Chapter 1
Pedagogy and Research. Notes for a Historical Epistemology of Science Education

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1.1 Transmitting Scientific Knowledge

"Those who can't do teach, and those who can't teach, teach gym." Woody Allen's scornful comment on the role of teaching in *Annie Hall* summarizes fairly well one very popular view. For many, there is a clear-cut distinction between the creative intellectual activity of research and the mere repetition of what someone else has produced to a classroom of students. To be sure, this view affects not only teaching and learning. Rather, it is more or less implicit in any occurrence of the exposition, communication, or transmission of scientific knowledge from the community of experts to the external world.

More importantly, this view is sustained by a certain model of science and its relations with society. The basic tenet of this model—sometimes attributed to Robert K. Merton and therefore called *Mertonian* (Cloitre and Shinn 1985), sometimes more simply called the "classical image of science" (Renn and Hyman 2012b)—is that knowledge produced within the scientific culture is radically different from any of its disseminations to the broader society. More precisely, the classical image of science pictures the scientific community as a highly structured and organized elite of experts, who produce a carefully defined and thoroughly validated—and therefore true—body of knowledge, which is in turn transmitted to an audience (students, informed public, laymen). Finally, this heterogeneous audience is, to various extents, incapable of fully appreciating the products of scientific inquiry without an adequate re-elaboration, and consequently, it is totally unable to feed anything back to the scientific elite.[1]

Although completely discredited by the scholarly work of the last thirty years, this model has maintained its grip on public representations of science. The main reason is that, even though successful in criticizing each of the tenets of the classical image, philosophers, historians, and sociologists of science have not been able to provide an alternative account that is as intuitive and all-embracing. This failure should not be exclusively ascribed to the contemporary tendency of scholars in science studies to insist on the disunity and locality of scientific culture (Galison and Stump 1996). It is also due to the fact that the several branches of specialized work on the transmission of scientific knowledge have grown at different paces. Thus, for example, popularization both aimed at the general public and at fellow scientists belonging to other disciplines received attention as early as the mid-1980s.[2] About the same time, the works of Harry Collins and Bruno Latour, among others, covered

[1] See for example (Whitley 1985; Hilgartener 1990; Olesko 2006).

[2] See the 1985 Yearbook of *Sociology of the Sciences* edited by Terry Shinn and Richard Whitley and especially (Bunders and Whitley 1985).

the analysis of the circulation of knowledge among experts and the transmission of scientific applications to social actors interested in their economic exploitation (Collins 1985; Latour 1987; 1988). By contrast, a systematic investigation of scientific pedagogy has taken off only in the last fifteen years. Instrumental to this general revamping of the image of scientific training has been a re-evalutation of the role of textbooks. Projects such as the volume edited by Anders Lundgren and Bernadette Bensaude-Vincent on the circulation of textbooks on chemistry from the French Revolution to the eve of World War II (Brooke 2000), the 2006 special issue of *Science and Education* on textbooks at the scientific periphery (Bensaude-Vincent 2006; Bertomeu-Sánchez et al. 2006), David Kaiser's edited collection of studies on pedagogy in science (Kaiser 2006), and the *focus section* in *Isis* in 2012 (Vicedo 2012), are just a few of the major steps taken in recent times towards a modernization of analyses of pedagogy and textbooks in science studies.

1.2 Creating Knowers, Creating Facts

However, one should notice that the attitude of scholars towards traditional views of scientific pedagogy has been complex and occasionally ambivalent. It is thus important to reconstruct some lines of development of this attitude.[3] One important line of inquiry many scholars have followed concerns the role of pedagogy and textbooks in producing *knowers*, that is a professionally organized group of people explicitly trained to perpetuate a certain kind of knowledge. It was Thomas Kuhn's deep criticism of the logical positivistic view of science as a purely theoretical activity that first highlighted, for many scholars, the role of training in determining the working style, the self image, and even the ontology of scientists, thus restoring dignity to the learning process (Kuhn 1962). As David Kaiser points out, "scientists are not born, they are made" (Kaiser 2006, 1), and the process of making a scientist has a profound influence on the way in which he or she will conduct future research. What is a good question, what is a satisfactory answer, what counts as a legitimate scientific procedure or a correctly conducted experiment, even what is viewed as a possible object of research is determined, according to the Kuhnian model, during the inculcation of the reigning paradigm, occurring at the training stage (Kuhn 1962, 359; 1963). Pedagogy is not solely a social phase in the formation of the "type" scientist, but is also crucially significant for the broader definition of disciplines and fields of knowledge.

Ironically, as he was giving new philosophical dignity to pedagogy, Kuhn was also playing a key role in keeping textbooks far from the inquisitive examinations of historians. Famously, Kuhn claimed that textbook writing is an activity almost exclusively performed during the peaceful periods he dubbed normal science. In his words, textbooks "are produced only in the aftermath of a scientific revolution […] [t]hey are the bases for a new tradition of normal science" (Kuhn 1962, 144). They "address themselves to an already articulated body of problems, data, and theory, most often to the particular set of paradigms to which the scientific community is committed at the time they are written" (Kuhn 1962, 136). From this point of view, textbooks are only written once a revolutionary process is coming to an end, and their role is basically to transmit the newly-accepted paradigm, never to pose problems for it. Although scientific training does have a critical bearing on scientific culture

[3] Some useful accounts of the role of pedagogy and especially textbooks in science studies are (Myers 1992; Brooke 2000; Olesko 2006; Kaiser and Warwick 2006).

as a whole, for Kuhn it still differs from research in a fundamental manner. This position is clearly stated in his paper "The Function of Dogma in Scientific Research," written only a year after *Structure*:

> Perhaps the most striking feature of scientific education is that, to an extent quite unknown in other creative fields, it is conducted through textbooks, works written especially for students. Until he is ready, or very nearly ready, to begin his own dissertation, the student of chemistry, physics, astronomy, geology, or biology is seldom either asked to attempt trial research projects or exposed to the immediate products of research done by others—to, that is, the professional communications that scientists write for their peers. (Kuhn 1963, 350)

Moreover, textbooks also have a hidden agenda: to erase any trace of crisis, of instability, of change, of historical contingency, and to present the ruling paradigm as an established, consistent whole—as the truth revealed. This trait not only transforms textbooks into repositories of dead doctrines, but it also disqualifies them totally as historiographical tools. Historians should keep away from the image of science conveyed by pedagogical texts. In his later paper "The Essential Tension," Kuhn insists on this view of the roles of textbooks:

> [T]he various textbooks that the student does encounter display different subject matters, rather than, as in many of the social sciences, exemplifying different approaches to a single problem field. Even books that compete for adoption in a single course differ mainly in level and in pedagogic detail, not in substance or conceptual structure. Last, but most important of all, is the characteristic technique of textbook presentation, except in their occasional introductions, science textbooks do not describe the sorts of problems that the professional may be asked to solve and the variety of techniques available for their solution. (Kuhn 1977, 229)

Kuhn seems to extend contemporary Western university education to all times and places when he says that "[t]ypically, undergraduate *and* graduate students of chemistry, physics, astronomy, geology, or biology acquire the substance of their fields from books written especially for students" (Kuhn 1977, 228). Almost certainly Kuhn's view of textbooks is autobiographically motivated, rooted in his own training. Educated in theoretical physics, Kuhn came to see textbooks as a collection of formulas, theorems, and formal techniques; i.e., a set of rules. But rules, Wittgenstein taught us, do not contain the conditions of their own application (Wittgenstein 1953). These conditions are eminently social, partly conventional, and surely cannot be formalized. Textbooks, by extension, would not have a history separate from the practices of their use and, more importantly, they would not be vehicles for history.

Apart from his harsh judgement on the epistemological and historiographical role of textbooks, Kuhn's conception of pedagogy, as functional to the formation of knowers, has been highly influential in several directions of research within science studies. For instance, the Kuhnian emphasis on disciplinary identity as the minimal unity around which knowers organize themselves has led to extensive historical investigations of the effect that pedagogical practices and texts have on the construction of disciplines. Pioneered by Owen Hannaway in the 1970s (Hannaway 1975), this line of research has been developed by, among others, Josep Simon (2011), and explicitly defended by Kostas Gavroglu and Ana Simoes, who argued that "textbooks from an early period in a discipline's history can also be viewed as a genre whose aim was to consolidate a consensus as to the language and practices to be adopted" (Gavroglu and Simoes 2000, 415–416).

Furthermore, Kuhn insisted that pedagogical practices, and therefore knowers, are temporally, spatially, and socially situated. The local aspects of scientific knowledge have encouraged many scholars to look more carefully into the mechanisms for producing national styles in the sciences and into the dynamics of incorporating novel knowledge into the pedagogical routine. Started as demographical studies at the end of the 1970s (Pyenson and Skopp 1977; Pyenson 1979; Jungnickel 1979), these investigations have originated important contributions on the microstructure of the day-to-day exchange between mentors and pupils, both in classes and in special seminars. Major examples are Andrew Warwick's deep study on the meaning of the Cambridge system of Mathematical Tripos for British mathematical physics (Warwick 2003), Karl Hall's account of the role of Landau's and Lifshitz's *Course of Theoretical Physics* in determining the style of physical research in the Soviet Union (Hall 2006), and the discussion of the influence of James J. Sylvester and Felix Klein on the developing American mathematical community pursued by Karen Hunger Parshall and David Rowe (1994).

Pedagogical practices can even lead to the establishment of "research schools" able to imprint a characteristic mark on subsequent research. The pioneering work of Jack Morell, who applied the notion of "research school" to the laboratories of Justus Liebig and Thomas Thomson was the starting point of a tradition that has provided new insights into the relationship between research and pedagogy in the sciences (Morell 1972; Brock 1972; Holmes 1989). Morell showed that Liebig's chemical laboratory owed its success largely to the regime of learning and production that he established in Giessen. From there, the tradition of hands-on training extended to university laboratories throughout modern Europe, encountering sometimes more, sometimes less resistance from those who thought of liberal education as a purely intellectual activity. Kathryn Olesko and, more recently, Suman Seth have extended this tradition to the research schools created around Franz Neumann in Königsberg and Arnold Sommerfeld in Munich, respectively, highlighting the importance of face-to-face interaction between professors and students in close, problem-oriented seminars (Olesko 1991; Seth 2010).

Finally, and more significantly for the purpose of this volume, even the teaching of theoretical physics, which does not need, in principle, the work of laboratories, can be understood to fit within this historiography of hands-on practices, of the transmission of a particular type of craftsmanship, and of specific social values, as shown in the work of historians such as Sharon Traweek, David Kaiser, and Ursula Klein, to cite only a few examples (Traweek 1988; Klein 2003; Kaiser 2005).[4]

Prominent as it was, Kuhn's view was not the only attempt to understand pedagogy in science. Along with the process of producing knowers, historians, philosophers, and sociologists of science have inquired into the effect of training in producing *scientific facts*. Ludwik Fleck wrote some of the most illuminating pages about this social phenomenon. In his 1935 book, which would inspire Kuhn himself many years later, Fleck distinguishes three

[4] This list of topics covered by the study of scientific pedagogy and textbooks does not aim to be exhaustive. Further interesting themes of research, together with a bibliography that includes studies in psychology and other human sciences, can be found in (Vicedo 2012, 85).

elements in scientific education: experience, cognition, and sensation. Through following a pedagogical path, young scientists-to-be are educated to see, feel, and conceptualize the world in a certain manner in order to become part of the established *thought collective* or *thought style*. Partially reshaping the scientific self, this process also reshapes the world around the subject: "a fact always occurs in the context of the history of thought and is always the result of a definite thought style" (Fleck 1979, 95). Fleck also separates sharply popularization from professional training: "in contrast with popular science, whose aim is *vividness*, professional science in its *vademecum* (or handbook) form requires a *critical synopsis in an organized system*" (Fleck 1979, 117–118). The vademecum is the medium of scientific pedagogy, the organized synthesis of what is relevant and worthy in the field. Like Kuhn, Fleck also insists on the difference between research—a creative activity that can even produce contradictory results—and pedagogy, which he represents through the metaphor of a carefully prearranged mosaic:

> The vademecum is therefore not simply the result of either a compilation or a collection of various journal contributions. The former is impossible because such papers often contradict each other. The latter does not yield a closed system, which is the goal of vademecum science. A vademecum is built up from individual contributions through selection and orderly arrangement like a mosaic from many colored stones. The plan according to which selection and arrangement are made will then provide the guidelines for future research. It governs the decisions on what counts as a basic concept, what methods should be accepted, which research decisions appear most promising, which scientists should be selected for prominent positions and which should simply be consigned to oblivion. (Fleck 1979, 119–120)

So far-reaching are the consequences of scientific training. Through the medium of the pedagogical text, both the self, and the world undergo a complete reconfiguration. This crucial insight has suggested to practitioners in science studies to look more carefully into the internal structures of these texts, the economy of their contents, and the communication techniques they deploy.[5] Bruno Latour and Steve Woolgar have provided an impressive analysis of the textual construction of scientific facts through a fivefold categorization of scientific propositions, ranging from *type 1* statements, which qualify the belief as belonging to a certain actor and certain conditions, to *type 5* statements, which black-box the belief as a generally accepted part of common knowledge. Textbooks, Latour and Woolgar conclude, usually do not hedge their claims, but deliver them as the bare truth about nature:

> Scientific textbooks were found to contain a large number of sentences of the stylistic form: "A has a certain relationship with B." […] Expressions of this sort could be said to be *type 4* statements. Although the relationship presented in this statements appears uncontroversial, it is, by contrast with *type 5* statements, made explicit. This type of statement is often taken as the prototype of scientific assertion. (Latour and Woolgar 1986, 77)

Accordingly, textbooks play an important role in sedimenting concepts, methods, experimental procedures, and orthodox interpretations. This aspect has been investigated by a

[5] An interesting development in this line of thought is the analysis of the rhetoric of science and its bearing on the creation of scientific facts; see for example (Fahnestock 1986; Prelli 1989; Gross 1990).

number of scholars, for example Mary Smyth in her reconstruction of the function of textbooks in creating consensus in psychology (Smyth 2001) or Antonio García-Belmar, Josè Ramon Bertomeu-Sánchez, and Bernadette Bensaude-Vincent, who, in their comprehensive account of French chemistry textbooks, trace the way in which the atomistic hypothesis was received and sustained in the scientific community (García-Belmar, Bertomeu-Sánchez, and Bensaude-Vincent 2006).

1.3 Towards an Epistemological Role for the Pedagogical Text

Reflection on scientific pedagogy and textbooks has hitherto generated an impressive amount of scholarly work, remarkable both in depth and in scope. A prime feature of this work has been the careful reconstruction of the pedagogical practices, the teaching procedures, the social negotiations, and the institutional settings involved in the transmission of knowledge from the scientific elite to those who are supposed to replace it in the near future. However, the fragmentation of this analysis into contingent and situated practices, does not restrict the ambition towards an encompassing model of knowledge transmission able to capture the rich material analyzed in a consistent view, and possibly to enlarge upon it. Quite the contrary, special interest has arisen in recent times in a more epistemological perspective able to illuminate persistent, long-term elements in scientific pedagogy, which tend to remain concealed in more detailed accounts. For this task, besides Kuhn, an obvious source to call upon is Michel Foucault.

In *Discipline and Punish* Foucault showed that, from the eighteenth century onwards, discipline steadily increased its efficiency by means of carefully partitioned spaces, calibrated times, and constrained behaviors (Foucault 1977). Control over the body of the individual is at work in prisons, in hospitals, in military institutions, as well as in schools. To be sure, it is precisely to schools that Foucault dedicates his most stimulating analysis. For Foucault, the pedagogical activity displays itself in three phases: hierarchical observation, normalizing judgement, and examination. The first phase requires the organization of space-time relations between the teacher and the students: the architecture of the classroom, the disposition of the seats, as well as the partition of time for lecturing, exercising, and resting. But it also requires a perpetual gaze from the teacher, which provides the control over posture, gestures, and behaviors. This control is always accompanied by a judgement, whose aim is to normalize the individuals to some preconceived orthodoxy. For this judgement one needs comparison and, more generally, examination, carried out according to rules, procedures, and routines, and evaluated according to normalizing systems.

While Foucault casts these disciplinary settings in terms of social alignments and the power relations established between the controller and the controlled, the master and the pupils, one cannot help but think about the similarities with Kuhn's pages on training. For one thing, the examinations can be really effective and normalizing only if the students have been suitably drilled in the "rules of the game," which is precisely what a paradigm is supposed to do. Docility and the cherishing of tradition are thus the essential ingredients of this approach. An approach whose implicit social alignment had been perceptively anticipated by John Dewey:

> Since the subject-matter as well as standards of proper conduct are handed down from the past, the attitude of pupils must, upon the whole, be one of docility,

> receptivity, and obedience. Books, especially textbooks are the chief representatives of the lore and wisdom of the past, while teachers are the organs through which pupils are brought into effective connection with the material. Teachers are the agents through which knowledge and skills are communicated and rules of conduct enforced. (Dewey 1938, 18)

It is with these similarities in mind that Andrew Warwick and David Kaiser have argued in favor of a "Foukuhnian" position as a possible general framework for the study of scientific pedagogy. In essence, this position boils down to an attempt to further historicize Kuhn's intuition that theoretical knowledge requires routinizing practices through Foucault's view of power as a productive force, acting by means of microscopic forms of social control, and it is developed in two points:

> [F]irst by noting the compatibility of Kuhn's emphasis on skill acquisition with Foucault's insight that power is the form of social relations does not inhibit or conceal knowledge, but is necessary to its production; and, second, by building on Foucault's claim that the minutiae of everyday practices have the power to generate new capabilities in human beings, thereby bringing about significant historical change. (Kaiser and Warwick 2006, 406)

This attempt at putting together the best of two worlds points us toward very interesting perspectives, but it still contains some fundamental difficulties. To begin with, the second point, referring to the production of historical change through everyday practices, seems to beg the question raised by the first. Kaiser and Warwick are certainly right in highlighting the similarity between Kuhn's notion of normal science based on paradigms and Foucault's normalizing regimes relying on disciplinary techniques. However, how these regimes can produce new knowledge, possibly knowledge that challenges the paradigm itself, is a particular sticking point in Kuhn's model and remains so in Foucault's. Occurring during periods of accepted paradigms and developing through normalizing procedures, the Foukuhnian pedagogy seems to leave little room for individual creativity. The strong emphasis that both Kuhn and Foucault put on the one-sidedness of the pedagogical relation between master and student makes it difficult to explain how training can turn a docile and obedient pupil into an independent researcher able, at some point, to metaphorically kill his/her master, that is, to challenge the paradigm itself. Suman Seth puts his finger on this problem when he writes: "disciplining, most specifically, cannot produce people who themselves produce new knowledge and it is the production of novel knowledge that distinguishes the researcher from the student" (Seth 2010, 69).

Furthermore, the daring combination of Kuhnian and Foucaultian insights seems at times to stretch too broadly and thinly the positions of both authors. On the one hand, as we noticed above, Kuhn's discourse on paradigm and scientific pedagogy appears to stem entirely from, and to be applicable especially to, physical sciences such as chemistry or theoretical physics. Foucault, however, famously eschewed entering into the genealogy of the physical sciences:

> [F]or me it was a matter of saying this: if, concerning a science like theoretical physics or organic chemistry, one poses the problem of its relations with the political and economic structures of society, isn't one posing an excessively

> complicated question? Doesn't this set the threshold of possible explanations impossibly high? (Foucault 1980, 109)

On the other hand, and more to the point of this volume, while Kuhn has much to say about textbooks and their relation with the whole body of knowledge—we provided ample textual evidence above—Foucault is almost silent about this topic; he prefers to focus upon the power relations displayed in specific patterns of social control and hands-on acquisition of knowledge.

Finally, their other similarities notwithstanding, it should not be forgotten that Kuhn and Foucault differ in at least one important respect, perceptively remarked upon by Hubert Dreyfus and Paul Rabinow (1983, 199–202). Foucault aims to characterize an interpretative dimension of the microscopic and macroscopic mechanism of society that is totally missing in Kuhn. Reflection on the intersections between power and knowledge inevitably entails an evaluation of the direction these processes take together with an evaluation of our society as a whole. This worry, absent in Kuhn, suggests that we should not underestimate the differences in aims and methods between the two writers.

These considerations lead us to the conclusion that the Foukuhnian approach needs to be complemented by further insights. This complementation should, we believe, derive from an insistence on the "knowledge" horn of the Foucaultian power/knowledge duality. Only in this way can the practice-oriented approach hitherto developed lead to an analysis of scientific pedagogy able to encompass two crucial, and interrelated, requirements. First, textbooks should become legitimate historiographical tools, used to illuminate not only the history of pedagogical practices, but, occasionally, the history of science as a whole. This perspective challenges head-on Kuhn's contention that textbooks provide historically and conceptually misleading perspectives on science making. Moreover, this requirement goes hand in hand with the second one: while both Kuhn and Foucault have insisted on the centrality of pedagogical practices in periods of stability and normal science, it is important to extend our gaze to what happens in times of scientific breakthrough. Theories can be in flux on the written page too, if science is in a period of crises. Thus, if we move our spotlight from the quiet days of normal science to the turmoil of an epoch-making crisis, we realize that textbooks cease to be the neutral repository of truth and enter a dialogue with active research. Through this dialogue pedagogy can offer us an original window on the production and dissemination of scientific knowledge.

Key to this twofold extension are the conceptual resources of historical epistemology and the insights they can provide us on the dynamics of scientific knowledge.[6] To begin with, by focusing upon the exploration of "the dynamics of scientific developments, as they can be extracted from an analysis of scientific texts and practices" (Feest and Sturm 2009, 3), historical epistemology has led to the conclusion that one should ease the Kuhnian distinction between normal science and revolutionary periods. Specifically, the historian of science should be entitled to look at textbooks not only as *products* of scientific change, useful only as tools in training regimes, but also as *active agents* in the creative process of scientific development. A new paradigm is not established overnight, and textbooks appear not only at the end-stages of scientific change.

[6]On the multiple forms that historical epistemology can take in different research contexts, see (Daston 1994; Renn 2006; Feest and Sturm 2009; 2011; Rheinberger 2010).

These thoughts nicely complement Foucault's power/ knowledge duality. "Political power always implied the possession of a certain type of knowledge" (Foucault 2000, 31) and especially true knowledge: "[w]e are subjected to the production of truth through power and we cannot exercise power except through the production of truth" (Foucault 1980, 93). Textbooks, Kuhn points out, are repositories of truths, but to reach that status a process of selection, re-evaluation, and redefinition must be put in place. Textbooks contain previously shared knowledge, which undergoes a process of elaboration and reconfiguration. Truth, historically taken, emerges against the background of inadequate knowledge and the investigation of the struggle for truth is precisely what power/knowledge is about:

> [T]o extend the claims to attention of local, discontinuous, disqualified, illegitimate knowledges against the claims of a unitary body of theory which would filter, hierarchize and order them in the name of some true knowledge and some arbitrary idea of what constitutes a science and its objects. (Foucault 1980, 83)

The studies in this volume aim exactly at de-black-boxing the process of construction of truth in textbooks during a period of crisis.

Second, and more generally, an important tradition of cognitive and epistemological studies on learning has led us to realize that research and pedagogy share the same epistemological fabric. Jean Piaget and, more recently, Peter Damerow highlighted the role of reflection on the resources and the tools of knowledge as a crucial step in learning,[7] but the same epistemological process also guides research, even those leading to revolutionary breakthroughs. Nancy Nersessian went as far as stressing a structural similarity between the learning process and conceptual changes:

> Students learning a scientific representation must also actively construct: they must form new concepts and new relations among existing concepts and integrate the new representation to such an extent that they can make use of it. […] [B]oth the nature of the changes that need to be made in conceptual restructuring and the kinds of reasoning involved in the process of constructing a scientific representation are the same for scientists and students of science. That is, the cognitive dimension of the two processes is fundamentally the same. (Nersessian 1989, 165)

Historical epistemology has internalized the piece-by-piece view of knowledge development that this tradition entails. New revolutionary ideas usually emerge at the boundary between different areas of knowledge as the result of internal tensions present in these areas. But a new idea, however radical, is not yet a scientific revolution or a new paradigm. Precisely because it stems from collisions at the boundaries between different theories, it belongs to none of them. At the beginning, innovative ideas are in 'epistemic isolation.'[8] The transition to a new science can be completed only through the long, intricate, and often tedious process of comparing the novel idea with the established body of knowledge (Renn 2006). This attempt at epistemic integration of novelty and tradition progressively unfolds the revolutionary potential of the new idea and generates the consensus about a new approach that characterizes a paradigm. Paraphrasing Kaiser's catchy sentence quoted above:

[7] See for example (Davis 1990; Damerow 1996).
[8] On the concept of epistemic isolation see (Büttner, Renn, and Schemmel 2003).

"revolutions are not born, they are made." Interestingly, and at this point unsurprisingly, the same epistemological drive can be found in scientific pedagogy during a time of crisis, as the articles in this volume show extensively.

Since textbooks, by necessity, bring into contact tradition and novel approaches, they relentlessly explore the potentialities of older tools and their connection with newer ones. This process, which Kuhn interpreted as concealing the tracks of a revolution, recapitulates in reality the essence of the research process. We can see this dynamic instantiated in the books of Planck, Sackur, Sommerfeld or Reiche discussed in this volume.

Again, this insight adds another dimension to Foucault's and Kuhn's positions. Organization of knowledge occurs at different levels and involves different disciplinary matrixes, leading to heterogeneity and blurring of the boundaries sharply drawn in periods of normal science. This is also a Foucaultian theme, best put by Joseph Rouse:

> Knowledge is established not only in relation to a field of statements, but also to objects, instruments, practices, research programs, skills, social networks, and institutions. Some elements of such an epistemic field reinforce and strengthen one another and are taken up, extended, and reproduced in other contexts; others remain isolated from, or conflict with, these emergent "strategies" and eventually become forgotten curiosities. The configuration of knowledge requires that these heterogeneous elements be adequately adapted to one another and that their mutual alignment be sustained over time. (Rouse 2005, 113)

It is on this complex process of combination of heterogeneous elements, of exclusion/inclusion, and of reconfiguration—a process typical of scientific research—that an investigation of "textbooks in flux" can provide illuminating insights.

1.4 Rethinking the History of Quantum Physics

This volume wants to contribute to the study of textbooks as agents of research by focusing attention on one specific episode in the history of scientific change: the so-called quantum revolution.[9] The emergence of quantum theory, in particular, represents an ideal setting because it is a multidisciplinary, delocalized, and multi-actor phenomenon. The canonical account of this chapter in the history of science starts with the *crisis* of black-body radiation and the solution put forth by Max Planck at the turn of the century. After this, Albert Einstein's 1907 theory of specific heats, Niels Bohr's 1913 model of the hydrogen atom, and the advent of Werner Heisenberg's and Erwin Schrödinger's quantum mechanics in the mid-1920s form the conceptual backbone of a story that, together with the development of relativity, has taken pre-eminence in the history of twentieth-century science. Historians of physics have, for decades, struggled to write a coherent account of a process that eludes simplistic explanations. There are too many, too diverse elements that contribute to the complexity of this particular story: the range of the conceptual changes that took place; the number and diversity of the actors involved; the institutional settings; the networks of power and complicities between scientists, popularizers, and science policy makers; the social and

[9] There are some studies concerning the transmission of knowledge during scientific change, such as the paper by Bernadette Bensaude-Vincent on the emergence of the chemical revolution (Bensaude-Vincent 1990). However, no application of this analysis to the quantum revolution has so far been attempted.

cultural ethos of the times around the two World Wars, not to mention the Manhattan Project, the bombs of Hiroshima and Nagasaki, and the Cold War. Furthermore, no other chapter in the history of recent physics, let alone in the history of science, has the same wealth of material available to the historian, including the gigantic project that produced the Archive for History of Quantum Physics.

The essays collected in this volume bring new light to this massive scholarship by concentrating upon early textbooks on quantum theory. This is one outcome of the large-scale, international project coordinated by the Max Planck Institute for the History of Science and the Fritz Haber Institute in Berlin, on the *History and Foundations of Quantum Physics*, that has worked to emphasize the importance of tradition and the conceptual reservoirs of classical physics in the establishment of the quantum revolution, thereby highlighting the continuous aspects within such a dramatic epistemological shift. The rationale behind this volume is that, since textbooks have seldom been treated either as relevant sources or as actors in the development of the new physics, it was worthwhile exploring the possibilities of treating some of these books as subjects around which to write new stories of the quantum.

A specific emphasis on the epistemological aspects of scientific pedagogy during the emergence of quantum physics can turn textbooks into useful research tools in two different senses. First, the study of how textbooks were conceived, projected, and written can elucidate many of the historical circumstances of the coming of age of the quantum revolution, aspects that remained hidden in the study of research papers. To begin with, it gives us access to the revolution on a different time scale because textbooks have a different life cycle from research articles. Furthermore, contrary to research works, pedagogical texts address a broader scope of topics, ranging from atomic theory to physical chemistry, and a wider audience, thus providing us with a wide-angle snapshot of the community involved in the quantum business.

Secondly, there is a particular character to the way textbooks are understood and composed that makes them especially useful for revealing some elements of the intrinsic dynamics of scientific knowledge. Textbooks, particularly in a moment of scientific turmoil, re-organize the inherited body of knowledge and try to integrate it with the emerging theories. This reflective process, which can involve new hypotheses, concepts, and assumptions, but also new formal techniques, procedures, and methods, is essential in igniting productive thinking. In other words, textbooks offer a privileged example of the systemic quality of knowledge, which seems to be a general feature of the transmission of knowledge in its globalizing dimension (Renn and Hyman 2012a).[10]

As the chapters in this book show, there are many different ways in which a textbook can become the subject in a history of early quantum physics, since the very process of writing a textbook, (i.e., of trying to organize a new doctrine in an accessible way for newcomers), together with its life as an object that is issued, used, changed, and abandoned, embodies the tensions between research and pedagogy developed in the first part of this introduction. Furthermore, the life of these textbooks can also help us better situate other actors in the history of quantum physics, by bringing into the picture the reasons, the context, the research

[10]By the same token, a re-evaluation of the epistemological role of textbooks is also necessary in terms of university policy making. A deep reorganization of the university curricula, essential to meet the challenges of the globalized society, requires a broader approach to how scientific knowledge is accumulated and how novelties have to be included in the pedagogical routine. On this topic see the project *Vom lokalen Universalismus zum globalen Kontextualismus* led by Yehuda Elkana and Jürgen Renn and its theoretical foundation in (Elkana 2012).

agenda, and other aspects that cannot be seen in the publication of research papers or in the abundant correspondence between the main actors involved in the story.

Obviously, the first question to address was how to qualify a book as an early textbook on quantum matters. Contrary to the case of chemistry, where there is a longer tradition of textbook writing, going back to the nineteenth century, some of the instances studied in this volume qualify as textbooks, not because they were formally and explicitly written as such, but mainly because they were used as tools to teach quantum physics in higher education. As David Kaiser has recently pointed out, textbooks possess a peculiar *plasticity* with respect to their collocation, their genre, and their boundaries (Kaiser 2012). During scientific re-alignments this feature becomes even more prominent. Furthermore, the complexities and technicalities of the discussions involved narrow the public to which these books were addressed: only professional physicists and advanced students of physics could have a real interest in and ability to follow the nuances present in these books. We, therefore, exclude popular books. The ten case studies presented here include books from well-known actors in the development of quantum physics, like Max Planck, Arnold Sommerfeld, Max Born or Paul Dirac, as well as names that never appear in extant histories of quantum physics, like Otto Sackur or George Birtwistle, but whose books played an active role in the evolution of the pedagogy of quantum physics.

The elaboration of an exhaustive list of textbooks is not easy, since, especially in the very early years, many books deal with established disciplines and include quantum matters only as solutions to specific problems. This introduces the disciplinary problem that some of the case studies in this volume illustrate. Where should quantum theory be pictured in the disciplinary division of the physical sciences at the beginning of the twentieth century? As is well known, Planck developed his hypothesis in the context of a very abstract theory of black-body radiation. This hypothesis, however, did not take root in an incipient community until the quantum hypothesis was compared with the established statistical mechanics and radiation theory. For this process to happen, it was very important to reconfigure the presentation of traditional disciplines so as to indicate the limitations in the classical approaches, but also its hidden potentialities, and its forgotten riches. Marta Jordi and Massimiliano Badino show us, in their studies of Paul Drude and Otto Sackur, respectively, that the pointing out of such limitations and potentialities was not always a pedagogical tool done a posteriori, with the aim of justifying the need for the new theory, but was, at times, prior to the actual development of the theory. Thus, Drude's *Lehrbuch der Optik* fully reconfigured the presentation of optics, moving away from a purely geometrical optics. Bringing the traditions of optics and electromagnetism together, the student was led into the boundaries at the interface between both fields as central topics for research, and not as marginal issues that one might easily overlook. With it in hand, when the quantum solution eventually appeared, the student of Drude's book was ready to understand the new theory in the context of the shortcomings of the reigning models of the interactions between the ether and matter.

Also, Sackur's 1913 book on thermodynamics and thermochemistry shaped the research agenda of a whole new discipline with a crucial change in emphasis in dealing with the long-standing conundrum of specific heats. Whereas traditional discussions started with the specific heats of gases and then extended the analysis to the specific heats of solids, seen as a still unexplained anomaly, Sackur's book presented the issue in the opposite direction, as a means to consolidate his own particular research agenda in his potential students. After

Einstein's 1907 work that solved the problem of the specific heats of solids using the quantum hypothesis, Sackur's was the first book to take the solid not only as an anomaly, but also as the starting point for a reconfiguration of the field. Thus, the old marginal problem became the first building block for ulterior research.

The examples mentioned above take us to the disciplinary boundaries of the emerging quantum physics. Another boundary seldom explored in the accounts of the quantum revolution is that of its publics. Contrary to the development of relativity, which was largely a one-man work, quantum physics evolved due to the creative interactions of a large number of actors. Even so, traditional historical accounts pay attention only to the community of scientists taking an active role in such developments, forgetting its 'popularization' for those professional physicists interested in the new science, but working in other areas of the discipline. In his interesting study on the popularization of the relativity revolution in France, Michel Biezunski argued that scientists from other disciplines wanted to catch up with the most revolutionary developments in order to maintain the socio-epistemic gap that separated them from the general public (Biezunski 1985). In their analysis of the cases of Fritz Reiche and George Birtwistle, Clayton Gearhart and Jaume Navarro show, in different ways, that, in the 1920s, there was already a market composed of physicists and students of physics interested in developing an introductory but sound, technical, and thoroughly mathematical understanding of quantum theory. In both examples, the pedagogy involved is more conservative, in that it struggles to introduce the new physics within old frameworks. The student is, thus, not led to new research problems but to questions that are, up to that point, broadly accepted. In the specific case of Birtwistle, he was no expert in quantum theory; he was not doing active research; but he had a general understanding that moved him to communicate his knowledge of it to other scientists looking for some introduction to the new physics. By contrast, Fritz Reiche, a PhD student of Planck's, was a first-rank physicist with a direct and profound knowledge of quantum physics. As Clayton Gearhart shows in his article, Reiche's lucid book, *The Quantum Theory*, grew out of a specific demand from other portions of the scientific community to get to know more about the new, exotic, but potentially useful quantum theory.

Better known actors, such as Sommerfeld, Born, Van Vleck, Planck or Dirac present us with other aspects of the various traditions of physics pedagogy. Writing a textbook, or a collection of lectures, has a bearing on the dissemination of a certain kind of knowledge and the prestige deriving from it. Dieter Hoffmann's account of Planck as textbooks author illustrates this point by highlighting the labor Planck devoted to bringing to perfection his books and to propagating, in this manner, his take on the emerging quantum theory. The issue of the research agendas implicit in pedagogical works is an important one. It substantiates a point we made in the first part of this introduction: knowledge is generally a struggle and, in times of crisis, it easily becomes a struggle for the establishment of orthodoxy and the simultaneous exclusion of heterodoxy. As Michael Eckert thoroughly documents, Sommerfeld's *Atombau und Spektrallinien* was not only a prominent advertisement of quantum theory but also a prominent display of *his* quantum theory, which was largely a theory of atomic physics and atomic modeling. By turning his lectures and seminars into a book, Sommerfeld was spreading his research agenda to a public eager to have a first big synthesis of quantum physics. Furthermore, by employing the mathematical techniques of celestial mechanics in his modeling of the atom, Sommerfeld was exposing a large community of astronomers and physicists to his own research agenda. It is not by chance that Sommerfeld toured the United States as well. But the American scientific community was not to remain a passive receiver forever. John Van Vleck was a protagonist in the process of critically recasting the new quantum theory in terms of what was gained and what was lost with respect to earlier traditions. His approach, which Michel Janssen and Charles Midwinter analyze using the concept of Kuhn losses, was beneficial for putting the American physical community on the map of the emerging quantum physics.

The dissemination of a particular perspective on quantum theory opens up the issue of the de-localization of scientific knowledge, that is its supposed universal character as opposed to national differences. Sommerfeld's extensive influence as a teacher both in time (on generations of students) and in space (through his extended trip in the United States) was crucial to the establishment of atomic theory and spectroscopy as *the* main problem of quantum theory in Germany and the world over. Van Vleck was implicitly highlighting this process of de-localization when he complained about the superabundance of attention given to spectroscopy at the expenses of other interesting problems, possibly closer to the American tradition. At the same time, though, some national figures stubbornly resisted the globalization of quantum theory. For instance, Cambridge scholars such as Birtwistle and Dirac insisted on viewing quantum theory from the angle of the British problem-solving approach relying on a substantial use of analytical mechanics.

From a different perspective, Born's, Dirac's, and also Pascual Jordan's efforts to axiomatize and systematize quantum theory as theoretical physics, offer us good examples of how the task of writing a book suitable as a textbook involves more than just the transmission of already published research. In these three examples, unfolded in different fashions by Domenico Giulini, Don Howard, and Helge Kragh, we are introduced to the philosophical background that leads these authors to look for the foundations of the new theory and the logical developments that stem from such foundations. These articles also show another crucial difference between textbooks and research papers. Only the former are suitable sites to muse about the foundations of the field. From this perspective, textbooks provide a seldom-recognized service to the active scientist, and to the historian as well. However unsuccessful one particular axiomatization might have been, as in the case of Born, whose *Vorlesungen über Atommechanik* was published at the same time as Heisenberg was introducing the new quantum mechanics, these efforts can be seen as a way to prioritize the need for immediate research into certain open questions above that into other, less-pressing ones.

Finally, many of the case studies discussed in this volume deal with books that were re-issued in subsequent editions. The evolution we find in these different editions shows the tensions embodied in the task of writing on quantum physics in a time of great change, to the extent that, as Eckert says, the book itself ceases to be one static entity but becomes a process.

1.5 About This Book

This book has a curious story. The idea to start a project on the role of textbooks in quantum theory came to the editors' minds in early 2009, when they were both working in the History of Quantum Physics Project of the Max Planck Institute for the History of Science (MPIWG) in Berlin. They thought that a good way to begin collecting ideas was to organize a four-speaker panel at the upcoming History of Science Society Conference. So they sent around a call for papers. The enthusiastic reaction of their colleagues surprised and almost overwhelmed the editors, who ended up submitting two special sessions of five speakers each.

The project gained momentum rapidly. To prepare the HSS conference, a workshop was organized between some of the presenters, members of the Quantum Project, colleagues, and visitors at the MPIWG. The workshop took place on 7 October 2009 and produced many exciting discussions. We would like to thank Arianna Borrelli, Jed Buchwald, Diana Kormos Buchwald, Ed Jurkowitz, Shaul Katzir, Christoph Lehner, Jürgen Renn, Arne Schirrmacher, Daniela Schlote and Dieter Suisky for their contributions to that meeting.

The two special sessions on textbooks in quantum physics eventually took place at the HSS Annual Meeting in Phoenix, AZ in late November 2009. On that occasions, talks were delivered by Massimiliano Badino, Michael Eckert, Clayton Gearhart, Don Howard, David Kaiser, Michel Janssen, Marta Jordi, Daniela Monaldi, and Jaume Navarro. Domenico Giulini could not make it for personal reasons. The sessions were a big success and we benefited tremendously from the discussion with the audience. Cathryn Carson and Richard Staley were especially generous in providing productive comments and encouragement to go ahead with our idea.

Back in Europe, we realized that it was time for the next step, that is the organization of our results into the form of an edited book. However, since we wanted more than just a bunch of papers tied together by a loose topic, but rather a new historiographical perspective on quantum physics, we took our time. The History of Quantum Physics Conference in Berlin was coming up and we decided that it was the ideal opportunity to define better our approach and to confront once again the community of historians which was our main intended audience. At the conference, in July 2010, the two editors of this book presented the definitive set-up of the project and discussed more thoroughly the structure of the volume with the authors, all of them in attendance at the conference.

From that moment the book project officially started. And, as any good editor or author knows all too well, it was just the beginning of another journey. Some of the original participants stepped down, some new joined in. In July 2011, we further discussed the structure of the book in a very interesting session devoted to scientific textbooks at the 11th Conference of the International History and Philosophy of Science Teaching Group in Thessaloniki. That experience was important for both of us. The ensuing process of writing, re-writing, re-discussing and negotiating the contributions and this introduction went on for many months. Of course, a series of technical problems cropped up, which were solved with commendable dedication by the editorial team (Irene Colantoni, Oksana Kuruts, Jonathan Ludwig, Marius Schneider, and Chandhan Srinivasamurthy) headed by Nina Ruge. Kai Surendorf took patient care of our requests concerning the fine-tuning of the LaTeX infrastructure and Jeremiah James did wonderful editing work at various stages of the production process.

The History of Quantum Physics Project at the MPIWG has been a stimulating common effort to look at the complex developments of quantum physics from new and sometimes unorthodox angles. For several years we have been discussing and exchanging ideas on a daily basis and it would be futile to isolate individual contributions to the overall setting of this volume. Therefore, we feel that we have to thank all colleagues whose various suggestions permeate this book: Alexander Blum, Arianna Borrelli, Shaul Katzir, Martin Jähnert, Jeremiah James, Christian Joas, Ed Jurkowitz, Christoph Lehner, and Arne Schirrmacher. Jürgen Renn represented an inexhaustible source of inspiration. Many readers will immediately perceive his presence lingering in this introduction. All the rest must be ascribed to (better: blamed on) the editors.

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