INTRODUCTION

Research Practice, Experiment, and Concept Formation

> And when you look more closely at "what scientists do," you might be surprised to find that research actually comprises both the so-called day science and night science. Day science calls into play arguments that mesh like gears, results that have the force of certainty. Its formal arrangement is as admirable as that of a painting by da Vinci or a Bach fugue. You can walk about in it as in a French garden. Conscious of its progress, proud of its past, sure of its future, day science advances in light and glory. By contrast, night science wanders blind. It hesitates, stumbles, recoils, sweats, wakes with a start. Doubting everything, it is forever trying to find itself, question itself, pull itself back together. Night science is a sort of workshop of the possible where what will become the building material of science is worked out. Where hypotheses remain in the form of vague presentiments and wooly impressions. Where phenomena are still no more than solitary events with no link between them. Where the design of experiments has barely taken shape.
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> —François Jacob (1998, 126)

Research Practice

What molecular geneticist François Jacob has here so colorfully and accurately described is the contrast between the image of research science shows to the outside world and the actual research practice of the laboratory. It is no coincidence that Jacob, like Ludwik Fleck, whom he invokes, was active in the life sciences. What he describes as the night side of the sciences has traditionally been more clearly recognized within the biosciences and chemistry—we need only recall (physical) chemist Michael Polanyi—than in the exact sciences, thought to be more advanced, or to have been lent greater rigor in their research process by dint of mathematization.[1]

As Jacob's book makes clear, his depiction is not meant to suggest that the research practice of science is merely fortuitous, or even chaotic, and thus resists closer study. Jacob's distinction aims rather at drawing attention to the fact that representations of science are typically geared toward only one of the two sides: toward the side of systematic accumulation of knowledge. The actual practice of research, however, reveals the complexity of scientific investigation. In the experimental sciences, investigations demand both thought and the interaction with and experimental manipulation of instruments and materials. These activities depend on resources, technical assistance, cooperation with others, equipment and consumables, considerations of space and time, and of course also on money. All such things weigh heavily in the daily life of the laboratory, and sometimes experiments are performed in a particular way because only certain resources are available and not others, while other experiments, perhaps of equal or even greater theoretical interest, are never performed at all, and for similar reasons.

What's more, the laboratory is not an isolated place. Researchers must communicate, and in doing so they face a plethora of additional considerations: with whom to communicate, why, and by what means; what to share and what to hold back; at what stage to publish and in what format; and how to secure priority and ensure the best career outcome. Attempts at communication and the responses they provoke frequently factor into the planning of particular experimental trials and even into the very direction of research. Communication or, more generally, social activity, is an essential component of research practice.

Research is driven not only by the quest for knowledge but also by the biographical situation of its agents, their place within a community and the safeguarding thereof, social and cultural conventions and conditions, and not least by sheer coincidence—in short, by everything that drives human action in any other context. The human agents at work in this process also have lives and interests besides science. It would be naïve to simply assume that such matters have no effect on the course of research. To be sure, in typical first-person accounts, they recede into the background or disappear altogether, leaving only the systematic quest for knowledge and the necessities this quest strictly entails.

But even this remaining aspect, when it comes up in public presentations, finds itself dressed up and organized from a point of view that has been reached as the outcome of scientific research. Many of the criteria and concepts that now structure the presentation were unavailable during the research process itself and were instead developed within that process. Jacob's talk of tapping in the dark, stumbling, retreating, doubting, and resuming is particularly applicable to such developments. So long as we attend only to accounts published by the scientists themselves, we will have little prospect for understanding the generation of knowledge in all its complexity. To be sure, the point is not to accuse natural scientists of falsifying their reports. Their published accounts are always aimed at a particular audience and necessarily shaped by that goal. In most cases, and for good reason, their primary purpose is not that of historical narrative but rather the systematic presentation of results from a perspective whose attainment was made possible by the very research leading to those results. And even if a historical account would be attempted, it would be difficult to describe the previous situations of deep conceptual uncertainty from a later position in which everything has been sorted in its proper place, often by means of concepts that were not available at the time.

In focusing on research *practice,* I am not concerned with contrasting such practice with *theory* but rather with the picture that science presents to the public about its process. Nor am I exclusively concerned with *experimental* practice, for research practice can also be studied with regard to *theoretical* disciplines, such as theoretical physics and mathematics.[2] Research practice, thus understood, has only recently come within the scope of historical investigation.[3] In doing so, historians have become highly aware of the particular kinds of source materials required here. Research practice cannot be simply read off of documents; it can only be historically reconstructed. To be sure, published first-person accounts by relevant agents can serve as a useful starting point, though they often reveal more about the expectations and clichés of their authors than they do about experimental activity and the motives behind it.[4] By contrast, materials originally meant to assist the agents in their own work, with no thought of presenting them to others, provide more insight. Examples include laboratory records, sketches, and loose notes, some of which may display an idiosyncratic or even cryptic character. But there are no firm boundaries between the genres; in larger laboratories, records might also be intended as a medium of communication. Other sorts of documents, such as purchase orders, laboratory access logs, and so on, can enrich the picture.[5] To what extent and in what respects such sources enable the construction of a detailed picture of research practice always depends on the disposition of the sources in the particular instance. Because the resulting picture often diverges in characteristic ways from first-person accounts, such investigations can offer important insight into the conventions of these accounts.

In this book, studies of research practice form a core element. I reconstruct historical episodes and try to learn some more general lessons from their analysis. The expense and complexity of producing reconstructions of this kind typically constrain them to relatively narrow time periods and specific lines of inquiry. As in every historical narrative, it is especially important to account for the specific interests that guide the investigation and motivate the selection of time period and source materials. These issues are briefly addressed in the sections that follow.

Experiment

The past three decades have seen increased attention to the role of experiments in the sciences.[6] The so-called standard view, in which the role of experiment, as handmaiden to theory, is confined to the testing of hypotheses and theories, has been found wanting. Numerous studies have identified a plethora of other, equally important roles of experiments, leading Hacking (1983, 166) to speak even of a "Baconian fluster."[7] Most of these roles are important in the *formation* of theories, as well as in the conceptual, linguistic, and visual media of research. For a long time, the philosophy of science has ignored such "generative functions," as Heidelberger calls them, considering them irrelevant. The divide between the context of discovery and context of justification seemed to many to imply that the only epistemically important role of experiment was that of testing theories that had themselves been developed by other means.[8] But as insight into the inadequacy of this dichotomy (or at least of its specific interpretation) grew (Schickore and Steinle 2006), the prospect of taking other roles for experiment came prominently back into view. There is, after all, a broad field between theory testing and mindless fumbling about with an apparatus. Despite a few attempts at taxonomy, that field remains largely unexplored. This field is one of my primary research interests.

One characteristic of recent studies of experimentation, even those of primarily philosophical thrust, is that they are based on the analysis of historical or recent episodes in science. For philosophical reflection to proceed, a wide range of "empirical" materials seems to be required, without which we cannot hope to become aware of otherwise unanticipated issues.[9] In this regard, the study of research practice has much to offer. The fact that, within the philosophy of science, the received view of the role of experiment, or the context distinction, could become so entrenched and last so long has much to do with the fact that its originators, who were certainly well versed in contemporary science, were inclined to defer to the published self-representations of participating scientists. But these representations, in turn, were often shaped by philosophical prejudice or by general expectations concerning the proper conduct of science. The investigation of research practice allows us to see past these self-representations and enables us to have a more direct view of research activity.

Uncovering the manifold roles of experiment in the research process, and understanding their significance, is a chief aim of the present study. Even after numerous studies (e.g., Gooding 1990a; Hacking 1992; Hentschel 1997; Graßhoff, Casties, and Nickelsen 2000), the question of the various different epistemic goals pursued through experimentation and their consequences for experimental endeavors have still not been studied in appropriate breadth. This question constitutes the central focus of my work. The study of scientific practice will, for the first time, allow us to sketch a differentiated picture of those issues. Among other consequences, it will allow experimental endeavors of the kind I call "exploratory" to be characterized in detail and their far-reaching, if heretofore unrecognized epistemic significance, exposed.

The Language of Research and Formative Periods

Scientific thought takes place on many different levels, involving items such as categories, classifications, modes of presentation, concepts, conceptual schemes, empirical rules, theories, and theoretical entities. Wildly divergent "epistemic things" emerge, sometimes stabilizing, sometimes being discarded.[10] Sometimes the boundaries between them are not drawn clearly, and sometimes indeed they cannot be. But that does not mean such distinctions ought to be abandoned, subsuming all of this diversity under the heading "theory," as has sometimes been done. After all, there remain clear cases. For example, it is one thing to classify effects as electrical or magnetic on the basis of their observed characteristics and quite another to investigate the prospect of some hidden, unobservable emanation behind them all. The morphological classification of plants or animals is different in kind from ruminations on a possible life force, much as the establishment of regularities in spectral lines is different from their quantum-mechanical explanation at the atomic level. To ignore such differences would be to sacrifice the potential for significant differentiation and to undermine any attempt to come to grips with the range of possible epistemic goals that drive scientific research. In this work, as in other efforts in the recent literature, I shall be defending and exploiting a distinction between concepts, empirical laws, and theories.[11] This distinction is suggested by the practices and reflections of historical actors and becomes, once critically deployed, an important tool with which to grasp the differing epistemic goals in play within a given scientific practice.

I have conducted this study with a view toward a particular kind of epistemic situation, one in which there is insecurity at the basic conceptual level and in which, consequently, the reliability of not only special theories but also established conceptual schemes, forms of thought, and modes of representation has been profoundly shaken. Such situations of conceptual insecurity, even speechlessness, can be brought about by unexpected experimental results that, while clearly observable and reliably repeatable, remain resistant to treatment by means of customary concepts and are sometimes frankly ineffable. Such situations have arisen repeatedly in the history of the sciences; in Thomas Kuhn's conceptual framework they would count as serious anomalies. The developments traced within this book took one such situation or anomaly as their point of departure. Research conducted in such situations typically aims at articulating and developing a conceptual framework required to stabilize engagement with the new effects. I shall call such phases "formative periods." Often, though not always, the conceptual innovation of such periods goes hand in hand with intense experimentation. In such cases, experiments take on a special role that cannot be captured under received conceptions of experimentation, for the testing of hypotheses or expectations can take place only against the backdrop of a reliable language.[12] Within that language, such hypotheses can be formulated, a particular experimental question posed, and the experiment itself designed and assessed. But when such a language—such a conceptual framework—is lacking or when the reliability of the extant framework has been shaken, experimental activity must necessarily take on a very different character. Its study under such conditions is thus of particular interest.

The detailed case studies of experimental work presented in this book concern such formative periods. In the constant give-and-take between experimental activity and conceptualization, new concepts are formed and stabilized—or destabilized. With successful stabilization, a new perspective on the field is gained, along with new terminology, permitting reliable experimental engagement and thus fundamentally shaping all future work in the field. In subsequent efforts, the new concepts are no longer subject to revision; instead, they may simply be used in the formulation of further research questions. While concepts may be flexible and provisional in the formative period, they later take on a stable, fixed character. Formative periods are like branching nodes or, to adapt Reinhart Koselleck's phrase, saddle periods, in that in a relatively short span of time they determine the long-term direction of scientific research. The case studies in this book display this feature very clearly. The full generality of this phenomenon might be illustrated by further examples, such as the first clear classification of electrical and magnetic effects by William Gilbert, or Charles François de Cisternay du Fay's early eighteenth-century introduction of the distinction between two different electricities. In all such cases, intensive experimentation was at the core of the effort, not for theory testing but in an essentially constructive role.

Electromagnetism in 1820–1821

By the mid-nineteenth century, and in the wake of the rapid expansion of electrical telegraphy, electromagnetism and electrodynamics became prominent fields of academic research.[13] It became apparent at that point that there were already two competing theories. Based on very different concepts, they were thus not amenable to any effort at direct comparison. On the one hand stood electrodynamics, first developed by André-Marie Ampère in Paris and since widely disseminated. It dealt with the mutual attraction and repulsion of infinitesimal elements of electrical current in a fully mathematical way. The forces involved were conceived as central, acting at a distance along the lines connecting the pointlike centers of force. In 1846, Wilhelm Weber greatly expanded this theory by extending it to induction effects. On the other hand, Michael Faraday in London had qualitatively, while still very precisely, captured electrical and magnetic effects and the connection between them and developed completely different concepts. His central notions were those of electrical and magnetic lines of force whose interactions with each other and with matter gave rise to all electrical, magnetic, and electromagnetic phenomena. There was no action at a distance; it occurred only at points of immediate contact. Faraday's framework, unlike Ampère's, encompassed a huge range of extraordinarily diverse phenomena, from electrochemical decomposition to electrical discharges in rarefied gases, electromagnetic induction, the magnetic properties of various materials, and the effect of magnetism on light. Despite the universal acclaim with which Faraday's numerous experimental discoveries had been greeted, his concepts and explanations had found very little resonance. Among his contemporaries who knew of them at all, most found them nearly unintelligible. This began to change only in the late 1850s, when James Clerk Maxwell began to develop a mathematical formulation of Faraday's principles, in the end presenting field theory, as it was now called, as a viable alternative to Ampère-Weber action at a distance. With this development, the stark contrast between the two frameworks became especially striking. During the second half of the nineteenth century, the divide between the two theories and their respective frameworks, along with a third, Carl Neumann's potential theory, would be one of the most important debates in physics.

The roots of this controversy extended to a much earlier date. Ampère worked out his theory between 1820 and 1826, and he had already formulated its basic conceptual structure in the fall of 1820. Faraday, for his part, entered the field of electromagnetism in the fall of 1821 and quickly developed the essential structures, concepts, and procedures that would shape the course of all of his future work. The formative period of both of the major theories and conceptual schemes of electrodynamics was thus 1820–21, beginning with studies conducted in response to Hans Christian Ørsted's discovery in July 1820 of an electromagnetic effect.

Ørsted's discovery had been a sensation, sparking far-reaching research efforts across Europe. Almost no one had been prepared for an electromagnetic interaction, and, though none had any trouble replicating the effect, most researchers were left profoundly puzzled. It was not merely that they could not explain it; even capturing regularities or reporting individual experimental results proved enormously challenging. They simply lacked the concepts and language needed in order to coherently express their results and the complex relationships among them. In particular, the experimental results resisted formulation in terms of the concepts of attraction and repulsion, at least at first blush. But these were the very concepts that had shaped scientific thought at a fundamental level, and not only in physics. As a result, early reports of experiments took on a baroque character, as the mere statement of an experimental result demanded a meticulous accounting of the positions of battery and experimenter, couched in terms of such elementary spatial concepts as the cardinal points of the compass. The description of the experimental arrangement (which, after all, consisted only of battery, wire, and compass needle) often took half a page. Yet, such a description applied only to particular experiments and did not allow generalizations to be formulated. Early electromagnetism thus offers a distinctive example of the kind of situation described above, one in which an accepted conceptual scheme has been shattered.

It is therefore not surprising to find that a significant proportion of the many research activities undertaken within this situation were geared toward the construction of adequate concepts. Even a cursory sampling of reactions from across Europe reveals astounding similarities in the reaction of the researchers who found themselves in this epistemic situation. To be sure, there were also significant differences, which in turn reveal how diverse particular local contexts and traditions of scientific research remained throughout Europe in the early nineteenth century. A broad-based comparative study of reactions to Ørsted's discovery (of which I shall provide only a sketch) might yield an insightful panorama of early nineteenth-century scientific cultures and their diversity.[14]

Such a study, however, is not my primary objective. With the later dispute between field theory and action-at-a-distance theory in mind, I am concerned with the formative period in which the conceptual foundations for both theories were laid down. Because Ampère and Faraday are the two principals of this period, I concentrate on work undertaken in Paris and London, respectively. It is remarkable that such different, incompatible conceptual schemes were developed in response to one and the same discovery. The fact that this development took place in such a short time and, as I show, under conditions of at least partial mutual awareness demonstrates all the more clearly the significance of this episode for the study of formative phases.

Two Research Objectives

My goals are twofold. First and foremost, I attempt a reconstruction of working scientific practice in the two episodes under study. For Ampère, the relevant period runs from September 1820 through January 1821, whereas for Faraday it spans four months almost exactly a year later. There were other noteworthy congruencies: both projects dealt with the same problem domain, both researchers undertook their reflections amid intensive experimental labors, and both were newcomers to the fields of electricity and magnetism. At the same time, while Ampère began his efforts as a well-established professor of mathematics and academy member, Faraday was a mere laboratory assistant and scientific autodidact, albeit one who had already begun to gain a bit of prominence following some minor publications in chemistry. There are further points of contrast. Ampère's milieu was French physics, with its strong tradition of mathematical treatment, while Faraday's was the chemical laboratory of the Royal Institution, along with several self-organized groups for the dissemination of scientific knowledge to the broader public. Ampère conducted his research in great haste and under strong competitive pressure, when Ørsted's circular contained all that was known about electromagnetism. By the time Faraday's effort was under way, a scant year later, a great deal had already been published. Finally, Ampère had no easy access to experimental facilities, whereas Faraday had unlimited access to one of the best-equipped laboratories of his time. Weighed together, the similarities and differences strike me as balanced enough to sanction significant points of comparison. Such comparison turns out to be extraordinarily fruitful, making possible a clear portrait of the two research practices.

Both episodes have already been the subject of historical research.[15] Though authors such as Christine Blondel, L. Pearce Williams, James Hofmann, and David Gooding have contributed much, and my own study would have been impossible without them, there remain important gaps with respect to the two episodes, and they impede an adequate understanding of the formative phases. In Ampère's case, this has to do with the very challenging disposition of the necessary sources. My work was made possible only by recourse to previously unknown archival materials, laboriously uncovered. In Faraday's case, prior studies were confined to only a small portion of the whole episode. Partly as a result of this fragmentary sampling, they have led to misinterpretations. The comprehensive comparison of the two episodes thus demanded a new analysis of both, with broader temporal horizons and greater attention to the wider historical context.

The second goal of this book is an analysis both of the relationship between experimental activity and concept formation during a formative phase and of the open-ended process whereby new concepts are formulated and stabilized. Such analysis rests on the foundation of a detailed reconstruction of scientific practice. I should state from the outset that one of my central results is the conclusion that experimentation can proceed systematically, in accordance with clearly recognizable guidelines, even when it is not strictly driven by theories. The procedure of "exploratory experimentation," as I have called it, can be explicated and systematized in considerable detail. Attention to its features clearly reveals how concepts are questioned, transformed, or replaced, and eventually stabilized in the context of experimental activity—a remarkable insight for the philosophy of experimentation. For history of science, moreover, there is the surprising historical discovery that even Ampère had his exploratory phase. It was during this phase, brief though it may have been, that decisive conceptual innovations took place. Furthermore, Ampère's procedures throughout this phase of concept formation are similar, in the main, to those discernible to a much greater degree in Faraday's work.

In this analysis, as much as in the reconstruction of scientific practice, I find myself in uncharted territory. Nothing of the sort has previously been attempted for Ampère. Williams's and Hofmann's studies are devoted to epistemological questions, and their depiction of Ampère's work now appears inadequate in several important particulars, as, for example, with respect to Ampère's use of equilibrium experiments. These studies have also entirely missed Ampère's exploratory phase. In Faraday's case, however, Gooding has certainly pursued goals similar to mine, though he has passed over several important points with surprising consistency. Most significant of these is the explanatory goal of "reduction to the simple case," not only discernible but downright prominent in Faraday's work. Gooding thereby misses a central aspect of Faraday's method of devising new concepts. His general epistemological picture is thus remarkably vague when it comes to concept formation. My study seeks a much more nuanced representation.

Overview of the Book

All scientific research is conducted within a particular situation, defined both by cultural and biographical circumstances and by the theoretical, experimental, and instrumental state of the art. Studies of scientific research practice must thus be framed by due attention to this background. In chapter 1, I survey the state of knowledge and experimental culture(s) in whose context early nineteenth-century studies of electricity took place. This survey is a necessary condition for the remainder of this work.[16] Besides sketching the general state of the research field, I selectively focus on Paris and London. As a result, some other developments, including those in the German-speaking world, which were never fully appreciated in those venues, will get less weight in the present study than they would in any truly comprehensive survey. Instead of employing a bird's-eye view, I aim to capture the perspective of actors in London and Paris from street level.

The core of chapter 2 is the discovery in 1820 of electromagnetic action. Ørsted announced his findings quickly, self-consciously addressing a wide audience across Europe. So as to convey a sense of the excitement aroused by his announcement, I extend my gaze, for a few sections, beyond Paris and London. Following a sketch of the contents of Ørsted's report, I offer an overview of reactions throughout Europe that brings to the fore significant features common to most localities. I then turn back to the main stage with a detailed accounting of events in Paris, where the academicians were surprised by the intensive involvement of an unexpected researcher, André-Marie Ampère.

Chapter 3 is a study of Ampère's scientific practice during the first weeks of this involvement. These first weeks are of particularly far-reaching importance, for they saw the formation of the concepts that would shape Ampère's work thereafter. Ampère did not have a clearly defined research program until the end of this period, a point that has garnered almost no attention in previous historical studies. This seeming neglect can be traced both to the challenging state of the sources and to the way in which Ampère would later describe his own work. As a result of my investigation, the received view, which has more or less explicitly shaped prior historical efforts, must be substantially revised. There is now a new and for the first time satisfactory answer to the old question as to how Ampère could have achieved such far-reaching innovations in a mere three weeks. The revised view was made possible by the discovery and reconstruction of source materials unknown to earlier scholars. For the first time, we now have rich materials at hand that Ampère produced not retrospectively but at the very moment of his research. In an effort to convey a sense of the nature and scope of the archival work necessary to carry out this study, I present some of these materials in the two appendices to this book.

Chapter 4 is devoted to Ampère's work from October 1820 through January 1821. Historians have agreed that Ampère's lecture to the Académie des Sciences in Paris in January 1821 represented a turning point in the development of his theory, which was then followed by a longer pause in his research efforts. I have dedicated a whole chapter to the period from October through January, despite the fact that it has been dealt with in prior historical studies, in order to show how Ampère's various activities hang together, with each other and with aspects of his particular situation in Paris. This is the phase in which his competition with Jean-Baptiste Biot is most keenly evident, and some of Ampère's moves can be understood only against this previously neglected backdrop. By taking account of Ampère's now transformed experimental practice, his intensive efforts toward precise measurement and the conclusions he drew from their failure, and his numerous outreach activities and consciously crafted public image, it is possible to shed new light on his research practice. In particular, important departures from those first three weeks, as discussed in chapter 3, become recognizable, exposing the peculiarities of the initial period with even greater clarity.

In chapter 5 I turn to the scientific milieu of London around 1820. It differed markedly from that of Paris, and not only in its institutional structure. Even in this context, however, Michael Faraday's career was rather unusual. My survey of the London responses to Ørsted's discovery reveals characteristic differences relative to those of their Parisian counterparts. These may even be observed in the meticulous efforts undertaken by Humphry Davy, the most important of the London researchers, efforts that were of great importance for Faraday. Only in the summer of 1821, when an extensive literature on the topic had already been published, did Faraday commence his own research activities. His motives, and the conditions under which he entered the field, were completely different from those of Ampère. I explore them in some detail toward the end of this chapter.

Faraday's first work on electromagnetism is the focus of chapter 6, which thus becomes the companion piece to chapters 3 and 4. Faraday quickly arrived at his spectacular discovery of electromagnetic rotation and, thus armed, immediately joined the first rank of researchers in the field, despite being a newcomer to physics. Previous historical studies of this finding have left a number of important questions open. Some of these concern the continuity, only now apparent, between his apparently sudden discovery and his earlier attempt to write a survey of the field. To a much greater extent than the authors of prior studies, I have drawn on Faraday's efforts *after* his great discovery. It is here that the exploratory character of Faraday's work becomes apparent—all the more so when we consider his later integration of the rotation effect into a classificatory system he had yet to devise. This picture is further fleshed out by attention to the studies he carried out that December, which have not received the scrutiny of historians. The net result is a clear and nuanced portrait of Faraday's research practice during the initial phase. With this picture in hand, I am in a position, in my concluding comparison between Ampère and Faraday, to render the distinguishing characteristics of both figures crisply.

As this overview already indicates, throughout this book, historical narratives are often interrupted by reflections on more general themes. The seventh and final chapter of this book brings these reflections together, systematizing and extending them. The subject of experimentation itself forms its core. I begin by reviewing the recent discussions of this issue, then turning to the implications of my own studies of scientific practice. They make possible a detailed account of the roles of experimentation outside the realm of theory testing. Of particular significance is the kind of experimentation I call exploratory, which I characterize in detail. I also provide at least a partial answer to the question of why such exploratory experimentation typically recedes into the background, or disappears entirely, in self-representations of science. In addition, based on my historical case studies, I argue for the fundamental epistemological significance of exploratory experimentation, which the philosophy of science, captivated by the distinction between context of discovery and context of justification, has thus far overlooked. Of crucial importance are the processes of concept formation, the delicate relationship between concept formation and experimentation, and the mutual stabilization or destabilization of concepts and experimental activity. A glance at other, similar processes in the development of the sciences demonstrates the generality of this perspective, while simultaneously pointing toward the irreducible complexity of scientific research, as an activity undertaken by human actors.

Micro- and Macrohistory

Large portions of this book are presented as historical narratives. In the various chapters, however, they unfold on very different levels and timescales. They run the gamut, from a continent-wide overview of two decades in the history of a scientific field to detailed case studies of the research practice of two individual agents over periods of one to three weeks. At medium resolution, they canvass the whole research activity of a particular place over several months. In short, their spectrum ranges from microhistory to the borders of macrohistory.

The relationship of micro- and macrohistorical treatments is not one of mere juxtaposition; to the contrary, chapters with different spatial and temporal resolutions complement each other in mutual dependence. The microhistories, studies of daily events in the laboratory, would be isolated, nonspecific, and sometimes even incomprehensible without due attention to the researchers' broader spatiotemporal frame, within which they self-consciously position themselves. Among other elements, this frame encompasses theoretical traditions, scholarly debates, material cultures, experimental resources, academic customs, training and career trajectories, and publication structures, as well as even larger national and international academic constellations. Without such macrohistorical elements, microhistory would have little to teach us. On the other hand, a pure macrohistory could hardly deliver what I am most interested in: insight into research practice. What's more, it is in the context of microhistorical studies that new and fruitful questions for macrohistorical analysis itself come into view. This book attempts to do justice to this mutual dependence.[17]

The narrative approach, which characterizes long stretches of the book, is the only way I could envision presenting an account of the relevant historical material that is both sufficiently detailed and readable. In my writing style I have tried to keep close to the historical actors' perspectives, especially in my detailed case studies, so as to capture and render plausible the many contingencies of the daily research process. Reflective distance is offered through many interpolated passages of varying lengths, as well as in the concluding summary in my final chapter. Taken together, my presentations will, I hope, illustrate my earlier observations regarding the complexity of scientific activity and the consequent need to include, beyond programmatic purity, a rich diversity of aspects in studies of research practice.

History and Philosophy of Science

Scientific development is driven by a heterogeneous variety of different factors. While they can often be isolated for the sake of specific historical analysis, an adequate account of particular historical episodes may resist such isolation, instead demanding an inclusive treatment. Such an account must rely as much on an understanding of the social, cultural, and biographical situation as on the particular scientific domain with which the researchers were concerned, as well as a nuanced appreciation for the epistemic process as such in all its ramifications. After all, to study the development of a science is to study the course of a social enterprise whose goal is the search for and generation of knowledge. Like other social enterprises—politics, industry, and the arts, for example—this one has its distinctive characteristics. For the behavior of an individual agent and for the scientific development of a given period and a given field, particular conceptual, theoretical, and experimental constellations, along with questions of methodology and epistemology, are every bit as important as cultural, social, and biographical situations. Any attempt at a comprehensive understanding of the scientific enterprise must do justice to this multiplicity.

In recent decades, the exploration of social, political, cultural, and economic aspects has gained increasing attention in the historiography of science. This turn constituted a reaction to the traditional, narrower focus on theoretical developments that predominated into the 1970s. Long-neglected questions of rhetoric, modes of presentation, and material culture are now being investigated, establishing the status of science as a part of, and significant factor within, broader cultural developments. The turn to general history and cultural history has led, and continues to lead, to important insights, profoundly transforming our image of scientific process. Nonetheless, it strikes me as running the risk of neglecting the complexities of science "from the other side," as it were, by losing sight of the epistemic dimension of science as such, and thus of the avowed target of the whole undertaking. On the other hand, the philosophy of science, to which this epistemic dimension is central, can still assert that, from the epistemological perspective, all the newer insights, interesting though they may be, remain a sideshow of no real relevance to the understanding of knowledge generation itself. A glance at the philosophical journals is enough to show that, even in recent years, many of the debates in the philosophy of science continue to be conducted in formalistic sterility, without making contact with new insights from the history of science.

Serious attempts to reintegrate epistemological and historical research and insights have only recently received wider visibility.[18] The promise of such integration for the understanding of science—the potential benefits of integrating historical and epistemological reflections, questions, and methods in order to study how scientific knowledge, in all its complexity and irreducible historical contingency, is formed and developed—thus is far from being fully realized. The many-layered character of the epistemic process, and especially its ineliminable historicity, becomes truly visible only by means of thorough and comprehensive historical investigation. Should the analytic scrutiny of philosophy of science begin to take it seriously, there may yet be the chance to achieve a new and integrated picture of the generation of scientific knowledge. It may well be that the conceptual tools required to formulate such a picture have yet to be developed. Such integrated studies are an admittedly laborious undertaking, for urgent methodological issues arise (for example, the question of how to deal with historical case studies).[19] In addition, the age-old problem of "truth and history" lurks always in the background. But such problems are typical, and unavoidable, if we seek to understand science, and increasing numbers of reflective approaches seek to address them.[20]

One point, however, seems to be obvious: such a picture can be achieved only with proper attention to scientific practice. Indeed, such a turn toward practice can be observed in many recent attempts at integration. Such efforts allow us to discern the scope and breadth required for a given historical case and may also offer a glimpse of characteristic structures. In the process, we may go well beyond the particular case, gaining intimations of broader historical processes and epistemological generalities. The central issue of this book, the problem of experimentation, is just an example, albeit one well suited to demonstrating how both historical insight and philosophical analyses may be enriched by an integrated study. Experimentation illustrates with particular poignancy the aptness of a slogan, often highlighted by my esteemed academic mentor, the late Lorenz Krüger, in homage to a famous Kantian dictum: "History of science without philosophy of science is blind; philosophy of science without history of science is empty."[21]