *tell* the reader;

> ... understanding always involves something like the application of the text to be understood to the present situation of the interpreter (Gadamer 1979, p. 274).

In this spirit the presentation of hermeneutic theory will commence—in the context of an investigation of what it might mean for understanding science.

I will start out by listing some results of phenomenological thought that appear directly relevant to science education, in motivation for the perhaps puzzling and unfamiliar ideas of this section. Then, in the course of the ensuing exposition, I will try to show how these results emerge from an ontologico-hermeneutical account of science. This account offers perspectives on learning, understanding, and for that matter on our very being *and* that of science, that are deeply different from the familiar ones of analytic philosophy. After describing the hermeneutic circle, and discussing whether and how doing science involves breaking out of that circle, I will discuss how ontological hermeneutics redefines problems of relativism, subjectivity and objectivity by eliminating the subject/object cut. That discussion will expand on the argument that with the boundary between the subject and the object thus bracketed, the question of whether scientific meanings are located within our minds or in the external world loses its significance. In this respect ontological hermeneutics is an alternative to both the subjectivism of radical forms of constructivist theory, and to the commodity model of knowledge typical of traditional scientific objectivism.

### 3.3.1 Relevance for physics education

This section simply lists some conclusions that are relevant to science education, *without justification.* Arguments are provided in later sections. Many hermeneutically inclined authors would probably disagree with the claim that these points follow from hermeneutic thought in all cases. This list draws heavily on Martin Eger's writings, which I generally find both congenial and convincing, although other authorities in the field have expressed objections against his views.

- The activities of *studying* physics and *developing* physics, or between learning and doing research, can not be sharply drawn. Rather, learning should be construed as part of the being of science itself.

- Students' preconceptions about the phenomena, far from being the obstacles to learning that they are often supposed to be, are essential preconditions for the development of understanding. They have a positive role in the learning process.

- Students' preconceptions can be distinguished by their degree of groundedness in the lifeworld. This has implications for how the resulting *mis*conceptions of scientific concepts can most effectively be approached.

- If understanding is to be achieved, the physics learnt must make contact with the students' lifeworld. This includes their everyday experience, but is not limited to it. In many cases historical knowledge may bridge the gap between science and the lifeworld.

- Textbooks should generally *not* be read as self-sufficient introductions to physics. Textbooks must be regarded as dehydrated products that require interpretation to be brought to life, to fully *be*.

- The function of school experiments and demonstrations should not generally be taken to be 'proving' or 'illustrating' the theory. Nor is their purpose that students should discover the theory for themselves. School experiments are *performances*, in pretty much the sense of 'performances' of plays, and are necessary for the experiments to fully *be*, for them to be real *as* experiments.

- The aim of science is not limited to prediction, control and technical manipulation. Science can, and should, provide some understanding of the natural world—and of ourselves.

- The existence of a plurality of accounts of the same phenomenon does not necessarily mean that the phenomenon is poorly understood. Rath-er, a proliferation of accounts typically goes hand in hand with increased understanding.

- Constructivism as a theory of knowledge and learning, and hermeneutic theory of understanding and interpretation, overlap in significant ways. However, constructivism tends to emphasize processes inside the mind of the learner at the expense of the 'external' objects that the learner is seeking to know. Hermeneutic theory specifically gives more attention to that which is to be interpreted. Some subjectivistic features of constructivism can be avoided in this way.

## 3.3.2 The hermeneutic circle

'The hermeneutic circle of interpretation' routinely crops up in accounts of interpretation and understanding in the human sciences. It is also often called upon in order to show how the humanities are *different* from the natural sciences—the role of the hermeneutic circle is then applied as a criterion of demarcation of the humanities from the natural sciences. After describing what the hermeneutic circle is about I will nevertheless discuss how it applies in the natural sciences, and raise the question of how the applicability of hermeneutics to science affects the objectivity of science. The discussion of these problems will point to the following section, on ontological hermeneutics, which solves many of these problems.

### 3.3.2.1 Two versions of the circle

There are two descriptions of the hermeneutic circle; they may be taken as interchangeable (Taylor 1987, p. 36).

There is the circle of reformulation of meaning. In trying to interpret a text that we do not immediately understand, we are trying to find ways of saying the same thing as the text does, but in a way that makes the original text seem less confused, unclear or puzzling. While in the original reading the text was cloudy and obscure, if our interpretation was successful, the second reading is less so. But if our new formulation is of the *same* text, if it says the same thing, may not another reader find it as mystifying as the first? How can we convince a critic that our reading is reasonable? "The answer, it would seem, can only be more of the same" (Taylor 1987, p. 36), another reading of the text, a further reformulation. This fact that in interpretating a text we can only move from one formulation to another supposedly equivalent one, while our argument for the identity of the meanings in the two versions can only be yet another reading, is one way of expressing what the hermeneutic circle is about.

There is also the circle of projection and part. This account casts the circle as one of motion between whole and part in a text. The problem of finding the meaning of a text lies in the fact that the meaning of individual words and sentences will depend on the meaning of the text as a whole, and the meaning of the whole on the individual words. How can an interpretation ever get started? The interpreter has to guess, or *project*, or 'throw out ahead' a meaning for the text as a whole, and then test this preconceived notion by checking whether the parts make sense in the light of this conjectured whole. Sometimes the very first words will persuade the reader to revise the preliminary idea of what the text means, and to proceed with the reading with this corrected pre-judgement. In other cases close reading, careful attention to the parts, is necessary in order to find out whether the parts harmonize with the projected whole. The back-and-forth movement between whole and part, and the mutual adjustment of each, ideally terminates at a point where no more corrections are required in order to bring whole and parts into agreement, and understanding of the text is then achieved. (Eger 1992, p. 338).

Note that only coherence of the text, a mutual fit between whole and part, can be achieved. There is no independent way of checking the meaning of parts or whole except by returning to the text. From this we see the agreement between the two accounts of the circle.

#### 3.3.2.2 Breaking out of the circle

Can we 'break out of the circle'? That is, can we do more than found one interpretation on further interpretation? The natural sciences have been seen as doing just that. Empiricists have argued that the basic building blocks of scientific knowledge are sense impressions or 'brute data', which have "by definition no element in [them] of reading or interpretation" (Taylor 1987, p. 37). That is, the validity of the data cannot be questioned by offering another interpretation or reading (p. 38).

Note that such a view of science is compatible with an awareness of the underdetermination of theory by data—though the conclusion is that the aims of science are instrumentalist in character. "As for the surplus meaning in a theory which could not be rigorously co-ordinated with brute data, it was considered [by logical empiricism] to be quite outside the logic of verification" (Taylor 1987, p. 38).

The difference between the natural sciences and the humanities is often construed in terms of whether or not appeals to readings or judgement can be disposed of. It is thought that the humanities require hermeneutic method, interpretation, and a dialogue with that which is to be investigated. These approaches correspond to the *aim* of these fields of inquiry, which is *understanding*. The natural sciences require no such method, as their characteristic aim is *explanation*, it is technical mastery, and knowledge of fixed laws and regularities, not understanding. Pioneer hermeneutical philosophers such as Heidegger and Gadamer have agreed with classical sociologists on this image of science. These philosophers

> take the aim of natural science to be essentially pragmatic, ordered to the control and manipulation of people and things, its characteristic method being the construction of theoretical model systems referring to radically imperceptible elements which do not, and cannot, have a place in any World[10]; scientific models of this kind, they say, are not more than surrogates for Nature. (Heelan 1983, p. 186)

Such use of the hermeneutic circle as a criterion of demarcation between natural science and the humanities reveals both the perceived attractions and losses of 'breaking out of the circle'. It also suggests why representatives of the humanities have admired the results of natural science, while simultaneously vigorously rejecting any notion that they should copy its methods, and jealously guarding their territory against 'scientism'. Inside the circle, objectivity in any strong sense is precluded. But only inside the circle is *understanding* and meaning to be had, and some sort of connectedness between the investigator and that which is studied. Outside the circle there is sheer detached manipulation of objects that are not intelligible. The image is one of alienation.

Is this image of science a necessary one? Martin Eger suggests otherwise in rhetorically asking whether it is "possible for natural science itself to be afflicted by scientism?" (Eger 1992, p. 344). As we shall see, there is much more to be said about the role of hermeneutics and the character of the natural sciences. Since 1962, when Thomas Kuhn's "The Structure of Scientific Revolutions" was published, developments within the sociology and philosophy of science have "thoroughly exploded" (Siegel 1988, p. 91) the image of science as a linearly progressive, purely rational, all but algorithmic systematization of brute data, and have highlighted interpretive aspects of doing science. It has been shown that in the natural sciences too, the knowledge 'produced' in some or other way is shaped and coloured by the approach taken by the scientist, by her projection of that which is under study. On the other hand, the broadening of traditional hermeneutics to deal not only with the study of written texts, but also with the study of other symbolic material, has to some extent invited attempts at applying hermeneutics to the context of understanding science.

#### 3.3.2.3 Importance of the fore-structure

In classical hermeneutics it was thought that arriving at the right meaning of a text was a matter of skill and perseverance: if the interpreter were sufficiently clever and open-minded, the circle of projections and corrections would eventually *converge* on the true sense of the text. Of course, real persons might not be perfect in terms of intelligence, open-mindedness or persistence, but that is true of logicians going about their subject as well; the important thing was that there were no *in-principle* obstacles to finding a unique best interpretation. This view was abandoned in the course of the 20th century (Eger 1993a, pp. 5–7).

The early 20th century saw a dramatic broadening of the *scope* of hermeneutics. Strong parallels to the 'hermenetic circle' in textual interpretation were discovered in many other situations as well: "in . . . trying to understand a foreign culture, in law, in art, and quite generelly whenever someone is trying to tell us something unfamiliar or complex" (Eger 1992, p. 339). By the time Hans-Georg Gadamer's major work in hermeneutics, *Truth and Method*, was published in 1960, the scope of hermeneutic philosophy had widened to the extent that

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[10] 'World' corresponds to the 'lifeworld' discussed in section 3.3.2.3

Gadamer called "the whole human experience" hermeneutical, "for the process we are describing is repeated continually throughout our familiar experience" (quoted in Eger 1992, p. 339).

Some time before this it had been realized that finding *the* true interpretation was not as straightforward as had been supposed. It could not be taken for granted that the hermeneutic circle would *converge* on one single meaning regardless of the interpreter's starting point. Rather,

> an irreducible contribution is made by the interpreter through the approach he takes, especially as it shapes the conceptual orientation (Eger 1992, p. 339).

Further, not only conscious, articulated and examinable pre-judgements come into play in the interpretation of a text or text-equivalent. Expectations grounded in the the "lifeworld", in the "pre-theoretical, pre-predicative, pre-conceptual activity that is prior in our thinking" (Heelan 1998, p. 278) also come into play. In fact, all comprehensible projections must somehow draw on this sphere of immediate experiences and unreflective activity of the everyday world. And,

> [by] relating knowledge first to the pre-scientific world of practice, to the depths of human existence itself, then to the inherited language, philosophical hermeneutics closed off for itself any hope of objectivity in the absolute sense (Eger 1993a, p. 8).

The contribution from the interpreter, then, can not be subtracted or abstracted from what is known.

On the other hand, this fore-structure that shapes knowledge is what make it possible to get a grip of the object in the first place. It is what makes it possible for interpretation and hence understanding to get underway at all. In Höttecke's words,

> Having a preconception, pre-judice, intuitive idea (or whatever one may wish to call it) is a precondition of, and not an obstacle to, understanding. (Höttecke 2001, p. 161)

The fore-structure is what makes the known something *we* can know, and not merely something hypothetically knowable from some hypothetical God's eye perspective. While we cannot rise above the bias due to our lifeworld background and pre-reflective practices, "because of its *positive* role, it is now clear that we cannot wish to do so" (Eger 1993a, p. 9).

These developments in hermeneutic theory have striking parallels in the recent history of the philosophy of science (Eger 1993a, pp. 8–11). The role played by hypotheses in Karl Popper's falsificationist theory of science, where knowledge always starts with a hypothesis to be tested against nature, is closely analogous to the function of projections of a text's meaning. The radicalization of philosophy of science due to Thomas Kuhn and others also has its counterpart in hermeneutic theory: Their emphasis on tacit knowledge, paradigms, styles of thinking, and non-propositional *skills* mirror the importance assigned to the 'lifeworld' in hermeneutic philosophy. In both cases a serious re-evaluation of the possibility of secure knowledge has been the result.

A surprising reply to these skeptical trends comes from within hermeneutics itself, with the rise of *ontological* hermeneutics. This direction of thought recasts the whole subject-object-relation in a way that has implications for all questions of 'subjectivity' or 'objectivity' of scientific knowledge. Before describing how this happens, in section 3.3.3.3, we need a grasp of hermeneutic aspects of natural science.

#### 3.3.2.4 Science as texts and signs

How can hermeneutics be relevant to natural science? Objecting to the idea that science is not about interpreting meaningful material, Eger points out that in *education*, at least,

> it is not nature itself but a *language of nature* that one encounters initially (Eger 1993a, p. 2).

The student of physics is engaged with *texts* rather than with nature directly, and spends most of her study time learning the symbols and representations of a human-made language for speaking about the natural world.

This is true also at rather advanced levels of inquiry in physics. Even in 'doing science', well beyond the stage of formal learning, the researcher is to a large extent preoccupied with studying texts, seeking knowledge through their interpretation. The discussion of section 3.1 showed the importance of re-interpretations of established knowledge in physics. Such reformulations are often motivated not by any perceived deficiencies of the theory with respect to empirical adequacy; frequently "the impulse to interpret arises from a *vague unease*, a lack of *understanding*". The study of signs, of textual expressions of relatively unproblematic and accepted knowledge, is interpretive at all levels, from school physics to research. Hermeneutic activity of this kind is pervasive in science.

So much for the study of physics *texts*. Höttecke sees *experiments*, as the construction and interpretation of signs, to be hermeneutic in character (Höttecke 2001, pp. 163–172). The 'Book of Nature' is written in laboratories, where scientists use devices such as counters, oscillographs and plotters to produce signs whose interpretation then becomes the focus of scientific discussion. Deciding what the graphs and plots produced signify is a hermeneutic process, involving the complex mutual fitting of the part (the sign under study) and the whole (an overview of what the relevant experiment is supposed to provide insight into). Sometimes signs or signals from something not immediately accessible to the senses are translated into other signs several times before interpretation by physicists, as when, for example, information about a collision in a particle accelerator, processed by detectors and computers, becomes the subject of study in the form of numbers and graphs. Understanding the resulting sign requires some understanding of the transformation processes it has gone through, and these embody accepted theories of that which is under study.

But such tracking of causal connections, is that *interpretation*? Is knowing how and why a certain "click" of the Geiger-Müller counter was caused by a passing particle not different from understanding a sign warning that the paint is wet? The former is the end product of a causal chain, the latter is a sign made by a person to express a meaning.

In a detailed article titled "Natural Science as a Hermeneutic of Instrumentation" (1983), Patrick Heelan argues that the reading of instruments can indeed be hermeneutical. For one thing, the fact that the natural world neither speaks nor acts is no objection against the hermeneutic character of experimentation. Nature 'writes' the 'texts'[11] of experimental science, by interacting with a standard instrument (Heelan 1983, p. 193). In literary analysis, settling the meaning of a text does not necessarily mean reading the mind or intentions of the author of the text, for understanding a text may just involve finding an interpretation typical and reasonable in the context in which the text was written. Similarly,

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[11] Heelan introduces 'text', 'read', and 'write' as technical terms for entities and activities that are analogous to texts, reading and writing in a sense to be described below.

> [a 'sign' produced in the laboratory] can have a meaning apart from any implications that Nature has a mind, though not apart from the cultural circumstances in which the 'text' [or sign] is produced (Heelan 1983, p. 188).

Secondly, the differences between man-made, meaningful signs and physically caused instrumental readings do not translate into differences in kind of *interpretation* of the signs. Heelan draws on the analogy between reading a text and 'reading' a 'text'. He lets 'texts' refer to text-like materials which an experienced person can 'read' with all the ease and immediacy with which texts in a familiar language are read.[12] "Reading in the ordinary sense is a form of direct knowledge which presupposes a text which is an artifact of human culture" (Heelan 1983, p 184). It is this directness with which we see what a text is telling us, usually without even noticing the print on the paper (p. 190), that Heelan wants to point out in our interpretation of 'texts'. It is not important for the ability to 'read' such a 'text' that we are aware of the causal connections between the instrument read and the physical state that the 'text' represents. For example, we can 'read' the temperature off a thermometer without knowing any thermodynamic theory (p. 192). This fact makes the analogy between the signs produced by an instrument, causally produced by the state we want to know, and symbols made by humans to tell something, as close as it need be for experimentation to be hermeneutical. It is also important to note that the "click" og the Geiger-Müller counter is not a *sign* independently of human purpose—as a mere causal effect without human interest it is just noise. But when taken as a sign and 'read' successfully, it provides direct knowledge of a passing particle.

Heelan's thorough account of instrumentation is not easily summed up i a few paragraphs, and here clarity has undeniably been sacrificed for brevity. Some of his ideas will be cursorily revisited in section 3.3.3.5. Hopefully the case for the hermeneutic character of both physical theory and experimentation still does not seem too contrived after this account.

### 3.3.3 Phenomenology and ontological hermeneutics

The aim of this section is to show what learning physics as well as doing research might look like from the perspective of ontological hermeneutics. As we will see, ontological hermeneutics is closely intertwined with phenomenology, and 'ontological hermeneutics' and 'phenomenological hermeneutics' appear to be used somewhat interchangeably. These directions of thought bracket the subject/object cut and attempt to see human beings as embedded in the world in a certain sense. 'Understanding' is seen as a mode of being-in the world.

One possible virtue of this picture is that it avoids a somewhat claustrophobic image of learning and research due to at least some versions of empiricism and constructivism. These construe the learning process as one in which the subject, confined within its own skull, grapples with constructing a theory that somehow fits with the sense impressions that penetrate into its mind. Phenomenology, on the other hand, tries to "envisage a world from which the human being, as a subject, is not always excluded" (Eger 1993a, pp. 303–4).

Another possible virtue is its anti-relativist aspect. The empiricist and radical constructivist picture sketched above not only makes the learner an outsider to its world—it also admits skepticism about the possibility of knowledge of that world. How can access to it ever be achieved? Whatever knowledge can be had must, it would seem, be of a purely instrumental

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[12] Not all scientific instruments can be 'read' in this way, or are what Heelan calls "readable technologies".

character, concerned with prediction and control of sense impressions—there can be no talk of *understanding*. In contrast,

> [the ontological turn in hermeneutics] is the feature that limits, counterbalances, and places in proper perspective that subjectivism and relativism with which hermeneutics is often associated (Eger 1993a, p. 11).

These ideas, then, will be fleshed out somewhat in this section. A detour into phenomenology is necessary by way of introduction.

#### 3.3.3.1 Phenomenology

The current of *phenomenology* arose toward the end of the 19th century, in reaction to the sudden growth of abstraction and the accelerating fragmentation of disciplines, which had brought about a 'loss of meaning' in knowledge:

> Phenomenology, in its broadest sense, is that approach in science and philosophy which tries to stay close to the phenomena by avoiding as much as possible all abstraction and imposition of constructs, and by relating always the object of study to the experiences of the subject who does the studying (Eger 1993b, p. 303).

Phenomenology insists on describing our experiences as they most immediately present themselves to us. The case of human speech, a favorite example of Hubert Dreyfus's[13] may illustrate the characteristic approach of phenomenology. When somebody talks to us, an empiricist and an analytic philosopher would have it that what we see or sense is this: An orifice opens up in the other person's head, and an accoustic blast comes out. The problem, then, is to account for how it can be that we partition this blast into smaller units and recognize them as words and sentences. How do we ever manage to assign meaning to this noise? The phenomenologist sees this description of what is going on as misguided from the outset. We hear no such thing as an 'accoustic blast'. Our direct, immediate and pre-reflective experience is one of meaningful speech. Often we are not even aware of the individual words, but only of what the other person is *telling* us. This, experientially and phenomenologically, is our starting point, is what is primary. The account of the accoustic blast is derivative. From the meaningful speech we can perhaps somehow arrive at the question of how sounds and meaningful words are coordinated. The idea is that empiricists have mixed up what is to be explained with the empiricist explanation (Heelan 1983, p. 190). Unlike what empiricists would say, the basic unit of experience is a perceptual object, not 'organizations of sensations' (p. 190).

While phenomenology aims at non-theoretical description of experience, it is no anti-theoretical direction. Edmund Husserl, who led the development of the phenomenological approach, was a mathematician who "had a deep appreciation for mathematics and natural science . . . [His] objection was not to science itself, but to the Galilean assumption that the ontology of nature could be provided by mathematics alone, bypassing the life-world" (Crease 1997, p. 259). Further, phenomenology is no empiricist rejection of the possibility of knowledge about what cannot be seen with the naked eye; for example, Patrick Heelan argues that electrons may have the appearance of perceptual objects in the life-world (Heelan 1998, p. 290). That is, in a reasonable sense, given suitable equipment and skills, we can and do *see* electrons

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[13] See the lectures on his introductory Heidegger-course at UC Berkeley, available on http://socrates.berkeley.edu/~hdreyfus/185_s04/html/lectures_185_s04.html through spring 2004.

and experience their presence directly. More will be said in section 3.3.3.5 about how the idea of perceiving theoretical objects is less complicated for a phenomenologist than for an empiricist.

What about *knowledge* of the objects of experience? Husserl introduced the 'variation of profiles', a distinctive procedure of phenomenology:

> The object of attention is viewed by the subject from different angles, so to speak, against different backgrounds and in different contexts. Each viewpoint affords the subject a different 'profile' of the object; and it is only by examining many such profiles that a conception of the essence of the object is attained. (Eger 1993b, p. 304)

There are constraints on these profiles. On the one hand they must, of course, be faithful to the object. On the other hand they must be meaningful to the interpreter. The phenomenological imperative is to stay close to the phenomena as they show up in our experience or imagination,

> One 'interprets' the meaning of the thing from such-and-such a viewpoint in such-and-such a context, by relating it to the background, ... one does not postulate pure concepts or invisible entities in terms of which the thing may be understood (Eger 1993b, p. 304).

Thus the requirement that understanding be *for us* is ensured by the emphasis on interpretation of the object "in terms of contexts, of experiences, and of specific standpoints", and by means of "metaphorical and analogical descriptions" (Eger 1993b, p. 304). The requirement that understanding be *of the object* is fulfilled by sampling a plurality of such profiles—for

> examining the object from several viewpoints allows the object to reveal its own outline (Eger 1995, p. 175).

In this way, pluralism and understanding are intimately connected.

#### 3.3.3.2 Ontological hermeneutics

*Ontology* is the study of *being*, of what it means that 'something' *is*, and of the modes or ways of being. Some ontological issues present themselves as soon as we start listing a number of ontologically diverse entities—stones, tables, fears, numbers, angels, proportionalities, quarks, virtues, selections, misunderstandings, for example. These entities *are* in different ways. What kinds of things these entities 'are' (if "things" is the right word at all; perhaps that word already carries more ontological assumptions than we would like for now?) certainly is a matter on which there can be disagreement. What *is* a number? An angel? *Is* a rock in the same way that a misunderstanding *is*? Probably not, and that is what ontology is about.

Phenomenological ontology is what we get if we inquire phenomenologically about what *is* in the world, when we consider the being of entities that we immediately experience *as we experience them.*

Heidegger and his followers draw our attention to the differences between 'presence-at-hand' and 'readiness-to-hand' as *modes of being.* The former is the mode of being of trees, stones, stars and other entities that 'just are there', and are what they are independently of our relation to them. Other entities, such as pencils, chairs and hammers, and generally 'equipment' in a rather broad sense, are not what they are without their human purpose or function.

Thus a chair of course *is* some entity (a slab with three or four cylinders attached, say) independently of our concerns—but it is not a *chair* except through its perceived availability for sitting on. The *being* of a chair as a *chair*, then, is something relational, something constituted in a relation between chair and human, and this mode of being of the chair Heidegger calls 'ready-to-hand'.

A third significant mode of being is that of 'being-there', or Da-sein as Heidegger's German construction is usually retained in English translations. Being-there is *our* mode of being, in that we are there, in the world, with and involved with entities surrounding us. It is a mode of being that allows other entities to 'show up' for us. The concept of Da-sein is not an easy one, and I will not try to elucidate it much further. In this context the interesting thing is that Heideggerian ontology casts a different light on *understanding*, of what sort of an entity that is, and on what is going on when we interpret something. The crux of the story is the way in which the cut between the subject doing the studying and the object under study is moved, or even removed. How this is done is the topic of the next section.

#### 3.3.3.3 Dissolving the subject-object cut

Descartes is generally cited as the originator of the subject-object distinction in the form we take it for granted. Starting from "I think, therefore I am", his project was to demonstrate the existence of an external world, and further a number of propositions about it. Phenomenology questions what may seem obvious in asking whether we might not be able to describe experience *without* recourse to a fixed boundary between our thinking selves and the world around us—whether, in fact, a description of immediate experience is not actually free from such Cartesian language. How can this be done?

Adding a bit of detail to the description of the mode of being labeled "readiness-to-hand" above is one way of starting off. It was noted earlier that the being of 'equipment' in a broad sense, of chairs, pencils and hammers, say, is not a self-sufficient and independent kind of being, but depends on human practices and concerns. For example, a hammer is not a hammer apart from its function and apart from a referential totality comprising such things as nails and timber. 'In itself' the hammer is only an oblong piece of wood with a lump of metal attached. More surprisingly, perhaps, *our* being in turn is bound up with that of such equipment. We may, for example, ask whether a carpenter can *be*, be a *carpenter*, except given certain connections and relations with the equipment of carpentry, and the answer given by phenomenology is that he can not. Hence 'being a carpenter' without reference to a referential context that involves hammers, nails and timber makes no sense, and *being* a carpenter means being part of a whole that involves humans and equipment in certain relations to each other. Saying that "the carpenter uses the hammer" somehow misconstrues the situation inasmuch as the carpenter would not *be* a carpenter without the hammer, nor would the hammer *be* a hammer (as opposed to some oblong piece of wood with a blob of metal attached) if it were not for its place in the equipmental totality of carpentry. One might as well say that the hammer uses the carpenter—at least, the hammer is there through the carpenter's handling it for hammering.

Is this account a compelling alternative? I don't know—but then it would be contrary to the spirit of hermeneutics to demand that an account be the single, exclusive right one in order for it to be enlightening—for, "to insist that differing interpretations matter only when they conflict, is to miss the whole point of philosophical hermeneutics" (Eger 1992, p. 346).

For some reason, probably the fact that Heidegger used it first, the hammer example is typically used by way of introduction to the idea of the dissolution of the subject-object cut. There are other examples, however, that in my opinion are more clarifying, illustrative and convincing. The following two sections sketch two broad metaphors of science that show how the being of *science* is not the being of something present-at-hand, of something that exists self-sufficiently, and that the boundary between scientist and science is not something clearly cut. This has implications for what learning and doing science is conceived to be about.

Before going on to the mentioned metaphors of science, we may take a brief look at a first-hand account of doing research. Eger quotes several passages from Evelyn Fox Keller's biography of the Barbara McClintock, who won a Nobel Prize in 1983 for her pioneering work in genetics. In this excerpt, McClintock describes her experience of being absorbed in this work:

> I found that the more I worked with them [chromosomes] the bigger and bigger they got, and when I was really working with them *I wasn't outside, I was down there, I was part of the system.* ... I was even able to see the internal parts of the chromosomes. ... As you look at these things, they become part of you. And you forget yourself. The main thing is that you forget yourself. (Cited in (Eger 1993a, p. 16))

"Far from being a mystical experience", Eger comments, "what is described here is simultaneously a state of cognitive detachment and attachment: detachment from the everyday self ... required for an attachment to the object or to the instrument that brings one closer to the object—in McClintock's case, the microscope" (p. 16). Research, on this account, can experientially be being-with, being-there, having direct contact with that under study, being open to the world. This connectedness, where the subject/object cut is far from awareness, is an aspect of the phenomenology of understanding.

#### 3.3.3.4 Science as performance

One picture of science that avoids the subject/object cut stems from an application to physics of Gadamer's treatment of performing arts, an application arrived at independently by Martin Eger and Robert Crease (Eger 1995, p. 183). In asking what a certain play or a certain musical composition *is*, it is clear that the script of the play or the score of music is not the whole answer to that question ("the notation *is* not music" (Eger 1993b, p. 315). In order to fully *be* as a play or as music it must be performed. But "if these art objects must be performed to actually *be there*, then their very *being* may *include* their performance."

Since all performances are interpretive, and the possible performances myriad, any actual performance also tells something about those who interpret and perform the script or the score. So a piece of performative art is not a self-contained entity 'out there', although its features are constrained by a permanent, objective 'core'—the script or the score. Those who interpret and perform are also involved in its being, and so in such art the 'cut' between subject and object is just not clear.

Eger argues that this picture is largely valid for physics, theoretical and experimental, as well. We do not worry about the fact that we never get access to the *true Hamlet, unmediated by interpretive performances*—such a complaint would involve a misunderstanding of what a play *is*, of the kind of *being* of theatre arts. Analogously, if we fret that our scientific knowledge does not provide 'bare facts', we have on this account misconstrued the nature of knowledge, the *being* of science.

In the case of theoretical physics the 'core' may consist of a formalism, say—but interpreting and reinterpreting this core is a central and serious scientific activity. Far from being optional embellishments, the interpretations are part of the being of science, and we never understand the 'core' except through some interpretation. While for example Einstein's postulate of the constancy of the speed of light, and Minkowski's four-space formulation of relativity, are 'mere' reinterpretations of Lorentz contractions, we do consider Einstein's and Minkowski's reconceptualizations to be genuine and novel contributions to science. The choices we make between the myriad possible representations inevitably also tell something about ourselves—about what we find intelligible, for example. The 'works of science', then, are neither inside our minds nor independent objects out there, but both interpreting scientists and some interpretative core are involved in their being.

In the case of experiments, that also are 'works of science', in Eger's terminology, it is clearer yet that their being involves performance. A description of, or a guideline for, an experiment *is* not an *experiment.* And again, the meaning of an experiment is underdetermined by any set of instructions for carrying it out, for no list of guidelines can uniquely determine all the myriad details of manipulation and emphasis that go into performing the experiment.

This ontology of experiment provides a fresh perspective on the laboratory exercises of science education. Often historical experiments are 'repeated' in classrooms—comparing the periods of pendulums with different lengths and masses is one classical example. On the phenomenologico-ontological account, the reperformance of this experiment is not primarily an illustration of the theory of mathematical pendulums, nor an open investigation into the character of pendulums—it is one of the myriad performances that constitute the *being* of that experiment. One consequence of this view that the students in doing their labwork are participating in the being of the experiment is that we could, somewhat curiously perhaps, say that the *experiment* is 'using' the students to be performed—another expression of the questionable character of the subject/object cut as traditionally conceived.

#### 3.3.3.5 'Putting on' or 'entering into' natural entities

A second picture of science that shows how the subject/object cut is complicated is the one of 'embodiment'. Eger, drawing on writings by Heelan and Polanyi, works out the idea that scientists, in preparing for doing experimental work, *enter into* their subject matter, frequently in a quite literal, physical sense.

The apparatus and instruments used by a scientist, once the scientist is trained in using them, drop from his or her awareness, and are as little a part of experience as are our hands and eyes when we are absorbed in successfully watching or handling something. To that extent the equipment, "by extending his perceptual reach, in effect function in the manner of an artificial body" (Eger 1993b, p. 307). Inhabiting such an extended body is the *mode of being* of the modern scientist *qua* scientist, for only by means of such a 'body' can the scientific view of nature come into focus (Eger 1993b, p. 307).

Importantly, the scientist's extended body is part of the being of science itself, as a space suit is part of astronautics. For the instruments used are not arbitrarily chosen, but already 'embody' partial knowledge of that which is under investigation (Eger 1993b, p. 308). What is *incorporated*, so to speak, in the 'extended body', are previously accessed features of whatever is under study. For example, for investigating entities of a certain size and character, a microscope of a certain kind will be chosen, and for exploring a place with a certain range of temperature and pressure, a space suit with certain properties will be chosen. Because of this sort of relationship between the equipment the scientist works with—or in—and whatever is to be investigated, the scientist can be seen as 'entering into' the realm of entities under study by learning to cope with the equipment that provides access to them. As the researcher's acquaintance with the entities thus made contact with increases, the possibility of making more suitable instruments arises. As the scientist in turn becomes familiar with these, to the point where they drop from awareness, they also—phenomenologically speaking—become part of the scientist's body. They become transparent, like windows on the world (Heelan 1983, p. 190), bringing the entities studied even closer. And so on. In this way, the cut between the subject and the object is moved.

Note that this account need not be limited to experimentation. Eger speaks of "cognitive tools like theories and models" as the "software of a scientist's profession" (Eger 1993b, p. 307)—they also extend the scientist's perceptual reach and provide access to entities in the world, and they are also part of the scientist *qua* scientist, in a natural, un-selfconscious way, like language. The important thing is how theory and instruments bring us closer to the entities we wish to know:

> To *inhabit*, as it were, the structured world consisting partly of experimental hardware, partly of the software of scientific tradition, partly of one's own emerging addition to that software—and from the center of such an interpreting system ('extended vision') to find oneself 'seeing' or 'touching' something 'out there'—can be taken as an *ontological* state in which the subject, no longer the human alone, is the interpretative system as a whole, *tuned* to something 'other' than itself. (Eger 1993a, p. 17)

Here it might be in order to briefly revisit a difference between phenomenological hermeneutics and certain versions of constructivism and empiricism, introduced in section 3.3.3.1. It is the place it affords for 'touching something out there' that, according to Eger, is an advantage of phenomenological hermeneutics over constructivism, which rather provides a picture of "dismal isolation, of the student [or researcher] as hopeless outsider" with respect to the universe (Eger 1993a, footnote 37, p. 28). If the subject/object cut is left intact, we have the problem of accounting for how we can build the complex and extensive knowledge we have of the 'external world' from mere sense impressions and speculation, and these sources of information seem somewhat meagre compared to the knowledge we do think that we have. On the other hand, on the phenomenological account, observing an electron with appropriate equipment is not in principle different from observing billiard balls with the naked eye. In neither case do we need to know the mechanisms that cause our impressions. The deciding factor is the immediacy and accuracy of our observations, and allowing for an extension of the subject toward the object, these need not be lesser in the case of observation of electrons. Much more detail of this argument can be found in Heelan's (1983) article on science as a hermeneutic of instrumentation (Heelan 1983, pp. 289–90).

## 3.3.4 Physics education and hermeneutics

### 3.3.4.1 'Misconceptions' and the lifeworld

Eger (1992) sees evidence of an orientation toward hermeneutic thinking in the literature of the 1980's on 'misconceptions' (p. 342). But while a good deal of the literature on 'misconceptions' emphasizes that students must be confronted with the inadequacy of their preconceived ideas, so that they will see that the scientific accounts are superior, hermeneutic theory accentuates the positive role of *prejudice*—in the literal, neutral sense of *pre-judgement*—in the process of interpretation. Any attempt at understanding a text must start with checking some *preconceived* notion of what the text as a whole is about against the text's parts. In Höttecke's words,

> . . . the conceptions (*Vorstellungen*) and concepts already possessed by the learners constitute the platform from which the process of understanding departs. Having a preconception, pre-judice, intuitive idea (or whatever one may wish to call it) is a precondition of, and not an obstacle to, understanding. (Höttecke 2001, p. 161)

The perspective of hermeneutics on this point harmonizes well with evolutionary varieties of constructivist accounts of learning—and also with a recommendation based on diSessa's (1993) very different theoretical approach to 'understanding'. diSessa emphasizes that physics teaching must engage students' naive 'sense of mechanism,' so as not to build a wall between prior knowledge and disciplinary knowledge. Students' naive physics becomes a part of expert knowledge, and is not to be abandoned (diSessa 1993).

Eger argues that hermeneutic categories could prove valuable in that they differentiate between different *kinds* of 'misconceptions'. For instance, "[h]ermeneutic categories distinguish *preconceptions due to experience in the life-world* from other kinds" (Eger 1992, p. 343, emphasis added). The common notion that the seasons are due to variations in the distance between the earth and the sun, and the nearly universal, and highly recalcitrant, idea that the acceleration of an object at rest must be zero, in hermeneutic terms belong to different classes of conceptions. The former is a case of a 'bad theory' that the learner may willingly trade for another theory once it is pointed out that south of the equator the seasons are reversed. The latter, on the other hand, is a case of a conception formed prior to scientific reflection, originating in the life-world where 'acceleration' is learnt and used in cases where there is motion.

In order to make sense of such a phrase as 'the acceleration of a body at rest' at all, a complex *extension of the concept* of 'acceleration' must take place, in an interpretive movement back and forth between the *part* (the kinematical case under study) and the *whole* (the language of science with its limiting process, its laws of motion, and the rest). It should be expected that several different formulations, examples and analogies may be required before the student can grasp or relate to this acceleration of bodies at rest. In this case "hermeneutics suggests at least this: since the student's use of the word 'acceleration' was not really 'wrong' within his own horizon, in the sense of the life-world, the whole exercise should not be treated as a 'correction' " (Eger 1992, pp. 343-4).

History may provide clues about which misconceptions are rooted firmly in the pre-reflective everyday experience and will require extensive interpretative efforts to be changed. Remarking on mechanical notions such as the one that motion requires force, Eger points out that such misconceptions "appear as a recapitulation of beliefs widespread at the very beginning of the scientific revolution", and that we therefore "may suppose them to be the most primitive, unexamined effects of a 'fore-having'[14] in the lifeworld" (Eger 1993a, p. 9).

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[14] 'Fore-having' refers to the most unarticulated, practice-rooted component of the fore-structure. The term was introduced by Heidegger. See (Eger 1993a, pp. 7–8)

#### 3.3.4.2 Textbooks, lab exercises, and history

An acute and constant awareness of the need for interpretation is one contribution of hermeneutics to science pedagogy—a contribution that overlaps with that of constructivism. Hermeneutics differs from constructivism in (among other things) the importance and value it assigns to *historical* contexts as resources for interpretation of textbooks and laboratory exercises.

**Lab exercises:** A school experiment is not a simple encounter with 'nature', with an obvious scientific lesson. The experiment needs interpretation in the light of broader physical knowledge. Höttecke describes how, in a classroom situation, students will 'observe' different things even in relatively straightforward experimental situations. In fact, what students notice is frequently things that experts would characterize as disturbances or irrelevancies (Höttecke 2001, p. 171). Höttecke refers to an example of Hoffmann's, of balls of different mass hung by strings of equal length, that with some skill may be made to swing in phase for about twelve cycles before the lighter ball slows visibly down relative to the other. After that the pendulums do not swing in phase, and do not follow the Galilean ideal. Perhaps the students will 'see' that the pendulum period is independent of the swinging mass, but perhaps they will rather notice the increasing discrepancies of the pendulum periods. Rather than pretending that the conclusions to be drawn from the experiment are self-evident, the teacher should take advantage of the potential for *differing* emphases and interpretations, and see them as fruitful starting points for class discussions.

Phenomenology further emphasizes the role of interpretation by making an ontological point. Any individual classroom performance *is* necessarily an *interpretation*, or the interpretation involved is part of the *being* of the experiment. The experiment is a certain instantiation, one among many possible, of an entity circumscribed jointly by a lab instruction serving as a 'script', and a certain performance of that script. Eger on such grounds disagrees with educators who oppose repeating classic experiments in which "the outcome is known in advance":

> I think this misses the point, and the theatrical analogy is just what is needed to make that clear: When we go see to *Oedipus Rex* for the third of or fourth time our enjoyment is not diminished but enhanced by the fact that the outcome is known in advance. It all depends on how the re-enactment is *presented.* (Eger 1995, p. 187; footnote 13)

This has implications for the role of experiment in education. The function of lab exercises is not to illustrate theory, nor to allow the students to 'discover' the theory for themselves:

> To me it seems that hermeneutics points in the direction of laboratory as an *experience*; as taking part in *presentations* of phenomena; as *interpretation* of theory rather than its proof or independent discovery; and as *history*—since, in any case, much of high school and undergraduate laboratory time is spent repeating classic experiments (Eger 1995, p. 184).

The role of history briefly alluded to in this quotation is not only to shed light on the origins of the experiment performed. For reasons discussed in section 3.3.3.4, the students' performance of the classic experiment can be seen as part of the *being* of the experiment as an historical entity, and so the students are genuinely *participating* in the being of science.

**Textbooks:** Speaking of textbooks in physics education, Eger states that

> often these books are mere residues of a kind of dehydration process, designed to be "reconstituted" by teachers and students (Eger 1995, p. 180)

Again, ontological issues are at play. A textbook *is* not a package of cut-and-dried information to be ingested by students; rather, it should be considered "as the 'script' for an interpretive performance in class, laboratory and home—something to be *made* to come alive" (Eger 1995, p. 180). That is done by adding contexts, one by one, by approaching the text from various perspectives, by finding out what it says in a variety of applications. Particularly valuable contexts may often be found in the history of the ideas discussed in the textbook. These contexts illuminate the material with the concerns of the scientists who created the theories or models under discussion. They may provide understandable connections between these scientists' lifeworld and their contributions to science, and add meaning and perspective to the scientific work. More will be said about these issues in section 3.4. For now, another passage by Eger may conclude this section:

> Of course interpretation is not just internal analysis of one and the same text. The whole point of talking about hermeneutics in regard to teaching and study is to emphasize that all texts, be they elementary or advanced, *need* to be interpreted (not consumed) in order to be genuinely understood; that the required interpretations are of several kinds, occurring on several levels, including, though not limited to, first order semantic understanding of new terms; and that contextual interpretations (using external materials) are, *of course*, required as well, though not all contexts are historical.[15] (Eger 1995, p. 179)

#### 3.3.4.3 'Understanding physics'

Understanding physics, then, according to hermenetics, is a motion of the subject/object cut and coming closer to the objects that physical theories are about. It is participating in physical science and changing it, while simultaneously being changed by it. It means 'touching' physical entities, and knowing them through perceiving them from a range of vantage points. Understanding is connecting with, and gripping, by means of resources of one's own lifeworld. It is finding, appropriating, construing *meanings* according to requirements described by Heelan in this way:

> Meanings are adopted from traditions of interpretation, or constructed or reconstructed in keeping with the responsibilities, constraints, and presumptions of rational hermeneutical method . . . One of these responsibilities is that each legitimate meaning be appropriately fulfilled in a reader's experience or imagination. One of the constraints is the relative richness or poverty of the linguistic and cultural resources available to the reader. One of the presumptions is that there is no single legitimate meaning relevant to all readers of, say, a text (or suchlike material), for meanings depend on use. (Heelan 1998, p. 279)

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[15] Perhaps an explanation of Eger's somewhat polemical tone here might be in order. This passage is part of a dispute with Bevilacqua and Giannetto, who argue that understanding the historical context of the origin of a theory is the *only* way to 'bridge the life-world and the science-world' (Bevilacqua and Giannetto 1995, p. 123) and truly understand the theory. Eger agrees that including the historical context *frequently* is useful, but insists that it is not *always* necessary and clarifying to do so.

The last sentence brings up the question of meaning and significance, or perhaps rather of meaning *versus* significance, when Heelan writes (of physical theories, among other things) that "meanings depend on use". In much of the literature it may at first seem that hermeneuts sloppily conflate two very different questions by not distinguishing clearly between two senses of the word "meaning". It would seem that they confuse the issue of what (permanent) meaning a *text* may have with the (variable) significance it may have *for someone* (Eger 1995, p. 186; footnote 10). Is not the question of what a chapter in a textbook 'means' different from the question of what it may 'mean' to me, if it can be 'meaningful' to me at all?

This ambiguous usage of the word is deliberate, however, and reflects the belief that the two senses are in some way continuous. Taylor argues with care that the issue is not one of "a bad pun" (Taylor 1987, pp. 40–47) (though he explicitly excludes the the natural sciences from the realm of 'meaningful' knowledge in this sense). Eger, on the other hand, explicitly states that the dichotomy of meaning and significance should, even in the natural sciences, be replaced with a continuum (Eger 1995, p. 186; footnote 10). An interpretive 'core' defined by the object under study is surrounded, in some sense, by 'potential interpretations'. Each interpretation is both *of* the core, and constrained[16] by it; each makes the core *intelligible*. But each proper interpretation is also *for* an interpreter, and may allow the object to appear *meaningful* to the interpreter. In studying physical theories, for example, the interpreter may find that what a theory 'means' in the narrow, semantical sense is bound up with philosophical issues of determinism, causality and freedom, and in this way with issues of significance *for the interpreter.* The claim seems to be that nothing can be intelligible to us without also in some way being meaningful. Whenever we succeed in understanding anything, we have understood it *as* something of significance to us.

This issue of to what extent intelligibility and significance are continuous is less than clear to me. Understanding a poem may of necessity involve understanding what it means *to the interpreter*, here intelligibility and significance may be inextricable, but I do not see that this also applies generally to science. Does Eger mean that I cannot understand Maxwell's equations without them meaning something to me, without them being related perhaps to some human purpose? Can Maxwell's equations not be understood except *as* something with a role in the life-world? There may be some sense in which I would understand the equations differently depending on whether I saw them as tools for solving a problem in radio transmission, as an expression of Maxwell's mechanical theory of electromagnetism, or as material for a physics exam. Still I am hardly convinced that my understanding the equations necessarily must presuppose their having significance for me. At any rate I am inclined to think that it is useful to uphold the distinction between these two senses of 'meaning'—even while perhaps showing that in at least some cases the one entails the other. Whether my attempt here at interpreting Eger's denial of a clear-cut distinction between intelligibility and significance captures the issue at all I do not know.

Nevertheless, I consider it is an important contribution of hermeneutics that it attempts to relate physics to the broader culture, and addresses issues of physics as *meaningful* or *meaningless.* The problems of physics education today, after all, are not limited to the difficulties of getting students to get a grasp of the basic concepts and applications of the science, to see physics as *intelligible*, but—I think more importantly—to get students to see the *point* of

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[16] "...while potential interpretations surround a text (or text-equivalent) and the 'distance' between any two of these may be considerable, nevertheless they are not running around all over the yard to be scooped wherever one pleases. Where they can be found and what sort of 'space' they occupy depends on the core, the text itself. Interpretation is *not* invention, there is something *there* to interpret." (Eger 1993a, p. 13)

physics, to see it as *meaningful* or *significant.* A great number of students, after all, turn their backs on the science not for any lack of mastery of the subject, but because it does not appear relevant, not *meaningful* to them. Efforts toward shedding light on understanding physics as meaningful should therefore be welcomed.

## 3.4 Pluralism, history and understanding

In the previous section it was seen that according to hermeneutics, the ability to take a plurality of perspectives on an issue is a defining mark of understanding it. Some further issues on pluralism, history and understanding are addressed here. A further defense of pluralism is included first. The question whether it is really at all possible to provide a single, coherent account in physics education is then raised, and answered in the negative. Finally the importance of history in the context of understanding such a multiplicity of interpretations is very briefly discussed.

### 3.4.1 Pluralism and attention to 'good reasons'

Harvey Siegel (1991) is concerned with how to communicate scientific method "not as a particular set of procedures or techniques but rather as a general commitment to evidence" (Siegel 1991, p. 52). How can science education be conducted in such a way as to emphasize reasons and evidence in science? Siegel suggests utilizing philosophy of science in the classroom, and studying cases of pseudoscience, and then goes on to argue that

> A science education focused on reasons and evidence in science ought also to embrace what might be called a *pluralist* epistemology (Siegel 1991, p. 54).

Among the virtues of pluralism are the philosophical recognition "that scientific knowledge is never final or certain, but is always subject to amendment and revision" (Siegel 1991, p. 54)—the acknowledgement of this open-ended character of science is a primary rationale for a "willingness to tolerate and utilize a diversity of ideas and approaches" (p. 54). A *pedagogical* virtue of pluralism is that "the conflict of ideas can serve to stimulate students, and to spur them on to deeper understanding of the matter at hand" (p. 54). Most significant, however, is the opportunity a pluralistic approach offers for "a *focus on the reasons* for regarding current theory as worthy of our attention and embrace" (Siegel 1991, p. 56, emphasis added).

In this connection Siegel quotes Michael Martin (1972), who borrows Feyerabend's phrase "proliferation of theories" and writes that

> [S]tudents of science should be taught a number of different theoretical approaches in a domain of research. If necessary, discarded theories from the history of science should be restructured and re-examined. Students should not only be exposed to different theoretical approaches, but should also learn to work easily with different theories, now seeing the domain from the point of view of one theory, now seeing it from the point of view of another, switching back and forth to get various theoretical perspectives and insights (Martin 1972 in Siegel 1991, p. 55).

Martin argues that "the more theories one is used to working with in a given domain, the less likely it is that one will be blinded by one's commitments to any of them" (in Siegel 1991, p. 55).

While on a theoretical level this all sounds fine, the physics teacher may well wonder about how practicable it is to realize this pluralism in the classroom. Can the students really absorb more than one account of a phenomenon, given the scarcity of time, and the limitations in ability and interest on the part of the students? Some issues raised in the following section need to be addressed before this question may be answered, however.

### 3.4.2 Textbook physics and 'currently accepted physics'

Apart from any value educators may or may not assign to pluralism in physics teaching, there are some questions about what the alternative can be. While it might be tempting to claim that students just should learn the currently most successful theory, there is some reason to doubt that this is at all possible or even makes sense.

Complaining about the quality of standard textbooks in physics, Fabio Bevilacqua and Enrico Giannetto charge that

> Ordinary textbooks for high school and undergraduates do not offer a coherent scientific theory of the phenomena: they offer layers of scientific results, coming from competing interpretations, deposited during centuries. A quantitative correspondence between the layers cannot hide the general lack of a coherent meaning and the conflation of contrasting models (Bevilacqua and Giannetto 1995, p. 119).

They elaborate this view that textbook physics is incoherent for the case of electromagnetic theory:

> Students of electromagnetism learn of Coulomb's law of action at a distance, but also of potential theory, of Faraday's and Maxwell's contiguous action and field theory, to mention only the best known models of electrical phenomena. Is the quantitative correspondence in the appropriate domains between most of these alternative interpretations ("numerical convergence") sufficient to give coherence to the students' conceptions? Certainly not ... (Bevilacqua and Giannetto 1995, p. 123)

Bevilacqua's dismissal of textbook electromagnetism as a coherent system is based on the careful work of his doctoral dissertation (1984). There he compares presentations of classical electromagnetism by leading 20th century physicists, and highlights differences in interpretation. He traces these differences back to debates in the 19th century, when electromagnetism was in the making. Teachers and students today frequently do not notice these variations in interpretation, however, and according to Eger this may be explained by the fact that

> in science today we are so focused on the 'bottom line' that many kinds of meaning escape us. (Eger 1993b, p. 322)

To the extent that Bevilacqua's complaints about textbooks are well-founded, the project of teaching students 'the currently accepted theory'—in the singular—is not feasible.

A quite different kind of objection to the idea that students should be given what is today seen as the 'right version' comes from reflecting on the scientific status of what is actually taught in high school and in undergraduate courses. It certainly is not the currently 'best theory'—and for pedagogical and practical reasons, it can not be. Most of the physics taught in high school, at least, has long ago been superceded by quantum mechanics, relativity theory, quantum electrodynamics ... and for good reasons. Much of the (largely classical) physics still taught at high school level has been deemed insufficient or flawed by the scientific community for many decades, or even a century.

### 3.4.3 Historical and pluralistic physics

Is the fact that the physics taught almost necessarily is outdated a problem for education? It certainly need not be. The fact that it is not possible to communicate the most modern of physics to beginner students is a problem only for the view that students should be told some sort of final truth. Learning classical physics can *of course* be useful and enlightening, and not only as preparation for further studies, but also for what it tells about the world. However, the limitations of what is taught should be addressed in the classroom.

To some extent it is already done in high school physics classes. For example, when students learn about Bohr's atomic model in physics classes, they are often immediately told about the limitations of the model, about what a model is, and about why it is worthwile to understand Bohr's atom properly even though it was refuted as a 'true' theory even before it was published. A similar degree of explicitness about limitations of the physics taught might be in order over a broader range of topics. For example, the differences between instantaneously acting distance forces, distance forces with a time delay, and contiguous forces could be pointed out in connection with lessons on Newton's law of gravitation and Coulomb's law. It could be emphasized that Coulomb's law as it stands applies strictly to the static case, and why. Frequently students who have learnt both Coulomb's law and the basics of special relativity do not notice any tension between the apparently instantanously acting forces of Coulomb's law and the principle of a maximum velocity laid down by special relativity. Classroom discussions of these questions could quickly raise a host of issues in field theory.

Elements of incoherence in high school physics should not be seen as problems, but rather be seized upon as opportunities for learning. Pointing out conceptual ambiguities might encourage an awareness that more than one theory may account for the same data. Identifiable tensions provide occasion for philosophical discussions, conceptual clarifications, and a highlighting of the need for further knowledge. Implementing this is probably not more complicated than teaching physics is in any case—but it might perhaps require some changes in teacher training programmes.

Bevilacqua and Giannetto (1995) are not so optimistic about the possibility of making sense of current high school physics. They judge modern textbooks at that level to be useless in relation to developing an *understanding* of science, and insist that

> From one point of view textbooks are good for indoctrination, like catechisms, from another they offer a technical view of science, closer to operating manuals of modern artifacts than to science texts (Bevilacqua and Giannetto 1995, p. 119).

Bevilacqua and Giannetto apparently want do do away with textbooks altogether, replacing them with original texts and thorough studies of the historical events that gave rise to the physics. Referring to the multiple interpretations of electromagnetic phenomena that co-exist in textbooks (previous section) they dismiss efforts at finding a "unique, final meaning" of electrical action through further textbook studies:

> What would be needed instead is a historical clarification of the specific individual interpretations of the authors implied, and of the reasons they had for shifting from one meaning to another, reasons embedded in their own lifeworld. This can be done focussing on sources other than textbooks. (Bevilacqua and Giannetto 1995, p. 123)

Perhaps a clear account of tensions within school physics does require recourse to history. Once the theories are seen *not* to be systems of final conclusions, or copies of the external world, or imprints on our knowledge of the laws of nature, each theory must be understood in terms of evidence available at the time when the theories were made, and in terms of characteristic concerns and resources of those ages. The physical theories taught in high school may in some respects be seen as fossils of theories once at the forefront of physics, fossils that must be understood in terms of the life forms that gave rise to them.

On the other hand, while I am sympathetic to many of the ideas expressed here, I can not see such wholly historical approaches as realistic in high school physics. I suspect that the the potential for misunderstandings *of the historical material* could soon come to match the misunderstandings clarified by going through all this history. I do not see that full histories must always be necessary in order to distinguish the various meanings or interpretations of electromagnetism that can be found in textbooks. However, I do agree that these variations in meaning should be made explicit. A quotation from Bevilacqua and Giannetto's (1995) may then very appropriately conclude this chapter:

> If we want to teach "science" these alternative interpretations have got to be outlined and clarified, not hidden and mistified (sic). (Bevilacqua and Giannetto 1995, p. 120)

# Chapter 4

# A Focus Group Study

## 4.1 Background and motivations

A limited study of some university undergraduates' conceptions of some issues was carried out as part of this work on applications of history in physics education. The aims were twofold: to get some first reactions from the students about the idea of historical content in physics curricula, and to compare students' reasoning about some fundamental electromagnetic ideas with historical arguments.

One hypothesis I wished to test was a hunch that students' knowledge in electromagnetism did not rely much on acquaintance with simple electrical phenomena: I expected students to have trouble with, say, explaining how one can demonstrate the presence of current and provide a measure of it without too much recourse to theory and prefabricated instruments. I also suspected that instantaneous distance forces and contiguous forces would not be strongly and consciously distinguished by the students, and that the role of the field in this debate would not have been a significant issue for them. Finally, I imagined that the students would have considerable reservations against the introduction of history into physics courses.

My expectations about the students' ideas about electricity were based on my own experiences when reading history of electromagnetism. I had been surprised—and fascinated—by learning that, for example, demonstrating the identity of currents produced by electrolysis and by galvanism had been a matter of experimental concern. Showing that electric current consisted in the flow of charge, and for that matter that 'charge' was something (a fluid? a collection of particles?) that could be moved around and transferred from one object to another had not been simple either. I realized that my own notions about charge and current derived their meaning mainly from their place in a network of concepts I had learnt in physics courses. The anchoring of these concepts in elementary physical *phenomena* was rather weak. I would have been hard put to show a non-physicist that I had, say, a given amount of charge on an object before me, or to show that it was positive or negative. And how would I demonstrate that an electric discharge was the same as a very short-lived current? My fascination was coupled with an element of embarrassment: ought I not to know these things after two years of high school physics and a college course in electromagnetism? The complexity—or at least the unfamiliarity—of argument and demonstration required in order to establish the electrical concepts *from scratch* bemused me. On what basis had I taken these concepts for granted? Part of my project, then, was to get an idea of how widespread this state of (un-)awareness of the evidential foundations of electromagnetism was among university physics undergraduates.

Another matter of concern was the question whether physics students characteristically have little patience with the history, and whether their interest in physics hinges strongly on a desire to arrive at ‘right answers’ by the shortest route possible. The view that physics students as a group are particularly unreceptive to historical approaches, being selected for an inclination to look for unique and definite answers, has been discussed in section 1.3, and so no more detail will be added here.

Perhaps I should add that this introduction was written a year and a half *after* the questionnaire used (section 4.3), and that the aims were somewhat less articulated at the time—as is reflected in the questions actually asked...

## 4.2 Practicalities of the study

### 4.2.1 Methodology—focus group interviews

The study was conducted in the form of focus group interviews. Focus group studies are a widely used method of qualitative research into people’s conceptions and attitudes. Generally about three to five groups, each of six to ten subjects, are formed. Each group meets to discuss a number of relevant questions under the guidance of a ‘moderator’. The discussions are usually taped, and this material, together with any notes taken by the moderator or an assistant during the interviews, form the basis of the analysis. The aim is *not* to determine the incidence of specific responses—statistical information is not sought for, and generally there is no pretense that notions or attitudes identified are representative. Rather, the aim is to capture a certain breadth and variety of responses. Frequently focus group studies are carried out in preparation of quantitative surveys. Insights mined from focus group interviews may suggest items for quantitative questionnaires, and may aid the interpretation of responses to such items.

While there is much literature on the methodology of focus group interviews, I will say no more on these issues here. Two graduate students before me at the Physics Education Group, Øystein Guttersrud and Bente Bjørkhaug, conducted comprehensive focus group studies and wrote thorough chapters on the theory of this kind of research (Guttersrud 2001, Bjørkhaug 2004). My study was to be much more limited, and so placing great emphasis on such background seemed to be methodological overkill. Here the scope of the study almost necessarily limits the generality of the conclusions more than do the lapses in method.

### 4.2.2 Recruiting informants

Subjects for the study were recruited at the University of Oslo during the fall semester of 2002. Efforts were made to recruit students doing the first course in electromagnetic theory. Finding a sufficient number of students willing and able to participate in the interviews proved to be far more difficult than anticipated, though, and so students doing the first course in thermodynamics were also targeted. This thermodynamics course is usually taken one year after the electromagnetism course; like the latter it is taught during the fall semester. Even including the volunteers from the thermodynamics course the total number of subjects was really far too small. In the spring of 2003 I attempted to form some groups from first semester students taking the mechanics course, but without any success.

In order to find volunteers I contacted the professor who taught the electromagnetism course, and was given permission to publish a notice about my project on the course website, and to inform his class in person during a lecture. I presented the interviews as informal lunchtime discussions lasting about an hour and a half—the students could bring their lunchbags and I would bring coffee and cake. One student drawn at random from the participants would get a gift certificate from a music store. The lecturer encouraged the students to sign up. There were few responses, though, and about a week later I once more told the class about my project during a lecture, but that did not make much difference. I repeated this procedure for the thermodynamics class. In the mechanics class the following spring I informed the class one single time during a lecture, and the professor reminded the students about the project later. Not one student took contact, however.

Not all the students who volunteered to participate could be included. The students' schedules were frequently very busy, so that finding a time at which more than two students were available simultaneously was very difficult. In the case of the first group, only half of the four expected participants turned up, because a network error had delayed an e-mail message about final place and time. A total of 12 students were questioned during October and November 2002, in three groups of respectively two, six and four students. Originally the first group was intended to function as a 'pilot interview' for testing the questions, and was not supposed to be included in the study. Because the material was so limited, and since there were many interesting responses from this group, it was incorporated into the report anyway.

I have wondered about why it was so difficult to persuade students to volunteer for the study. Of course the students did not really have any incentive for joining, other than solidarity with a graduate student, and that is not very much of a reason to sign up. The most decisive factor probably was just shortage of time—a look at the schedules of those who actually did turn up strongly suggested this as a problem. There may also have been elements of reluctance to risk displaying misunderstandings about elementary electromagnetism. At any rate, researchers planning to conduct comparable studies among students at the University of Oslo should probably not count on getting a sufficient number of volunteers. Maybe it would be possible to refashion the study into a form suitable for a teaching unit (perhaps even obligatory), which could be conducted during hours students already have allotted to the course. A good deal more thought would be required in order to figure out whether—and how—this form could be used for such an investigation.

### 4.2.3 Limitations of the study

The very small size of the study of course limits the validity of any conclusions drawn. Further, about half of the interviewees were acquaintances of mine. The overlap between their interests and mine would therefore probably be greater than with a randomly selected group.

The interviews were conducted in Norwegian, and so the quotations included are in translation. Transforming colloquial Norwegian into colloquial English is not always easy. I have now and then sacrificed natural flow for literal accuracy. If any of the students' phrases should contain stiff or awkward vocabulary, then, that is quite likely an effect of the translation.

A difficulty in interpreting the interviews afterwards was due to the simple fact that I had not been careful enough about getting the students to speak one at a time. Consequently, at a number of points in the discussion it is impossible to disentangle the dialogue, as everybody is speaking simultaneously. Of course this problem arises precisely where the discussion is at its most interesting, when the respondents are eagerly trying to make their point all at once...

Several of the questions turned out be leading questions to a greater extent than I had been prepared for. This effect of the formulations was probably reinforced by the fact that most of the students were aware that I was interested in history. One consequence was that it sometimes took a bit of coaxing and specific questioning to get the students to discuss *objections* against using history in physics education. Often when focus group studies are conducted, the moderator can adapt and reformulate questions that during the first interviews appear to prompt rather predetermined responses. With only three groups, and no prior practice, the opportunity for such weeding out of unfortunate formulations—and more generally, of honing moderating skills—was limited.

A final, rather obvious reservation: I do not, of course, imagine to have identified stable conceptions or opinions in the interviewees. Perhaps that can be done when people are interviewed about deeply felt and long held political convictions. Here the notions queried about were in the making, formed through the questioning, and chance features of the questions and the course of the conversations would determine many responses. The students might well have answered some questions very differently a day later. That does not interfere with the purposes of this investigation.

## 4.3 Questionnaire

This section provides the interview guide. The questions below were not always asked in quite this order or in exactly this formulation, but the list guided the conversations.

In several places I have asked the students to formulate their replies as if they were explaining something to their fifteen-year-old sister. The idea behind this perhaps curious phrasing was to avoid a focus on formally sufficient definitions, on linguistic issues at the expense of the conceptual. I also hoped that the students would be more explicit about things they might expect their co-students to find obvious. As far as I could tell, this worked well.

### Introduction

I am writing a thesis that is to be concerned with, among other things, using history in physics teaching. There are several things am interested in finding out about in the course of these discussions. My main concern is to get an impression of the ways in which you think about a number of phenomena within electromagnetism. The purpose of that is to compare with historical electromagnetic theories. I would also like to hear about what thoughts you might have about the use of historical approaches in the teaching of a physics topic such as electromagnetism.

### Previous experience with history in physics education

- Which persons and events do you remember from the history of electromagnetism? In what contexts did you hear about them?
- Have the textbooks or teachers you have had included history of physics in the physics courses? In what ways? (As biographical material about great physicists? Material about conceptions that were replaced by the new theories?) Which topics included historical material? (Mechanics, electromagnetism, astronomy...?)
- To what extent do you think it might be useful to study theories we now have abandoned? What, if anything, can we achieve in that way? Does it make any difference what kind of material we use, whether it is biographical material, information about

society at the time, material about the arguments between thentime physicists for and against the new theories, or details about the central experiments in the development of electromagnetism?

- What problems can you see about the inclusion of history in physics teaching?

## Some central concepts in electromagnetic theory

### Charge

- How would you explain what electric charge is? Or, if you were to explain to your bright 15 year old little sister what charge is, how would you go about it?

- How do you know that there is charge? How can you demonstrate, show or prove that what you have in front of you is a charge?

- How could you measure how much there is of it? (What physical phenomena can you imagine you could use if you were to measure a charge?) Can you suggest a scale or a measuring unit for charge so that other physicists, with other equipment, would understand just how much charge you mean when you communicate that you have a certain amount of charge? (Or, put differently, if you could freely define 1 Coulomb, how would you do it?)

- It has been said that if there were only one single charge in the universe, the concept of 'charge' would be meaningless. What do you think was meant by this statement?

### Current

- What is a current? (Or, if you were to explain to your bright 15 year old sister what a current is, how would you proceed?)

- How would you demonstrate the presence of a current?

- How would you go about measuring how much there is of it? (What physical phenomena can you imagine could be used for measuring electric current?) Can you suggest a scale or a measuring unit you could use for quantifying current, so that other physicists, with different equipment, could understand just how great the current you have been working with might be? (Or again, if you could freely decide the meaning of 1 Ampere, how would you do it? You have not got an ammeter at hand—unless you can explain how it works!)

- How would you show that there is a connection between current and charge, and what sort of a connection it is?

### Electric field

- Let's bother your bright, teenage sister again. How would you get her to understand the concept of an electric field?

- We demonstrate, or verify the presence of, an electric field by looking at the forces on a small test charge. Is the electric field still there when we remove the test charge? If so, how can we know that it is there?

- Do you think about the electric field as something physically 'real', or rather as a mathematical entity that is convenient for relating measurable quantities?

- Suppose that there are electric forces acting on an object. We can say either that there are forces acting on our object from a charge Q some distance off, or we can say that there are forces acting on our object because of the field surrounding Q. Can you see any way of deciding whether our particle is interacting with an electric field or directly with an other charge?

- In the latter half of the 19th century, Weber's electrodynamics was a strong alternative to that of Maxwell. In this theory the field concept did not occur, and charges exerted forces directly on each other over distance. Critics of Weber's theory felt that the distance forces of his theory were suspicious. It seemed unreasonably mysterious that anything should be able to act on anything else directly across a distance, without the forces being mediated by anything. The field theory, in contrast, made it possible to describe electric forces as contact forces (though we need not go into the details of just how in this context). What do you think about this objection from Weber's critics? Are distance forces in themselves so 'occult' that they should be rejected in a physical theory? Are such metaphysical arguments about what may or may not exist in the world of interest for physical theory?

- Did you think of the Coulomb force as a distance force or as a contact force when you learned it?

### Comparison of the concepts' ontological status

- Some of the concepts of electromagnetism may appear more fundamental than others, and there are some that it seems quite clear that we could do without. For example, it would not be very difficult to formulate a theory that avoids D-fields, and relies on E-fields instead. We can readily think of the D-field as a mathematically convenient construction rather than as a physically real quantity. Is this account reasonable, do you think?

- We may consider some better known concepts in electromagnetism. Do you think about *voltage* as something physically real, or rather as a mathematical construct that simplifies calculations of electric problems? Do you think it would be possible to develop a theory of electricity that avoided the voltage concept?

- Consider the concepts *charge*, *voltage*, *current* and *electric field*. If you were to rank these according to how physically 'real' they are, and how indispensable for electric theory, in what order would they come? Which concepts can most readily be sidestepped by mathematical reformulation of the theory?

### Conclusion

- Do you have any comments? Any additions on any of the issues, whether about using history of physics or concerning any other of the questions you have discussed?

## 4.4 Student responses

### 4.4.1 Preliminary remarks

First, a typographical note: In this report, unlike in the rest of the thesis, ellipsis (. . . ) will not mean that material has been omitted. Here it will rather indicate a brief pause in natural speech. I will indicate omissions like this: <. . . >

Second, student statements are marked with a 'G', or sometimes with a 'G1', 'G2', and so forth when several students are taking part in a discussion. The labels are not permanent, in other words 'G1' may refer to different students in different excerpts. I have not labeled the students according to which of the three groups they participated in. My own statements are labeled with an 'M' (for 'moderator').

### 4.4.2 History in physics courses

**History in physics courses** The interviews started with some questions about the students' previous encounters with history of physics, through textbooks and other media. The purpose of these questions was primarily to warm up a bit to the topic and start thinking about the history of physics; and so, for example, the details of which historical physicists the students remembered best will not be recounted although such information was asked for. Some scattered bits of dialogue from this section may perhaps still be worth noting.

While it is impossible to say anything very general about students' interests based on this study, there appeared to be a preference for more recent history of physics. Pre-Galilean science is harder to relate to.

M: *But there was more history in the parts on modern physics?*

G: *At least that's the parts I remember . . . But that might be just because it was more interesting. Not as alien as the old Greeks rubbing a lump of amber . . .* <laughter>

The students interviewed tended to enjoy the brief historical accounts found in textbooks, as a kind of lighter reading between the mathematically more demanding parts. Texts on the physical content of historical physicists' work was of more interest than dates and details of their private lives:

G1: *My high school textbook had a good deal of boxed material that was rather biographical, but it was . . . very brief, short summaries. . .*

G2: *If it's well written its fun to read, I think.*

G1: *More fun than reading equations, anyway* <laughter>

M: *Does it matter whether these textbooks are biographical, deal with experiments, or concern changes in theories?*

G3: *If there's anything at all literary in the physics book that can help you understand things, you read it!* <laughter>

G2: *It is the theories one has most interest in.*

M: *Not so much when people were born and so on?*

G2: *No ... It's OK to know something about the person, but, like, that's not what I'm most interested in. It's their work, the theories...*

The students were asked whether they could see any value in studying discarded theories (admittedly a leading question...) There were many suggestions; one was that historical theories might actually contain useful ideas:

G: *You can of course ... study the period, study the period and the way of thinking, you can ... it may be also be that perhaps there was something to it, that there maybe was a way of understanding—I mean, another approach perhaps, in thoughts that have been discarded—I mean, you don't know for sure, there may be something to it, maybe...*

M: *In the old theories?*

G: *One can get inspiration and ideas, perhaps...*

Another advantage that was mentioned was that attention could be given to reasons and arguments for the *current* system in studying the transition from the old one.

G: *It ought to increase creativity, if you look at the old theories and try to explain new things <...> gets your creativity started, instead of being just told this is how it is...*

The students discussed how the theory that the earth is at the center of the universe has long been refuted, how any child today knows that the earth revolves around the sun. Still there may be some point in looking at the geocentric theory, for

G: *when you have the two [theories] against each other you have to, in a way, make up your mind about* why *the one theory is more right than the other...*

The students were also asked about objections to the inclusion of history in physics courses, about problems they could see resulting from this. The tight time schedule of typical physics courses was mentioned—there is not room for more material. Besides, the purpose of studying physics is to acquire the knowledge and skills to solve problems with the best currently available theories.

G2: *There's of course with history of physics, there's, it isn't about applications, like you can't* use *the history directly, like a method you learn from the course.*

G1: *There are many who would think it a waste of time to learn history of physics, because you, you learn what is correct* today, *not things that are proved to be wrong.*

M: *What do* you *think?*

G1: *I don't think I would want to use half the semester learning history, for example...*

G2: *I'm most interested in learning the theories that are thought to be correct, want to learn the...*

> G1: *Learn physics as a tool...*

> G2: *That's the idea, that we can use it, to research, to move on ... and then that's what has priority, instead of learning that that's the way it was...*

This statement was modified for the case of physicists planning to do fundamental research; apparently the students thought they might have more use for historical knowledge:

> G3: *I think it can be about two different things. Suppose if you want to make cell phones, you don't need, and if you're studying physics in order to do that, you don't need history as such, to the same extent as if you're going to do research, for if you then, if you study, history of physics maybe you get a kind of humility toward the discipline, that, that you need in research, like, in order to say that your theory turned out to be wrong, after all, and that...*

Finally, a remark about historical elements as mnemonic devices may be included:

> G: *But with respect to physics textbooks, I don't know, I think it's a good thing—I guess one can get too much of it, I havn't thought much about this, but the fact that I can think, like, this is the* Schrödinger *equation, it's not just any equation, and that's* Faraday's *law of induction, it's not just... In part just to remember what's what, and also to remember, well, honour to those who deserve it as you've said, you remind yourself that somebody's worked at these things, but it's also to have some hooks to hang things on, and also a bit in order to remember when things came, like, for its easier to rememember when Schrödinger and Faraday lived than when this and that equation was invented.*

**Fallibility of physical knowledge** In all the groups the students mentioned the value of history for demonstrating that established, taken-for-granted knowledge can be replaced by very different ideas. Frequently quite animated discussions of such once accepted ideas followed.

> G: *At least it shows that everything may not be exactly as we believe it to be just* now... *They believed things very strongly* then *that do not seem so true today, to put it that way. And so we can think that what we today think is true perhaps is not true either. For example there's Newtonian physics that suddenly was... different.*

> M: *So you have no trouble imagining that the physics we learn today may end up in the history books in some years?*

> G: (emphatically): *It will.*

The reason why such awareness of change was important was taken to be that physicists in that way could become better prepared to relate to novel ideas in their field.

> G: *But something you can learn by looking at for example how old theories are falsified and replaced by new ones, I guess it can make you think that, OK, maybe what I'm doing just now is totally wrong, and, like, how can we show that this is wrong and get further, seeing that it after all has been done many times earlier <...> and if there comes a new theory, I'm not to oppose it because it then is against what I have learnt, and like, recognize that, perhaps, OK, what I have been doing is actually falsified and we've got to begin to think in a different way.*

The students generally seemed convinced that the physics they were currently learning would look different in the future, but they differed as to how dramatic the changes would be. Thus, while some students unreservedly agreed that today's physics might be refuted in a strong sense, others were more guarded:

M: *Do you think it likely that much of the university physics you are learning now will be in the trash bin in some years?*

G: *Not in the trash bins. But we must be prepared to make changes.*

In one group a wish for explicit attention to problematic aspects of *current* physics was expressed. Physics as encountered in educational settings did not have the open character that students thought the science as such did have:

G1: *Should be more concerned with points where the physics doesn't fit completely, instead of always pointing to the showcase examples.*

G2: *We take small anomalies away and put them in the drawer and lock it. Throw away the key.*

G1: *That's what is exciting about studying physics—that you do not get the impression everything is discovered, everything is ...*

M: *But would you say that from the way physics is taught you do get the impression that discovery is not completed, that there is a lot that is still open, or is your impression ...*

<interrupting laughter>

G2: *It seems quite settled ["bastant"], really.*

G1: *Things aren't taken to be problematic.*

**Role of Examen Philosophicum** Several students mentioned that they had developed an awareness of scientific change while taking Examen Philosophicum ("Ex. Phil."). This course of about half a semester's work is a prerequisite for all university degrees in Norway. It encompasses a history of philosophy from the ancient Greeks to the 20th century, some philosophy of science, argumentation theory, and logic.[1] The students transferred what they had learnt in this course to physics.

G: *That is the most useful thing I learnt from Ex.Phil. also. Just what he said there. When you read the history of all that was believed for a couple of thousand years, but that we laugh about today, but that must have seemed quite convincing at the time ... Just the same thing, why should it be so much more correct what we believe today? That was I think the most useful thing about Ex.Phil. anyway.*

<...>

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[1] At least, it did when the students interviewed took the course—since then the syllabus has been revised considerably.

M: *You mentioned that you in the course of Ex.Phil. got the feeling that theory changes. But does that insight readily transfer to physics, or is it neccessary to study old phyical theories in order to understand that* this *field of physics is subject to change? Many would say that Aristotelian physics* [which is briefly dealt with in the Ex.Phil. course] *is not really* physics, *according to our methods ...*

G: *No ... but there was a lot about natural science in the course, if not much Aristotelian physics ... But still, generally ... just the fact that so many that intelligent and enlightened people for so many years could take for granted something that for us seems so stupid ... even if you say Aristotelian physics is not really physics, it was, it was still theories that people clearly must have thought about and accepted ...*

**Motivation for learning fallible physics** The students, who had all declared that they believed physical knowledge was subject to change, were asked whether this skepticism might not erode motivation for learning current accounts. One student did suggest that the patience and practice required in order to develop calculating skills might suffer:

G: *Perhaps if there's too much of the qualitative one may forget the somewhat gray work of it, getting the calculations right, perhaps the respect for calculation rules can be weakened.*

However, by far most of the respondents emphatically disagreed when asked whether their motivation was reduced by their expectation that at least part of what they were studying might be refuted later. The following response was typical:

M: *How does that affect your motivation to study physics, if you think this is all temporary, and you can't believe it too strongly?*

G: *Cool! That's what's exciting. Definitely.*

I made some attempts at finding out *why* the idea that physics was in flux carried such fascination, but without much luck.

G: *You have to learn the theory anyway to see what is wrong with it to improve it... That's the boring thing about maths, things are so fixed, you can't change anything...*

M: *Is it exciting because you personally may change physics one day, or is it interesting in itself, that the theory is subject to change?*

G: *Both! ... Always something new, always something changing... that's like, it's nice. If things were static, like, we could learn it all and there would be nothing more...*

Several students more than suggested that their belief in scientific change, or perhaps an interest in foundations of physics, had been a factor that influenced their choice of higher education. If they had thought otherwise of physics they would have either opted for engineering, or studied something other than science.

G1: *I would never have studied physics if I'd thought that way, I think, it wouldn't have been so exciting. Then I'd have studied at NTNU and become a civil engineer if that were the idea, sort of.*

G2: *I study physics because there is change, if it's completed, its not so interesting. You can't do anything more there...*

M: *Some would say new applications provide enough novelty, that technology can be exciting enough in its own way?*

G1: *In medicine, say, to help people—you can have such motives? That's very good, but it's not my motivation. Egoistic, yes... but that's how it is.*

G2: *If physics were complete now, I would have been at the Department of History and Philosophy noe.*

M: *Studying the development of physics?*

G2: *Further back, I think. Hittite martial history, or something?*

**Historiographical issues** In two of the groups, historiographical issues were raised by the students. One student thought a primary reason why textbooks included history was to give credit to those who deserved it. He was aware that a history is affected by the purposes it is written for:

G: *I remember reading about Feynman, he wrote that there is something called physicist history. And that's history that is completely different from ordinary history, like, the discipline of history. And that was a kind of resume, it did not necessarily need to be historically accurate, with dates and the order of things and all that, but it was a kind of, like, compact version of how things started and how certain things hung together, historically, it was a kind of, of—we've talked about physicist-math or engineer-math; here we're talking about physicist-history...*

In another group the students asked historiographical questions in discussing whether or not history in physics courses was a good idea. Knowing what kind of history was at issue was necessary in order to decide whether or not it was worth learning.

G: *When we're talking about history of physics, are we talking about the development from, the disciplinary* ["faglige"] *development, or are we talking about the history of physics more like the persons... ? For like the history of the physics itself, I think that must be quite important for a* physicist *too.*

### 4.4.3 Some electromagnetic concepts

**What is charge?** The students were asked to "explain, not come with a necessary and sufficient definition of, but explain to your 15 year old little sister" what electric charge was. In each case this question was followed by a moment of silence while the students exchanged surprised glances—before beginning to laugh.

G1: *Eh ...* <laughter> *You may start!*

G2: *No idea ...*

G1: *Eh ... My little sister is only fourteen.* <laughter> *So ...*

Some students immediately guessed at the intentions of the interviewer:

G: *Eh... I think one would have to use a bit of history!* <laughter>

In several cases the students began with atomic theory, talking about hydrogen atoms and electrons. It was not always very clear what explanatory function the elementary particles were thought to have.

G2: *Charge ... Have we got any definition of it at all? Something pulling at something else, but ...*

G1: *Think I would have explained in terms of particles ... Considered the electron as a point charge or something ... Particle explanation, I think.*

M: *That would have been very complicated for those people in the 16th to the 18th century, who started studying charge, for they certainly didn't know about point charges ...*

G1: *But charge is quantized, so one must be able to ... Some spherical thing...*

G2: *Something pulling at something else, so that both can move ...*

One group suggested starting with a familiar electrostatic phenomenon, and debated at length the case of an electrified balloon stuck to the ceiling.

G1: *Or begin with some examples. Very simple.*

M: *For example?*

G1: *No... Like a balloon on the ceiling or something. Explain that ... Then, there are two opposite charges that ... that there are differences that attract each other. That when you have a balloon that is completely normal, if you rub it against your head there's a difference between your head and the balloon, and then there'll be a difference also between the balloon and the ceiling, so that the balloon sticks.*

G2: *I think it's easier to explain that sort of thing if one can find, find something that is similar, but is simpler...*

G1: *Then you must talk about atoms?*

<everybody talking at once>

G2: *There are atoms where the electrons are strongly bound, and others where they...*

G1: *To be quite honest, we don't know why there is charge, we're just trying to describe it in this and that way. Why there must be electric forces, we don't know that.*

I have some trouble following this dialogue, which continues below. But from the last quotation above it seems to me as if the respondents are trying to find out why charge arises—or perhaps why it exerts forces—rather than defining charge in terms of the forces between charged objects. In such terms the conversation makes sense:

G1: *Charge—charge is something an object already has, yes, a quantity—this is difficult...* <slowly> *The reason why the balloon sticks to the ceiling ist that you have...* <pause> *Talking to a 15-year-old about forces is difficult!* <...> *Charge is something an object has an excess or deficiency of. And it will try, I mean if you bring them close that charge will try to ... even out so that that there is the same amount...*

G2: *The difference between the rich and the poor?* <...>

G1: *If the balloon sticks because charge is flowing from the balloon to the wall... and then...*

G3: *But this tells me nothing about what charge actually* is.

G1: *No, it doesn't... No.*

G2: *Perhaps it's a* property *of an object?* <...>

G1: *That may be the only way to explain it if one is not to use atomic theory.*

G2: *Is it a noun or an adjective?*

G1: *That charge is a quantity that we associate with an object. And that, well, two things that have different charge—that is, one that has positive charge and one that has negative charge are attracted to each other, because, the one has an excess of something and the other a deficiency of something, so the two try to equalize, and so...*

If, as suggested above, the students are here trying to figure out why the forces arise rather than just defining charges in terms of the characteristic patterns of mutual forces, they are in good company with 18th and even 19th century electricians. Much effort was then put into accounting for how the electrostatic forces might arise from the flow of some sort of electric substance between the interacting objects. Perhaps the "what is"-formulation of the question suggested to the students that this sort of account, an explanation of why the forces arise, was called for—at least, some students objected to this formulation:

G: *I guess one can't really say that "this is charge" or "this is a charge or a charged particle", one has to show some effects and then say that the cause of this is that charge is created...*

The same point is made very clearly toward the end of the following exchange:

G1: *Isn't it just ... a property we have attributed to things because it's easy to explain in terms of charge ...*

G2: *It's just—we call it charge ... it's useful for us to use that term, the theories we have about these things fit ... On Mars they would have called it something different ... and had other theories ...*

M: *Fair enough, they could have called it something different—but would that have made it a different thing?*

G2: *No ...*

M: *But what is charge, then? Suppose I were quarrelsome and hostile to physics, and insisted that charge just does not exist, what would you say to convince me that of course there is charge?*

G1: *We couldn't just say that charge exists, but we might demand an alternative explanation for the phenomena we explain in terms of charge now...how would you explain that? . . . Taken some suitable examples, with pith balls ["hyllemargskuler"] and so on, and ask, how would you explain all this?*

<. . . >

G1: *But I've understood it this way, that in physics we shouldn't talk about what things are, and what is, everything is just models that just fit very well and explain things and predict things in a good way . . . What is there is in a way not interesting.*

One student suggested explaining charge in analogy with magnetism:

G: *Think I would have used a magnet. Two magnets, they're fixed. They either attract each other or repel each other. And say that electricity is a form of magnetism, but different.* <pause> *They act over a distance, that's a little odd. And there's nothing between that pushes or pulls, any physical contact.*

When asked about how to distinguish between magnetic and electric forces this student immediately knew that electrostatic forces could be induced by friction, and that there was no mutual action between charged objects and magnets.

**Measuring charge** This section combines responses to the question about demonstrating the presence of charge and suggestions to the problem of quantifying charge.

The charge of the electron was, very reasonably, suggested as a unit for charge:

G: *It's very tempting to take the elementary charge again, and give that an appropriate unit . . . It's rather small, though, but I'd start with that. . .*

At least, that would be fine in this connection if coupled with a transparent procedure for determining the elementary charge—or, rather, for determining how many multiples of the elementary charge make up the charge to be measured. Unfortunately I did not think of asking the respondent about this.

One student suggested balancing gravitational forces and electrostatic forces in order to quantify charge:

G: *You can for example talk about it in relation to gravitational forces and if you have a charge acting upwards and the gravitational forces downwards then you can compare or something, for you must have a reference value. . .*

Another group, creatively, suggested a measure of charge in terms of the deflection of a stream of water by a charged object. Since water molecules are electric dipoles, a thin stream of water flowing from a faucet is attracted to an electically charged object of any sign. Given a controlled and quantified flow of water, the magnitude of the deflection could serve as a measure of the charge on the object.

One group suggested that the presence of charge could be demonstrated by showing that there was an electric field in the vicinity.

G1: *Yeah, but a charge sets up a field, doesn't it—*

M: *Yes?*

G1: *And I guess it's that field, maybe, that you can look at, a charge sets up an electric field and if you observe that there's a field there must be a charge there—possibly?* <. . . >

G2: *You can use something else that you're* sure *has electric charge. And then if this other thing is either pushed away by it or attracted by it, it must necessarily have a charge.*

This tendency to consider the field rather than just the forces exerted by the charge can be seen in following exchange, from a different group, as well.

M: *But, at least given some fairly simple theories of charge as particles or a fluid, you can distribute it equally on two spheres and . . .*

G: *. . . look at attraction . . . If you are allowed to use electric fields without first explaining what they are . . .*

<laughter>

In one discussion, a measure of charge in terms of electric current was suggested. Charge and current are both familiar concepts to any modern student. The non-triviality of determining the quantitative connection between the two may therefore come as a surprise.

G1: *Attraction . . .*

G2: *We need another charge, then, and we measure both, but measuring them independently . . . A lump of charge. . . ? We need an instrument to measure it, then.*

M: *OK, imagine a simple instrument. . .*

G2: *For example, an ammeter that can measure current . . .*

M: *But does that help you to measure* charge*?*

G2: *No idea, depends on where your charge is . . . If you're sending charge through a wire you can use it—if you have the right theories to calculate afterwards. . .*

M: *Then we get a quite theoretical connection at any rate, for an ammeter works through the magnetic forces the current exerts, so we measure a force that we trace back to magnetism, which we trace back to current, and then to charge. . .*

G2: *Then you have to calculate a lot.*

This group soon arrived at the essentially Coulombian measure of charge, however:

G2: *How much it pushes on something else . . . When you have equal amounts of charge on two spheres . . . You can go from there. Say that if it pushes* that *much the charge is* so *big. . .*

G1: *You can define it directly in terms of forces, then.*

G2: *Newton ... and Coulomb, who was first of the two?*

M: *Coulomb after Newton*

G2: *So then we can use Newton.* <...>

G1: *What about the international Coulomb in Paris or something?*

<laughter>

**Detecting electric current** The students were of course all familiar with the concept of current as a flow of charge:

G1: *That's what a current is, like, that displacement of charge...*

G2: *... Or that a body with charge moves...*

Of course, that does not meant that the concept is easy to come by in the first place. One student's outburst in reaction to the "what is"-question for current expresses the experience of many, I am sure:

G: *O, I remember that* so *well from science class in secondary school... We were to learn about current, but what this current was, what* is *current, I never got an answer to that, I almost gave up! <...> I never understood that.*

A confusion of voltage and current marked one of the dialogues on demonstrating the presence of a current. This was one out of a number of cases where an electric discharge was suggested as an indicator of other quantities than potential difference.

While all the students undoubtedly knew about the magnetic field surrounding an electric current, in two out of three cases a bit of prompting was required before the respondents mentioned the magnetic effects that can signal the presence of a current.

G2: *You have the easiest way* [of detecting a current]. *You grab the wire with your hands and see what happens. If it's strong enough...*

M: *Hmm...*

G2: *... or voltage and so on.*

M: *Yes, the voltage. Suppose you had a current, but knew nothing about the voltage ... ?*

G2: *That's worse. Then you have to insert som nice little measuring instruments...*

M: *Which you could make in what ways?*

G1: *There are lots of them lying around in the lab ...*

<laughter>

M: *No, imagine you lived in 1820 and wanted to convince a skeptic that you did have a current there that appeared in several phenomena.*

G1: *Look at the magnetic field produced ...*

G2: *And go find a compass on a ship.*

The fact that the students did not immediately think of the magnetic effects may be an artifact of the questionnaire—since charge had been discussed a moment before, the students seemed to think they were expected to reason in terms of charges when talking about current, too. Consequently, in one case the question about how to detect a current seemed to be taken to be about how to show that a stream of charges was present—a more difficult problem.

G1: *If you have two charged bodies, if you have a sphere that you know has, has a certain charge, and then you set up something so that a current can pass to another...*

G2: *A piece of wire?* <...>

G1: *And then measure the charge on the sphere once more, by the sphere pushing away something else that is charged with the same sign ...so you would be able to measure a difference in how charged the sphere was...*

G2: *Yeah!*

G3: *I can't really see it if you have an electric* circuit. <...> *I don't see that any other charges would act on it.*

<silence>

G2: *How did an ammeter work, again...?*

<laughter>

M: *Do any of you remember?*

G4: *No. When were we supposed to have learnt that again? In high school?*

<debate about whether this had been taught, and when>

M: *But you* have *learnt about the properties of a current that ammeters rely on.*

G2: *Magnetic fields?* <...>

G1: *But wasn't the point that we were supposed to explain current from charges without thinking about a new property that we have to introduce again.*

G2: *Yeah, there we have it. We just introduce properties for the sake of explanation...*

G1: *If you have an extension cord and you ask, do you get a current in it, and you say yes, and you ask how you know that, it's not so easy to...*

M: *It certainly is legitimate to use the magnetic properties of current; the question was only about how to show that you have a current...*

This question was followed up with one about demonstrating a connection between current and charge, when you measure the current through its magnetic field and charge by electrostatic measures. In one group an animated debate about the momentary magnetic deflection during discharge of a conductor resulted. How would the brief pulse of current be detected?

G: *If it were my sister I'd ask her to remove the insulation from the wire and put her tongue against it.*

**Measuring current** There was one suggestion for measuring electric current that did not in some way rely on the magnetic properties: the idea was to define the unit of current in terms of the amount of heat given off by a current-carrying wire to the surrounding water. The issue of a standardized resistance for such an arrangement was not discussed.

In one group the suggestion was to measure the magnetic field surrounding the current in units determined by a standardized magnet. One problem then was to calibrate other magnets for measurement of currents:

G1: *Yeah... Then you've got to do as... you have to say, this is the* fundamental *magnet that we measure with, and then you have to, sort of, check that all the other magnets are just as strongly magnetic as that one... Then you have to use that, like, as a needle or something...*

G2: *You have a standardized compass, and then you see the deflection, that it's 3 degrees, or 7 degrees.*

G1: *Yeah.*

M: *"Standard compass", I'm afraid that needs some clarification...*

G2: *I guess it doesn't exist, but like we have a standard meter, a defined meter, and in the same way we can define a compass that's this heavy and that long and broad and everything, and when you then get 3 degrees deflection, then you know how strong the magnetic field is. And the larger the current, the larger the magnetic field.*

<debate with everybody speaking at once>

G1: *Or you can check by making several magnets, and then you have this fundamental unit. And then you have a* long *conductor and I put these magnets next to each other, and those who have the same deflection are the same compass, or they give the same unit. Then I can use those other compasses, and tell others to measure with this needle here...*

G3: *But are you completely sure that the magnetic field is proportional to the current?*

Curiously, neither students nor moderator thought about the fact that a magnetic needle would not show the *strength* of the field, only the direction... In order to show the strength of the field, the magnetic needle would have to be restrained by, for example, the torque of a twisted cord, or some other quantifiable force.

Another group, having rejected the idea of a standard magnet in Paris from the outset, quickly arrived at the standard way of defining the unit of current:

M: *Okay, same question as for charge … How would you say how much you've got of it?*

G1: (joking) *I think I'd suggest the ampere …*

<laughter>

M: *Fine, but will you define the ampere?*

G1: *I don't remember the definition, but that is perhaps the point … Hmm … We were to measure current in terms of the magnetic properties. But then we need a measure for magnetism—for the magnetic field … Or can you define current in terms of how currents pull at each other in different directions … then you get to* <laughter> *cancel away some magnetic field…*

In the context of these discussions of measurement, where the roots of the theoretical concepts in the physical phenomena have to be clarified, several students expressed an interest in understanding the historical background for the physical units. I mentioned how electrolysis had been used for a while to give a measure of electric current in terms of mass and time (a fixed mass of water is decomposed to hydrogen and oxygen in the course of a certain amount of time by one unit of current). This, apparently, aroused some fascination:

G1: *But learning about* such *connections can be* very *fortunate, to increase understanding of the subject, simply… For example of how measuring units were introduced…*

**The electric field** One group described the electric field by analogy with the gravitational field, which they expected the 15 year old sister to have some understanding of.

G2: *Electric field? Something that is around a charge…*

G1: *Perhaps go via gravitational field. That's something we can feel with our bodies…*

G1: *Electric fields pull at electric charges (don't ask what that is!). In the same way as the earth pulls at you. Even though you are not touching the earth.*

G2: *You'll have trouble explaining that some things push, though.*

G1: *Yes—it will be like a gravitational field, only it works the other way. Of course you have the problem of explaining what a gravitational field is. You can't explain what it is, only hope they'll be satisfied with knowing how it works.*

A brief exchange on the possibility of finding negative mass followed, with one respondent wondering whether his co-student had also read too much of Hawking…

Another group used the metaphor of a map to describe the field.

G1: *It's the same as a map… it's a map in a way.*

G2: *A map of what?*

G1: *A map of the forces that will act on the charges…*

G3: *Yeah, it is like he says, a kind of map of where, where the electric forces will act.*

G1: *And in which directions.*

G3: *And of course its easy to compare with gravity...*

One respondent made a more unusual suggestion in comparing the electric field surrounding a charged particle with the heat around the sun:

G4: *I think if I were to explain something like this to a child—perhaps this is all wrong—I would perhaps talk about the sun, for example, a glowing sphere, from there light and heat spreads out in all directions, and you can call that a field, and it will fall off by and by. And in that way at least the electric field from a point charge would ... Perhaps it could be understood that way.* <pause>

G4: *You can picture that, like.*

G2: *Yeah, and that's a field, like, and the strength of the field falls off with distance...*

**The field—map or agent?** My questions on this matter were badly formulated. I had wanted to learn something about what status the students assigned to the field, whether they thought about it as something physically real, a causal agent—or rather as a mathematical description of forces between material objects. I had hoped to capture some of their ideas on this issue by asking:

M: *We demonstrate, or verify the presence of, an electric field by looking at the forces on a small test charge. Is the electric field still there when we remove the test charge? If so, how can we know that it is there?*

This question was not successful at all. In all three cases it was interpreted as asking whether the electric field, present at one moment, disappeared or not when we stopped observing it. One group started discussing the question whether a tree falling in the forest makes any noise if there is nobody there to hear it. Another group argued about whether it was sensible to think that the moon was still there when nobody was watching it. In the third, Occam's razor was called upon to argue that the electric field should be taken as being there even when the test charge was removed.

G1: *As long as that is a simpler theory than assuming that it disappears* <...>

G2: *Occam's razor.*

M: *So the simpler theory is to assume that the field is there?*

G2: *Yeah, otherwise you have to explain why it disappears...*

A good deal of time was lost on this unfortunate question, but in some of the groups some very interesting comments were made once the question had been clarified. In one group the respondents were familiar with particle physics, and tended to think about forces as being transmitted by particles moving with the speed of light. By including finite speed for the interaction as well as a mechanism for it, this account (at this level of precision) makes the field superfluous:

M: *Yes ... But, in showing the presence of an electric field, the only thing we* measure *is the electric force, so we measure the* forces *on a charge and then say there is a field there. So, why do we need the field?*

G1: *As I think of the field, I think of something that isn't anything, but is very nice for drawing lines... I don't think I see the field as something existing physically in space.*

G2: *It nicely transmits what is to be transmitted...*

G1: *It's a very convenient way of describing things, much like we introduce fictional forces in mechanics to explain things; we simplify things by introducing the field... for how the force is transmitted, force particles...*

G2: *Particles transmitting gravity...*

G1: *Yes. I don't know how it is with electric fields... Anyway ... I don't have an intuitive understanding that the field is something that exists there ...*

In another group, the question about the role of the field revealed disagreement, and a lively discussion resulted. Some students argued for the idea of the field as a map of the interactions between particles, while one student in particular tried to object to this view.

G4: *But it has an effect on things that are in that area <...> It's much like with heat, then, okay, if you're* there *you'll feel* that *heat, and if you're* there...

G1: *And it gets warmer the closer you get.*

This debate soon continued as one about the field as a causal agent:

G2: *The electric field must be what* pushes, *pushes these charged bodies apart...*

M: *Is it the* field *that pushes them apart?*

G2: *Yeah, the field in a way is a way of describing it, that they push each other apart, and they'll push less if they're further apart...*

M: *That's in a way the question, is it the field that pushes them apart, or does the field, only as a mathematical formulation, describe what will happen?*

G1: *It's a nice simplification for calculating mathematically <...> What gives rise to the forces, physically, is just the charge of one body ... that's here and another that's there.*

M: *So it's forces between the bodies, not between the fields?*

G1: *Yeah, that's what it always is, it's just that we use the fields because that's easier to calculate with.*

Can these views on the field be experimentally distinguished? In discussing the question this group rushed through basic field theory in the course of some minutes:

M: *Is there any way of deciding the matter, that you can imagine, to decide whether there's a physical field, there's energy stored in space out there, and the field acts on particles, or whether there are direct interactions between the particles?*

G1: *Remove those charges that set up the field?*

M: *Well... but according to field theory you would then also remove the field.*

G2: *So there's no way of* showing *it. So we've introduced the field to describe things <...>*

M: *So is it impossible to imagine any experiments that might decide the matter, it's all the same, you can have different metaphysics, so to speak, you can picture space as filled with energy from the fields, or that it is not, and you get just the same results, it doesn't make any difference?*

<everybody talking at once, disagreement>

M: *You mentioned removing the charge... Perhaps if you push it away very quickly, and see whether the other particle reacts at once or whether the field must change first; does the field change as immediately as the charge is moved? <...>*

G3: *It propagates with the speed of light and all that?*

G4: (surprised) *Okay?!*

G2: *But then you have to move that charge very,* very *quickly...*

<laughter>

G1: *Then you can for example set some charges moving with very high velocity and have a test charge at rest, then one can check whether, for example, the field then, which is moving will affect the charges at rest. For if the field propagates with the speed of light... and the particles move with nearly the speed of light the field will, like, lag behind a little all the time, be a little delayed. Perhaps one can look at that?* <pause> *If you know exactly where the other particle is at any given time, and the first particle, then you know, can calculate whether the particle will move with the other particle or the retarded field in a way...*

A discussion of the Doppler Effect followed, with the students placing imaginary charges here and there on the table, moving them abruptly and discussing how the particles and the fields would move relative to each other, musing about what would happen if a particle turned in its tracks and moved back into the field it had sent out a moment before, and so forth. At one point the conclusion seemed clear;

G2: *Then I would say that it's actually the* field *that affects* [the charges]...

However, a moment later the realization that introducing forces with a time delay would give the same results as the field description left the issue unsettled as before:

G1: *We're talking about the field, that it spreads out with the speed of light. But if we also introduce, we also let the forces between the charges propagate with the speed of light ... Then we can't distinguish anyway!*

### 4.4.4 Two philosophical issues

**Metaphysical assumptions** The students were asked about what they thought about a metaphysical objection to Weber's electrodynamics, namely that distance forces were "occult" and unintelligible. Two of the groups thought such objections were unreasonable, arguing that we could not, and should not try to, determine in advance what kinds of physical phenomena there might be:

> G: *Then we wouldn't have had quantum mechanics and that sort of thing, either ... If one should reject an idea just because it seemed disturbing.*

In one group, however, the reaction was different, with one student claiming that

> G: *I think its quite okay to say that, that it doesn't fit, it doesn't fit with our understanding and so we reject that theory.*
>
> M: *Okay?!*
>
> G: *I think that's very sensible.*

In the discussion that followed the need to be explicit about our conceptual limitations and metaphysical assumptions was raised. In that connection one student suggested that knowing history might contribute precisely to such awareness:

> G: *But then there's something wrong now that we have in a way lost the historical part, and think that there are fields, like. That's a reason why I think it's important to learn history, so that we learn it, that some have actually thought about it as direct forces, but that we in a way just can't understand that, or we can't picture it, so we choose something else.*

**Ranking concepts** In conclusion of the interviews, the students were asked to rank the concepts 'charge', 'current', 'voltage' and 'electric field' according to how physically 'real' they were. Two of the groups quickly ranked the terms in just that order, while suggesting that 'charge' and 'current' should share the first place, having pretty much the same ontological status.

One group, however, barely agreed on any ranking, and had a long discussion on this point. This was the same group that had representatives arguing for the physical reality of fields—and perhaps that was not mere coincidence.

Historically, it has varied according to time and place which of these concepts have been considered ontologically primary. While in Weber's theory charges, together with currents, were taken to be the fundamental and most 'real' entities, Faraday and the Maxwellians held the field to be primary. At the time discrete charges were not known experimentally, but they may (presumably?) be thought of as singularities in a field, so that it can still make sense to take the field as the fundamental 'reality'. And some of the 19th century German formulations of electrodynamics in terms of potentials and energy considerations, perhaps they can be taken as representatives of systems where voltage was more primary than the other three concepts mentioned above?

There is much freedom of interpretation with respect to what is real in the world.

## 4.5 Summary remarks

Since as I have explained in section 4.2.3 this study has strong limitations, I present these remarks as suggestions for further inquiry rather than as 'conclusions'. In introduction to this chapter I stated three hypotheses, or suspected findings of the focus group study. Here they are revisited in reverse order.

I had expected considerable reservations on the part of the students against significant history components in physics courses. The students were much more interested than I had assumed. That is of course in part due to the selection of interviewees, as explained above. More significantly, the interest in history appeared to increase in the course of the discussion. This was a hoped-for effect of some of the problems posed to the students. In the questions there had been an emphasis on basic concepts and phenomena, and in particular on explaining and measuring the former in terms of the latter. Such problems, related to the founding of the science, have been the great concern of pioneer electricians and have been beautifully solved by them. The value of this early work becomes apparant only when one starts to reflect on such problems anew—on how these scientific terms can most easily be justified in terms of simple, comprehensible experiments. Once we begin to reflect on such questions, historical arguments and experiments become not only abstractly interesting, but currently useful and relevant.

Another suspicion before the study was that the students would not hold well-developed distinctions between contiguous action, instantaneous distance forces, and delayed distance forces. This would lead to some degree of vagueness and conceptual confusion about electromagnetic action. I found this supposition confirmed. What I had not at all anticipated was the amount of knowledge the students, especially in one of the groups, would generate themselves through one discussion about this issue. Once the question about experimentally distinguishing these various modes of action for forces was raised, the students sorted out the conceptual tangles for themselves.

Finally, though already touched upon above, there was the hunch that the students' physical concepts were weakly rooted in familiar physical phenomena. The students would for example be expected to have more trouble with explaining how to show that electric current consisted of moving charges, than with integrating a current with respect to time to determine the charge. The physical concepts derive from classroom situations and textbooks, and are tested and refined in similar situations. With some very notable exceptions, the students' suggestions for defining fundamental units, for example, were as awkward as I had suspected (that is, almost as bad as my own would have been).

Does that matter? Likely not much, and perhaps not at all. It can make a lot of sense to think of knowledge as defined by a coherence of ideas rather than by stringent reasoning from a secure basis (such as, for example, simple physical phenomena). Another way of expressing this is by asking whether the historical problems of establishing science 'from scratch' are really relevant to us, or whether they are quite artificial now that the conceptual network has been so much further refined and tested. I hardly think that our physical knowledge would be more *certain or secure* if we were more adept at deriving it from, or illustrating it in terms of, simple physical phenomena or hands-on, transparent experiments. As far as the well-foundedness of our scientific beliefs go, I would not argue that it is important.

If the issue is important at all I think it would rather be in terms of questions of the meaning of science for us, and of our sense of science as intelligible. Perhaps we understand electricity better if we can find it, and let it play out, in readily comprehensible phenomena.

The statement that taking the historical, ‘from scratch’-approach can deepen our understanding of physics may not be easy to establish with great rigour, but I will venture it. I will also claim that it can contribute to making physics more meaningful and significant to the learners. For a physics *teacher*, such matters are important too.

# Chapter 5

# Afterword

This thesis has touched upon rather too many different topics to make for a clear and concise conclusion. A thesis on physics education should perhaps conclude with some concrete advice for physics teaching. While a more focused piece of work would have been required in order to be specific, some general suggestions that may be warranted on the basis of this text will be aired below, together with some summarizing remarks.

In chapter 2 I have described a wide range of possible applications of history for physics teaching. I would not single out one for special recommendation, though, since what would work in any particular educational setting would depend strongly on the knowledge and interests of the teacher. However, I do think that the breadth of applications speaks for the inclusion of history of physics in teacher training programmes. After a general course in the history of physics, and some individual specialization following that, a physics teacher would be able to pick and choose among the resources provided by historical knowledge, and find something that he or she could apply effectively and enthusiastically.

At this point it may be noted that if not carried through with care and good judgement, historical content included in physics lessons may just become further dead facts to be memorized along with the equations and definitions of physics. While history *can* be a wonderful resource for bringing physical problems and puzzles to life, it does not automatically have this effect. And if it fails to serve this purpose of breathing meaning and purpose into dusty old experiments and arguments, it should not be heaped on top of the current syllabus for its own sake.

I have devoted much space to hermeneutic theory of understanding. One major recommendation to follow from that work would be an emphasis on interpretation in physics learning, and on relating the material as far as possible to the world of the learners. This may be an educational platitude. Now, any sound theory of education *should* churn out a number of truisms; any theory must account for some basic facts of learning. If hermeneutics can make an obvious point in a new way, or from a different perspective, that is also a contribution.

A more original aspect of hermeneutics is the emphasis on pluralism, and the value accorded to multiplicity of account and formulation. I think this strategy of seeking out differing accounts of the same phenomena should be pursued. Hermeneutics provides arguments for pluralistic teaching that will not be recounted now, but there are many and various reasons to believe that students' understanding of physics would increase if pluralities of formulation and interpretation were highlighted.

A reader may ask whether I consider hermeneutic theory to be *true*. I am not sure, but it is a thought-provoking approach that generates insights and perspectives not readily come by otherwise. A new vantage point is usually of at least some value, I think, and hermeneutics provides an impressingly rich and different story of interpretation, understanding and knowledge. Among the attractions are certain deviations from the big picture provided by constructivism and instrumentalism. While providing recommendations that to a not small extent overlap with those of constructivism, hermeneutics, in speaking of interpretation rather than of construction, seems to accord more weight to that which is to be interpreted, to the entities in the world. Hermeneutics would, for example, emphasize that it is *the electron* that we wish to understand, while constructivism remains silent about electrons and speaks of our invented models about them. Hermeneutics also provides a framework that combines pluralism with realism, it is a way of thinking that does not make it necessary to conclude from the plurality of theories that we can know nothing about the furniture of the world, and must be content with the ability to predict and control. It is a different and difficult question whether hermeneutics is *warranted* in claiming such access to what is real in the world, and I will not enter into any such arguments here. At any rate, I think there are further resources of value for science educators in hermeneutic theory, and that more studies and discussions of these matters would be useful and enlightening.

# Bibliography

Allchin, D.: 2003, Lawson's Shoehorn, or Should the Philosophy of Science Be Rated 'X' ?, *Science & Education* **12**, 315–329.

Allchin, D.: 2004, Pseudohistory and Pseudoscience, *Science & Education* **13**, 179–95.

Angell, C.: 1996, *Elevers fysikkforståelse: En studie basert på utvalgte fysikkoppgaver i TIMSS*, PhD thesis, University of Oslo.

Assis, A. K. T.: 1994, *Weber's Electrodynamics*, Kluwer Academic Publishers, Dordrecht/Boston/London.

Baune, Ø.: 1991, Vitenskap og metode. Textbook for *Examen Philosophicum* at the University of Oslo, Norway.

Benseghir, A. and Closset, J.-L.: 1996, The electrostatics-electrokinetics transition: historical and educational difficulties, *International Journal of Science Education* **18**, 179–191.

Bevilacqua, F.: 1984, *The Principle of Conservation of Energy and the History of Classical Electromagnetic Theory (1845–1903)*, PhD thesis, University of Cambridge.

Bevilacqua, F. and Giannetto, E.: 1995, Hermeneutics and Science Education: the Role of History of Science, *Science & Education* **4**, 115–126.

Bjørkhaug, B.: 2004, *En fokusgruppestudie av fysikklæreres oppfatning av fysikkfaget i videregående skole*, Master's thesis, University of Oslo.

Brush, S. G. and King, A. L. (eds): 1972, *History in the Teaching of Physics: Proceedings of the International Working Seminar on The Role of the History of Physics in Physics Education*, University Press of New England, Hanover/New Hampshire.

Butterfield, H.: 1950 [1931], *The Whig Interpretation of History*, G. Bell and Sons Ltd, London.

Couvalis, G.: 1997, *The Philosophy of Science: Science and Objectivity*, SAGE Publications Ltd, London/Thousand Oaks, CA/New Delhi.

Crease, R. P.: 1997, Hermeneutics and the natural sciences: Introduction, *Man and World* **30**, 259–270.

Cushing, J. T.: 1991, Quantum Theory and Explanatory Discourse: Endgame for Understanding?, *Philosophy of Science* **58**, 337–358.

Cushing, J. T.: 1995, Hermeneutics, Underdetermination and Quantum Mechanics, *Science & Education* **4**, 137–146.

D'Agostino, S.: 2002, From Rational Numbers to Dirac's Bra and Ket: Symbolic Representations of Physical Laws, *Physics in Perspective* **4**, 216–229.

Dancy, J.: 1985, *An Introduction to Contemporary Epistemology*, Blackwell Publishers Ltd, Oxford, UK/Malden, Mass., USA.

diSessa, A. A.: 1993, Toward an Epistemology of Physics, *Cognition and Instruction* **10**, 105–225.

Duschl, R. A. and Hamilton, R. J. (eds): 1992, *Philosophy of Science, Cognitive Psychology, and Educational Theory and Practice*, State University of New York Press, Albany.

Duschl, R. A., Hamilton, R. J. and Grandy, R. E.: 1992, Psychology and Epistemology: Match or Mismatch When Applied to Science Education?, *in* Duschl and Hamilton (1992), chapter 1, pp. 19–47.

Eger, M.: 1992, Hermeneutics and Science Education: An Introduction, *Science & Education* **1**, 337–348.

Eger, M.: 1993a, Hermeneutics as an Approach to Science: Part I, *Science & Education* **2**, 1–29.

Eger, M.: 1993b, Hermeneutics as an Approach to Science: Part II, *Science & Education* **2**, 303–328.

Eger, M.: 1995, Alternative Interpretations, History, and Experiment: Reply to Cushing, Crease, Bevilacqua and Giannetto, *Science & Education* **4**, 173–1–88.

Feynman, R. P.: 1965, The development of the space-time view of quantum electrodynamics. Nobel Lecture, December 11, 1965, URL: http://www.nobel.se/physics/laureates/1965/feynman-lecture.html, accessed April 2004. The paginations have been removed from the online version since I printed out the lecture in June 2003.

Gadamer, H.-G.: 1979, *Truth and Method*, Sheed and Ward, chapter 4, pp. 267–278.

Goodman, N.: 1973 [1955], *Fact, Fiction and Forecast*, 3 edn, Bobbs-Merill, chapter 3: The New Riddle of Induction, pp. 59–83.

Guisasola, J., Zubimendi, J. L., Almudí, J. M. and Ceberio, M.: 2002, The Evolution of the Concept of Capacitance Throughout the Development of the Electric Theory and the Understanding of its Meaning by University Students, *Science & Education* **11**, 247–261.

Guttersrud, Ø.: 2004, *En fokusgruppestudie av fysikkelevers oppfatninger av fysikk og deres grunner for å velge fysikk i den videregående skole*, Master's thesis, University of Oslo.

Hacking, I.: 1983, *Representing and Intervening*, Cambridge University Press, Cambridge.

Heelan, P.: 1983, Natural Science as a Hermeneutic of Instrumentation, *Philosophy of Science* **50**, 181–204.

Heelan, P. A.: 1998, The Scope of Hermeneutics in Natural Science, *Studies in the History and Philosophy of Science* **29**, 273–98.

Heering, P.: 2000, Getting Shocks: Teaching Secondary School Physics Through History, *Science & Education* **9**, 363–373.

Heidegger, M.: 1962 [1927], *Being and Time*, Harper & Row, New York. Translation from the German *Sein und Zeit* by John Macquarrie and Edward Robinson.

Höttecke, D.: 2000, How and what can we learn from replicating historical experiments? A case study, *Science & Education* **9**, 343–362.

Höttecke, D.: 2001, *Die Natur der Naturwissenschaften historisch verstehen: Fachdidaktische und wissenschaftshistorische Untersuchungen*, Logos Verlag, Berlin. Also PhD thesis, Carl von Ossietzky Universität Oldenburg, 2000.

Kitchener, R.: 1992, Piaget's Genetic Epistemology: Epistemological Implications for Science Education, *in* Duschl and Hamilton (1992), chapter 4, pp. 116–146.

Klein, M. J.: 1972, The use and abuse of historical teaching in physics, *in* Brush and King (1972), pp. 12–18.

Kolstø, S. D.: 2001, What place is there for the history of science in a science curriculum for citizenship?, Lecture submittet for a doctoral dissertation at the University of Oslo.

Lie, S. and Angell, C.: 1990, *Fysikk i videregående skole: Hvem velger faget, og hvorfor?*, number 5 in *Skrifter for realfagundervisning*, Senter for lærerutdanning og skoletjeneste og skolelaboratoriet, avd. fysikk, Universitetet i Oslo.

Matthews, M. R.: 1994, *Science Teaching: The Role of History and Philosophy of Science*, Routledge, New York/London.

Matthews, M. R. (ed.): 1991, *History, Philosophy, and Science Teaching: Selected Readings*, OISE Press, Teachers College Press, Toronto/New York.

Maxwell, J. C.: 1998 [1891], *A Treatise on Electricity and Magnetism. Volumes I and II*, Oxford University Press, Oxford/New York.

Meya, J.: 1990, *Elektrodynamik im 19. Jahrhundert: Rekonstruktion ihrer Entwicklung als Konzept einer redlichen Vermittlung*, Deutscher Universitäts-Verlag GmbH, Wiesbaden. Also PhD thesis, Carl von Ossietzky Universität Oldenburg, 1990.

Miller, A. I.: 1987, Introduction to the Reissue of Whittaker's Volumes I and II, *in* Whittaker (1987).

Moravcsik, J. M.: 1979, Understanding, *Dialectica* **33**, 201–216.

Nersessian, N.: 1992, Constructing and Instructing: The Role of "Abstraction Techniques" in Creating and Learning Physics, *in* Duschl and Hamilton (1992), chapter 2, pp. 48–68.

Nersessian, N. J.: 1989, Conceptual Change in Science and in Science Education, *Synthese* **80**, 163–183.

Nersessian, N. J.: 1995, Should Physicists Preach What They Practice? Constructive Modelling in Doing and Learning Physics, *Science & Education* **4**, 203–266.

Næss, A.: 1982 [1969], *Hvilken verden er den virkelige? Gir filosofi og kultur svar?*, Universitetsforlaget, Oslo/Bergen/Tromsø.

Prosser, M., Walker, P. and Millar, R.: 1996, Differences in students' perceptions of learning physics, *Physics Education* **31**, 43–48.

Siegel, H.: 1988, *Educating Reason: Rationality, Critical Thinking, and Education*, Routledge, New York/London, chapter 6: Science education, pp. 91–115.

Siegel, H.: 1991, The Rationality of Science, Critical Thinking, and Science Education, *in* Matthews (1991), pp. 45–62.

Sjøberg, S.: 1998, *Naturfag som allmenndannelse—en kritisk fagdidaktikk*, Ad Notam Gyldendal, Norway.

Solomon, J. and Aikenhead, G. (eds): 1994, *STS Education: International Perspectives on Reform*, Teachers College Press, New York.

Taylor, C.: 1987, Interpretation and the Sciences of Man, *in* P. J. Rabinow and W. M. Sullivan (eds), *Interpretive Social Science*, University of California Press, Berkeley/Los Angeles/London, chapter 1, pp. 33–81.

Viard, J. and Khantine-Langlois, F.: 2001, The Concept of Electrical Resistance: How Cassirer's Philosophy, and the Early Developments of Electric Circuit Theory, Allow a Better Understanding of Students' Learning Difficulties, *Science and Education* **10**, 267–286.

Whitaker, M. A. B.: 1979, History and quasi-history in physics education, Part I and II, *Physics Education* **14**, 108–112 and 239–242.

Whitaker, M. A. B.: 2000, Analysis of some Arguments on Quantum Interpretation, *Annales de la Fondation Louis de Broglie* **25**, 309–324.

Wittje, R.: 1996, *Die Frühen Experimente von Heinrich Hertz zur Ausbreitung der 'Elektrischen Kraft'*, Master's thesis, Carl von Ossietzky Universität Oldenburg.

Ziman, J.: 1994, The Rationale of STS Education is in the Approach, *in* Solomon and Aikenhead (1994), chapter 3.