New Directions in the Philosophy of Science

A SOCIAL EPISTEMOLOGY OF RESEARCH GROUPS

Susann Wagenknecht

New Directions in the Philosophy of Science

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Indianapolis, IN, USA Susann Wagenknecht
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Introduction

Today's natural science has become a highly collaborative endeavor. To create scientific knowledge, scientists with specialized expertise observe systematically, analyze data and interpret combined experimental evidence to formulate a scientific knowledge claim. In so doing, most scientists depend deeply and immediately on their peers. Experiments have typically become too time-consuming and resource-intensive to be carried out by any one single scientist. Only in research groups can scientists accumulate the necessary expertise, labor, financial means and physical infrastructure to carry out cutting-edge research. For this reason, this book analyzes the collaborative creation of scientific knowledge in research groups, thereby addressing two questions that are continuously troubling philosophy: What *is* scientific knowledge—is it genuinely collective? And *how* can it be created, particularly under the conditions of actual experimental scientific practice?

When answering these questions, I seek to add two factors—one thematic, one methodological—to reflections upon collaborative knowledge creation. Thematically, I examine research groups. This examination proceeds "from within" research groups, studying the collaborative practices

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that constitute such groups and inquiring of research group members about their experiences with research group collaboration. Methodologically, I probe a way in which first-hand empirical insight, gained through qualitative methods, can inform philosophical reflections. My methodological approach reflects the difficulty that comes with my thematic focus. When collaborative scientific practice in research groups is the object of philosophical inquiry, then philosophers cannot solely rely upon intuition and imagination.

Drawing upon comprehensive empirical insight, I investigate different configurations of the within-group division of labor and the forms of epistemic dependence that they entail. I study how practicing scientists deal with epistemic dependence, how they come to trust one another, and how they individually relate to the scientific knowledge claim that they collaboratively create. To do this, the book is based on a comparative case study, drawing upon observations and interviews that have been carried out in two research groups, a small interdisciplinary group in planetary research and a larger molecular biology laboratory. The book is, hence, also about interdisciplinarity and the question of how interdisciplinary research collaboration compares to mono-disciplinary collaboration.

Based on these investigations, I argue for an inter-individual account of research group collaboration—an account that recognizes the role of individual knowing for scientific practice, but acknowledges the collective character of some of the scientific knowledge that research groups create collaboratively. With this account, I seek to make a contribution to two specialized fields of philosophical inquiry: social epistemology and philosophy of science. In particular, the book contributes to a "philosophy of science in practice" (Ankeny, Chang, Boumans, & Boon, 2011; Soler, Zwart, Lynch, & Israel-Jost, 2014).

For a philosopher to inquire about "science in practice" means to inquire about the practices in which scientific norms are interpreted, scientific methods applied and scientific knowledge made. It means to inquire about knowledge in-the-making, knowledge as it is created in processes of collaboration under contingent material, cognitive and social conditions (Rouse, 2002). This requires philosophers to give up "minimizing and externalizing the social dimensions of scientific knowledge" (Rouse, 1996, p. 168). It also requires empirical insight into how science is *actually* practiced. There are several ways to acquire such insight, one of which I exemplify when I mobilize first-hand qualitative data for philosophical reflection.

1.1 An Epistemology of Research Groups

In this book I study research groups, a thematic focus that is plain and novel at the same time. A philosophy of research groups is only now emerging, although both philosophy of science and social epistemology have long been interested in the "social" character of science. But although most philosophers would agree that scientific knowledge creation involves "social" aspects, precisely in what sense science should be understood as "social" remains the subject of debate. So far, philosophy of science has focused on the role of peer communities, a focus for which research groups have been, at best, of peripheral interest. Social epistemology, in turn, is only beginning to develop pronounced interest in the domain of science.[1]

But while research groups are a relatively novel object of inquiry for social epistemologists and philosophers of science, they have been the undisputed norm in natural science for many decades. To find practicing scientists discussing research groups passionately and remarking upon their novelty, one has to go back to the mid-twentieth century when the prospect of large-scale technocratic research coalitions, later to be christened "Big Science,"[2] sparked debate. Against those who were concerned by this prospect, David Green published a letter in the journal *Science*

[1]As a field of philosophical inquiry, social epistemology is, as much of the philosophy of science is, rooted in the tradition of analytic philosophy. To inquire about the "social epistemology" of science means to inquire about the possibilities of well-founded belief and the (scientific) knowledge that scientists, individually and collectively, possess. Social epistemology, as a branch of general epistemology, is a relatively young field of philosophical inquiry. The programmatic beginnings of social epistemology as a field can be traced to the work of Fuller (1988), Longino (1990), Goldman (1999) and, for example, (Schmitt, 1994). Some social epistemologists have begun to engage with comprehensive, empirically detailed case studies (see, e.g., Bergin, 2002; Rehg & Staley, 2008; Staley, 2007). Despite the field's interest in the social dimensions of knowledge, however, philosophers in the field of social epistemology typically do not refer to social-scientific studies of science or social-scientific empirical methods.

[2]The expression "Big Science" was coined by Weinberg (1961).

in 1954. In this letter, Green, at the time a leading biochemist, defends research groups as "one of the most powerful instruments yet devised for conducting experimental research" (Green, 1954, p. 445). As he argues, research groups bundle the increasingly specialized expertise of individual scientists and focus it on a selected set of research questions, questions that are too laborious and too daunting for any individual scientist to pursue without the support of committed group members. In sharing success and failure among group members, Green points out, research groups act as an "insurance" against the epistemic risks of experimental science. Yet, Green admits, so little is known about the ways in which research groups work and adapt to the contingencies of scientific practice that such group themselves remain "an experiment in human relationships" (Green, 1954, p. 445).

Over recent years, philosophy has slowly become more and more interested in research groups, triggered largely by the widespread reception of John Hardwig's (1985, 1991) work on research groups and the application of Margaret Gilbert's (1989, 2004) notion of collective belief to research collaboration (as, e.g., in Cheon, 2014; de Ridder, 2014; Wray, 2001). I will consider Gilbert's notion of collective belief, but particularly Hardwig's work which has been a major source of inspiration for me. Hardwig argues that contemporary natural science fundamentally relies upon trust and that "[…] the alternative to trust is, often, ignorance" (Hardwig, 1991, p. 707):

> In most disciplines, those who do not trust cannot know; those who do not trust cannot have the best evidence for their beliefs. In an important sense, then, trust is often epistemologically even more basic than empirical data or logical arguments: the data and the argument are available only through trust. (Hardwig, 1991, p. 693f.)

Trust becomes an obligatory element of experimental progress, Hardwig explains, when experiments are too labor-intensive to be carried out by one scientist, too costly to be repeated without serious doubts, or too complex to require just one field of expertise. Importantly, Hardwig points out that the relevance of trust does not concern merely the formulation of scientific knowledge claims, but plays out in their justification as well, since the logical and evidential justification for those claims is too comprehensive to be understood in deep detail by a single scientist (Hardwig, 1991, p. 696). When he first formulated these claims, Hardwig was bluntly challenging the ideal of the autonomous, individual knower that was (and in many respects, still is) underlying epistemology (cf. Fricker, 2006b). Against this ideal, Hardwig's work calls for a new way of thinking about knowledge and scientific knowledge, a philosophical angle that is broader than an individualist focus:

> [. . .] we need an epistemological analysis of research teams, for knowledge of many things is possible only through teamwork. Knowing, then, is often not a privileged psychological state. If it is a privileged state at all, it is a privileged social state. So, we need an epistemological analysis of the social structure that makes the members of some teams knowers while the members of other teams are not. (Hardwig, 1991, p. 697)

In which sense knowing could be exactly understood as such a 'social state' and features a genuinely collective character needs to be discussed. In any case, philosophical analysis of social structures and their role in scientific knowledge creation cannot be an armchair undertaking only—the experiences, intuitions, norms and limitations that shape the ways in which professional scientists create knowledge are too domain-specific.

1.2 Empirical Insights for Philosophy

In this book I seek to establish a way in which such philosophical study can be grounded in empirical insight, for example, through first-hand data gained via qualitative empirical research methods, such as observation and interviewing in a comparative case study of two research groups. The reason for collecting first-hand data is the conceptual contribution that a dialog between philosophical abstraction and empirical description can yield.

As research in the social epistemology of group collaboration has begun to show, empirical insight helps to contextualize and differentiate analytic concepts. An example of conceptual contextualization can be found in

Kent Staley's application of the concept "group belief" to large-scale collaborations in high energy physics (Staley, 2007). Another example is the account of interdisciplinary teams that Hanne Andersen and I have proposed and that emphasizes the diversity of constellations in which scientists can interact (Andersen & Wagenknecht, 2013). Contextualization and differentiation will often limit the abstract scope of philosophical concepts. But while abstraction is certainly one of the strengths that philosophical concepts possess, concepts cannot unfold their reflective force if they elude actual practice.

The philosophical concern for science as it is actually practiced, or, more broadly, knowledge as it actually can be acquired by human beings, is not new and reaches back at least to William v. O. Quine. His essay on "naturalized epistemology" (1969) has inspired many philosophers of science to "naturalize" their way of studying science as an object of philosophical interest.[3] Quine envisions a form of philosophical inquiry that is continuous with natural science, ascribing philosophical claims with the status of empirical hypotheses and treating them as revisable in the light of empirically gained insight. In a strict interpretation, Quine's program of a naturalized philosophy rules out the attempt to give an *a priori* (or at least largely non-empirical) justification for knowledge in general, and the progress of scientific knowledge creation in particular (Giere, 1988, p. 11).

Over the last decades, however, the term "naturalized" took on a broader meaning. Especially with the reception of Thomas Kuhn's work, a much more loosely defined conception of naturalized philosophy has spread (Giere, 1985). Naturalized philosophers want to analyze science "as it really is," not as one may rationally reconstruct it (see, e.g., Callebaut, 1993, p. 72; also Bechtel, 2008; Nelson, 1990). This motivation has led a range of philosophers to draw upon knowledge from other disciplines, such as psychology and the cognitive sciences (e.g., Giere, 1988; Goldman, 1986) as well as history (e.g. Nersessian, 1984; Nickles, 1995), and it has also supported the development of an (integrated) history and philosophy of science (HPS).

[3] For a thorough epistemological analysis of Quine's position see Haack (2009, ch. 6).

This book builds upon naturalist traditions, but it also departs from the way in which a majority of philosophers of science and epistemologists formulate naturalized accounts of science and (scientific) knowledge creation. While it is common for naturalized philosophy to rely upon interpretations of empirical data provided by research outside philosophy, that is, research in natural sciences, this book presents first-hand empirical data, gathered by qualitative, social-scientific methods. The research process that underlies this book, hence, has little to do with the methods that the natural sciences pursue. It is rather an open-ended, iterative process of interpretation—"hermeneutic" rather than "naturalized" (Schickore, 2011).

Although the crossroads between the methods of the philosophy of science and history have been explored eagerly, combinations of philosophical inquiry and social-scientific methods have been far less popular—a rift between philosophy of science and social studies of science that reaches back to the "Science Wars" (Wagenknecht, Nersessian, & Andersen, 2015). Nonetheless, philosophers of science have begun to make use of social-scientific methods. So-called "experimental philosophers" are using quantitative methods such as standardized interviews to learn about the frequency of people's intuitions (Griffiths & Stotz, 2008; Knobe & Nichols, 2008; Machery & O'Neill, 2014). Other philosophers have pioneered qualitative empirical inquiries within philosophy of science (Calvert & Fujimura, 2011; Kastenhofer, 2013; Riesch, 2010; Toon, 2012). Leonelli (2007, 2010), for example, has studied scientific practice in the life sciences with the help of ethnographic methods. Nersessian and collaborators (2003, 2006, 2010, 2011, 2015) have made use of a mix of qualitative methods to explore laboratory collaboration and scientific modeling in fields such as bio-informatics. It is this strand of philosophical inquiry—an inquiry into scientific practice grounded in first-hand empirical data—that I seek to continue in this book.

1.3 Scientists as Reflective Practitioners

In a most fundamental sense, this book is about scientists, about how they collaborate with one another, and how they come to rely upon their collaborators. This book is also about observations of scientists and interviews with them, bringing out what they think about their work, how they think it should be done, and how they reflect upon the experiences of the collaboration that they have been involved in. In this book I approach scientists as reflective practitioners, who have something to say about their work when interviewed. They are able to offer accounts of scientific knowledge creation that reflect their education and their experiences. One premise of this book, a premise concerning its empirical method, is that philosophers should take these "native" expert accounts seriously, even though they may lack the terminology, the subtlety and the argumentative structure of philosophical reasoning.

As reflective practitioners, scientists continuously reflect upon their work and measure it against the standards of scientific practice. To practice experimental science involves manual activity as much as it involves cognitive activity. Scientists reason; reasoning is part of their work. Therefore, I conceive of scientists as cognizant actors—actors who form beliefs in the light of what they know, observe and infer. These beliefs concern the epistemic significance of experimental results, but they also concern, for example, the trustworthiness of collaborators who produced these experimental results.

Addressing scientists as subjects who form beliefs and reflectively seek to justify them, I analyze empirical insights in terms of *knowledge as justified belief*, the predominant albeit not uncontested paradigm in epistemology (Haack, 2009; Ichikawa & Steup, 2013). Within this paradigm, knowledge is understood as attributable to an epistemic subject or a "cognizer" (cf. Longino, 2002, p. 77ff.).[4] Conventionally, the paradigm of an epistemic subject is the individual human being, a paradigm which allows us to conceive of knowledge as individually held justified belief. Though a widespread notion in epistemology, the understanding of knowledge as individually held belief has been criticized by social and feminist epistemologists (see, e.g., Bechtel, 2008; Gilbert, 2004; Nelson, 1990), and I reflect upon some of their critiques in Chap. 9.

[4]An alternative to knowledge as justified true belief is reliabilism, which understands knowledge as reliable belief (Goldman, 2011). Note that a reliabilist position remains within the paradigm of knowledge as belief. For a more fundamental critique of knowledge as belief see, e.g., Vendler (1972), Craig (1990), Welbourne (2001), and Kusch (2002).

What it takes to justify a belief as knowledge is a fundamental epistemological question to which different strands of epistemology have given different answers. For example, the long-standing debate between foundationalism and coherentism discusses what it means for beliefs to be warranted with regard to their logical structure and their logical relation to other sets of belief (see, e.g., Haack, 2009). Virtue epistemologists, in turn, focus on the epistemic agent and argue that the warrant of his or her beliefs is tied to the possession of virtuous faculties or the exercise of virtuous traits (e.g., Greco, 2001; Zagzebski, 1996). Contextualism, in turn, suggests that the warrant of beliefs is dependent upon the mobilization of particular standards of justification (e.g., S. Cohen, 1986; Pritchard, 2000; Rysiew, 2016), and parts of feminist and social epistemology argue that these standards are community-borne (e.g., Longino, 1990; Nelson, 1990). The argument that justification refers to context-dependent standards will become important in later parts of the book when I explore how far collaborators' scientific trustworthiness can justify belief but is not equivalent to the evidence needed to justify a scientific knowledge claim (see Chaps. 7 and 9).

Proceeding from a notion of knowledge as justified belief, I am able to build upon previous epistemological analyses. Some of the issues this book addresses—relations of dependence and trust—have been discussed instructively by social epistemologists in terms of individually held knowledge and its exchange through testimony. It is important to note, however, that while the notion of knowledge as justified belief is predominant in epistemology, it is not widely shared in philosophy of science. Echoing Popper's "third-world knowledge" (1972), philosophy of science commonly endorses a notion of (scientific) knowledge as discursive content, that is, as an inter-individually accessible body of well-corroborated scientific claims in a given peer community. "In this usage, knowledge is what piles up in books and journals in the form of verbal or two-, three-, or four-dimensional representations" (Longino, 2002, p. 82, 83). I do not attempt to tackle this divide between different strands of philosophy. Rather, I proceed from the understanding that scientific knowledge (i.e., the kind of knowledge philosophers of science are typically talking about) is the result of collaborative efforts to integrate individual knowing (see, e.g., Andersen & Wagenknecht, 2013).

Another concept that will appear throughout the book is *expertise*. Scientific expertise is what senior scientists are supposed to have acquired throughout their professional maturation and what enables them to practice science without immediate guidance (but not without help, collaboration or the feedback of their peers). I understand expertise as an individual attribute, comprising his or her declarable and tacit knowledge and skills. Even as an individual attribute, however, expertise necessarily relates to social aspects beyond that individual. What counts as expertise is constituted through the practices of a community of acknowledged experts, and the acquisition of full-blown expertise requires individuals to immerse themselves in the practices of an expert community. Only in this manner will individuals acquire the expertise necessary to contribute to the practices of a given expert community (Collins, 2013; Collins & Evans, 2007).

Conceiving of scientists as reflective practitioners means to conceive of them as actors whose expertise comprises both knowledge-that and knowledge-how, both declarable as well as tacit, that is, inarticulate resources. While scientific research creates scientific knowledge-that, the performance of experimental scientific method relies crucially on knowledge-how. And while reflection requires articulation, practices have a corporeal component that may resist articulation—for practicing scientists themselves, and in parts also for observing philosophers (Goddiksen, 2014; Polanyi, 1962; Soler, 2011). For practicing scientists, experimental practice may remain epistemically "opaque" in the sense that it may be impossible to provide an exhaustive description of experimental activity and complete justification of experimental. As Soler (2011) points out, much of this epistemic opacity is due to the fact that scientific practice requires skill or incorporated knowledge-how.[5]

[5]The notion of knowledge-how, and particularly its relation to the notion of knowledge-that, i.e., propositional knowledge, is debated. Arguably, not all knowledge-how can be reduced to knowledge-that (Fantl, 2012; Ryle, 1971). This seems to be the case even in scientific practice where knowledge-how serves the aim of producing scientific knowledge-that. More important, however, is the relation between knowledge-how and articulation. Both knowledge-that and knowledge-how can be imagined to remain unarticulated in a given context. But insofar as knowledge-how pertains to incorporated knowledge, it stands to reason that it poses a particular challenge for exhaustive

Since incorporated skill is an element of scientific expertise, the latter itself becomes difficult to articulate. That, in turn, makes scientific expertise difficult to learn and difficult to observe in others. Expertise must be learned through immersion in practice. For junior experimental scientists, nothing is more valuable than lab time shared with senior scientists. And shared lab time is equally important as an opportunity to gauge the expertise of peers. In fact, being able to determine the expertise of peers is part of scientific expertise.

Yet, what is the role of scientific expertise for research collaboration? As this book will show, expertise is one important criterion in determining the trustworthiness of collaborators, that is, the scientific quality of their research contribution (cf. Hardwig, 1985). The question whether or not to trust a research colleague is closely tied to the question to which degree that colleague possesses the relevant scientific expertise in a given domain. To determine colleagues' expertise is complicated by the fact that scientific expertise is limited in two respects. First, it is accumulated over long periods of time. Second, it can be more or less comprehensive, though rarely do scientists possess senior expertise in more than one specialized domain of scientific inquiry. Scientific expertise is tied to seniority and disciplinary background, rendering interdisciplinary collaboration, and collaboration between senior and junior scientists, particularly fragile. I will investigate how fragilities of this kind are dealt with in collaborative practice and examine how the confines of individual expertise are mended through research collaboration.

1.4 Chapter Overview

Chapter 2 introduces the reader to research groups, the phenomenon which this book seeks to investigate. For philosophers and social epistemologists of science, research groups are interesting insofar as group

articulation. For this reason, it is questionable whether we should consider knowledge-how to be "knowledge" in the sense of justified true belief. "Skill" or Polanyi's term of "inarticulate intelligence" may be a more apt vocabulary (Polanyi, 1962, p. 71).

members' interactions immediately concern the collaborative making of scientific knowledge. So far, social epistemologists have approached research groups either from an individualist or collectivist perspective, and I provide reasons for endorsing the first perspective, commenting upon the latter only at the end of the book. Moreover, in elaborating the gradual differences between mono- and interdisciplinary research groups, I prepare for the comparative case study that latter chapters unfold.

Chapter 3 addresses questions of method, describing the way in which I rely upon first-hand qualitative empirical data. The use of such data in the context of philosophical theorizing raises a number of meta-methodological and procedural questions. To address these questions, I reflect on the interplay of empirical data and philosophical concepts by mobilizing the notion of dialog between the concrete and abstract. I also provide a detailed description of the case study, the methods of data collection and the process of data analysis that underlie this book. Finally, I consider the challenge of presenting a qualitative empirical case study within an analytic philosophical discourse.

Chapters 4 and 5, then, introduce the reader in greater detail to the two research groups investigated—the planetary science group and the molecular biology laboratory. Throughout this book, I will also refer to these two research groups as group1 and group2, respectively. While the planetary science group is a smaller interdisciplinary research team, the molecular biology laboratory is a relatively large mono-disciplinary team. Both groups have developed a distinct modus operandi, and the differences that the comparison of these two groups will bring out inform my philosophical reflections.

Chapter 6 focuses on the division of labor in research groups, that is, the division of cognitive labor, but also the manual labor of experimental practice and the "social" labor that it takes for group members to interact. I examine how research is divided in the two groups studied, distinguishing two forms of division of labor, and I discuss how far these forms align with notions of division of labor put forward in the existing philosophical literature.

Chapter 7 offers a perspective on epistemic dependence that is grounded in theoretical discussion and field observation at the same time. Since instances of epistemic dependence are multifarious in scientific practice, I propose to distinguish between two different forms of epistemic dependence—opaque and translucent. A scientist is opaquely dependent upon a colleague's labor if he or she does not possess the expertise necessary to carry out independently, and to assess profoundly, the piece of scientific labor his or her colleague is contributing. If the scientist does possess the necessary expertise, then his or her dependence is translucent. Many dependence relations, however, are neither entirely opaque nor translucent. I discuss why this is the case, and show how we can make sense of the gray zone between opaque and translucent epistemic dependence.

Chapter 8 sheds light on the issue of epistemic trust between collaborating scientists. Only on the basis of such trust can collaborating scientists enter relations of epistemic dependence. But while trust pervades scientific practice, scientists do not trust blindly and completely. Rather, as I illustrate with empirical data, they continuously fine tune their attitudes of trust towards collaborators through dialoging practices, eliciting explanations and probing understanding. Moreover, scientists supplement personal trust with impersonal trust and seek to reduce the personal trust relations that are necessary through hierarchical modes of collaboration.

Chapter 9 analyzes how far collaboratively created scientific knowledge can be characterized as collective knowledge. For this analysis, the chapter discusses two existing socio-epistemological approaches to group belief and collective knowledge. While the first of these approaches seeks to "collectivize" the belief component of knowledge as justified (true) belief, the second approach seeks to collectivize the justification component of knowledge and argues that scientific justification is often too complex to be possessed by any one individual scientist. In this chapter, I will reject the first approach and endorse the second, thus providing a concept of collective scientific knowledge that accounts well for the collaborative practices I have observed in my comparative case study.

Finally, Chap. 10 concludes the book, summarizes the arguments and offers some closing remarks.

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2

Research Groups

Since in this book I am attempting to provide a social epistemology of research group collaboration, this chapter introduces research groups both as an empirical phenomenon and as an object of philosophical analysis. To do so, let me begin by saying what I mean by "research group." I use the term to refer to teams of closely collaborating scientists. Such teams are relatively stable and often embedded in the formal structures of research organizations, such as universities. Yet, group membership cannot always be clearly defined; nor is it stable. Also, research groups are by no means the only form of collectivity that research collaboration may rely upon. There are informal networks, academic friendships, authorship coalitions—and there are peer communities, the communities of scientific fields and disciplines.

In fact, notions of community have so far been the dominant analytic "lens" through which the collaborative character of much of scientific practice has been conceived of in philosophy of science. While notions of community have long had a strong foothold in philosophy of science, notions that attend to micro-structures of collaboration have

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not received much attention—an issue for which the interdisciplinary dynamics between philosophy of science and social studies of science can account. At the time when philosophy and history of science were open to incorporating influences from sociology, social studies of science were generally dominated by macro-sociological approaches (see, e.g., Merton, 1965; Zuckerman, 1967). As social studies of science later began to focus on micro-structures of scientific collaboration (Knorr-Cetina, 1981; Latour & Woolgar, 1979), the Science Wars and deep divides concerning the ways in which "the social" is seen to relate to "the epistemic" prevented the reception of sociological influences in the philosophical discourse (cf. Wagenknecht et al., 2015, p. 5).

I will make a modest attempt to help mend this shortfall in interdisciplinary connect. For this reason, Sect. 2.1 characterizes the research group as a phenomenon of scientific collaboration by drawing extensively upon social-scientific research. Section 2.2, then, considers research groups as objects of philosophical analysis, introducing two analytic approaches to them and the scientific knowledge that they create. While the collectivist approach holds that such knowledge is to be analyzed as irreducibly collective, the individualist approach maintains that collaboratively created knowledge can be understood in terms of individually held knowledge. In this book, I base my investigations upon the individualist approach, and I will provide three reasons for doing so. Section 2.3 offers a concise orientation regarding philosophical accounts of interdisciplinarity, situating my concern for interdisciplinary research groups in ongoing discussions in philosophy of science.

2.1 The Phenomenon

Research groups are a historically contingent expression of the fact that what we simply call "research" is an involved effort that takes effect through multiple forms of collaboration (Katz & Martin, 1997; Maienschein, 1993). While social epistemologists and philosophers of science have only just begun to show interest in the structures of collaboration other than specialist communities, social scientists have long studied research groups as a diverse, multifaceted empirical phenomenon. For this reason, this section draws on the work of social scientists to characterize research groups as a distinct, but utterly varied, empirical phenomenon of scientific collaboration.

The significance of scientific collaboration for contemporary science plays out in numbers. As collaborative authorship is arguably a robust measure for close research collaboration (Beaver & Rosen, 1978; Clarke, 1964; Meadows & O'Connor, 1971), bibliometric studies suggest that close collaboration has become the preferred way of conducting research in many fields. The last century witnessed a continuous increase in co-authored papers in almost every scientific discipline, and especially in experimental natural science multi-authored papers dominate by far.[1] Scholars have also found that research collaboration is co-related with individual productivity and visibility (Beaver & Rosen, 1979b; Lee & Bozeman, 2005; Zuckerman, 1967).

Research groups are a crucial element of scientific collaboration, as Edward Hackett observes: "Research groups are an elemental form of scientific collaboration and knowledge production" (Hackett, 2005, p. 788). Scientific collaboration can take many forms but "small batch production" in research groups has become typical for the way scientific knowledge is created at Western universities at least since the mid-twentieth century (Hagstrom, 1974a, p. 757). In a similar vein, Henry Etzkowitz calls small and medium-sized research groups of "moderate scale" the distinctive feature of science at American universities as it has emerged in the last century (Etzkowitz, 1992, p. 28).

Usually, a university-based research group has between three and thirty members. In terms of their academic personnel, most research groups would consist of at least one senior scientist, commonly in a tenured position, who assumes group leader responsibilities, a number of students and post-doctoral fellows as well as further senior scientists. Additionally, technicians and administrative staff may support the research group. The

[1]Scientometric studies have produced a large body of data testifying to the increase in multi-authored publications (Balog, 1979/1980; Beaver & Rosen, 1978, 1979a,b; Braun, Gomez, Mendez, & Schubert, 1992; Cronin, 2005; Cronin, Shaw, & Barre, 2004; Meadows, 1974; Meadows & O'Connor, 1971; Moody, 2004; Wagner & Leydesdorff, 2005). For an overview of the literature see Subramanyam (1983) and Katz and Martin (1997). Note that the number of colleagues thanked in acknowledgments has also increased (Cronin, 2005; Cronin et al., 2004).

size of research groups can change quickly when funding runs out or new research opportunities open up (Etzkowitz, 1992, p. 36). Yet, for a group to function, it should be small enough so that its members can develop ongoing face-to-face relationships. A group is too small when it cannot ensure enough varied interaction among group members. A group is too large when its members' activities cannot be "kept on track," when their activities cannot be sufficiently coordinated, especially when junior scientists cannot be sufficiently supervised and trained and when they may produce significant results that senior group members miss out on(Etzkowitz, 1992, p. 38). The less routine the work, the smaller a group has to be to ensure an accurate exchange of information among its various members (Hagstrom, 1974a, p. 756).

Research groups can be coordinated in different ways. A great variety of factors shape research group leadership, such as the degree of interdisciplinarity involved, work ethos and employment culture, but also group leaders' professional experience, their academic biography and personality. While some groups centralize leadership, other groups will distribute its responsibilities among its members (Cohen, Kruse, & Anbar, 1982). A crucial responsibility of group leadership is to manage the scientific focus of group members' research activities. Here, group leaders focus on an ambivalent choice, as Hackett observes: "tighter focus may intensify competition, while looser focus may dissipate energy and relinquish fruitful synergies" (Hackett, 2005, p. 818). While some competition among group members may increase their initiative, too much competition constrains collaboration among group members and may prove detrimental to the group's research performance.[2] Furthermore, group leadership needs to balance continuity in research focus against "the continual, incremental freshening of a group's membership and capabilities that expands the boundaries of its sphere of inquiry" (Hackett, 2005, p. 794), continuously adapting to the ways in which scientific fields evolve.

Research group members, as Hackett observes, typically "work face-to-face, sharing work space, materials, technologies, objectives, hypotheses

[2]For competition among members of a research group see Edge and Mulkay (1976); Hagstrom (1974b), and for the complicated mediation between collaboration and competition in research groups see Traweek (1988, p. 88); Poulsen (2001).

and, to a significant degree, a professional reputation and fate" (Hackett, 2005, p. 788). Shared resource access, as the historian Jane Maienschein notes, is a major motivation for an individual scientist to enter a research collaboration in the first place, as they are "eligible for resources that individual researchers could not obtain" (Maienschein, 1993, p. 167). The sharing of resources helps increase both the efficiency of scientific labor and the credibility of research outcome. Increasing efficiency may "simply be a matter of needing more hands doing the same kind of work, or it may involve bringing together specialists who provide different types of expertise" (Maienschein, 1993, p. 167). To bundle specialist expertise through collaboration can have a positive impact on the credibility of a scientific knowledge claim: "collaborations among different individuals may produce greater credibility because each brings to the project his or her own credentials and acceptability in a different research community" (Maienschein, 1993, p. 167). For research groups, it can be a challenge to keep these cognitive, technical, social and financial resources "in stock" despite the constant fluctuation of group members.

Beyond resource sharing, research groups also facilitate hands-on learning. Face-to-face collaboration helps group members to acquire the tacit skills that are an important element of scientific expertise, helping junior scientists mature professionally—a responsibility that particularly research groups in academic contexts attend to. In fact, for university research groups, the interaction between senior and junior scientists will often be one of the collaborative relations that determine the way in which a group divides labor among its members.

The collaborative relations within a research group can be multifarious, and a number of typologies have been suggested to describe them. Drawing on scientometric studies, Subramanyam (1983), for example, lists teacher–pupil collaborations, collaborations among colleagues, supervisor–assistant collaborations and researcher–consultant collaborations. Toomela (2007) distinguishes "dialogical collaboration" and "unidirectional collaboration." While the first designates a truly collective research effort, the latter is ultimately an individual enterprise where only one person determines the direction of research. This person hires in other "helping hands" and "basically absorbs knowledge provided by others" (Toomela, 2007, p. 202). For philosophy and epistemology of science, Thagard (1997) suggests a typology that consists of "employer/employee," "teacher/apprentice," "peer-similar" and "peer-different." A mono-disciplinary collaboration takes place among peers with a similar, largely overlapping expertise, while an interdisciplinary collaboration involves scientific peers with utterly different fields of expertise (see also Sect. 2.3). Such typologies, however, say little about the collaborative practice by which individual efforts are entwined. In fact, the task of later chapters (such as, e.g., Chaps. 6, 7, and 8) in this book will be to investigate how different types of relation frame dependence and facilitate the trust that it takes for group members to rely upon one another.

2.2 Individualism or Collectivism?

There is a growing body of epistemological analyses of research groups in social epistemology and philosophy of science. Among epistemological accounts of research group collaboration, Kristina Rolin discerns "two approaches to understanding the epistemic structure of scientific collaboration," an approach that I will call "collectivist," and an approach that I will call "individualist" (Rolin, 2014, p. 74). The collectivist approach argues that collective belief or acceptance are necessary for research groups to create, and have, scientific knowledge—in fact, the scientific knowledge produced by a research group is, according to the collectivist approach, an irreducibly collective group belief or acceptance (see, e.g., Andersen, 2010; Bouvier, 2004; Cheon, 2014; Rolin, 2010; Wray, 2002, 2006). The individualist approach, in contrast, suggests that this need not be the case, arguing that the kind of scientific knowledge that research groups produce can be analyzed in terms of individually held testimonial knowledge (see, e.g., Andersen & Wagenknecht, 2013; Fagan, 2011; Frost-Arnold, 2014; Hardwig, 1991). As Rolin observes, the two approaches can be seen "as two parallel models for understanding the special nature of scientific knowledge produced in collaborations" (Rolin, 2014, p. 74).

Rolin's distinction concerns the status of scientific knowledge: Should scientific knowledge, collaboratively produced by research groups, be analyzed as an irreducibly collective group view; or, rather, in terms of (testimonial) knowledge that can be attributed to single individuals?

This distinction is analytically important, and it reflects the debate that animates social epistemology's ongoing interest in research group collaboration. What I suggest in this book, however, is a way to navigate past a rigid individualist/collectivist divide and add more nuance to Rolin's distinction. In the course of this book, I formulate an inter-individual notion of group collaboration that combines elements of both the individualist and collectivist approach. As I will elaborate in Chap. 9, such a combination is possible because the distinction that Rolin offers primarily concerns collaboratively created scientific knowledge, casting such knowledge as group belief, acceptance or view. It does not concern the justification of scientific knowledge. While I will argue that the justification of a proposition as scientific knowledge can be irreducibly collective, this proposition need not be believed in irreducibly collective ways—an argument that, in fact, will be based on my analysis of inter-individual dependence and trust among collaborating scientists.

So, before arguing for the collective status of scientific justification in Chap. 9, I will pursue an individualist approach to scientific knowledge creation in research groups, examining (testimonial) exchanges between individual scientists on the basis of dependence and trust. I do so because I believe the individualist approach to be better suited to an empirically informed analysis of research group collaboration for three reasons.

First, the analysis that I unfold in the chapters to come relies upon first-hand empirical data (see following Chap. 3). An important part of my data collection consists of my interviews with individual scientists, through which I have gathered the perspectives of scientists as reflective practitioners that justify their actions and their beliefs to themselves and others. Since my methodological approach, hence, foregrounds individual reflection, an epistemological approach that reflects this emphasis seems apt. Moreover, the collected interview data testify to the highly individualist ethos that the scientists I encountered maintained. In addition to interviews, I also conducted ethnographic fieldwork. At no point throughout my field work did I encounter a situation that resembled one of those examples that Margaret Gilbert uses to illustrate what she calls "collective belief" (Gilbert, 1987), the concept which inspired the collectivist approach to research group collaboration (see also Chap. 9).

Second, the individualist approach appears more parsimonious and fine-grained, remaining open to the diversity of research group collaboration. The individualist approach presupposes less than the collectivist approach. It presupposes the existence of inter-individual relations of trust, dependence and testimony, but makes do without group belief or group acceptance. This is advantageous because such inter-individual relations are "smaller" building blocks. A perspective that "zooms" in on inter-individual relations is a more fine-grained perspective that maintains a greater openness to the question of how scientific knowledge is ultimately created and justified. Various constellations of trust and dependence are conceivable. Even if scientific knowledge would be, in one instance or another, aptly analyzed as group belief or acceptance, even then it stands to reason that group collaboration in science is embedded in relations of trust and dependence—that distrust and independence inspire a group of scientists to accept jointly a group view seems a rather odd assumption to make.

Third, the individualist approach is better suited to cope with the ephemeral character of research group collaboration, accommodating difficulties in determining membership. As mentioned in Sect. 2.1, research groups are continuously changing. Many feature a constant turnover in members, and often membership can only be loosely defined. Moreover, research collaboration is not confined to group boundaries and singular authorship alliances often stretch across different research groups. In scientific practice, research groups are rather "fluid" (Andersen, 2010, p. 262). They are also "porous" in that they do not "contain" the creation of scientific knowledge. To create scientific knowledge, scientists do not rely on fellow team members only. They also rely upon far-reaching personal networks and their wider peer communities. For this reason, the collectivist approach may unduly reify research groups as self-sufficient epistemic actors—within and beyond research groups—which constitute scientific collaboration on the group level.

All of these three reasons have, in part, to do with the pragmatics of an empirically informed investigation. For such an investigation to be conceptually constructive, its analytic focus needs to tie in with the empirical data gathered. The collectivist approach formulates a rather specific hypothesis: research groups create and have scientific knowledge qua group belief or group acceptance. As I will elaborate in Chap. 9, this hypothesis rests upon a notion (group belief) which is hard to match with empirical observations. That makes it difficult to support this hypothesis empirically, to falsify it or to modify it. The individualist approach, however, formulates a hypothesis that is better suited to an empirically informed investigation that formulates a broad hypothesis: the scientific knowledge that research groups create can be analyzed in terms of dependence and trust, and thus be reduced to individually held knowledge. This hypothesis crucially relies upon concepts (dependence, trust) that are not entirely disconnected from quotidian language, that can be assumed to be observable to some degree both by practitioners and ethnographic observers of collaborative scientific practice, and for which the field of social epistemology offers a broad range of nuanced formulations—a rich vocabulary to choose from and work with, to probe and develop.

But the collective approach raises an important question that an analysis proceeding from the individualist approach must eventually confront: where does research group collaboration set the limits of individual knowing? That is, what can individual collaborators *know*? This question is particularly palpable in the context of interdisciplinary research. Therefore, in this book, I examine both interdisciplinary and mono-disciplinary research group collaboration. To situate my examination of interdisciplinary research in the philosophical discourse, the next section provides a concise overview of the discussions of interdisciplinarity that philosophy of science has had.

2.3 *Excourse*: Interdisciplinary Research Groups

As observed above, research groups can vary distinctively, an important variable being the breadth and diversity of expertise that group members possess or seek to acquire. In mono-disciplinary research groups, all members possess or seek to acquire roughly the same kind of expertise. In interdisciplinary research groups, this is not the case. Since this book builds upon a comparative case study involving a mono- and an interdisciplinary research group, a preparatory comment on philosophical accounts of interdisciplinary collaboration is appropriate here.

The term "interdisciplinarity" characterizes research across disciplinary boundaries, a multifaceted phenomenon that defies any straightforward definition.[3] Interdisciplinary endeavors differ substantially in their intellectual scope and cohesion, scale, organizational constitution, internal structure, purpose, results and stability over time. Furthermore, interdisciplinarity describes the character of collaborative scientific practice as well as the discourse, the scientific content, that such collaboration may produce—a conceptual split reflected in the approaches to interdisciplinarity that philosophers of science and social epistemologists have formulated.

Generally, one can discern two different angles from which interdisciplinarity is studied: as discursive integration of scientific research or as collaboration between scientists from diverging disciplinary backgrounds. The dominant approach is to investigate interdisciplinarity as discursive integration, analyzing the syntactic and semantic relations between theories, concepts, models and evidence which stem from different branches of the scientific discourse (Bechtel, 1986; Darden & Maull, 1977; Mäki, 2009; Mitchell, Daston, Gigerenzer, Sesardic, & Sloep, 1997; Wylie, 1999). A second approach is to investigate interdisciplinarity as a collaboration between scientists, studying the ways in which they cooperate across differences in disciplinary backgrounds so as to integrate individual contributions into a piece of interdisciplinary research (Andersen & Wagenknecht, 2013; Mattila, 2005; Osbeck & Nersessian, 2010; Paletz & Schunn, 2010; Rossini & Porter, 1979). This is the approach that in this book I seek to contribute to.

Both approaches make frequent reference to Kuhn's work, translating his argument about incommensurable scientific paradigms into the hypothesis that there are significant socio-cognitive gaps between scientific disciplines—gaps that can, or cannot, be "bridged." Thus, the notion of "interdisciplinary integration" that would bridge cognitive

[3] For an overview see Frodeman, Klein, Mitcham, and Holbrook (2010); Huutoniemi, Klein, Bruun, and Hukkinen (2010); Klein (1996, 2010); Weingart and Stehr (2000).

differences, or the rejection of this notion, underlies many philosophical accounts of interdisciplinarity (though this is seldom explicated) (Gillespie & Birmbaum, 1980; Holbrook, 2013; Mansilla, 2006; Petrie, 1976).

Concerns for the challenges of interdisciplinary integration should not lead, however, to an over-emphasis on the difference between mono- and interdisciplinary modes of research. Instead of being a fundamental difference in kind, the difference between mono- and interdisciplinary research can be conceived of as a difference in degree, a conception that forms the premise for the comparative approach taken here. Two arguments can support such gradual conception of the difference between mono- and interdisciplinary research. On the one hand, the fact that interdisciplinary research is possible and new fields emerge at the intersection of established disciplines should lead us to consider whether the relation of disciplines and sub-disciplines is one of neighboring resemblance rather than exotic difference (Campbell, 1969). On the other hand, there is good reason to believe that disciplines and sub-disciplines are, in themselves, much more fragmented than any talk about interdisciplinarity as gap-bridging usually would suggest. In fact, as Abbott (2001) has shown for the case of sociology, paradigmatic gaps can be constitutive to the very backbone of a discipline's identity. In a similar vein, MacIntyre has argued that a scientific tradition is constituted by "a conflict of interpretations of that tradition" (MacIntyre, 1980, p. 62). And philosopher of science Joseph Rouse has argued that philosophy's "standard view" on scientific communities either overstates consensus or formulates it too vaguely (Rouse, 1996, p. 168).

When mono- and interdisciplinary research collaboration are seen as different, but not fundamentally so, a comparison between them can yield insightful results. Therefore, in this book I also provide a comparative case study between a mono- and an interdisciplinary research group. I will probe the assumption that scientists in both mono- and interdisciplinary research groups grapple with similar issues of collaboration, such as issues of trust and dependence. To study ways in which these issues play out differently in different types of research groups, which is my assumption, can help formulate a more comprehensive account of collaborative scientific practice than a study in mono- or interdisciplinary groups alone.

2.4 Conclusion

In contemporary natural science, scientific knowledge is created collaboratively in research groups. Therefore, any attempt to understand comprehensively the social dimension of scientific knowledge creation has to account for the epistemic role of research groups. While philosophy has long focused on notions of community to account for epistemologically significant matters of social structure, more recent research in philosophy and social epistemology of science has begun to analyze research groups, choosing either a collectivist or an individualist approach. I pursue an individualist approach to research group collaboration, a decision resonant with the book's empirical grounding in qualitative data obtained from observation and individual interviewing. Furthermore, I pursue a comparative angle, investigating the differences and similarities of mono- and interdisciplinary group collaboration. The following Chap. 3 will detail the empirical method and the comparative case study that underlie this book.

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3

Method

In this chapter, I will explain how I combine philosophical theorizing with first-hand empirical insights that I have gained through the use of qualitative empirical methods. When I decided to make extensive use of these methods, I decided to move out of philosophy's comfort zone. Understandably, for philosophers this move raises a number of questions. How can empirical observations be related to philosophy's conceptual considerations? Can they be related at all? How can philosophical theorizing be combined with qualitative methods? What do I mean when I speak about qualitative methods, and what is the investigator's role in the process of inquiry that such methods entail?

This chapter answers these questions regarding the study that underlies this book. In Sect. 3.1, I provide meta-methodological considerations about the manner in which I combine philosophical theorizing and empirical insights. Thereafter, I describe the details of my empirical case study (Sect. 3.2), my methods of data collection (Sect. 3.3) and my data analysis (Sect. 3.4). At the end of this chapter, I reflect upon the challenge of presenting a qualitative empirical case study within an analytic philosophical discourse (Sect. 3.5).

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3.1 Engaging Empirical Insights in Philosophy

Before elaborating on the details of the empirical method that I have chosen to make use of, I want to consider in a meta-methodological way the manner in which I combine philosophical reflection with the empirical insights gained via social-scientific, qualitative methods. Although few philosophers mobilize such methods for their work, the use of empirical insight for philosophical reflection has been extensively discussed in the context of an integrated history and philosophy of science (HPS)—a discussion to which I return here because it helps articulate how the normative and the descriptive, the particular and the abstract, can be fruitfully interwoven for an empirically grounded social epistemology of scientific practice.

"But shouldn't philosophy be normative?" is a question I frequently encountered when I presented the project of this book to broader philosophical audiences who were unfamiliar with the use of empirical methods. The assertion that philosophical work should be normative often implies the suggestion that it should not be too descriptive, voicing the concern that descriptive care for "time-bound particulars" is not easily reconciled with philosophy's interest in "timeless truths" and normative standards (Caneva, 2011, p. 51). As philosopher of science William Wimsatt sees it, "[w]ithout normative force, studies of methodology, however interesting, would translate as a catalogue of fortuitous and mysterious particular accidents, with no method at all" (Wimsatt, 2007, p. 26). And yes, indeed, philosophical analyses of scientific practice need to be normative when they address questions of scientific knowledge, characterizing some beliefs as "knowledge" and others not, deeming some propositions "scientifically justified" and others not.

But not all philosophical analysis needs to be outright prescriptive, deriving precise and ultimate claims about how science should be conducted and scientific knowledge should be created. Rather, philosophical accounts of scientific practice can be normative in that they account for the limitations of actual scientific practice and reconstruct the normative judgment inherent in that practice, a judgment reflected in the experience of practicing scientists. For philosophy to be normative in this sense is an undertaking that complements outright prescriptive accounts in worthwhile ways.

In this book, I refrain from making outright prescriptive judgments about "good," "bad" or particularly "efficient" science. Rather, I trace scientists' vernacular notions of normativity which are embedded in personal experience and individual perspective. I do not offer philosophical advice on how to improve the creation of scientific knowledge, and I fall short of prescriptively outlining which conditions lead most efficiently to the most secure knowledge and why. Instead, I seek to achieve something much more modest. Guided by an interest in epistemological concepts, I provide first-hand insight into what scientific collaboration actually looks like, how practicing scientists go about collaborating, and how they assess its epistemic challenges. I discuss how these empirical insights resonate, complement and help to modify or challenge existing accounts of collaborative knowledge creation in philosophy of science and social epistemology.

How empirical insight can relate to philosophy of science is a question that has been discussed many times regarding the integration of HPS, which has been problematized by a range of scholars and deemed impossible by some. For example, Pitt (2001) argues that anybody who seeks to combine historical case studies and philosophical abstraction is caught in a dilemma: if one takes philosophy as point of departure and picks a case study to elaborate on philosophical claims, "[…] then it is not clear that the philosophical claims have been supported, because it could be argued that the historical data was manipulated to fit the point." But if one takes historical data as a point of departure, "[…] it is not clear where to go from there—for it is unreasonable to generalize from one case or even two or three" (Pitt, 2001, p. 373). Nonetheless, scholars do integrate historical case studies and philosophical reflections, and it has been suggested that Pitt's dilemma can be avoided.

The dilemma of either generalizing too much or too little is avoidable, Richard Burian holds, arguing that the historian-philosopher must neither be "philosophically innocent" nor must he or she "proceed to grand conclusions by induction from absurdly small samples" (Burian, 2001, p. 388). Burian agrees with Pitt that approaches which work from first, philosophical principles and then draw on historical data for deductive inference or mere illustration are not helpful. Therefore, he advises the historian-philosopher to work his or her way up from the study of particular scientific practices to context-dependent, "limited generalizations" (Burian, 2001, p. 399). Such inductive case studies, Burian argues, can yield well-established claims of "regional" scope which are fallible but valid (Burian, 2001, p. 400).

While sympathetic to an inductive approach, Hasok Chang has more recently voiced the opinion that it is not rigorous enough. Instead, he argues, "[…] we need […] to see if we can get beyond an inductive view of the history-philosophy relation, which takes history as particular and philosophy as general" (Chang, 2011, p. 110). He proposes that we should conceive of this relation as that between "the concrete and the abstract," two components intricately interwoven with one another. Without each other, they are meaningless. Any description of analytic depth will necessarily contain both the concrete and the abstract. "This necessity should not be resisted or avoided, but actively embraced as a great intellectual opportunity" (Chang, 2011, p. 111). In this perspective, the task of a philosopher-historian is to "extract abstract insights" (Chang, 2011, p. 110) from the detailed study of historical episodes and show their "cogency" and applicability: "successful application functions as confirmation, but without the presumption of universality in what is confirmed" (Chang, 2011, p. 111). Put differently, concepts show their philosophical value in the ways in which they can be transferred and, I would like to add, lend themselves to fruitful modification, variation and refinement in different, "concrete" contexts.[1]

The rejection of both a naive inductive as well as a deductive view of the relation between history and philosophy is not new and has been elaborated by other historically minded philosophers in similar ways before. As Jutta Schickore argues, the integration of historical insight and philosophical conceptualization implies a complicated movement back and forth in the course of which empirical insight and conceptual framing

[1]Compare with reflection in qualitative social scientific method as in George and Bennett (2005) and Gerring (2007).

constitute one another in a sequence of hermeneutic iterations, eventually leading to a saturated understanding (Schickore, 2009, p. 102). Similarly, Nancy J. Nersessian calls case-study-based reasoning in philosophy of science a "bootstrap procedure" (Nersessian, 1991, p. 683) which, as Paul Thagard puts it, comes to a close when a "reflective equilibrium" is reached (Thagard, 1988, p. 119). On such accounts, historians do not provide "facts" on the basis of which philosophers could draw inductive conclusions in any straightforward way. Nor should historical insights be regarded as "evidence" against which philosophical hypotheses have to be tested (Giere, 2011, p. 64).

My work takes inspiration in the approaches that Chang, Nersessian, Schickore and others have outlined for the integration of historical and philosophical work. I use philosophical concepts to guide my empirical inquiry and interpret empirical data, using empirical insight to reflect on existing conceptual approaches in philosophy of science and social epistemology. I do not seek to "confront" empirical insight with philosophical concepts, but rather to explore the resonances and dissonances between them in a cyclic, dialogical back and forth movement(see also Mansnerus & Wagenknecht, 2015).

My work aims to mediate between the established conceptual ("abstract") discourse in philosophy and empirical ("concrete") insights gained through qualitative observation and interviewing. Yet, already before philosophical concepts are reflected against empirical insight, the abstract and the concrete are incorporated into one another in complex ways. Any concrete experiential insight gains shape and, more importantly, meaning only when it is molded into concepts. These concepts need not be philosophical concepts; they can be vernacular notions. To combine empirical insight with philosophical reflection means to add *another* conceptual layer, another sense in which the concrete relates to the abstract.

In creating a dialog between abstract and concrete, I try to refrain from quickly subsuming empirical insight under philosophical concepts, avoiding the fitting of the one unduly to the other—an ideal that is difficult to achieve given the imbalance between philosophical concepts and first-hand empirical insights that any attempt to mediate between them faces. Existing philosophical concepts are rooted in an established discourse; and they have their adherents and defendants. In contrast, empirical insights are voiced, tentatively, at first only by the researcher who gathers them. Therefore, any empirically grounded philosophy is challenged to find ways to build empirical insight up, consolidate it and retain a sensitivity to its character while engaging with abstract philosophical thought.

In the result of such a dialogical process of inquiry, philosophical concepts can be further differentiated, contextualized and modified. Accordingly, this book offers a conceptual distinction between different forms of epistemic dependence (Chap. 7), provides a contextualized account of epistemic trust (Chap. 8) and discusses how a more practice-minded understanding of the collective character of scientific knowledge can be developed, a philosophical understanding that accounts for the socio-epistemic entanglements which underlie the formulation of natural scientific knowledge by a group of researchers (Chap. 9).

As will become clear in Chaps. 7, 8 and 9, the manner in which I bring together empirical insight and philosophical concepts varies. When I analyze epistemic dependence in Chap. 7, I search for empirical observations that correlate with an established philosophical concept, and on the basis of these observations, I suggest a way to differentiate the concept further. In contrast to epistemic dependence, the notion of trust is much more rooted in the vernacular. Every scientist, it seems, has rich experiences with trust, breaches of trust and the problem of gauging another person's trustworthiness. In my interviews, scientists were able to provide comprehensive reflections upon trust. For this reason, my work in Chap. 8 has been to present scientists' quotidian reflections upon trust and provide an account of it that reflects their experiences. My approach in Chap. 9 is different, since the philosophical concepts there are rather abstract, and particularly the concept of joint commitment has little traction with the forms of research collaboration I studied. Therefore, drawing on Melinda Fagan's work (2011), I try to assess the applicability and the analytic value of these concepts in a more indirect way.

In the remainder of this chapter, I describe how I access "concrete" scientific practice through a comparative case study, relying on ethnographic observation (Sect. 3.2) and interviewing (Sect. 3.3). I regard interviewing as a means of gathering empirical insight in which the dialog between the philosophically abstract and the experientially concrete is, in parts, enacted in an actual dialog between philosopher-interviewer and scientist-interviewee. In Sect. 3.4, I explain how my data analysis intertwined philosophical concepts and the qualitative data I gathered. Finally, in Sect. 3.5, I reflect upon the challenge of conveying a dialog between abstract and concrete to the reader.

3.2 A Qualitative Case Study

To study the collaborative scientific practice of research groups, I have adopted qualitative social-scientific methods for my empirical study, building upon the experiences with such methods that other philosophers of science have made in recent years (Calvert & Fujimura, 2011; Kastenhofer, 2013; Leonelli, 2007, 2010; Nersessian, 2006; Nersessian, Kurz-Milcke, Newstetter, & Davies, 2003; Osbeck & Nersessian, 2010; Osbeck, Nersessian, Malone, & Newstetter, 2011; Riesch, 2010; Toon, 2012).[2] Qualitative methods approach the empirical not as something that is to be measured and expressed in quantifiable variables. Instead, qualitative methods are geared towards a hermeneutic understanding of the empirical object of study, approaching this object in real world contexts of practice. In so doing, qualitative methods are committed to creating detailed, highly context-sensitive empirical data and value depth, rather than empirical range. To this end, qualitative methods crucially involve the researcher's hermeneutic effort, relying upon him or her as a highly sensitive "instrument" of data generation (Osbeck & Nersessian, 2015).

I used qualitative methods to study research group collaboration as experienced by scientists in scientific practice, that is, through their situated, practice-bound sense-making efforts, seeking to understand their reasoning and reconstruct the epistemic rationale of collaborative scientific practice. In order to gain access to scientific practice and scientists'

[2] For general references on qualitative research methods as developed in the social sciences see Strauss and Corbin (1990) and Denzin and Lincoln (1994).

experiences with it, I employed qualitative methods such as observation and interviewing, methods that create an immediate encounter with practicing scientists and which enabled me to gain rich, multi-faceted insights.

As qualitative methods of inquiry prioritize context-sensitivity and hermeneutic depth over range, I have confined my study to two cases. I investigated two academic research groups at Danish universities, the planetary science group and the molecular biology laboratory (see Table 3.1). When I call these two research groups my "cases," I understand a case as a local context for data generation (cf. Flyvbjerg, 2007; Gerring, 2007; Platt, 2007; Stake, 2000). For philosophy of science, Burian specifies case studies as being "[…] concerned with scientific work carried out during a limited time period and [which] are usually restricted to a specified set of scientists, institutions, laboratories, disciplines, or traditions" (Burian, 2001, p. 384). In this sense, case studies enable the investigator to confine meaningfully data collection to a fragment of what is empirically observable. Qualitative data that involve actors' experiences, however, are not confined to the local and temporal context of a case study. When interviewed about issues such as inter-individual dependence and the role of trust in scientific practice, interviewees refer to experiences that have been accumulated through their professional careers and beyond.

Although much of philosophy of science seems to have a preference for "characteristic," paradigmatic cases (as, e.g., in Wimsatt, 2007, p. 28), my case selection has not been guided by such preference. I do not claim that the two groups that I study are particularly characteristic, nor do I claim that they are particularly important. I chose these groups for two reasons. First of all, both groups were welcoming and enthusiastic about my research project. (This was not the case for all research groups that I contacted.) Second, the two groups complemented one another in some of their characteristics—one being small, the other being relatively large; one comprising a high ratio of senior researchers, the other comprising a high ratio of junior researchers; one being interdisciplinary, the other mono-disciplinary (see Table 3.1). This choice enabled me to adopt a comparative perspective and to transcend analytically the configuration of the conditions of one case and provide impetus for philosophical reflection that differentiates between the conditions of, for example, mono- and interdisciplinary research (see also Sect. 2.3).

Selecting a case and going out into the field is more easily said than done. You have to identify and email "gatekeepers." In these first emails, you will try to convey your professional integrity, your trustworthiness and your sincere intentions. When you then come to meet your contacts, feelings of curiosity and uneasiness will accompany you. What will they make of you, a "philosopher"? Will they allow you to observe them? I was surprised and relieved to find that I was granted access rather quickly. Research groups opened their doors at my request, and they were welcoming, polite but also slightly puzzled about my undertaking. They did, however, believe that the functioning of their research group was indeed worth empirical investigation.

I established contact with the first of the two groups, the planetary science group, in early summer 2010. Shortly thereafter, in early autumn, I established contact with the second group, the molecular biology laboratory. In both cases, first contact was made by email, after which I went to have a conversation with the group's spokesperson or leader, who enthusiastically introduced me to the group's research. I, in turn, explained to them that my research interest was to investigate "how scientists collaborate in groups." After these introductory conversations, I began to observe the groups' weekly meetings. In a first phase of fieldwork I prioritized the planetary science group (group1), and in a second phase I prioritized the molecular biology laboratory (group2). I will describe both research groups in detail in Chaps. 4, 5 and 6.

My field observations can be divided into two different phases. In the beginning, I observed weekly meetings. Later I focused my attention more particularly on single group members. The participation in weekly meetings allowed me to familiarize myself slowly with the group. In these meetings both administrative and research issues were discussed. In both groups, I observed approximately 15 meetings. Leaving interruptions of my fieldwork aside, my observations of each group stretched, roughly, over the course of a year. Although my philosophical interest in the socio-epistemic dynamics of collaborative scientific practice was set right from the start, I began my fieldwork with a broad curiosity for everything that concerned the groups' functioning and was brought up by group members, including, for example, administrative issues and aspects of science policy. During my fieldwork, I also learned about the content of their research. I read articles that group members had co-authored and handbooks about the basics of their area of interest.

When I had familiarized myself with the group sufficiently, I started "shadowing" single group members individually. Shadowing is an ethnographic technique for following single persons through their daily life (Czarniawska, 2007). For one or more working days, I spent my time with individual group members in their office, left with them to go to the lab, went with them to short administrative meetings, chatted in the elevator and had lunch with them. I took as many notes as possible. This was often exhausting, but together with my observations of their meetings shadowing proved a good preparation for the subsequent interviews. With three exceptions, I shadowed all interviewees before I interviewed them (see Table 3.1).

During the groups' weekly meetings I behaved passively and took notes silently. I was, as the leader of the molecular biology laboratory liked

Table 3.1 The two groups studied

Planetary science group (group1)	Molecular biology laboratory (group2)
Interdisciplinary	Mono-disciplinary
Informal, stretching across organizational units	Formal, coextensive with sub-departmental unit
Egalitarian with a "spokesperson" dedicated to research coordination	Hierarchical with clear leadership, including about six subgroups
Six core members, among them five senior scientists and a post-doctoral researcher, more peripheral members	About 35 group members in total
High ratio of senior scientists	High ratio of non-tenured junior scientists
Observed circa 15 weekly group meetings over the course of a year	Observed circa 15 meetings over the course of a year, with interruptions, and observed three subgroup meetings
Shadowed six group members for one day respectively	Shadowed one group member for two and a half days, another one for two days
Interviewed six group members	Interviewed four group members

to describe me, a “fly on the wall.” Yet when I began to shadow single group members my role changed. While observing their lab work or joining lunch breaks with the scientists studied, I had many extended, informal conversations about their research groups and their collaborative networks, about the place that their research groups claim for themselves within the university organization, and national and international peer communities. When scientists revealed their curiosity about philosophy of science, I explained what they seemed to be interested in. I avoided philosophical jargon in the same way that they avoided biochemical or geological terms when explaining their work to me. I did not pretend to share their educational background and their research interests. Despite my physical presence in the scientists’ labs, meeting rooms and canteens, and given that I had received no academic training in natural science, I remained an outside visitor. The professional slack between us helped to establish myself as a trustworthy outsider, opening a space for conversations about scientific practice that took a few steps back, “abstracting” from the “concrete” everyday of research practice and speaking about the socio-epistemic challenges of collaborative research.

Philosophers of science have found different ways in which to relate to the scientists whose work and whose manner of working they are interested in studying. At one end of the spectrum, there are philosophers whose research interests substantially overlap with those of the scientists observed and interviewed. Borrowing a term which Hasok Chang has coined, one could call these philosophical studies “complementary science” (Chang, 2004, p. 238ff.). Reflecting upon her participant observation, Sabina Leonelli describes herself as being “[…] closely allied with the scientific goals pursued by the scientists I was studying” (Leonelli, 2007, p. 88). In fact, she calls herself a collaborator rather than observer (Leonelli, 2007, p. 89). Moving closer to the other end of the spectrum, there are philosophers whose research interests are dissimilar to the scientific interests of the researchers studied, for example, Nancy Nersessian’s work on scientific cognition. The research questions guiding her are largely disconnected from the questions pursued by the researchers studied. This does not imply that her work would be of no concern to practicing scientists. Nersessian has reasons to believe that her cognitive studies can contribute to academic training (Nersessian, 1995, p. 211).

A dissimilarity of interests between philosopher-investigator and scientists studied is characteristic of studies aimed at an understanding of collaborative practice. Maj-Britt Poulsen's study (2001) of how scholars in biomedicine balance competition and collaboration is of no particular biomedical interest. Similarly, the philosophical interests that I pursue in this book have little to do with the research in the two groups that I studied. I am interested in how natural scientists come to collaborate, a question that may fascinate scientists and relate directly to their work but is not the focus of their research.

3.3 Interviewing Scientists

I conducted six interviews with members of the planetary science group and four interviews with members of the molecular biology laboratory. In the planetary science group, I interviewed all core group members that were willing to participate, that is, four senior scientists and a post-doctoral researcher. To learn more about the perspective of junior scientists, I additionally interviewed a PhD student. Except for the post-doctoral researcher, all interviewees were male. In the molecular biology laboratory, however, it was impossible for me to interview all or most group members. Therefore, I chose to interview at least one researcher from each level of hierarchy and to include at least one female perspective, eventually conducting four interviews with a male PhD student, another male, a former PhD student who had just obtained his degree, a female associate professor who in the group's hierarchy formed part of the mid-level, and the group leader. All interviews lasted between 40 and 90 minutes and took place in interviewees' offices or, if available, the group's dedicated meeting room. Only the two PhD students from the molecular biology laboratory were interviewed in a university canteen.

To prepare for interviewing, I conducted two pilot interviews outside the two selected research groups and which provided a testing ground for my questions. When I later had familiarized myself with the selected research groups through extended observation, I began to interview group members. The shift from observing to interviewing was significant, since it implied a shift from a rather passive role in which I could keep my observations to myself to a role in which I structured the exchange between myself and the scientists more openly.

My approach can be described as "theory-generating expert interviewing" (Bogner & Menz, 2009, p. 47f.). Nevertheless, I do not perceive of my interviewing as the kind of expert interviewing that is geared towards particularly informed, exclusive or elite perspectives on scientific practice (as, for example, suggested by Zuckerman, 1972). Instead, I approached every interviewee as being "expert" in his or her individual daily professional practice, notwithstanding age or reputation. I employed a semi-structured question format for the interviews (Fontana & Frey, 2000, p. 653). Each time I used an interview guide with about 15 questions on it, which, over time, I slightly but not substantially adapted. Sometimes I asked the questions I had prepared literally; other times I reformulated them ad hoc so that they would not interrupt the conversational flow. Often, I would change their order by skipping questions that were brought up by interviewees themselves or that did not apply. When, for example, a PhD student told me that he had not yet written a journal article, I would not ask him about collaborative writing processes. My role in interviewing was moderately directive, allowing for detailed descriptions and digressions but guiding interviewees toward certain topics and questions. As I had observed most interviewees at their work beforehand, we could relate to specific persons, incidents or articles in our interview in order to illuminate more general points.

Interviewing is a method of data generation with an in-built verbal bias, privileging explicit reflection. On the one hand, the inexplicable and tacit can be difficult to access in interviews; on the other hand, the expectation to explicate themselves and the conversational dynamics between interviewer and interviewee may result in over-verbalizing and creating interview artifacts and ad hoc interpretations that tell more about the interview than about its subject. In my work, I have addressed these challenges by conducting multiple interviews, bringing each distinct individual perspective to the fore, and combining interview data with data gathered through observation.

From my experience, I conceive of interviewing as a dialog, a situated co-construction between interviewer and interviewee (King & Horrocks, 2010, p. 134), a discursive activity that forces individuals to face the "otherness" of their interlocutor in a process of dialectical negotiation. As Schostak points out:

> The interview […] is a place where views may clash, deceive, seduce, enchant. It is as much about seeing a world—mine, yours, ours, theirs—as about hearing accounts, opinions, arguments, reasons, declarations: words with views into different worlds. (Schostak, 2006, p. 1)

At the same time, for both interviewed scientist and interviewing philosopher, the interview is a compressed confrontation with their own work: for the scientist, because she is asked about her work; for the interviewer, because it is a test of the fruitfulness of her research question. Interviewing mobilizes intellectual resources of both interviewee and interviewer. Both have their own knowledge about the phenomenon in question, be it previously gained theoretical knowledge or practical experience, and both kinds of knowledge are necessary to establish an interview relationship between them. Hence, interviewing can be understood as mediation between different stocks of knowledge and experience. While the philosopher-interviewer draws on his or her conceptual discourses, the interviewed scientists will be asked to mobilize their professional experience in answering them. Interviews are thus a space where theory and experience, the abstract and concrete, meet through actual dialog.

The concept of co-construction enables one to make sense of the fundamental asymmetries involved in interviewing: the interview is jointly, but asymmetrically, co-constructed.[3] Any understanding of what an interview actually is should take into account the fact that interview data originate from a particular context. Interviews are unusual situations. They are not part of everyday life, but interrupt daily routines and thus offer a

[3]Yet, in contrast to, e.g., Hasu and Miettinen (2006) and Ellis and Berger (2003), my dialogical approach carries no "interventionist" motivation. The concept of co-construction should not lead the reader to believe that I have transcended the form of traditional interviewing—I certainly have not. I have restrained myself to asking questions, elaborating on these questions and offering reformulations. In single instances I have explained in simple terms how some philosophers would think about the issue in question. I have not, however, confronted interviewees with an elaborate description of my tentative, theoretically informed perspective.

niche for reflections that otherwise might not occur. Both interviewer and interviewee contribute to the interview, but they do so in very different ways. Neither during nor after the interview can interview partners be equal peers. Both of them are observers. During the interview, they observe one another and interpret facial expression, gesture, tone of voice, and implicit and explicit messages. But whereas the interviewer-analyst is primarily listening during the actual interview, the interviewee remains silent during the analysis process. The interpretation that is pivotal for a philosophical study is the one made by the interviewing philosopher, and in the end he or she comes to represent the co-constructed interview.[4]

Interviewing has been a pleasant experience. Most of my questions were met with goodwill. The challenge for interviewing with philosophical intentions lies, however, before the actual occasion. Preparing good questions is a time-consuming task. Useful questions have to be both meaningful to the interviewee and meaningful with regard to the philosophical issues at hand. In my experience, asking practicing scientists to take up philosophical theorizing does not yield results of the quality desired. Interviewing is a co-construction, but its grounds have to be carefully prepared by the investigator.

3.4 Structuring Empirical Insights

The qualitative data that I gathered through field observation and interviewing had to be processed—sorted, stored, carefully transcribed, analyzed, interpreted. This section describes how I handled the gathered data in the process of my analysis that enabled me to interpret observation and interview data as empirical insights. On the one hand, my data analysis needed to reflect my philosophical interests; on the other hand, I felt that I needed to do justice to the depth and abundance of the empirical data that I was able to gather. Therefore, I have chosen to code large parts of

[4]The issue of representation has been dealt with comprehensively in anthropology, see, e.g., Denzin (1994, p. 503). Interviewing philosophers represent the people who are participating in their study and they should deal with this burden carefully, see Fine, Weis, Weseen, and Wong (2000).

my data, that is, some of the field notes that I took during group meetings and shadowing phases, and all interview transcripts.

Coding is a method of analysis that treats empirical data as text and involves an analysis process in the course of which the data text is segmented and reorganized. Being a term adapted from grounded theory, 'codes' are "[…] conceptual labels placed on discrete happenings, events, and other instances of phenomena" (Strauss & Corbin, 1990, p. 61; see also Alexa & Zuell, 2000, p. 306). To code means to index text passages with descriptive or analytic categories that relate, if vaguely at first, to the research interest. Indexing text passages allows the researcher to familiarize him or herself with small data fragments. By breaking up a body of text into manageable segments, describing those segments, unfolding the information they convey, comparing and relating them, the researcher reorganizes the data with regard to the research focus and is able to interpret them as a response to the research questions (cf. Coffey & Atkinson, 1996).

The formulation of codes can be guided by the intention to describe data from the bottom-up, that is, with the conceptual resources inherent in them, or by the intention to relate them to theory, that is, to the repertoire of abstract concepts offered by the discourse in which a piece of research is to be embedded. An example of a strongly theory-guided code in my analysis would be "testimony." Labeling different text passages with this code, however, did not help me substantially in organizing my data material, because the label "testimony" did not correspond very well with the data that I had. I realized that the acts of informational exchange that I was looking for were embedded in socio-epistemic relationships that were better described as "dependence relations." Therefore, I started to code for "dependence" instead. This led me to distinguish two different forms of epistemic dependence (see Chap. 7). An example of a code that was, at least in the beginning, rather unconnected to my theoretical framework would be "frustration." While shadowing a PhD student, frustration was a recurrent theme in his conversations with me. Clearly, this was related to the high pressure to succeed, his anxiety about failure and his understanding that he had been given a high-risk project as his dissertation topic. His frustration, however, was also related to his work conditions. He perceived his research group to be only a "little interactive," a perception in sharp contrast to my observation that led me to reflect on my biases as an observer and to reconsider the interplay between deliberation and delegation (see Chaps. 5 and 6).

For my coding process, I did not formulate a template of codes prior to the analyzing stage. When I had transcribed the data gained from observing or interviewing (which forced me to go through my audio records at a painstakingly slow pace), I began applying ad hoc codes to semantic units in the text which seemed relevant to my research interest. I have used both rather descriptive codes, which did not appear to be theoretically relevant at first glance, and more interpretive, theory-inspired codes to label and organize text passages (cf. Strauss, 1987, p. 33f.; King & Horrocks, 2010, p. 153). At certain stages, I put existing codes into a systematized order. This helped me to envision new, complementary codes to match the existing ones. In between coding cycles, I went back to the data in their raw, unprocessed form. Such repeated phases of unstructured immersion ensured that I did not lose touch with the original data. After I had skimmed through scribbled field notes again, and relistened to the original audio files, I wrote encompassing case descriptions and composed characterizations of single interviews. In part, these descriptions form the basis of the portrayal of selected interviewees in Chaps. 4 and 5.

From coding and less structured immersion phases, I proceeded to the formulation of broader, more general categories which I call "themes." Thematic analysis is a generic technique of analysis in qualitative research (Attride-Stirling, 2001; Boyatzis, 1998; Charmaz, 2000).[5] Themes ideally resonate both with the investigator's interest and with the concerns of the people studied as expressed in the gathered data. In general, a theme "[…] captures something important about the data in relation to the research question, and represents some level of patterned response or meaning within the data set" (Braun & Clarke, 2006, p. 82). Whereas codes pertain to the micro-level of analysis, the focus on themes enables the analyst to draw a bigger picture. From iterative cycles of coding that

[5] For a critique of thematic analysis, questioning the representational status of themes, see Gomm (2004, p. 196).

were paralleled by my study of the literature from social epistemology and philosophy of science, two codes, "trust" and "epistemic dependence," emerged as broader themes in my analysis. These themes later became the basis for the discussions outlined in Chaps. 7, 8 and 9.

I understand the analysis that I have conducted as moderately theory-directed "editing" (Crabtree & Miller, 1992, p. 18), a form of qualitative analysis that is little formalized and has allowed me to flexibly follow various interpretive trails that connect empirical insights with philosophical concepts. Iterations of sequential, often theory-guided, coding helped me to mediate between philosophical interests and the existing abstract conceptual repertoire on the one hand, and the concrete empirical observations that I made in the fieldwork and interviews on the other hand.

3.5 A Short Note on Writing Empirical Philosophy

There is no philosophical genre yet which encompasses empirical studies in the social epistemology of science, and I could not resort to an established scheme into which I could have molded my writing. In working upon this book, I therefore have had to solve challenges concerning not just its content, its method and argument, but also concerning the narration of content.

To write as an empirically immersed philosopher means to be constantly caught between two spheres, between the abstract and the concrete, between abstract concepts and the locally observable, between the technical and the quotidian. It means to focus on empirically observable phenomena on the one hand, and to follow the trails of philosophical thought on the other, thereby exploring each of their subtleties and making visible how one bears upon the other.

Writing qualitative empirical research comes with a commitment to detailed observation, but also with the responsibility to guard the interests of the people studied. When presenting my empirical data, I have decided to refrain from describing research details that could compromise the anonymity of the scientists that have participated in the study. To this end, I have modified descriptions and interview quotations where necessary. Furthermore, all persons that are mentioned in the empirical material have been given pseudonyms. My choice to refer to the scientists studied by their first name reflects the rather informal workplace culture in Denmark.

When writing about empirical research for an audience of philosophers, you have to find a way to meet, if not entirely adopt, the expectations of style in philosophy. Social epistemology and philosophy of science are dominated by analytic approaches, and one of analytic philosophy's prime achievements is that its language is abstract and elegant, often slick and clean. Philosophy tends to speak with a rather authoritative voice. However, the gradual exploration of empirical phenomena with the help of qualitative methods, however, gives a more mixed, tentative picture that is gradually corroborated. Furthermore, qualitative empirical studies engage the investigator as a social, corporeal individual and as an instrument (Osbeck & Nersessian, 2015). Therefore, reflections on the role of the investigator and his or her embodied interactions with the field are essential. This reflective style is not easily reconciled with the impersonal, often bold, style of writing which is state of the art in philosophy of science and epistemology.

As the reader will notice, I have found a slightly different balance between philosophy's analytic style and the style of qualitative research in each chapter, placing emphasis sometimes more on the concrete, sometimes more on the abstract.

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4

The Planetary Science Group

The strength of our group is that we're experts in particular fields and all come together with our own expertise and we try not to step on each others' toes.

Adam, interview, group1

The first group that I have studied empirically through observation and interviewing is the planetary science group, which is a small but long-standing interdisciplinary collaboration among senior researchers and their graduate and post-graduate students. The group examines extra-terrestrial surface processes and combines expertise from geology, physics, chemistry and microbiology. Having searched for interdisciplinary collaborations at Danish universities online, I contacted the group in 2010 and soon began to observe their weekly meetings on Tuesday mornings. Later I shadowed individual group members, following them through their professional days. When I had familiarized myself with the group, I set out to interview the five scientists that formed the group's core at the time of my investigation.

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This chapter portrays the planetary science group and illustrates at the same time how I familiarized myself with it, providing examples of the empirical data I obtained. Section 4.1 describes the group meetings where my observations began. As it turned out, these are crucial for the group's functioning and continually interweave individual research interests into a collaborative experimental practice. Section 4.2 abstracts from my situated observations and describes the group more systematically. Finally, Sect. 4.3 draws on two of the interviews I conducted to provide a more nuanced portrayal of the group from the personal perspective of its group members. The interviews are not only deeply personal accounts of sentiment and experience—they also touch upon a range of issues that are crucial to discussions of scientific practice in social epistemology. The interviews, thus, provide a first glimpse of what it means to wrestle with epistemological problems, problems of epistemic dependence and trust in professional practice.

This chapter, as well as the following Chap. 5, provides the background against which the data that I refer to in empirically informed arguments in later parts of the book gain significance. With its main focus on qualitative empirical data and the way in which I have gathered them, these two chapters may be an unusual read for many philosophers. Regarding content, but also regarding tone and style, these chapters do not abide by the conventions of writing that dominate analytic philosophy of science and social epistemology. Once I have laid out the empirical foundations upon which my reasoning rests, however, later parts of the book return to a way of writing closer to the conventions of analytic philosophy.

4.1 Tuesday Morning Meetings

Not only did I start my empirical observations in weekly group meetings, it also became clear to me over time that the effective functioning of the planetary research group hinges upon these meetings. To provide a vivid impression of these meetings and how they proceed, but also to show the kind of empirical data that I gathered at the beginning of my field work, I will present fragments of edited field notes in this section. These edited notes are based on handwritten notes which I took while in the meeting and which I later rewrote so as to make them accessible to people other than myself.

Despite my editing, however, these field notes remain fragments, their nature reflecting the challenge that anyone faces who empirically observes ongoing research practice—the difficulty of understanding observed events as elements of a complex scientific undertaking, which unfolds on a trajectory reaching back to the past and extending into the future, which integrates past experimental results, setbacks, previously gained expertise and collective research traditions with visions of experimental results to be gained, visions of experimental success and future research. Given this difficulty, my field notes cannot attempt to provide an instructive, comprehensive description of what a research group is and does, and they are far from being consolidated enough to contribute to a philosophical account of scientific group collaboration. These notes are no more than the humble beginnings of empirical work.

What you read in the following are edited and shortened field notes from one particular group meeting at the beginning of 2011. As in all meetings in the planetary science group, this particular meeting was organized and chaired by Laurits, the group's geologist and spokesperson. Apart from Laurits, three graduate students, the group's technician and three senior scientists, among them two physicists and one chemist, were present at this meeting:

> Shortly before nine Laurits opens the door to a small meeting room. He leaves his keys in the lock and takes a seat. Laurits always takes the seat right next to the door; he is in fact the only person that always sits in the same chair. He has brought with him a pile of documents, which he spreads out in front of him. [...]
>
> Shortly after nine two senior physicists, Adam and Rasmus, the group's technician and three doctoral students arrive. One of them, Lucas, is attending the meeting for the first time. He has just started his four-year dissertation project in microbiology. His supervisor, Victor, cannot attend the meeting, because he has conflicting teaching duties. Another of his PhD students, however, is present today and she now introduces the new PhD to the group. They all shake hands. Christoffer, the group's chemist, arrives. Since there is no seat at the table left, he chooses an armchair in the corner of the room.

The first point on Laurits's agenda deals with a "shaking device" that he wants to have installed within a bigger instrument. Nikolaj, the technician, has apparently contacted a number of companies to find out where they could buy such a device and how much they would have to spend on it. Shaking tables that move horizontally are commercially available. Nikolaj would have to modify these so that vertical moves are also possible. Laurits thinks that such a modification would be a good solution, whereas Adam disagrees. He questions the resilience of horizontally shaking devices with regard to vertical movements. Adam is in charge of the instrument and his opinion is crucial. Therefore, they do not settle the issue for the time being. Nikolaj is asked to develop a different idea for building a shaking device in cooperation with the companies from whom they might purchase the device. On this occasion, Laurits also asks Nikolaj about the price of a cooling plate that they have agreed to order. […]

Laurits asks Christoffer about the results of an experiment that he has finished recently. They aren't good, but Rasmus insists that some time ago they produced reasonable results with a very similar experiment: "It was little, but significant." Christoffer agrees, Adam doesn't. They try to remember the exact details of the experimental set-up they used in the earlier experiments. "Anyway," says Adam, "we could put it on the list." By suggesting to 'put it on the list,' Adam proposes to repeat the original experiment. They all agree that it would be worthwhile to perform this kind of experiment in a number of variations, again at a later date. And Rasmus mentions in this context that he just ordered some more experimental materials for another experiment that they planned to carry out soon as well.

During the remaining time they talk about a potpourri of minor issues that are mostly brought up by Laurits. An application has to be finished. Why not include a colleague from another department with whom they used to cooperate? Instruments have to be built; tests have to be run. How are the lab students doing? The Xrd-detector in the chemistry department has been seriously damaged during the rebuilding and will thus not be available for some time. They can, however, use an instrument in another department. Their homepage has to be revised and a PhD defense has to be organized. Maybe one of the censors could deliver a talk? "And," Laurits checks with a quick look at today's agenda what still needs to be discussed, "there is this project meeting in Norway. Someone should go there." "Yeah," Adam replies, "someone should go there. I've looked into the program.

> I think Victor should go there. He is the biologist." Laurits has printed out the workshop program and hands it around. Laura has a quick look at it: "Oh, yes, I know these people. It's really biology." After studying the printout Christoffer says: "But Adam, you are on the program." "Yeah, but if Victor goes I wouldn't go. And I think it would be better if he went." "So," Laurits resumes, "we should ask him about that. I will write him an email." After a while he adds: "That's it for today."
>
> When the meeting comes to an official close, people have a casual chat. The new PhD student, Lucas, is the center of the group's attention. He is asked to explain his project. Laurits wants to know how he is financed and where he graduated. When the conversation is about to fizzle out, Lucas turns to Adam: "I guess you are the guy that is responsible for the big instruments." "Yeah, if you have time we can go down and I'll show you around." "Oh yes, sure. I have time now."

When Lucas and Adam descend to the basement where the group's large simulation facility is installed, I join them. I know that in the meantime Laurits will return to his office and write up today's minutes. At the bottom, he will list open to-dos, on which he will follow up at the beginning of next week's meeting.

4.2 Group Characterization

The planetary science group can be characterized as an informal interdisciplinary collaboration, driven by the good work relations that a handful of senior scientists have established with one another over many years. All group members study abiotic and/or biological processes on extraterrestrial surfaces, and all of them have an interest in combining the methods, concepts, literature and theoretical backgrounds from geology, physics, chemistry and microbiology. Group members perceive their collaboration as a success, and the group's research is published in high-ranking international journals.

Since the group was founded about 10 years ago, there have been but few changes in the group's core, that is, among those who regularly participate in weekly group meetings. At the time of my observations, the group consisted of five senior scientists and their students, doctoral fellows and post-docs who are working on a topic closely related to research on planetary surfaces. Usually, there are about two junior scientists involved in the group's activities. Of the five senior scientists two, Adam and Rasmus, are physicists. The three others come from microbiology (Victor), chemistry (Christoffer) and geology (Laurits). All of them are hired on tenure by their respective disciplinary department and have departmental teaching duties. All of them also have their own laboratory. In administrative terms, they are not bound to the planetary science group. And, in fact, for most of them the research that they conduct in collaboration with other members of the group is just one, if an important, aspect of their scientific work.

When the group's core gathers during weekly meetings, group members ask each other about the ongoing research and recent results, adapting the length and level of detail according to the listener's interest and his or her understanding of the topic at hand. Often, a topic would not be of interest to all participants. Nevertheless, group members usually do not conduct parallel conversations until the very end of the meeting. The regular face-to-face contact that weekly meetings facilitate is important because the coordination of the group's research does not follow any formal rules or plans in any narrow sense. Rather, "it is one question leading to another" as Laurits puts it (*Laurits, interview, group1*), and the group's mode of collaboration has to be understood as an open-ended deliberative process for whose continuous and iterative character the group's weekly meetings are vital.

The group's core is embedded in a wider network of collaborators most of whom are located at different universities. The wider network surrounding the group is amorphous and has unclear boundaries. For example, the minutes of weekly meetings are emailed to about 40 researchers at other universities in the country and abroad. The group also has cooperative relationships with other research groups in their field. And even more importantly, each senior core group member has accumulated his own personal contacts during his professional career. These contacts can be, and in fact are, drawn upon for research group matters, although they may be related to research interests outside the group's focus.

The planetary science group is informal insofar as it does not constitute an administrative unit within the university structure. It exists alongside university hierarchies but is not part of them. The university administration acknowledges the planetary science group as an independent group and allows it to keep an independent budget for research-related expenses. The group also disposes of two dedicated laboratories that complement the laboratory facilities to which each senior group member has access through his respective department.

The smaller of the two laboratories consists of two adjacent rooms opposite Laurits's office where abiotic material samples are stored and smaller instruments installed, among which are a high-temperature oven and a self-constructed device called the "tumbler." Below a casting chipboard, the device consists of an old washing machine motor that spins a wheel onto which shoe boxes can be mounted. Filled with sand samples or similar materials, the tumbling shoe boxes simulate surface erosion processes. Additionally, the group disposes of a much larger laboratory space in the basement below the physics department.

In addition, the group has a large laboratory space in the basement below the physics department where their central experimental facilities, simulation tunnels, are installed. The simulation tunnels are large submarine-like high-tech containers inside which planetary atmospheres—their temperature, their chemical composition, their winds—can be simulated. The tunnels have been designed and constructed by the group. At the time of my study, Laura, a post-doc and former doctoral student of the group, spends her time calibrating the newer, larger simulation tunnel and is learning how to handle simulation experiments under the supervision of Adam, one of the group's physicists, who is responsible for their maintenance and operation in collaboration with Nikolaj, a crucially involved technician who works part-time for the group.

The group has a decentralized structure, cultivating an egalitarian group ethos and allowing group members to collaborate in changing coalitions *inter pares*. There is neither a discernible hierarchy nor scientific competition among the senior core members of the group, as each of them has their own specific field of expertise. Accordingly, the group functions without a leader in any conventional sense and has a spokesperson instead, a role which Laurits has been fulfilling during recent years.

Laurits represents the groups—both internally, with respect to university matters, and externally, with respect to outside contacts that are not mediated through personal acquaintance. In addition to scheduling and chairing their weekly group meetings, Laurits manages the group's research budget, coordinates joint funding applications and conference attendance. His administrative tasks leave him with less time for research. Therefore, in contrast to other group members, he is forced to dedicate his research time almost exclusively to planetary surface matters. To be closer to other group members and the group's large basement laboratory, Laurits has long given up his office in the geology department and taken office space in the physics department. But although he carries out a number of administrative functions and is crucial to the group's internal communication, he does not claim leadership with respect to research. He has neither overall responsibility nor overall control or detailed oversight.

Lastly, to conclude my characterization of the planetary science group here, let me mention that there are varying accounts of the group's beginnings. Both Laurits and Adam, one of the group's physicists, have been involved in the planetary science group since it came into being about 10 years ago—and both of them describe differently its beginnings and the reasons for why it was formed in the first place. In my interview with him, Laurits foregrounds the role of his personal, geological interest in the research that the group carries out on extraterrestrial surface processes. He describes how he happened to discover a soil, a material that can be found in a place that is about a two-hour's drive from the university. As it turned out, this soil has unusual qualities and can be employed as an analog for the kind of soils that, according to hypothesis, cover some planetary surfaces.

Adam, in contrast, attributes the group's foundation to a side interest that a former physicist at the university had in the transport of small particles by wind. To pursue this interest, this physicist constructed a wind tunnel, a predecessor of the simulation tunnels that the group was operating at the time of my observation. According to Adam, the experimental potential of this first wind tunnel attracted the attention of colleagues from different disciplines. Successful publications and funding applications were written, and soon the cluster of collaborative activities that emerged around the wind tunnel morphed into a research group. Since the wind tunnel facility and the experiments conducted with it became more and more complex, Adam joined the group to take responsibility for the day-to-day operation and the technical development of the facility.

What the divergence in accounts of the group's foundation shows is how different group members approach the group and their interdisciplinary collaboration with other group members from the point of view of their personal, disciplinary research interests. Despite this difference in perspective, however, the planetary science group succeeds time and again in interweaving individual research interests in a shared experimental practice.

4.3 Individual Interviews

I conducted six individual interviews with members of the planetary science group that were regularly present at group meetings during my time in the field. To give a comprehensive impression of these interviews, I present two of them in greater detail, offering a selective overview that is organized to show my interview material in as much breadth and variety as possible while highlighting aspects relating to my philosophical interest. (Emphasis in interview quotes is always added by me.) What these interviews convey are individual perspectives, offering the personal experiences and opinions of practicing scientists. I will indicate which reactions and thoughts their utterances have triggered in me and which foreshadow the themes of epistemological significance that I will elaborate upon in later chapters.

4.3.1 Adam: 'I could work alone'

The conversation I had with Adam, a physicist, was one of my longest interviews in the group. In contrast to the other interviewees, he is a native English speaker and the interview quickly unfolded in a calm, but steady, flow.

In Adam's view, the simulation facility in the basement below the physics department is crucial to the group and its research. Adam has contributed substantially to the design and construction of their two latest simulation tunnels. As it is his job to run this facility and assist others in conducting simulation experiments, he is co-authoring a large number of journal articles, contributing information about the experimental set-up and adding to the interpretation of experimental data and the conclusion of several papers. For this reason, he describes himself in the interview as a "focal point" for other group members, a contact person for questions regarding their simulation method. His responsibility for a complex experimental facility means that his role could be perceived as mere technical support. Therefore, he goes to some length to emphasize the fact that he pursues his "own research":

> Originally I was I guess employed to run the [simulation facility] which is – could in principle just be like a technical position where you run a facility which is why I for many years had a technical type of employment. However, I do my own research. I always have, from the start I always did my own experiments. So, I have like at least two roles.

It is important for Adam to underline his intellectual autonomy as a researcher with a particular field of expertise. In this context, he emphasizes that he, like other core group members, pursues his individual research interests within the group *and* "on the side," collaborating with international colleagues outside the group. In that regard, he explains, the planetary science group is "a kind of unusual collaboration," as group members "[s]ometimes […] do experiments essentially alone, and sometimes it becomes very collaborative."

The group members' collaborative efforts, as Adam sees it, are not driven by shared research interests: "[B]asically we have different interests in [planets]. Like - life on [planets], that's one question, but that isn't the central issue that I deal with. A lot of what I think that I do is – I would call comparative planetology." Adam goes even so far as to say that he pursues his individual research interest "essentially on his own" and "together with students." His research interest as such, he explains, does not tie him to the group. “But it ties me to the [simulation facility],” he adds. Asked whether he could, in the long run, do his research without the planetary science group, he replies:

> I could work alone down in the [simulation facility] in the lab doing my physics things and still collaborate internationally with people. That would work, but I think we would miss out on a lot of, we would miss out on a lot of research and there are a lot of new avenues that have started up purely because we are a bunch of experts all talking together.

Like Laurits, Adam underlines the important role that regular meetings play and how they provide a forum for discussing recent findings, deciding on the next experimental steps and agreeing on the outline of the argument which is to constitute their next jointly authored paper. These meetings also provide an opportunity to discuss different viewpoints so that group members understand each other’s distinct disciplinary perspectives and can explain to one another how they would interpret the measurements that they have obtained. The back and forth between explaining and understanding is an interactive process that has the function of bringing “[…] all up to speed, [so that] we’re all sort of knowing the same sort of thing.” Such a common understanding forms the basis of collaborative experimentation.

To underline the role of such a common understanding, let me refer to an episode in the interview where Adam describes an incident of severe disagreement with a collaborator outside the group’s core. While it is usually not a problem to agree on a jointly authored paper, a couple of years ago members of the planetary science group were in disagreement with an outside collaborator, who refused to accept the conclusion which the group had reached on the basis of various measurements, some of which he had taken. As a consequence, he asked to “take his name off the paper” and the remaining authors had to find a colleague with a matching field of expertise who could step in. “[I]t’s sad when that happens,” Adam tells me, “but it’s kind of rare. It doesn’t happen a lot—not to us anyway. And it doesn’t happen in-house.” This is because “[…] if we disagree with the conclusions of it, then we would – we do some more measurements until we, we’re sure.”

The group offers its members the opportunity to engage collaboratively in an "ongoing research line," as Adam formulates it. There is a continuous discourse among group members and a continuous trail of collaborative experimentation, even though not all core group members are necessarily involved at one point in time. The biologists, Adam points out, have a different experimental rhythm, one of the reasons being that it takes them much longer than anyone else to prepare their organic samples. Still, their frequent and regular face-to-face contact allows them to "[…] perform ongoing research in a way that you can't do with [an outside] long range collaboration." With outside collaborators, Adam usually works on "well-formulated projects" that are likely to carry a short-time reward in the form of a joint publication. Inside the group, "[w]e could just try something a little bit crazy and see what happens." Like Laurits, Adam describes the collaborative practice within their group as a process in which one question leads to another: "[W]e think this, but we're not sure, I mean it could be this or this and how do we show which one of these it is? And then you start talking about experiments."

4.3.2 Laura: 'You have to be a knowledge base on your own'

At the time of my interview, Laura is in a transition phase. She has just obtained her PhD in physics for a dissertation on planetary science that was supervised by Adam and Rasmus, and now she is about to be hired as a post-doctoral researcher. Laura is trained to operate the facility on her own, but it will take at least another year until she will work without Adam's immediate help. Her reflexive awareness impresses me and I am delighted by the clear and determined formulations she finds for her experiences in an interdisciplinary research environment.

Laura emphasizes that the planetary science group has been very valuable for her. The group offers "a center of focus for [your] studies" and is a reliable source of both financial and administrative support as well as expert advice from scholars in related fields: "so, I know that I from these people can get different views on the same things and I get a lot of help, at least that's what I've used all of it for." Help from scholars with a different area of expertise is essential, she underlines, because planetary science as a research field necessarily involves a scientific perspective that spans across the many narrow specialized niches covered by individual scientists. Therefore, two particular kinds of knowledge are to be acquired in interdisciplinary research. On the one hand, Laura, contrary to Laurits, points out that group members learn from each other about each other's fields, particularly with respect to aspects specifically relevant to particular experiments such as, for example, the choice of materials and instruments. This knowledge is highly context dependent. On the other hand, group members learn "what [they] can use each other for," that is, to gain an understanding about "the field of expertise [they] each have."

Laura has been involved in interdisciplinary science not only at the planetary science group, but also as a participant in a larger international research project. Planetary science is an interdisciplinary field and characterized, as she tells me, by "[…] the fact that the areas of interest are very separate." Although they "overlap" in their common interest in planetary surfaces as an empirical phenomenon, scholars in the field and members of the planetary science group in particular "[…] don't do the same things, we haven't been trained to do the same things, we haven't been taught to think in the same ways and that can actually be quite a hindrance sometimes." As an example, she tells me about the difficulties faced by scientists from the international research project in which she participated when they talked about atmospheric particles:

> […] One of the things I found problematic, for example in the [international project], we had of course both geologists, physicists, atmospheric chemists, just talking about for example particles in the atmosphere, getting, just getting around the fact of what, what do we call the different particles. Are they sand? Are they dust? Is it silicates? I think we spent three months coming up with a definition of what size particles are what, because it turned out that everybody had their own system of size determination of particles. […] So sands and grains and dust was defined in three or four different ways, just within the about 50 or 100 scientists that were on [the international project].

Interestingly, Laura has made these observations not within the planetary science group but in an international project whose participants had not worked closely together before. This, along with other remarks that she made, suggests that the planetary science group has successfully gone through a phase of interdisciplinary familiarizing. All group members have developed a common "general understanding" of planetary surfaces and this understanding must have been established, Laura tells me, before she joined the group more than four years ago. Yet, when I ask her whether the group shares not only a common understanding but also a joint vision, it is one of the few moments where she hesitates and then answers that "[…] there is definitely a shared vision of wanting to understand [planets]," an "overall sense of direction," but that she cannot specify this sense any further:

> […] every person in the [planets] group probably has their own, will have their own interest in [planets] in some respect—ahm, whether you can say that there is a more narrow perspective for the […] group combined, I don't, off the top of my head, I don't really know.

To explain her experience of group collaboration, Laura repeatedly refers to a particular term—"knowledge base"—which has its roots in information technology. There, a knowledge base designates a repository which provides a means for the accumulation of information on a particular topic such as an operating system. Typically, online knowledge bases contain frequently asked questions (FAQs) and tutorials. Users consult a knowledge base as a manual that, unlike ordinary instructions, is tailored to the problems and issues they are likely to encounter. Thus, when Laura brings up the term "knowledge base" she implies a relational understanding of knowledge possession and expertise, emphasizing that in group collaboration knowledge is called upon dialogically—those who possess knowledge provide in reaction to the questions and the knowledge needs that their collaborators express.

Interestingly, Laura refers to the term "knowledge base" both in relation to research groups and in relation to individual scientists. When she, for example, compares the planetary science group with another, larger research group in the field, she explains that "[…] the more people you have, the better opportunity you have of having a group that has a wider knowledge base." At a different point in the interview, however, she also mentions that the core employees of a group, as in fact *any* senior scientist, can be described as a knowledge base in themselves. As she explains to me, as a post-doctoral researcher, you are "[…] seen as a knowledge base in yourself," that is, "[y]ou have your own area, start developing the area where you become the expert." As she is becoming a post-doc herself, she sees herself confronted with the challenge of developing and consolidating the kind of expertise she has to have autonomously— in herself —in order to respond to the knowledge needs of her interdisciplinary collaborators.

To conclude this chapter, let me point out that the concerns for professional autonomy in the interviews I have led with Laura and Adam are present in the interviews with other members of the planetary science group as well. Like other group members, Laura and Adam seek to build and retain a distinct disciplinary identity alongside interdisciplinary collaboration, thereby cultivating a sense of autonomy in the midst of thoroughgoing collaborative dependence. The next chapter begins to elucidate how these concerns play out in a molecular biology laboratory, a mono-disciplinary research group.

5

The Molecular Biology Laboratory

They all came to my lab, because they sort of respect what we do and what we can do for them.

Johan, group leader, interview, group2

Drawing on professional contacts that my supervisor happened to have, I began extending my fieldwork to a second research group—a research laboratory in the field of molecular biology that differs from the planetary science group in its mono-disciplinary orientation, its hierarchical structure and its size. The molecular biology laboratory is led by Johan, an internationally accomplished expert in the field; and it comprises about 35 people, most of whom are graduate students and post-graduate researchers who have spent only a couple of years in the group, collaborating with fellow group members, broadening their knowledge of experimental techniques and trying to make a name for themselves with high-ranking publications. Many of them see the laboratory, and the research collaboration it facilitates, as a springboard to an academic career.

S. Wagenknecht, *A Social Epistemology of Research Groups*,
New Directions in the Philosophy of Science,
DOI 10.1057/978-1-137-52410-2_5

To introduce the molecular biology laboratory, Sect. 5.1 describes the group's weekly meetings where my empirical investigations, again, began. Section 5.2 provides a comprehensive characterization of the molecular biology laboratory and the collaborative scientific practice it facilitates. Section 5.3, finally, presents two of the interviews I have conducted with group members to offer a more nuanced view of the group from individual perspectives.

5.1 Wednesday Mornings

The group as a whole gathers each Wednesday morning for the so-called "lab meeting." As with the planetary science group, I started my empirical observations by attending these weekly meetings. Witnessing these meetings on a regular basis, I quickly became aware that they follow a rather rigid structure.

The first part of their weekly meetings deals with organizational and technical information. Johan, the group leader, brings up important funding and research deadlines, announcing conferences, workshops, lectures and relaying news on administrative matters. Next, he would turn to "lab matters" and have technicians inform everyone about the purchase of supplies and equipment, and remind them about security guidelines and cleaning duties. Thereafter, group members, usually post-docs, report instrument breakdowns or software problems. The second part of their meetings is dedicated to the discussion of experimental problems and research results. When I started observing their meetings, Johan had just introduced a new routine. Given that he has less and less time for individual supervision, for each meeting Johan now schedules three or four of his students and post-docs to present their current work with a short slide show, reporting on preliminary findings and their plans for the next couple of weeks.

The following field notes were taken during one such lab meeting, the second meeting after the summer break during which many lab members were absent.

The meeting room is slowly filling, around 20 people present, less than usual during semester times. People appreciate the opportunity to chat before the meeting starts. Some of them haven't seen each other for a while, because they were on holiday or have been visiting other laboratories. I overhear a PhD student asking another: "Hey, what about the results you had? I'd like to know."

Shortly after nine o'clock, Johan appears, takes his seat, welcomes everybody, and without much ado begins talking about a funding proposal he is busy writing and for which he will need input from various group members during the coming days. Then, in his rather abrupt manner, he announces "lab matters" and quickly walks through a range of topics. He mentions two new PhD students who will join their lab and a visitor who will arrive in late autumn. Then he talks about their plan to move offices and lab space to an adjacent building. The moving date is approaching. Johan says it is essential that they "keep the coziness we have here, but to be at the same time closer to the rest of the department." He goes on to talk about a planned outdoor trip, the yearly lab excursion in early October. He takes a look at the handwritten notes he has jotted on the sheet of paper lying in front of him on the table. The next point on his agenda concerns their lab equipment. There is a problem with repairing an important instrument. Repairing this broken instrument "has been a nightmare." Other instruments that could serve as a substitute have been moved around the lab building in the course of the ongoing reconstruction process, so they are difficult to access. "In urgent cases we can use the one in biophysics," Johan says, "but currently we're using that one more than they are." Then they talk about other instruments, and discuss which ones they might want to purchase in the near future.

When it is time for individual research presentations, it turns out that it is unclear whose turn it is to present today. Since no one has prepared a slide show, and no one is volunteering to improvise ad hoc, Johan turns to a newly started post-doctoral researcher. It's only her first week in the laboratory, and she reports that she has started working on a protein together with a PhD student, trying to purify the protein and separate it from other compounds. Unfortunately, the concentration of a substance in a dilution they use for purification was too high, "so that we couldn't observe any activity." Johan comments on this encouragingly: "We should definitely have an eye on this." Then he begins questioning a student who has just returned from a lab at an American university. It turns out, the student reports, that specific experiments that they have been trying to conduct in Johan's lab earlier this year (alas, unsuccessfully) had "worked over there." They shortly discuss possible differences in the experimental set-up. Johan closes this episode with an optimistic remark, then asks: "Any other research summaries?" No. But suddenly a spontaneous conversation among four people in the room arises, involving the discussion of recent positive results that one of them has been able to obtain in purifying a protein. As usual, I observe, their discussion is a back and forth between a person whose experimental results are discussed and her commentators whose questions typically follow a distinctive pattern, trying to establish whether particular experimental options have been tried and with what success.

Finally Johan, as always, asks: "Anything else?" They quickly talk about planned trips to synchrotrons, experimental facilities in which the structure of purified proteins can be tested. The booking of autumn time slots for a beamline in a particular synchrotron is opening soon. Who is going there in person? Who else should go, and whose samples can they take with them? Then the meeting is over, and a half a dozen students are waiting to get hold of Johan.

These field notes, as cursory as they are, are a first illustration of the character of the molecular biology laboratory as a research group, its hierarchical structure, the trial-and-error character of its research, research that is highly specialized and focused on a handful of experimental techniques.

5.2 Group Characterization

The molecular biology laboratory is a mono-disciplinary research group, integrated into university structures as part of a section dedicated to structural biology in the department of molecular biology. The molecular biology laboratory comprises about 35 people whose work is dedicated to revealing the structure of a set of proteins which perform specific functions in bacterial, animal or human cells. The research that the laboratory carries out is laborious and resource-intensive, and it comes with a high risk of experimental failure and progresses through trial and error—challenges that the group tackles with concerted collaborative effort, grounded in a clear hierarchy among its members. In this respect, the laboratory resonates well with existing research on collaborative scientific practice in the field of molecular biology and the life sciences more generally, strands of research I will refer to below.

The group is organized hierarchically, which mirrors the levels of professional seniority that are present within the laboratory. Holding a full professorship, Johan presides over the lab. One tenured associate professor, three senior researchers with non-tenured positions and six to nine post-doctoral fellows constitute a medium level of hierarchy. The laboratory also comprises about 20 graduate students and undergraduate students. At doctoral student level and below, the laboratory features an equal gender balance, but fewer women are in evidence at the post-doctoral level and above. Three technicians take care of the laboratory and another three employees assist Johan in administrative tasks such as budgeting and internal communication. Johan also functions as managing director of a research center that comprises his own group as well as other research groups.

Given the importance of group leadership, the molecular biology laboratory is referred to by its members as "Johan's group" or "Johan's lab." As the group leader, Johan sets the scientific agenda. He formulates the research directions for his lab for the next couple of years and oversees the organization of financial, material, social and "human" resources in the lab. He acquires funding and hires people; he schedules and chairs group meetings. The purchase of new laboratory infrastructure and the use of experimental techniques have to be approved by him. He coordinates the group's research activity, publishing strategy and dissemination activity. Post-docs are, within a certain framework, free to choose their research subject. PhD students, however, pick a predefined project or are assigned one. Moreover, Johan is a gatekeeper for the group. Managing outside contacts, he ultimately decides with whom in the field his laboratory is competing and with whom it is collaborating.

Since much of Johan's time is accounted for by research management and administrative tasks, he has been forced to set limits to his "open door" policy. During the week that I interviewed him he announced at the lab meeting that if group members needed to speak with him it would be best if they only came to his office in the afternoons, and that if they felt they needed a longer conversation, they should book time via the online calendar system.

Johan himself does not perform laboratory work anymore. Over the last decade, his laboratory has grown constantly and he has gradually withdrawn himself from the work bench. With respect to the day-to-day supervision of undergraduates and graduate students in the laboratory, he delegates substantial parts of his supervisory role to the more experienced post-docs and senior researchers in his group, who are overseeing groups of three to four graduate students and undergraduates. These subgroups collaborate closely on a cluster of related, that is, structurally similar, proteins. Each subgroup holds a meeting once a week, often with Johan present, to take stock of their experimental progress and coordinate their efforts for the coming days.

As a group leader, Johan likes to emphasize the common theme around which all members of the laboratory work, namely the structural determination of a family of complex proteins that perform specific cell functions. Yet, there is no one shared research object. Typically, every group member pursues his or her distinct, individual project, which usually consists of an attempt to determine the molecular structure of a particular protein by crystallographic methods. From the molecular structure, then, conclusions may be drawn concerning the protein's molecular functions.

In general, all proteins are subjected to the same biochemical procedures. First, cells containing the protein are selected or manufactured and vast cell colonies are grown. Then, in various steps, the protein is isolated. This isolation process is called "purification." When a sufficiently large and pure sample of the protein is obtained, it is subjected to conditions that ideally induce its crystallization. In crystallized form, the protein can be analyzed with the help of laser beams in a high tech facility called a synchrotron. Some group members travel to synchrotrons all over Europe, up to three or four times a year. In synchrotrons, laser light will be directed at the crystal. When hitting the crystal, the light is scattered in different directions. If one can detect patterns in the way the crystal diffracts the laser light, one can model the structure of the protein.

Unfortunately, although all the proteins studied in Johan's lab belong to one family and thus resemble each other in both their structure and function, different proteins are very particular in their biochemical features. The dilution that works for one protein may or may not work for another. Thus, the purification and crystallization of proteins is to a large extent a time-consuming (and often frustrating) process of trial and error. Knorr-Cetina hence describes the dominant experimental approach in molecular biology as "blind variation" (Knorr-Cetina, 1999, p. 79ff.). Every minuscule step in the purification of a protein can be performed with a number of different chemicals, at different temperatures, at different times and with varying speed and length. A thorough literature review can give ideas, but will hardly provide the researcher with a successful "recipe" right away. Personal experience and informal access to the practical experience of other group members is vital. The questions are: What would be worth trying? What worked for others and is likely to work in the case at hand? The scientific insights generated by research in this area must, to a large degree, be described as knowledge concerning the technical manipulation of biological objects such as cells, cell parts and single molecular structures (Leonelli, 2009).

All scientists in the laboratory have an education in molecular biology or pursue a degree in this field. This implies that most group members have either roughly the same experimental skills and expertise or seek to acquire them. Yet, the fact that the molecular biology laboratory is a mono-disciplinary team should not veil the diversity of academic biographies. All scientists working in this group can be regarded as molecular biologists, but there is variation in expertise. Some PhD students have a background in programs other than molecular biology, such as medicinal chemistry or even theoretical chemistry, and some of them have chosen to take courses in neighboring fields such as bioinformatics. The variation in expertise continues at higher levels of seniority, although differences in individual specialization tend to be more subtle here. Post-doctoral researchers may, for example, specialize in proteins with a particular molecular function that have to be cultivated in particular cell environments.

It is essential for members of the laboratory to learn from one another, acquiring tacit knowledge while working side by side at the bench in the laboratory space they share. More than half of the group members are students and PhD students who, under the immediate supervision of a post-doctoral fellow, carry out large fractions of the experimental labor. Not only students but all lab members have a permanent interest in acquiring and improving the skill to master new experimental techniques. Technological innovation in the field of molecular biology moves fast, so that experienced as well as young researchers face the challenge of keeping up with the state of the art in experimental techniques. Students start by learning generic experimental routines; later on they proceed to complex, highly technology-vested procedures.

Only through learning-by-doing in the laboratory can junior scientists achieve the professional autonomy of an experienced researcher. To achieve and maintain this autonomy is the learning goal of all group members. Yet, laboratory practice is necessarily collaborative as it is practically impossible for any one person to handle all experimental procedures required in the pursuit of a specific project alone. These procedures are extremely time-consuming and knowledge-intensive. For the laboratory to run smoothly, group members need to help each other with the planning, set-up and monitoring of their experiments, thereby offering advice and counsel to one another.

But while scientific practice in the laboratory is highly collaborative, it also features competition, both among group members as well as between the group as a whole and other research groups elsewhere. First, it is not unusual for several groups in one research community to work on the same protein independently. The crystallographic determination of protein structures is often perceived as a "race" in which scientists compete with one another. While the discovery of a new protein structure allows for a publication in the highest ranking journals, the confirmation of a discovered structure receives substantially less attention. Therefore, information about ongoing research given on university websites is often outdated or intentionally vague. Research meetings are confidential and unpublished information or material samples are only exchanged within the laboratory or with collaborating groups whose research is complementary, not competing.[1]

[1]For an empirical study on the balance between collaboration and competition in the Danish life sciences see Poulsen (2001).

Second, there is a palpable element of competitiveness in relations among fellow group members, particularly among junior scientists. The ratio of junior and senior researchers suggests clearly that only some, not all, junior scientists will be able to stay within academia. Consequently, PhD students try to distinguish themselves with high-ranking publications. Journal articles have a high chance of being published in a high-ranking journal if they report a hitherto undiscovered protein structure, a finding that is usually the result of many months, if not years of research. Since graduate funding is limited to a fixed period of time—either three, four or five years, depending on previous qualification—junior careers are a risky business.

In his study on research groups within the life sciences, Edward Hackett has singled out the management of epistemic risks as a particular challenge for groups and group leaders. The pursuit of "risky" research projects is associated with reputation and fame. Investments in promising, but highly uncertain, research tracks are appreciated. Therefore, "[…] it is risky not to take risks" (Hackett, 2005, p. 805). Research groups can be a way to address the risky character of experimental research. Collaborative research practice allows group members to engage in more than one research endeavor at once, thereby balancing the risk of failure.

Given the highly collaborative, yet competitive, character of scientific practice in the life sciences, sociologist Ulrike Felt and collaborators observe "the tensions emerging from the requests of being simultaneously individually excellent and part of a collective" (Felt, Sigl, & Wöhrer, 2010, p. 4). Their study shows how young scientists are "carving out a 'self"' from the collaborative relations they engage in when they create a professional profile (Felt et al., 2010, p. 17). From a sociological perspective, thus, Felt and collaborators raise issues that reach into the core of my philosophical interest, namely issues of dependence and autonomy, and they argue that the collaborative character of life science research poses a challenge for scientists who need to establish independent careers—an argument that, as we will see, underlies my interviews with members of the molecular biology laboratory.

5.3 Interview Voices

In the following, I present two of the four interviews that I have conducted with members of the molecular biology laboratory. As in the previous chapter, this presentation is intended to provide a selective overview of my interview data—and to convey, exemplarily, the individual perspectives that different group members have developed on group collaboration.

5.3.1 Johan: 'I'm the memory'

Johan has been group leader for many years, a period during which he has accomplished significant scientific success and during which his lab has significantly grown in terms of lab members, research volume and in terms of the breadth of experimental techniques that lab members apply in their research. Given this development, Johan describes himself as retaining his "own sort of deep expert knowledge" within crystallography, a set of experimental techniques that has remained at the center of the lab's research activity over the years; yet he acknowledges that the members of his lab "[…] do a lot that I'm sort of less expert knowledgeable about, but where for example the technicians, and most of the post-docs also, are very knowledgeable."

When asked about his leadership and the kind of work relations he entertains with group members, he describes his role as group leader in terms of "mentorship," trying "to convince them of the good ideas" and "discourage them from what you see as a bad idea." When I want to know more about "bad" ideas, he explains:

> Yeah, [a bad idea] could be something where cost–benefit is really not good, where you end up spending a lot of time and effort on something that in the end doesn't answer the question you want. Or starting a project that isn't sort of ripe, I mean in the sense that we end up spending a lot of time on things we are not experts on. […] These are the kind of discussions as a group leader one needs to take. Also, in terms of for example which kind of equipment we use, and will invest in, which kind of new methods we use. I think it's important from time to time that we try and do new things.

> […] but we can't change our lab too much all the time. It has to have a balance, like, between conservative stability and innovation.

To strategically balance conservative against innovative research decisions is what Johan sees as a crucial challenge, an element of his leadership that he elaborates upon at length:

> […] so once a new idea or opportunity pops up, yeah well then one would think well who can actually incorporate that. Either as a new project or into [an existing] project, and then talk to these, go down the hallway, or set up a small meeting with three people. Should we do this […] and normally they are interested. Other times, if no one really takes the ball and says well that's not really—sometimes, one needs to be a little more insisting. Because I mean everyone wants to sort of get along and produce something but sometimes you need to invest also in trying something new, that's an important role where sometimes one needs to be a bit more sort of an authority, saying: we gotta do this, right. So, that, finding that balance is important I think. At the same time being an authority, at the same time allowing the independence.

In weighing the authority that comes with seniority against individual independence, Johan seeks to create a participatory "lab culture" that acknowledges the collaborative efforts that the lab undertakes as a group—it is good for group members to have "individual goals," but, he underlines, "[…] the important thing is to let them understand that they can reach even higher goals by working as a team."

A recurring motif in the interview with Johan is his emphasis on caring for the students in his lab: "[…] younger researchers have, often have bright new ideas, but they can also actually be quite conservative in the sense that they, they want to be sure that things work." Therefore, Johan sees it as his task to help particularly junior group members manage risks of experimental failure. High-risk projects are important, but "[…] if it doesn't work, you get absolutely nothing, […] then we have some back-up projects and we make sure that people in the end leave with something" and acquire an acceptable record of publication despite all experimental odds. Group members should not "[…] feel that they worked all this way, and then they are just being screwed in the end." Throughout the interview, Johan is eager to convey that he regards his laboratory as an "educational institution":

> I see it as a very important thing that what I do is to set out people for careers in the future. Not only should I publish good papers, but we should also publish really—no, no, sorry [laughing] produce or send out really good people. And you get to realize also among the more senior scientists, they actually also rate each other like that: are they good people, those people coming from those labs?

Since many people pass through the laboratory, coming from and moving on to other research institutions, Johan tells me, "[…] you need as a group leader to really have a good eye on to be sure that procedures and the science is really maintaining its high level." If done right, "[…] you have this fluent mass of people with a lot of expertise that all the time maintains in the lab." Given the constant arrival of new and the departure of old group members, Johan is, apart from lab technicians and administrative staff, "[…] the only one really continuing all the time – I'm the memory, so to say, of the laboratory." Yet overseeing the group's research has become challenging because of its growth, a development that forces Johan to delegate to others his day-to-day supervision and hands-on laboratory work. In order to manage a laboratory of this size, Johan has "[…] set it up more formally, that we have groups and these individual project groups meet with me and others, and I really encourage them to meet of course anyways, whether I'm there or not."

5.3.2 Martin: 'The template was not there'

At the time of my interview, Martin is halfway through a three-year PhD program. He earned his master's and bachelor's degree abroad, in his native country. There, he met Johan a couple of years ago when the latter was giving a presentation at Martin's former university. As Martin recalls it, he went up to Johan after his talk, they chatted and stayed in contact. First, Martin came for a week's visit to the lab, then for a whole summer and finally he was hired as a doctoral student. From early on, Martin showed interest in a high risk project, the structural determination of an “orphan” protein, an oddly shaped one at the periphery of the family of proteins that Johan’s lab is studying. Comparably less is known about such proteins, and to determine their molecular composition requires a sequence of experimental steps, each of which is fraught with experimental insecurities.

In fact, Johan gave the project to him—but not to him alone, as Martin learned later. When he started his PhD, he found to his surprise that a newly arrived post-doc, Nanna, would be working on the same research task. As Martin and Nanna pursue the same goal, they are closely collaborating. In the beginning, they had to find out how to organize themselves in order to avoid “inefficiencies” and “not to double […] work” unnecessarily. This has been complicated by the fact that Martin is supposed to work on his dissertation. In his view, Nanna’s understanding of what it means to write a dissertation is different from the understanding that Martin developed through the time he has spent in Johan’s group. According to Martin, Nanna, having graduated at a research laboratory in another country, regards a dissertation rather as a collection of experiments in which a student is supervised, but essentially works alone:

> […] she cares a lot about my PhD which is extremely nice and she is always kind of telling me that this is, “put this in your thesis” and so forth. In her mind, when she started, was the thing that in your PhD thesis should be only and exclusively your results. But what I experience here is that if you collaborate, of course you state who, that it’s someone else’s, but you can make a, make a coherent story with results from other people. And for a while that was unclear, that was trying to do something, everything that I did should have a coherent kind of storyline. And it was difficult kind of not to double, then, work. So, […] we had some inefficiencies, and yeah, we had to figure out kind of on the job. There was not really, the template was not really there.

After some time, however, Martin and Nanna were able to agree upon a modus operandi, a routine of synchronized, parallel experimentation that has proven to be most “efficient.” As Martin explains, he and Nanna

"[…] do simultaneous things and then if any of us have success, start from there and then split again." In this manner, they explore experimental alternatives at each of the steps that it takes to purify a protein for crystallization. If one of them has obtained good results with a particular badge—e.g. a particular chemical dilution—at a particular experimental step, they share the successful badge between themselves and, again, take it from there and purse parallel experimental tracks. Their shared goal is to determine the structure of their orphan protein as quickly as possible—as Martin emphasizes, "you want to publish, you want to get to the end." For the amount of time that Martin has at his disposal, this is an ambitious goal, and he relies not only on close collaboration with Nanna, but also on intense supervision and substantial help:

> In the Danish system […] you have only three years of PhD. I don't think you have time for actual whole projects, stumbling on every step, doing all the steps and having problems at all these steps to learn the techniques – and be able to publish. […] So, […] generally speaking, you never know all the techniques. So, whatever you do, you learn some techniques.

Therefore, Martin adds, "you want to be in an environment where there is a lot of techniques available, so whenever you have a particular problem, you can ask people to kind of supervise you." Because the group is large, Johan is not "[…] supervising us in a standard way," and Martin relies to a large degree on Magnus, an advanced post-doc who is leading a subgroup on orphan proteins and whose experience in the isolation of these proteins is crucial for the work of both Martin and Nanna.

Many experimental steps require pragmatic decisions—e.g., about the concentration of dilutions—for which no detailed explicit guidelines exist, therefore particularly younger scientists rely upon lab members with more experimental experience:

> The question always is why do we use 50 molar, 100 molar, like you don't know if 78 would be better. You just don't have the time to test every small detail. You have to go with, just things that work and just copy that. And then, on top of that, you put what you understand is happening, what you know generally about science.

In dealing with epistemic uncertainties such as these, “luck” and instances in which a colleague “stumble[s] semi-accidentally” over a result can be components of experimental success; but just as important are the sheer labor force, mutual help and a well-functioning laboratory: “it’s very difficult to compete as a new person, but if you are here [in the laboratory] you can actually, if you are lucky, you can go straightforward with the routine techniques that we have here and actually get an excellent publication. It’s possible.”

Let me conclude this chapter by pointing out that, for the two research groups I have studied and whose members I have interviewed, collaboration is the *sine qua non* of experimental scientific practice. As I hope to have illustrated with the interviews presented in this and the preceding chapter, by and large research group members perceive collaboration, and the relations of inter-individual dependence that it entails, neither as a threat to their intrinsic individual motivation nor to their professional autonomy as an established or aspiring expert in a given field. Rather, collaboration is a means of consolidating, developing and enacting expertise and pursuing individual interests. As this chapter makes clear, however, there is a basic difference in the pursuit of individual research interests between the planetary science group and the molecular biology laboratory. While members of the former seek to establish themselves as relative experts with distinct fields of expertise, members of the latter seek to learn from one another, incorporating in detail the experimental experiences that other group members have made. Only in this way can they pursue their individual research interests.

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6
Division of Labor

Research groups divide the kind of labor that it takes to create scientific knowledge among their members—cognitive labor, but also the manual labor of experimental practice and the social effort that it takes for a group member to interact. But *how* do groups divide labor? And hence, what does an epistemological approach to the division of labor in groups need to take into account? To answer these two questions, I begin by revisiting social epistemology's existing discussion about the division of labor in science, a discussion primarily focused on scientific peer communities (Sect. 6.1). Thereafter, I examine the division of labor in the two research groups I have studied, the planetary science group (Sect. 6.2) and the molecular biology laboratory (Sect. 6.3), providing a comparison in Sect. 6.4. Based on these two cases, I will argue in Sect. 6.5 that an account of the division of labor in research groups has to differ from existing community-focused accounts in four crucial respects in which there is a need to: (i) focus on within-group differences in expertise; (ii) consider both processes of differentiation and convergence; (iii) shift attention from competition to collaboration; and (iv) consider scientific knowledge in-the-making long before formal peer reviewing takes effect.

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6.1 Philosophical Perspectives

Philosophers of science and social epistemologists have long analyzed the division of "epistemic," that is, knowledge-creating, labor within scientific communities. More recently, philosophers have also begun to examine aspects of division of epistemic labor when they examine inter-individual exchange and scientific collaboration within research groups, building upon accounts of epistemic dependence or group belief and collective knowledge. To continue this line of philosophical inquiry, group-focused perspectives on the division of labor in science should be able to relate to existing, predominantly community-focused perspectives. But not only do group and community-focused perspectives apply a different "zoom" when they choose to foreground either micro or macro-phenomena, they also approach the division of epistemic labor with different conceptual emphases.

When philosophers of science and social epistemologists consider the division of labor for scientific communities, they are typically concerned with the ways in which socio-cognitive diversity at the community level serves efficiency, the elimination of unwarranted bias and epistemic risk-spreading (e.g., D'Agostino, 2009; de Langhe, 2010; Kitcher, 1993; Solomon, 2006). Philip Kitcher's work has pioneered community-concerned approaches to this division of labor, and I therefore present his account in some more detail. Kitcher (1990, 1993) explores how a scientific community fares best when forced to choose between competing theories or methods. When two rival methods or theories are on the table, which one should the community choose? Put differently, how can a community minimize the risk of scientific failure? Kitcher proposes the division of "cognitive" labor (Kitcher, 1993, p. 344) as a means to balance the epistemic risks attached to the method or theory choices. It is not reasonable for a community of scientists, he argues, to endorse only those options which appear most promising, that is, those which can be assigned the highest probability of yielding scientific success. Instead, scientific communities perform best when they divide the labor such that different research options are pursued in competition (Kitcher, 1993, p. 347).

With the help of mathematically formalized models, Kitcher discusses how such a division of labor can be achieved in a community of individual scientists who have to choose between different methods or theories to invest themselves in. If these scientists were purely "epistemically" minded, that is, if they only weighed scientific knowledge gains against the resources that the creation of such knowledge requires, then, so Kitcher argues, no broad division of labor would be achieved and the epistemic risks would be poorly distributed among community members. For individual scientists that take only strictly epistemic criteria into account, Kitcher holds, it is not rational to invest themselves in anything other than the theory or method option that has the highest probability of scientific success. Assuming that all members of the community would converge in their judgment of success probabilities, no division of labor would occur. This, however, is neither desirable from a community point of view nor actually observable in scientific practice. Scientists in communities do divide the pursuit of different theoretical or methodological routes among themselves, and they do so, Kitcher argues, because at least some of them take into account "non-epistemic" or "social" criteria when deciding which theory or method to pursue. They will consider not only the probability of scientific success, which the adoption of a particular method implies, but also the credit they could receive in the case of success. While little credit is to be gained by following the mainstream, much is to be gained in a group of vanguard outsiders. Given different preferences in balancing the risk of failure against gains in reputation, Kitcher argues, individual and community rationality can be aligned (Kitcher, 1990).

In the context of this chapter, two aspects of Kitcher's account of the division of labor on a community basis merit further discussion—its notion of community and the role of not strictly epistemic triggers of division of labor. Kitcher's notion of community does little to support analyses of group collaboration. Kitcher realizes that individual scientists belong to what he calls "fiefdoms" (laboratories) (Kitcher, 1990, p. 17), but he does not pursue this thought. Rather, he characterizes scientific communities as a set of individual scientists, whom he conceives of as independent, competing and rational decision-takers. Furthermore, he argues that the social arrangement of science as a credit-distributing institution, as one that addresses not just purely epistemic ambitions, triggers a desirable division of labor. However, to foreground institutional incentives easily obscures which other knowledge-concerned conditions may entail division of labor in science.

In fact, a number of philosophers have argued that the division of labor does not require non-epistemic values, credit and prestige, nor the mechanisms through which they are realized, to take effect. Instead, the uneven distribution of epistemic resources, such as experimental infrastructure, skill and expertise within a scientific community, may incite different scientists to pursue different, competing lines of research (Giere, 1988, p. 213f.). Moreover, differences in scientific background assumptions will lead individual scientists to assess the probability of epistemic success attached to theory and method choices differently (Goldman, 1999, p. 257). In addition, the epistemic values at play in scientific practice are, according to D'Agostino, sufficiently diverse and ambivalent so as to "[…] exhaust the differences that we require to support diversity in exploratory behavior and, hence, to spread risk" (D'Agostino, 2005, p. 205).

However, while community-focused approaches to the division of labor in science focus on individual decision-making, they tend not to consider the inter-individual relations that may be the trigger or result of the division of labor at the group level. The division of labor in inter-individual relations is, instead, considered by some social epistemologists in the context of epistemic dependence between experts and relative laypeople (as, e.g., in Hardwig, 1991; Ruloff, 2003). Christopher Gauker, for example, defines the division of epistemic labor as "a social arrangement in which people benefit from the expertise that others possess regarding subjects of which they themselves do not possess an expert understanding" (Gauker, 1991, p. 303). In a similar vein, Goldberg (2011) phrases the division of epistemic labor as a net of dependence relations among individuals within a scientific community. While I elaborate on the issue of epistemic dependence in scientific practice in Chap. 7, let me note for the time being that these dependence-focused approaches concern themselves primarily with isolated acts of inter-individual change, largely leaving aside the contexts of collaboration and the social texture in which these acts can be embedded.

New motives for the study of the division of labor at the group level and its epistemic relevance for scientific knowledge creation have recently come from accounts of research group collaboration. Yet, this strand of literature is not concerned with the division of epistemic labor per se but rather foregrounds the question as to whether collaboratively created scientific knowledge should be considered "collective knowledge" (see, e.g., Andersen, 2010; de Ridder, 2014; Matthiesen, 2006; Rolin, 2010; Wray, 2001). Much of this literature developed out of the concept of a "joint," that is, irreducibly collective "group belief," which Margaret Gilbert proposed (Gilbert, 1989) and which highlights the moral aspects of group collaboration, for example, elaborating upon the webs of commitments and dependencies that can bind group members to one another. I will discuss this literature in more detail in Chap. 9.

Many group-focused perspectives upon research collaboration display a growing interest in the nitty-gritty of group collaboration, an interest that is often pursued by means of case study. Let me briefly provide two examples to illustrate the bandwidth of the empirical phenomena that this literature has sought to deal with so far. Hanne Andersen develops her account of joint acceptance with reference to a historical case study on research on induced radioactivity that resulted from clusters of interdisciplinary collaboration in the 1930s (Andersen, 2010). In contrast, when Kent Staley and William Rehg study a case of contemporary high-energy physics, they study research collaboration on a much larger scale, involving the institutionally coordinated efforts of hundreds of scientists (Rehg & Staley, 2008). Both Andersen as well as Rehg and Staley, however, are ultimately less interested in the division of epistemic labor among collaborators than in characterizing the kind of consensus that collaborating scientists reach when they co-author publications.

Against the backdrop of this literature, in this chapter I empirically examine different patterns of division of labor in the two research groups I have studied. Investigating the nitty-gritty of group collaboration will show how far the existing notion of a division of epistemic labor, a notion that has by and large been elaborated in reference to scientific communities, can help articulating aspects of a social epistemology of research groups.

6.2 Complementary Collaboration

The division of labor in the interdisciplinary planetary science group, I argue, is best understood as complementary collaboration—an ongoing, open-ended collaboration in the course of which group members cooperate *inter pares* in order to complement their respective expertise in the pursuit of a cluster of differentially shared research interests. In the following, I will characterize the practices by which members of the planetary science group collaborate and divide labor among themselves, namely by shared planning and framing, deference and witnessing, and iterative deliberation. All three of these practices enable group members to draw on their mutually complementary expertise, weaving their distinct individual research interests together.

Two types of experiments are central to joint activity in the planetary science group: experiments with the simulation tunnels in the group's large basement laboratory, and so-called "tumbling experiments," upon which I will draw to illustrate the group's division of labor. In tumbling experiments, small samples of quartz sand or similar material are bottled and mounted onto an experimental device that the group's technician built to Laurits's specification. The device, powered by a washing machine motor, spins bottled samples in circles to simulate erosion processes caused by wind. It is a shared assumption among group members that processes similar to the tumbling of grains take place on planetary surfaces and that these processes change the geomorphology of planets.

To provide a vivid illustration of the experimental practices I have observed in the group, I refer to parts of a particular experiment in the following. The experiment in question addresses whether and to what degree hydrogen peroxide is produced when tumbled quartz comes into contact with water. The presence of hydrogen peroxide is of biological interest because it is known to have toxic effects on cells, though it is also known that some, supposedly "old," micro-organisms have developed mechanisms to cope with its presence. Moreover, hydrogen peroxide is a rather unstable compound that can trigger various geochemical reactions.

The decision to conduct these experiments was taken more than a year before they actually happened. It was also decided that these measurements would form part of a dissertation project in microbiology, which would be supervised by Victor. The measurements have eventually been delegated to Lucas, a newly started PhD student. With the help of Christoffer's chemical expertise, Lucas has been developing a protocol for the measurements needed during the past few months.

It is a November morning and the group meets as usual in the same small meeting room with the large round table. All core group members are present today—Laurits, Adam, Christoffer, Rasmus, Victor with his PhD student Lucas, and Nikolaj, the group's dedicated technician. When everyone is seated, Laurits reports that two glass flasks with quartz have been heated up to 110 degrees and are now sterilized. Laurits goes quickly through a number of other things (a student, an application, an invoice, a computer system, a new instrument), but then abruptly comes back to the two flasks: "What to do with them now? They have been cooling down."

It had been agreed earlier that the flasks have to be closed, one of them will be vacuum pumped and both will be tumbled for the same period of time, so that Lucas will be able to perform his hydrogen peroxide measurements on them. Adam and Nikolaj discuss briefly how actually to pump the air out of one of the flasks. Then, the conversation suddenly turns away from experimental details.

As Lucas has begun drafting an article about his envisioned measurements during the last week, he mentions that they need to talk about the focus that the article should have. The subsequent discussion becomes contentious. While some group members prefer a focus on the physical details of the experiment and the chemical mechanisms which can be measured by Lucas, others argue that an article with the character of a report would not "sell" and they propose to interpret measurements with regard to geomorphological processes which might have taken place on early Earth. Lucas's findings could be used, they argue, for a "back-of-the-envelope" calculation supporting the formulation of a rough quantitative hypothesis about the production of oxygen on Earth. Phrasing the article in this manner, they may be able to place it in a high-profile journal. Christoffer argues enthusiastically for this option. Other group members, however, are rather skeptical of these suggestions and doubt that there is sufficient reliable background literature to support any hypothesis about the role of hydrogen peroxide in oxygen production on early Earth. Finally, Victor suggests to include considerations of the early Earth's geomorphological processes, albeit with the proviso that "the story might actually be more complicated." The meeting comes to a close.

> After the meeting, they all gather in the lab opposite Laurits's office. With Nikolaj's advice, Lucas takes two quartz flasks cautiously out of an oven. The flasks are still very hot, and they transport them down to the basement laboratory where they plan to vacuum pump them. Adam and Nikolaj prepare the vacuum pumping and explain the process to Nikolaj. Christoffer is also present. They talk loosely. Christoffer wants to know: "Why did we actually never tumble in water?" Adam agrees: "That would be very interesting. We could do that at some point. We should put it on the list."
>
> Then, the vacuum pump is switched on. You can see the flask vibrating lightly. Lucas takes pictures. They discuss the best way to switch off the pump and disconnect it from the flask. The latter task will be taken care of by the glass-blower who has just come over from his workplace in the chemistry department and is now preparing for the task. Adam slowly switches off the pump while the glass-blower carefully seals the flask just above the base of its neck. Then they disconnect the pump and Christoffer labels the two flasks. Nikolaj measures them. Lucas tells me: "After so many days of work I'm so happy that we finally have the samples ready." *(field note, group meeting, group1)*

This field note describes a phase of close collaboration in the planetary science group in which the respective research interests and the expertise of group members—biological, geochemical and physical—are tightly integrated. However, the field note also exemplifies that, since the planetary science group is interdisciplinary, it is often not obvious to group members how experiments should be conducted and how experimental results should be framed for publication. Therefore, group members are forced to discuss the *planning and framing* of experiments that they jointly carry out at their weekly meetings in great detail, and sometimes controversially. Not only do they discuss possible experiments, their design and the outcome of their respective disciplinary viewpoints, it is also important for them to discuss the framework of interpretation, that is, from which perspective should they argue for the relevance of their experimental results. The framing of conducted experiments concerns decisions about "which story to sell." For which geomorphological, geophysical or biochemical phenomena are the tumbling experiments relevant? After all, what do these experiments reveal? How would different scientific communities interpret them? What broader theoretical context is of primary concern here? What journals should the authors target for submission? Which narrative would be appropriate for such an article? To which phenomena of general interest should a possible article relate? And taking these things into account, which further measurements and calculations are needed, and which experimental standards have to be met?

Right from the start of experimentation, planning discussions and framing negotiations are important for the planetary science group to define, assign and coordinate experimental work. Apparent differences in expertise between group members force them to rely upon each other, to step back and *defer* experimental work to their collaborators. Nonetheless, group members take an active interest in each other's work. Deferring labor to a collaborator is seldom an act of indifferent outsourcing. Rather, when group members have a shared interest in experimental results, deference is often complemented by enthusiastic *witnessing*, particularly in the group's dedicated lab facilities. Group members appreciate the opportunity to witness and share experimental experience even though, for pragmatic reasons, only one or two group members will actually carry out particular experimental steps.

Iterative deliberation accompanies all steps of joint experimentation in the planetary science group. The interpretation of ultimate, interim and envisioned results, as well as the question as to when results are actually definite and publishable—all this is a matter of open-ended discussion, in the course of which a common vision of joint experimentation (and jointly authored publication) is continuously negotiated. While single experiments or experimental steps are clearly endorsed with varying enthusiasm by different group members, methods and results should eventually be acceptable to all participating senior core group members. This acceptance evolves gradually in joint discussion in the course of which different group members at different points in time offer both their critical objections and their arguments for developing the experiment in question further. Here, a lack of agreement does not lead to dissent but ultimately reinforces group members' collaborative search for experimental results whose reliability, validity and interpretation can be accepted by all core group members. In fact, the planetary science group has established a continuous mode of collaboration in which one experiment, whether successful or not, leads to another experiment in the future.

Through shared, open-ended discussions, deference and witnessing, the planetary science group has adopted a pattern of division of labor that reflects the interdisciplinary character of its research. Senior group members possess complementary expertise, and they do not aspire to acquire the expertise that their colleagues possess—"we try not to step on each others' toes," as one member told me *(Adam, interview, group1)*. Each senior member seeks to carve out his or her niche within the group's research in a way that fosters collaboration with other members but allows the pursuit of distinct, individual research interests at the same time, continuously balancing research collaboration with professional individuation and interweaving individual research interests into a shared experimental practice.

6.3 Parallel Collaboration

The division of labor in the molecular biology laboratory is hierarchically structured, which reflects, by and large, professional seniority—the laboratory's senior scientists, most prominently the lab leader, delegate tasks to junior scientists and supervise them. To organize this supervision, lab members are clustered into subgroups, each of which is dedicated to the study of a set of similar proteins. These proteins are purified and crystallized to ascertain their molecular structure. Because much of this work is hands-on, time-consuming and labor-intensive, most research in the molecular biology laboratory is carried out collaboratively. Even though the crystallization of a protein is, as a project, typically assigned to one or two individual lab members, many different hands are involved in the experimental routines that are necessary to determine a protein's molecular structure.

For a more detailed discussion of the lab's division of labor, let me present the empirical material that I gathered while shadowing Alex, an advanced PhD student. The following field note describes an interaction between Alex and Sylvia, a less experienced PhD student. Both Alex and Sylvia belong to a subgroup supervised by Magnus, an experienced post-doc. They each work on their "own" protein but collaborate closely during day-to-day experimental labor, helping one another out, offering advice and discussing experimental issues. Since the proteins studied within one subgroup are selected so as to resemble one another strongly, subgroup members often rely on the same, or very similar, protocols for their experimental procedures. As we will see, this enables them to share material resources, such as certain dilutions to wash their proteins, which have to be carefully prepared from scratch and whose composition has to be fine-tuned to individual proteins.

> His schedule leaves Alex a little more than half an hour to help Sylvia with her "lipidization," a specific experimental step in the purification procedure. She has performed the step before, but she cannot remember the details of it. After a look at her protocol, Alex discovers a mistake in the protocol which she has formulated on the basis of Magnus's directions. But since she uses the dilutions Alex had prepared earlier for his experiments, not her own, it doesn't matter. She is lucky.
>
> As a student, Sylvia was educated in theoretical chemistry and she is not yet familiar with a number of standard procedures required for the purification of proteins. Alex and Sylvia talk repeatedly about Magnus's protocol and how exactly she needs to perform certain procedures listed there—e.g., that the sample should always be kept on ice, Sylvia! But how do you handle the things in between the lines that differ from lab to lab and from person to person? Alex: "So you have to ask different people, but not too many different people. I think Magnus and I are semi-consistent."
>
> Alex is telling Sylvia what to get and what to do: ice, buffer, defrost the protein. He does some calculations for her to get the right dilutions in the right amounts. They do a dry run of lipidization in which he is showing Sylvia each step. When she performs them herself on her protein sample, he corrects her. Suddenly something is going wrong. There are too many bubbles in the dilution with her protein. Their movements become hasty as they try to fix the problem. Alex is proposing a number of options to correct the failure, but she will anyway, as Alex explains quickly, "(a) have too little sample for a good crystallization, because she will be losing some of her protein by removing these bubbles; or (b), if you get crystals, they will not be reproducible, because you deviated from the protocol too much."

> Sylvia looks distressed. They try to remove the bubbles. "Once you are off the protocol, it's really shaky," Alex comments. *(Alex, field note, group2)*

As this field note shows, the division of labor in the molecular biology laboratory is not only a means of efficiently achieving experimental goals. It is also a means of hands-on learning. The purification of a selected protein has been assigned to Sylvia not because she would possess the necessary expertise to carry out the necessary experimental procedures—rather, it has been delegated to her to provide an occasion for hands-on learning. To purify "her" protein, Sylvia has to learn how to do it, and to do so she relies on Magnus's supervision as well as on Alex's help. Delegation, help and learning are key elements in the mode of collaboration that the molecular biology laboratory has adopted, and I will discuss each of them in the following.

Delegation is the primary means of coordinating research activity in the laboratory. What underlies delegation is the authority of scientific expertise, and therefore research tasks are typically delegated by senior lab members, that is, a lab leader or a subgroup leader, to less senior lab members. The lab leader, for example, assigns students to specific research projects and delegates their day-to-day supervision to the post-docs who take on the role of subgroup leaders, who coordinate the work of the junior students. In so doing, it is their responsibility to hold up scientific standards and guarantee the quality of all experimental steps performed. They do that by being present at the lab bench and making themselves available for consultation during most of the day. They also do that by enforcing a protocol, from which supervised junior scientists are not supposed to deviate without their consent. Yet, despite the hierarchical structure that underlies it, delegation in the molecular biology laboratory is typically phrased as suggestion or advice, and embedded in a participatory work culture.

Besides delegation, the molecular biology laboratory features many other instances of division of labor which neither involve a gradient in expertise nor authority. Often, labor is split up between group members in a benevolent tit for tat—group members *help* each other. Help is vital for the functioning of the laboratory, because experimental procedures are complex and stretch over long hours.

To help a colleague means to offer resources not currently at his or her disposal: physical strength (to help carry the nitrogen tank), a material object (a share of a chemical dilution prepared for "private use"), expertise (the know-how to run an experimental machine) or time (to look out after an experiment over the weekend). In a well-rehearsed team, help can be a quick fix in situations which have not been foreseen. When instruments are malfunctioning, when experiments go awry and time spent is mounting up, the willingness to help one another out is crucial. Help is vital in situations where those who are in need of support are in no position to delegate the task they are struggling with to someone else. In contrast to hierarchical delegation, help is asked for, not ordered. Help is based on solidarity, not on a hierarchy of expertise and authority.

Learning is another key element in the lab's collaborative scientific practice, and lab members often help one another to learn. In a field where important knowledge consists in the embodied ability to manipulate organic samples literally hands-on, teaching involves demonstrating experimental procedures and overseeing and correcting their execution *in situ*. As experimental practice in molecular biology is collaborative, it is not just those who are taught, but also those who teach that profit from the collective effort to learn. After all, it is better to rely on skilled collaborators, and it is easy to trust a colleague's skills if one has witnessed how these skills have been acquired.

What promotes learning in the molecular biology laboratory is the fact that all lab members work on projects, that is, proteins, that resemble one another in crucial ways. Collectively, lab members pursue a series of similar research interests. The seriality of their research enables them to learn from one another and to help each other, which facilitates supervision. Furthermore, all lab members have, or intend to acquire, roughly the same kind of expertise, which is, along with experimental techniques, ever evolving. Learning, from the perspective of junior scientists, is a way to overcome the hierarchy in expertise and authority that structures their relations to other lab members. As they continue to learn, junior scientists close the gap that separates them from the level of expertise that senior group members possess.

An exception to this rationale of learning is, curiously, the lab leader. Since he has not been working physically at the laboratory bench for the past few years, Johan has stopped learning to master new experimental routines (although he continues to learn *about* them). Instead, he focuses upon theoretical research perspectives and strategic management, an important part of which is the assessment and distribution of risk, that is, the possibility of experimental failure. This kind of specialization that "[…] involves the differentiation of theoretical activities on the one hand, and experimental activities on the other," and is tied to within-group hierarchy, has been called "vertical" specialization (Laudel, 2001, p. 764).

6.4 Comparison

At this point, it is instructive to compare the planetary science group and the molecular biology laboratory. The two groups clearly show distinct patterns in the division of labor among their members, a difference that is shaped by their research, the demands of experimental practice and also by the kind of expertise that different members bring to the group.

While the planetary science group is dominated by senior researchers who cooperate in an egalitarian way, the molecular biology laboratory features a distinct hierarchy between junior and senior scientists. While the planetary science group endorses a loose, rather exploratory and continuous mode of experimentation, the experiments carried out in the molecular biology laboratory are established procedures which serve a clear end. Therefore, planning and framing discussions, such as occurs in the planetary science group, are not necessary for the molecular biology laboratory, where the trajectory research activity is structured by generic experimental procedures—the purification and crystallization of proteins—in rather foreseeable ways.

While members of the molecular biology laboratory work on a series of resembling, yet distinct, research goals, members of the planetary science group pursue research goals that they, given their respective disciplinary backgrounds, differentially share. Since large parts of the research carried out in the planetary science group are perceived as a joint endeavor in which all involved scientists have their stake, the planetary science group displayed fewer instances of mutual support that might be described as genuine help.

What the differences between planetary science group and molecular biology laboratory show is that the division of labor can strengthen differentiation of expertise and professional individuation *as well as* it can, by creating learning opportunities, foster the convergence of expertise. In the planetary science group, the division of labor is organized so as to account for the resources that members bring to the group, that is, their expertise, their research interests, the infrastructure they have at their disposal, but also the time and the sheer labor they are able to put into a task. Members of the planetary science group specialize "horizontally," reflecting the differentiation between scientific disciplines and sub-disciplines. In contrast, members of the molecular biology laboratory often divide labor so as to account for the lack of expertise, that is, the need to learn and practice. Here, the division of labor is not only a measure of efficiency, but also a pedagogical means for helping all group members to acquire, roughly, the same kind of expertise—a pursuit of convergence that is, as we have seen, complemented by "vertical" specialization, which in different research tasks is in line with the hierarchy that distinguishes experienced senior scientists from less experienced junior scientists (cf. Laudel, 2001).

These differences in the division of labor between the planetary science group and the molecular biology laboratory carry over in reflections upon epistemic dependence and trust that I will unfold in the following Chaps. 7 and 8. As we will see, horizontal specialization entails different forms of dependence and requires different strategies to build up trust than a primarily vertical division of labor that seeks to create convergence in expertise.

6.5 Conclusion

My empirical investigation of research groups shows that community-focused approaches to the division of epistemic labor, such as Kitcher's, do not sit well with research group collaboration. Rather, distinct group-focused approaches are needed because the division of labor at the group level raises epistemological issues that community-focused approaches typically sideline. To conclude this chapter, let me outline four epistemological aspects that group-focused approaches need to account for.

First of all, my comparative study of research group collaboration reinforces the argument that not only non-epistemic but also epistemic factors trigger the division of epistemic labor. For the groups observed, the prime trigger for the division of labor among group members is the distribution of expertise, an uneven distribution that either stems from the research group's interdisciplinary composition or from the fact that the group features a strong gradient in seniority. In the planetary science group, division of labor capitalizes upon the uneven distribution of expertise, along with the material research infrastructure, among group members. In fact, the planetary science group exists as a group because it makes an interdisciplinary pool of resources available that group members otherwise, within mono-disciplinary research contexts, would not be able to access. The division of labor in the molecular biology laboratory, in contrast, takes into account the expertise that group members have, or lack, a lack that is to be mended by dividing the labor so as to create learning opportunities.

Second, the issue of learning draws attention to a peculiar aspect in which group-concerned accounts have to diverge palpably from community-concerned accounts of the division of labor. While community-concerned approaches such as Kitcher's tend to cast the division of labor in the light of differentiation, the case of the molecular biology laboratory particularly shows that the division of labor can be a means of convergence through learning, and need not be a means of differentiation.

Third, group-concerned approaches need to shift attention from competition to collaboration. The division of labor in groups can, certainly, involve elements of competition between group members. But as my case comparative study illustrates, and as other literature has shown (e.g., Poulsen, 2001), research groups divide labor among their members by way of collaboration—and it is their close collaboration, not competition, that makes research groups a powerful instrument of scientific knowledge creation. Through collaboration, research groups effectively combine the resources that members can bring to the group, and the risk of scientific failure is borne collectively. Like scientific communities, groups need to spread the risk of failure, as well as the rewards of success, onto many shoulders. In research groups, collaborating individual scientists typically engage themselves in several projects or experiments at once, and they co-author many more publications than they would be able to publish alone. What group collaboration, thus, takes into account is not only the collective need to distribute epistemic risks, but the individual need to do so as well.

Lastly, group-focused approaches to the division of labor concern knowledge in-the-making, that is, knowledge claims that have not yet passed formal peer review and cannot be taken for "established" knowledge. Such knowledge in-the-making is difficult to rely upon. Therefore, epistemic dependence emerges as a pressing issue in group collaboration, an issue that community-concerned approaches, such as Kitcher's, typically background. In Kitcher's account, competing individual scientists pursue alternative lines of research independently. Only when one line of research leads to evident scientific success (or failure) do scientists either disseminate their findings or abandon their line of work and refer to others' research. Epistemic dependence is not a predominant issue here because the scientific community at large has the means, the peer-review mechanisms and the critical mass to decide what counts as evident scientific success, as "established" trustworthy knowledge, and what does not. Research groups, however, do not possess these means; and since the division of labor in groups is collaborative rather than competitive, members have to rely on one another early on in the process of scientific knowledge creation—an epistemic challenge upon which the following chapters will shed more light.

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7

Epistemic Dependence

When scientific, knowledge-creating labor is divided among the members of a research group, these members come to depend upon one another. Evidence has to be gathered, arguments have to be formulated and collaborating scientists have to rely on one another for the bits and pieces of experimental evidence, interpretation and argument that are eventually to be integrated into a well-corroborated, publishable, scientific knowledge claim. As scientists form beliefs about the quality of the evidence and arguments, they crucially depend on the beliefs of their collaborators who have carried out experiments and gathered experimental data or who possess, for example, the necessary expertise to interpret the data and formulate an argument. In this chapter, I analyze dependence of this kind in terms of epistemic dependence.

Broadly conceived, epistemic dependence can be defined as a relation between two beliefs, a relation where one belief draws its justification from a second belief. This chapter, however, addresses epistemic dependence more specifically as an inter-individual problem. I deal with the relationship between beliefs that are held by different individuals, and that is characterized by an asymmetry in intellectual authority—the dependent

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individual relies upon somebody else because he or she has good reason to believe this other person to be in an epistemically superior position. Such a relation between individuals' beliefs has to be mediated, either through verbal communication or the exchange of material objects such as data charts, written reports and material probes. When a belief is verbally communicated from one individual to another by way of assertion, and when, thus, epistemic dependence is a matter of reliance upon others' say-so, then epistemic dependence is akin to reliance upon testimony. Therefore, in this chapter I draw not only on epistemology's accounts of epistemic dependence but also, in parts, on accounts of testimony as a source of justified belief and knowledge.

Examining inter-individual epistemic dependence as an issue of collaborative scientific practice, I analyze epistemic dependence as a relation between one scientist's believing and another scientist's beliefs and/or, as I will elaborate, the product of his or her labor. Three remarks are in order here. First, when I speak of individual believing or individually held beliefs, I imply that beliefs can qualify as knowledge if they are "well-founded," that is, justified, in terms of the standards that apply in the context of scientific practice. What, hence, underlies my understanding is a contextualist notion of knowledge as justified (true) belief.[1] Second, when I understand knowing as believing, I leave issues of tacit knowledge largely aside. This is not to say that tacit knowledge would be peripheral to scientific practice. Rather, knowledge that cannot be easily explicated and communicated does not sit well with the epistemological approaches discussed here. Third, when I focus on epistemic dependence in collabora-

[1] It lies beyond the scope of this book to defend epistemic contextualism (Rysiew, 2016), but as will become apparent in this chapter and in Chap. 9, a contextualist understanding of what it means to justify a belief as knowledge allows us to accommodate the fact that second-order reasons may justify a collaborating scientist's individual knowing—but does not constitute the kind of justification that it takes to formulate a scientific knowledge claim. Without such a contextualist understanding, one would either have to deny individual scientists the possibility to know qua trust or to allow for scientific knowledge to be justified upon the basis of testimony. Both options are unattractive. To deny scientists to know qua trust overly limits their epistemic agency and casts collaborative scientific practice as inferior guesswork. To allow for scientific knowledge to be justified in terms of testimony does not resonate with the conventions of scientific method and scientific authorship.

tive scientific practice, I also sideline all those instances in which scientists depend upon one another for other than epistemic purposes.[2]

Drawing on the empirical data I have gathered, I ground my analysis of epistemic dependence on observations of actual scientific practice in research groups. From a practice-minded point of view, philosophical accounts of epistemic dependence need to take into consideration that the relations of epistemic dependence between collaborating scientists can vastly vary. No one size fits all. In order to get a grip on the configurations of epistemic dependence in scientific practice, a subtle and differentiated terminology is needed. As I will argue, crucial to configurations of epistemic dependence in collaborative scientific practice is whether, to what extent and in which way the dependent person has the possibility to make use of relevant scientific expertise. For this reason, I propose to distinguish between translucent dependence (which is supported by the dependent person's experimental expertise) and opaque dependence (which is not). As I will point out, this distinction does not exhaust the phenomenon of epistemic dependence. Nevertheless, it provides two cornerstones in relation to which a variety of instances of epistemic dependence can be understood.

To unfold this distinction, the chapter proceeds as follows. Sect. 7.1 reviews some of the work on epistemic dependence that has been undertaken by epistemologists so far. Section 7.2, then, harkens back to my comparative case study and the division of labor in research groups, which, as I argue, reinforces or creates the epistemic asymmetries in which epistemic dependence is rooted. Thereafter, Sect. 7.3 elaborates on my distinction between opaque and translucent epistemic dependence.

7.1 Theoretical Groundwork

To lay the theoretical groundwork for my distinction between opaque and translucent dependence, in this section I approach epistemic dependence from two different angles. First, I consider it from a bird's-eye

[2] For a broader, social-scientific account of dependence in science see Whitley (1984, p. 87ff.).

perspective, describing it as a relation between two collaborating scientists. Then, building upon the work of Hardwig (1985, 1988, 1991), I consider it from the perspective of the dependent scientist, analyzing the reasons an epistemically dependent individual has for relying on others.

7.1.1 Belief–Belief Relations and Beyond

Let me begin by considering epistemic dependence from a bird's-eye perspective. According to Robert Audi, epistemic dependence is "[…] (roughly) the sort of relation that holds between one belief and another (or between a belief and something else) when the former depends on the latter either for its status of knowledge or for whatever justification it has" (Audi, 1983, p. 119). For Audi, hence, the paradigm of epistemic dependence is a belief–belief relation that concerns the epistemic justification of one belief in relation to another. What Audi offers is an abstract view on the architecture of beliefs and their justification, which may qualify respective beliefs as knowledge. Whether or not these beliefs are held by individuals, and how inter-individual relations of dependence may be facilitated, is of little concern to Audi.

In contrast to Audi, I am interested in the relations between a scientist's justified belief and another scientist's expertise, as manifest in his belief and/or the products of his labor.[3] When a scientist's knowing, that is, her believing-that and the reason for her believing to be justified, crucially involves what her colleagues know or the efforts they have undertaken, then we have a case of inter-individual epistemic dependence. Taking into account the products of labor, I move beyond an exclusively cognitive, internalist notion of epistemic dependence, like the one proposed by Audi. In so doing, I seek to involve the material dimension of scientific practice in epistemological reasoning because experimental evidence is, in

[3]Goldberg (2011) shows that epistemic dependence extends beyond two-person relationships. In fact, he argues, in the formulation of well-founded beliefs a person depends not only on eyewitnesses and experts from whom he might adopt knowledge, but he also depends on his larger epistemic community. This aspect is important, but for the purpose of investigating collaboration in scientific groups, I limit my analysis to immediate person-to-person relationships.

fact, mostly conveyed in a material form—that is, as a photograph, a copy of the laboratory notebook, a print, a frozen sample, a concrete model, a diagram or a file.[4]

Let me explain with an example why I consider reliance upon "foreign" labor to be an instance of epistemic dependence, albeit not one that is appropriately characterized as a belief–belief relation alone. An experimental scientist A depends on her colleague B for a couple of measurements. B may hand A a file with all the measurements made and A may access the data in the absence of B. If these measurements show *that p*, then A can form the belief *that p* when she sees those measurements or is informed about them and she can justify her believing *that p* by reference to those measurements. As these measurements possess a material quality that make them physically accessible, that is, accessible not only through measuring scientist B's testimony of his believing, the dependent scientist A may justify her believing in ways other than by direct reference to B's believing *that p*.

One may object that B's role, here, is merely instrumental, that B is acting as the prolonged arm of the measuring device. But taking scientific measurements is a process requiring reflection and discriminatory judgment (parts of which may remain tacit, cf. Soler 2011). When B passes his measurements on, A must assume that B believes them to be good. If B does not believe them to be good but passes them on anyway, he should provide A with an explanation of their weaknesses. Nevertheless, it would be short-sighted to characterize this instance of epistemic dependence as a simple belief–belief relation. Note that the measuring colleague B may interpret the measurements to show *that not-p*. It is, after all, possible to imagine that B is good at handling a measuring device but bad at interpreting his measurements. But under the condition that A, who has not performed the measurements herself, possesses the required expertise to interpret them, A can take them to indicate, and be justified in doing so,

[4]Latour's observation that scientific practice involves sequences of material "inscriptions" comes to mind (Latour, 1999). The material dimension of science has been explored paradigmatically by Rheinberger (1997), and philosophers have developed an increasing interest in the concrete artifacts of scientific practice as well (Bechtel & Abrahamsen, 2012; Carusi, 2012; Knuuttila, 2011). For the cognitive value of concrete artifacts more generally, see the literature on distributed cognition (Hutchins, 1995, 2001; Nersessian et al., 2003).

as showing *that p*. As I argue, this remains a case of epistemic dependence between two collaborating scientists as scientist A relies on her colleague B to have performed the measurements accurately and to have reported their results truthfully.

But why does it matter about emphasizing the material dimension of scientific practice for an account of epistemic dependence? When A relies upon the results of her colleague B's labor, she need—even in cases of epistemic dependence—not necessarily rely on his believing *that p* or *that not-p*. She is in a position where she can make her own judgment as to whether B's epistemic labor is evidence of *p* or not. In making this judgment, A has the possibility to make use of her own expertise. So, given that A has the necessary expertise on the issue in question, she is able to assess the evidential significance of B's labor with some degree of intellectual autonomy. This may help to contain A's dependence upon B. In Sect. 7.3, I will characterize instances of dependence such as this as translucent epistemic dependence.

7.1.2 First and Second-Order Reasons

Next, let us see how such a practice-minded understanding of epistemic dependence plays together with Hardwig's (1985, 1988, 1991) account of dependence relations, which is formulated explicitly with regard to collaborative scientific knowledge creation. Hardwig reflects upon epistemic dependence from the perspective of the dependent scientist. The question that frames his reflections is the question of how a person can acquire "well-founded" beliefs—in my wording: justified beliefs—in the absence of sufficient first-hand evidence. His answer to this question is, in short, to argue that a person can acquire well-founded beliefs by appealing to the intellectual authority of a second person. For him, thus, epistemic dependence is a matter of reference to another person's intellectual authority, which, I would like to add, can reside in scientific expertise but also in "first-hand," eyewitness knowledge of experimental procedures and their outcomes.

The point of departure for Hardwig's argument is to introduce the notion of "reason," saying that to have a well-founded belief means to have good reasons for holding this belief. A good reason for holding the belief *that p* is to have sufficient observational and/or inferential evidence that *p* obtains. I will also call evidence of this kind "immediate" evidence. What, however, if an individual, for whom it would be important to know whether or not *p* is the case, cannot have sufficient observational and/or inferential evidence, neither for believing *that p* nor for believing *that not-p*? Does that mean that this individual has no possibility whatsoever of acquiring a well-founded belief as to *p*? No, Hardwig argues, and he introduces what I call second-order reasons for having the belief *that p*.

Translating Hardwig's argument into my terminology, *first-order reasons* for *p* are reasons that spring from having observational and/or inferential evidence for *p*. If first-order reasons cannot be had, then it is rational for an individual A to rely upon another individual B affirming *that p*—provided A has good reasons to believe that B has sufficient evidence for *p* (Hardwig, 1985, p. 336). Such good reasons to believe *that p* when B is saying so (or the products of his labor are indicating it) are *second-order reasons*, which are reasons concerning B's intellectual authority, that is, his qualities as an eyewitness, his honesty and expertise (Hardwig, 1985, p. 337, 339; see also Hardwig, 1991, p. 700). As I will elaborate in Chap. 8, second-order reasons concern the trustworthiness of B as a scientific collaborator.

Following Hardwig, I maintain that second-order reasons concerning a collaborator's scientific trustworthiness can justify a scientist's belief *that p*—even though second-order reasons provide neither inferential nor observational support for the proposition *that p*. Note also that second-order reasons do not directly concern the question as to whether or not *that p* actually obtains. Second-order reasons concern somebody's trustworthiness, and as such they are not identical to reasons which would justify the assumption that somebody's testimony would *always* be reliable (or, that his or her labor would always be error free).

Summarizing, Hardwig characterizes instances of epistemic dependence as situations in which a dependent collaborator A, in her believing *that p*, resorts to second-order reasons relating to the trustworthiness of B (who, in turn, possesses immediate evidence for *p*). In the scenario of epistemic dependence that Hardwig presents, the primary question for A to answer is not whether *p* is right or wrong. Rather, the crucial question for A is how she can reasonably rely on B. Is she warranted to rely upon

B's intellectual authority in the question of *p*? If A possesses no expertise as to *p*, A will have to establish B's reliability as to the belief *that p* on grounds other than the expertise at stake. In this case, A's judgment to adopt or reject *p* is not based on expertise as to *p* and the evidence for *p*, which at least B is supposed to have, remains opaque to A (Hardwig, 1985, p. 341ff.).

My intention is to add more nuance to this picture. If the dependent scientist A has some expertise on the issue in question, she may be in a position to acquire some first-order reasons, too—only that these first-order reasons may not suffice to justify A fully in believing *that p* in accordance with scientific standards. Therefore, A's first-order reasons need to be supplemented with second-order reasons as to the intellectual authority of her collaborator B. In a scenario where A, albeit ultimately dependent on B, is able to make use of her own expertise, the character of epistemic dependence is different from situations in which A fully relies on B. In order to account for this difference, I suggest distinguishing two forms of epistemic dependence in Sect. 7.3. Before I do that, I will elaborate upon the empirical insights that have guided me in drawing this distinction.

7.2 Epistemic Asymmetries

With Hardwig, we understand that epistemic dependence among collaborating scientists arises from asymmetries in intellectual authority which are rooted in the uneven distribution of knowledge-related resources and the division of epistemic labor. As I have observed in previous chapters, collaborating scientists differ from one another with respect to the resources they possess, such as expertise, access to experimental infrastructure as well as the sheer labor that it takes to perform an experiment and the time that it takes to witness an experimental procedure first hand. Taking into account how these resources are distributed across individual group members, research groups divide the labor among themselves.

The division of epistemic labor, in turn, creates new epistemic asymmetries—allowing some group members (but not others) to specialize in an experimental task or, for example, to allow some group members (but not others) to witness an experiment first hand. While, thus, some group members are able to form justified beliefs based on their "own" perceptual experience and/or inference, others are not able to do so and are forced to rely upon the measurements taken and the interpretations made by their colleagues. It is convenient to use "first hand" and "second-hand knowing" to describe constellations such as these (Fricker, 2006). A caveat, however, is due here. To speak of first-hand knowing in the case of scientific practice should not mislead us into thinking that scientific knowledge can be had by any individual in completely self-reliant ways. Even knowledge which is related seemingly *im*mediately to observation is likely to be mediated by theory and theory-based instrumentation. But it would be wrong to infer from this that there are no epistemically significant differences between a scientist observing an experiment and a fellow colleague relying upon his or her observation.

Both the planetary science group and the molecular biology laboratory show an uneven distribution in expertise that is partly reinforced, partly mended, by the patterns of division of labor that both research groups adopt. As elaborated in Chap. 5, all lab members of the molecular biology laboratory have, or intend to acquire, roughly the same kind of disciplinary expertise; and in many instances labor is divided so as to create learning opportunities for junior members. There is a difference in specialization, however, between the lab leader and all the other researchers in the group. As the lab leader has not been working physically in the lab a lot for the past few years, he is not expected to master manually recently developed experimental routines. He instead focuses on strategic management, and I have described this form of specialization as "vertical" (cf. Laudel, 2001). "Horizontal" specialization, in contrast, is characteristic of the planetary science group, an interdisciplinary research group in which members generally do not aspire to acquire the expertise which their colleagues possess. In relation to one another, group members are experts on issues within the realm of their expertise and relative laypeople for issues outside this realm. To collaborate with one another across disciplinary boundaries, group members develop "interactional expertise" (Collins & Evans 2007; cf. Gorman 2002), a form of expertise I elaborate upon in Sect. 7.3 below.

What the comparative case study of a mono- and an interdisciplinary research group vividly illustrates is that some epistemic asymmetries are more fundamental than others. Differences in expertise that have been accumulated throughout a scientific career can hardly be cleared out. Some epistemic asymmetries, however, can be mitigated. Data measurements that were taken in the absence of a collaborator can be repeated in his or her presence. If there is time for it, the interpretation of the results from an experiment already concluded can be discussed again with the help of a team member who was occupied in other duties. Yet, time is precious, materials expensive and sometimes attempts to cross-check each others' work within the team can be perceived as inappropriate skepticism.

7.3 Opaque and Translucent Dependence

To refine the vocabulary available for analyzing collaborative knowledge creation from the point of social epistemology, I suggest making a distinction between *opaque* and *translucent epistemic dependence*.[5] A scientist is opaquely dependent upon a colleague's labor if she does not possess the expertise necessary to carry out independently, and to assess profoundly, the piece of scientific labor her colleague is contributing. I suggest, however, that if the scientist does possess the necessary expertise, then her dependence would not be opaque, but translucent. As we will see, the distinction between opaque and translucent epistemic dependence does not exhaust dependence relations in scientific practice. A range of dependence relations are neither entirely opaque, nor translucent. I will discuss why this is the case, and show how we can make sense of these relations within my distinction of opaque and translucent epistemic dependence.

[5]The expression "epistemic opacity" has been used by philosophers of science in different contexts before. Soler (2011) uses the expression to highlight the fact that tacit components of scientific practice cannot be explicated and thus remain "opaque." Lenhard (2006), in turn, uses "opacity" as a synonym for the limited comprehensibility of mathematically complex simulation models, and incomprehensibility is also what I denote by the expression.

7.3.1 Opaque Dependence

What I designate as "opaque" epistemic dependence is the kind of dependence that epistemologists usually talk about. It is the extreme case of an epistemic dependence relation, the relation that Hardwig describes and that can be formulated as a testimonial exchange: Speaker B tells listener A *that p*, and A has no means to establish the truth *that p* other than assuming that B is a competent and truthful testifier. A has no way whatsoever to justify her believing *that p* without recourse to B, because A lacks the expertise to acquire the justified belief *that p* by means other than reliance upon B's testimony. Nevertheless, she can try to establish the reliability of B, and succeed in doing so to varying degrees. The more A knows about B, his moral and professional qualities, the better she can judge his qualities as a testifier *that p* without possessing expertise pertaining to *p* herself. Thus, the dependent person in opaque dependency relations should not be described as completely naive, ignorant or undiscerning. She can have, as Hardwig pointed out, very good second-order reasons to enter an opaque dependency relation.[6]

A lack of expertise cannot easily be mitigated. Expertise cannot be simply learned from the book. The acquisition of scientific expertise is a complex learning process that typically stretches over years. Therefore, exemplary cases of opaque dependence can be found in both groups studied. In the planetary science group, instances of opaque epistemic dependence are particularly apparent because the group has an interdisciplinary profile. In this group, scientists with different disciplinary backgrounds collaborate in an intricately interwoven manner. Every one of them contributes to their collaboration with the particular combination of epistemically relevant resources he or she possesses. The use of the group's simulation facility by members without physical expertise is a good illustration of opaque epistemic dependence. When the biologist runs an experiment in the group's simulation facility, he or she relies entirely upon Adam, the physicist who is responsible for this facility.

[6] Strategies for establishing second-order reasons for the reliance upon a collaborator are analyzed in Goldman (2001) and Sperber et al. (2010).

The biologist is not able to operate the simulation facility, so he or she delivers a bacterial sample to the physicist who then places the sample in the facility, runs the experiment and returns the sample afterward to the biologist for further laboratory analysis. The physicist will also report on the measurement of all experimental parameters, such as wind speed. Another example of opaque dependence in the planetary science group would be the reliance upon Moessbauer spectroscopy to determine the iron components in geological samples. This is an example that Laurits, the group's geologist, brings up during our interview:

> Q: Mhm. Do you think that the things you have in your papers, in principle you could do them on your own? Or you could only do them with others?
> Laurits: Ahm, if I think back I would say that, that ahm, in many of our publications Moessbauer spectroscopy has been used as a useful method. I started with that before Rasmus [physicist in group1] came here. With the Moessbauer laboratory in Physics. And I started to publish things ahm with one of the persons from there, so I knew or I never had, it hasn't been necessary for me to do Moessbauer spectroscopy myself. And it's a field where you need a lot of experience to be sure of what you interpret out of the spectra you see. So, I would not be able to get the same interpretation out of that if, if I didn't have an expert like Rasmus. So that, this wouldn't be a good way doing publications, try to do this as an amateur yourself. So you need to collaborate with people that know more about different fields. And when it comes to sand transport, I would say that Oliver [geologist associated to group1] has used all his life on that and he of course has a basic knowledge there that I don't have. So if we come to experience and publications in that field, I would never do it without him. *(Laurits, senior geologist, group1)*

Instances of opaque dependence relations come to the fore in the molecular biology laboratory as well. There are two sources of epistemic opacity here. The first source is a learning lag between less experienced and more experienced group members, and the second source is specialization, both in vertical and horizontal forms. Paradigmatic for within-group relations in the molecular biology laboratory is the first source of opacity. Opaque epistemic dependence typically characterizes students' relation to senior scientists. For PhD students it is key to rely on the hands-on expertise of junior group leaders for the purification and crystallization of particular families of proteins. For example, Martin, a new PhD student, depends crucially on a post-doc's skills (Magnus) for the treatment of proteins that are known to require particularly intricate purification procedures. At the time of my interview with Martin, however, it was clear that Magnus would be leaving the laboratory soon to start his own group at a different university.

> Q: Magnus is leaving, is that a problem?
> Martin: I don't know. Initially I was very anxious about that, but I feel that we are getting to a point that within, because we have progress, basically, I think that within two months let's say we can do everything, like, because after he gave us the basic experiences, then we have to just fiddle around by ourselves. Also Magnus is very accessible by emails, skype and he clearly wants to help us and he said that of course he wants to consult with us. If we got further, then it's not really, his experience stops to be that necessary.
> *(Martin, PhD student in molecular biology, group2)*

Here, opacity has, or is imagined to have, a transient character. Time is a crucial element in understanding these dependency relations. Because of a lack of experience, junior researchers rely on senior ones, though the former expect to gain the professional capacity they are lacking at some point in the future. As students' expertise grows, they develop greater professional autonomy.

7.3.2 Translucent Dependence

Different configurations of epistemic dependence are possible. Many instances of the latter in scientific practice are not opaque, and some of these are what I refer to as "translucent," which I characterize as instances in which the dependent scientist does possess enough expertise to be able to work independently, but he or she does not do so for pragmatic reasons.

My terminological choice may seem curious at first glance. It would be obvious to choose the attribute "transparent" for forms of epistemic dependence that are not opaque. However, there is no such thing as "transparent epistemic dependence." If all epistemic issues were completely transparent to a dependent scientist, then he or she would not be epistemically dependent. To be more precise, let me return to Hardwig's terms. What is *not* transparent in cases of epistemic dependence (but should be transparent in cases of epistemic *in*dependence) are the first-order reasons for why the statement *that p* should be regarded as reliable. However, as I will elaborate in the following, the intransparency of first-order reasons can have different sources.

In the measuring example from Sect. 7.1, having sufficient evidence for *p* means to fulfill two conditions. It means to possess the necessary expertise to perform and understand the measuring procedure *and* actually to perform the measurement or eye-witness them scrupulously. The first condition posits an expertise requirement, and the second condition posits both an expertise and information requirement. When these requirements are not fulfilled, then we have a case of epistemic dependence.

While the expertise requirement may go without saying, the information requirement might seem superfluous. Clearly, when A does not meet the expertise requirement, then we have a case of opaque dependence. But as I have argued above, without having performed the measurement herself (or having overseen it closely), A cannot be entirely sure that the measurement has been conducted skilfully and reported truthfully by her colleague B. In experimental science, evidential contributions can be entirely transparent only to those who have created or eye-witnessed them because the creation of evidential contributions typically rests upon complex experimental procedures, many of which may require tacit knowledge (cf. Leonelli, 2010; Soler, 2011).

When A does possess the necessary expertise, but doesn't have sufficient information about how the contribution *that p* has actually been created, then we have a case of translucent epistemic dependence. Translucent dependence is a form of genuine epistemic dependence because the evidence for the claim *that p*, which A is interested in endorsing, is not entirely transparent to A. Translucent dependence is a form of epistemic dependence in which the dependent scientist A engages for reasons not related to her expertise in a relation of dependence with scientist B. Instead, A relies on B for pragmatic reasons, often related to considerations of time. But because A possesses expertise pertaining to the claim *that p*, A can at least partially assess B's claiming *that p*—and, if available, A can assess the experimental evidence that B provides for this claim. Also, A is in a position to question B about the details of his claim *that p*, so that B is forced to argue for his claim *that p*. Moreover, A has more possibilities to assess B's qualities as an experimenter in general. Therefore, A's dependence runs not as deep as in instances of opaque dependence. Still, I want to emphasize the fact that translucent dependence is a relevant and epistemically significant form of epistemic dependence in scientific practice.

De Ridder has suggested a distinction between "cognitively necessary epistemic dependence" and "practically necessary epistemic dependence" (de Ridder, 2014, p. 46). This distinction is similar, albeit not identical, to mine, and I will refrain from endorsing it for two reasons. First, laboratory skills are a fundamental component of scientific expertise so that it is fair to say that expertise clearly involves more than cognition. Thus, an opaque epistemic dependence relation, rooted in an asymmetry in expertise, may concern more than just cognitive asymmetries. Second, the circumstances of "practically necessary" dependence have significant cognitive consequences: if you cannot run an experiment yourself due to a lack of time, infrastructure or labor, you cannot acquire first-hand reasons concerning the experimental evidence produced. Therefore, I believe a distinction between "cognitively necessary" and "practically necessary" dependence to be potentially misleading.

What is more, I also believe it inadequate to rephrase the conceptual distinction that I draw between opaque and translucent as "insurmountable" and "surmountable" epistemic dependence. Opaque dependence is not insurmountable; it simply needs considerable time and effort to do so. In addition, to label translucent dependence as surmountable may imply that translucent epistemic dependence *should* be overcome. It is not my intention, however, to make such a claim. Translucent epistemic dependence is a valuable instrument in the efficient organization of research collaboration—and as such it is not surmountable. It has to be acknowledged that the creation of knowledge in scientific practice is shaped, and rightly so, by the uneven distribution of various resources, which are not restricted to scientific expertise. Clearly, experimental expertise is important, but the experience required to, for example, set up, maintain, and fund an experimental infrastructure is important, too, though not every scientific expert does possess it.

A case in point for translucent epistemic dependence are the relations that subgroup leaders entertain to PhD students. When Martin describes this relation, he explains:

> I don't think there is a thing that Magnus [a subgroup leader] would do worse than me, in the lab, nowadays. Just that he cannot do everything. […]
>
> […] Now Magnus is starting his own group and he will be moving out until the end of the year. And he probably spent half or a year getting money, writing applications, going to funding interviews. Of course, he was doing a lot of different stuff as well. But the more kind of, the higher you go—track record—and the more it's about just getting financing. Because it's so costly. And then you have hands, you can think about this as your hands that do your experiments, in a way. …
>
> […] so a lot of problems that we have, and we kind of solve them by ourselves, is something [Magnus] would solve also or even quicker. It's just impossible for him to do everything. So, […] there are collaborations which are because of the expertise and those are usually outside the group. But within the group […] I think it's mainly the work load. *(Martin, PhD student in molecular biology, group2)*

Magnus depends on Martin and other doctoral students to establish certain intermediate experimental results, but his dependence is translucent, because he possesses the necessary expertise to understand fully all experimental steps and, if need be, perform them himself. If he did not possess the experimental expertise, his dependence would be opaque.

7.3.3 The Gray Zone

The distinction between opaque and translucent epistemic dependence hinges upon this difference: having or not having expertise pertaining to the piece of epistemic labor at stake. It should be noted, however, that this is a gradual difference and that there is such a thing as "partial" expertise. We hence have to speak of a large grey zone that lies between opaque and translucent epistemic dependence. In the following, I elaborate on the question of what partial expertise can be and how it may play out in relations of epistemic dependence.

At least two aspects, I believe, need to be added to the distinction between opaque and translucent epistemic dependence. The first aspect concerns the role of time and learning in the acquisition of expertise which might result in it being partial. The second aspect concerns forms of scientific expertise which have not been discussed so far and which may be described as "partially enabling" and not fully fledged forms of expertise.

Regarding the first aspect, scientists gain expertise over time. This is a capacity that is acquired gradually through immersion in specialist communities and the learning processes that this immersion entails. Being in the midst of this gradual learning process, students may come to possess partial expertise. Through learning, the relationship of collaborating scientists may change over time. This is particularly obvious for student–post-doc relations in the molecular biology laboratory. The more expertise the student acquires, the less he or she depends upon others for their expertise. This requires us to consider the gray zone between opaque and translucent epistemic dependence in which the dependent scientist A possesses partial expertise relating to the piece of epistemic labor for which she is depending on B. On the trajectory from novice to expert, students pass through this gray zone, imagining their status as "half-baked" experts to be only temporary.

While the gray zone may be a transient phenomenon from the perspective of an individual student, it should not be considered as passing or unimportant. Not just students, but mature scientists as well will find themselves in the gray zone of partial expertise. While having full-fledged scientific expertise in one domain, they may have acquired some experience in other fields. To be an expert in one field does not mean one is an expert in a second field; nor does it mean one is a complete layperson either. This leads me to the second aspect that I would like to add to my discussion of the gray zone between opaque and translucent epistemic dependence.

Until now I have solely considered one form of scientific expertise. I have limited myself to the kind of expertise that distinguishes a fully competent scientist in a given research field from someone who may be considered knowledgeable, but not an expert. Following Collins and Evans' (2007), I call this form of expertise "contributory," by which I designate the kind of specialized scientific expertise that enables scientists to engage themselves fully in the scientific practice of a given scientific field—for example, to perform experiments, formulate arguments, write articles, exchange views with colleagues, etc. Collins and Evans distinguish this form of expertise from other forms which may be described as "partially enabling," such as "interactional" expertise. With the latter, Collins and Evans describe the skill needed to master the language of a scientific community, a proficiency clearly important for exchanging views with colleagues but not sufficient for making substantial contributions to the field (Collins & Evans, 2007, p. 28ff.; see also Goddiksen 2014; Gorman 2002). Contributory expertise entails interactional expertise but not vice versa.

For my purpose, the concept of interactional expertise is useful insofar as it sheds more light on the gray zone between opaque and translucent dependence. One way for a mature scientist to have partial expertise is to have interactional expertise on the question at stake (while having contributory expertise in a field that is not of immediate concern). For interdisciplinary research, some interactional expertise seems to be indispensable. As Laura, post-doc in the planetary science group, describes the issue:

> Astrobiology is a really, really good example [of interdisciplinary research] because it combines physics, geology, biology, chemistry, astronomy and you need to know a little bit about everything. And of course you have your little niche in this field, but even though you're a chemist, you still need to know something about everything else in order to be able to take what you're doing and put it into the broader perspective of astrobiology. *(Laura, post-doc in physics, group1)*

Furthermore, interactional expertise can enable the scientist to transfer parts of his or her contributory expertise to a different field, a field to which his or her expertise does not originally apply. Collins and Evans conceive of such transfers as "referred expertise" (Collins & Evans, 2007, p. 64). The authors' examples of the kind of judgments that referred expertise allows have a rather "managerial" character (Collins & Evans, 2007, p. 66). I will, however, give you an example of referred expertise as it is mobilized for genuinely scientific judgment. In my interview with Adam, I asked him how he judges the competence of potential collaborators from different fields:

> Q: OK, so what would be an example for a signal that tells you this person is maybe not competent? Not competent enough?
> Adam: Ah, if they are missing, if they are missing fundamental ahm lines of argument, if they are confusing cause and effect for example, that would be bad. And if they are vague on something which is really quite important—and if and if I explain something, something that is quite basic, and they still don't, still don't get it.
> Q: But I can imagine that this works out nicely with another physicist, but if you are talking to another let's say biologist, can you then be so sure?…
> Adam: Yeah, I think mainly the arguments are the same in that in that they should be able to explain their thing to me in a way that I understand it. And in the way they explain it, you can tell, normally, whether they understand it themselves. *(Adam, senior physicist, group1)*

Despite the fact that Adam does not possess the same expertise as his interlocutors, he can make an informed scientific judgment about them and the things they have to say by means of referred expertise, mobilizing professional experience gathered in the field he has been trained in. If he was to rely on the things they have to say for his personal knowing, he would thus not be dependent upon them in a completely opaque way.

What this discussion shows is that, as concepts, opaque and translucent epistemic dependence provide useful handles on the diversity of dependence relations in collaborative scientific practice. But as a dichotomous distinction, they tell us only one half of the story. In practice, there is a large gray zone between opacity and translucency. Within this gray zone, there are clearly instances of more and less expertise. A third-year student is not yet an expert, but will usually possess more expertise than a first-year student. I will refrain, however, from speaking of a continuum between epistemic opacity on the one hand and translucency, or even epistemic transparency, on the other. A rationale for computing the "opacity coefficient" of a given epistemic dependence relation is not to be had. The issues of expertise that are entangled with epistemic dependence are too intricate. With interactional and referred expertise I have pointed out how dependence relations in the gray zone can be understood.

7.4 Conclusion

To conclude, let me summarize what forms of epistemic dependence are observable in the two research groups I have studied. In the planetary science group, members from different fields of expertise contribute to the joint pursuit of research questions. For this reason, relations of opaque epistemic dependence are frequent. Epistemic opacity can be handled, because scientists in the group have come to know each other's professional qualities during a decade of close collaboration. Furthermore, they have acquired interactional expertise that helps them to bridge their own and foreign fields of expertise. Interactional and referred expertise can render dependence relations less opaque.

The situation is clearly different in the molecular biology laboratory with its comparatively high fluctuation rate. Here, relations of epistemic dependence have a different character. Post-docs and senior scientists, who delegate work to doctoral students and students, will usually be epistemically dependent in a translucent form. Students, in turn, may be opaquely dependent on more senior researchers, but since they perceive themselves as experts-to-be, the epistemic opacity they are confronted with may be regarded as transient. As they learn to master experimental techniques more and more independently, they gain gradually more and more expertise. Therefore, the epistemic dependence that students are involved in is mostly a gray-zone dependence—not opaque anymore, but not yet translucent either.

To conclude, I hope to have shown in this chapter how the distinction between opaque and translucent epistemic dependence helps to characterize the empirical variety of research group collaboration in scientific practice, articulating thereby how, while epistemic dependence pervades collaborative scientific practice, not all dependence runs equally deep.

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8

Epistemic Trust

Trust, in its vernacular meaning, is a blurred concept with many facets. This being the case, it should be all the more important for philosophical accounts to be selective about the aspects of trust that are to be studied and the function that it is supposed to perform. It should also be important to be domain-specific. After all, what applies to interactions between children or friends in non-professional everyday life need not apply to the professional interaction between scientists who collaborate to create scientific knowledge.

Can one know from trust? This question is particularly acute in the context of scientific knowledge creation, in which strong emphasis is placed on grounding knowledge in inferential and observational evidence and where the imperative of "organized skepticism" is supposed to reign (Merton, 1973 [1942]). Drawing upon John Hardwig's work, I argue that one can indeed ground knowledge on trust and that, in fact, scientists do so in scientific practice (Hardwig, 1991, p. 697)—a position, however, as we will see in Chap. 9, that leads me to distinguish between individual, trust-based knowing and scientific knowledge.

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In this chapter, I elaborate how epistemic trust enables epistemically dependent scientists to acquire knowledge they otherwise would not be able to have. After having introduced the notion in Sect. 8.1, I argue that epistemic trust in scientific practice has a tentative character and is accompanied by epistemic vigilance in Sect. 8.2. Thereafter, I elaborate the ways in which collaborating scientists address these issues in Sects. 8.3, 8.4, and 8.5. As these sections are grounded in empirical data about the planetary science group, Sect. 8.6 considers issues of epistemic trust for the molecular biology laboratory.

8.1 Theoretical Groundwork

The question of what trust actually *is* has long interested analytic philosophers of various fields. Moral philosopher Annette Baier suggests that trust is a three-part relation, that is, a "relationship where A has entrusted B with some of the care of C and where B has some discretionary powers in caring for C" (Baier, 1986, p. 237). In caring for C, B is supposed to act on behalf of A in contexts where A does not take care of C. As the care for C rests in B's hands, B is expected to endorse an attitude of dedicated goodwill towards A. A, in turn, has to be confident about B's goodwill (Jones, 1996).

When epistemologists analyze trust, however, they are typically interested only in a particular element of such trust relations. They are interested in A's trusting B, her expectation of care towards B. For epistemologists, *trust attitudes* are the problem; and the questions that they concern themselves with are questions of whether A is warranted in trusting B and what A can reasonably expect to gain from trusting B. Yet not only do epistemologists tend to focus on trust attitudes, they also focus specifically on *epistemic trust*, discussing its epistemic warrant and its possible epistemic function.

What is epistemic trust? It is knowledge-concerned trust. Many epistemologists who consider epistemic trust hold it to be a *knowledge-facilitating* attitude in that it can result, under certain conditions, in knowledge that without epistemic trust could not be had. Epistemic trust can enable A, who does the trusting, to acquire the knowledge *that p* by relying upon a trusted person B (see, e.g., Fricker, 2002; Goldberg, 2011; Hardwig, 1991; Kappel, 2014; McCraw, 2015). Precisely under which conditions, A is warranted to place epistemic trust in B is debated. It is, however, generally assumed that for an adult A to place epistemic trust in a person B it is necessary for A to regard B as being "epistemically well-placed" with respect to the belief in question (McCraw, 2015), a condition I have described with reference to Hardwig (1985, 1991) as B's possession of intellectual authority in Chap. 7.

Epistemic trust, too, can be construed as a three-part relation between a trusting person A, a trusted person B and a something (C), the care of which A entrusts B with. Here, C is the justification for the belief *that p*, a justification for which A relies upon B. Note, however, that B cares for C only indirectly. A's justification for the belief *that p* need not be B's primary concern. Rather, what B is supposed to care about is his intellectual authority concerning the observations, measurements, arguments or advice he passes on to A. If B testifies *that p* to A, conveying his testimony as a piece of (scientific) knowledge, then B should have a justification for the belief *that p* that conforms with those (scientific) standards of justification that apply. Yet, as I have elaborated in Chap. 7, B's justification will not be identical to A's justification for *that p*—but B's justification will be "folded into" A's justification because A relies upon B's intellectual authority.

While today's debate about epistemic trust is, generally, benign to the idea that trust can indeed result in justified believing, Hardwig began to work upon trust at a time when epistemologists were rather skeptical about its epistemic potential. This may explain why he construes trust in strict analogy to evidence gained from observation, inference or memory. Like such evidence, he argues, trust in a person B provides a "reason" for A's adopting the belief *that p* in reference to B's maintaining *that p*. In Chap. 7 I have characterized reasons of this kind as second-order reasons. Since these reasons do not pertain to "immediate"—i.e., observational or inferential—evidence for the belief *that p*, Hardwig characterizes trust as "blind" (Hardwig, 1991, p. 693).

However, that Hardwig calls trust blind must not lead to the conception that trust attitudes were de-coupled from empirical evidence concerning the trustworthiness of the person trusted. In fact, Hardwig instructively distinguishes two facets of trustworthiness that enables us to specify how and in which sense it can be empirically evidenced. According to him, the trustworthiness of a person concerns both moral and epistemic character. While moral character concerns dedication and honesty, epistemic character concerns skillfulness and expertise (Hardwig, 1991, p. 700). A trustworthy person, hence, is one who is sincere and makes as few mistakes as possible.

The belief (*that p*) whose justification is at stake in epistemic trust relations can take many forms in collaborative scientific practice. As elaborated in Chap. 7, it can be a belief based on a collaborator's testimony, asserting that "the measurement is x." Or, it can be a belief based on what a collaborator's labor implicitly conveys. By, for example, making a chemical compound available to a colleague, a scientist implies that this compound has been prepared correctly. But a prerequisite to work *with* these contributions is to form beliefs *about* collaborators' contributions and their trustworthiness.

Epistemic trust relations involve trust attitudes (A's trusting B), the epistemic status of which is debated among social epistemologists. Should one conceive of attitudes of epistemic trust as beliefs and, if so, do the standards of justification that apply to knowledge apply to them? Hardwig seems to be inclined to argue that trust attitudes can, indeed, be "known" when he argues that the trustworthiness of a collaborator is something that a scientist can rationally rely upon (Hardwig, 1991, p. 699; see also, e.g., Fricker, 2006, p. 232). Other authors have taken a different, if not opposing, stance. In rejecting Hardwig's account of trust, Klemens Kappel, for example, characterizes trust as a non-inferential disposition, a "feeling of confidence about someone or something" (Kappel, 2014, p. 2017). Such a feeling cannot and need not be evidentially justified. As a non-inferential disposition, thus, trust cannot be "known"; and Kappel accordingly claims that collaborating scientists need not, in fact cannot and "do not know, and are not justified in believing, that other agents are trustworthy within their specialised domains" (Kappel, 2014,

p. 2017). Nevertheless, Kappel argues, when epistemic trust as a non-inferential disposition "is discriminating and defeater-sensitive, it can ground knowledge and justification" (Kappel, 2014, p. 2011). Integrative accounts of trust have sought to overcome the divide that Kappel sees between accounts of trust attitudes as justifiable beliefs or non-justifiable feelings. Karen Frost-Arnold, for example, argues that trust involves cognitive attitudes that can involve beliefs, though they are to be considered broader than that (Frost-Arnold, 2014).

It matters what precisely trust attitudes are because that will tell us how trust can and should be acquired. When epistemic trust requires A merely to adopt a non-inferential disposition, then a trust can be warranted by an a priori entitlement. Burge (1993) and Coady (1992), for example, argue that trust in a person's testimony can be justified a priori. When, however, epistemic trust requires A to establish reasons for believing in the trustworthiness of B, then A has to put considerable effort into warranting an attitude of trust toward B, establishing reliable criteria for assessing B's trustworthiness (cf. Goldman, 1999; Kitcher, 1993; Origgi, 2004). Again, some authors suggest ways to integrate both positions, combining a priori entitlement and empirical warrant. Differentiating between facets of trustworthiness, Kristina Rolin argues that, while scientists are expected to establish an empirical warrant for the competence and skillfulness of collaborators, "the moral character of the collaborator is to a large extent taken for granted" (Rolin, 2014, p. 76; see also Andersen 2014; Rolin 2015). Other authors have argued that, while trust may initially rest upon an a priori entitlement, this entitlement becomes more and more irrelevant the longer a relation of epistemic dependence lasts and the trusting person A is able to collect evidence for the trustworthiness of B (Adler, 1994; Fricker, 2002).

The ambivalent, divided portrait that epistemologists draw of trust and its epistemic status leaves us to wonder how collaborating scientists deal with epistemic trust in scientific practice, that is, under conditions in which collaboration is a necessity. Do they trust by default? How much care do they take to establish the trustworthiness of collaborators? How much care do they feel they should take? These are the questions that my empirical investigation addresses.

8.2 The Tentative Character of Epistemic Trust

The interviews I conducted with scientists of the planetary science group suggest that attitudes of trust towards scientific collaborators are akin to hypothetical projections, assumptions that are in part evidenced but not satisfyingly so. The interviews reinforce the notion that trust is inevitable and ubiquitous in scientific practice, but remains, at the same time, tentative and fragile. In fact, the interviews bring a sense of uneasiness to the fore that seems to accompany trust towards collaborators. In part, this sense of uneasiness might be an interview artifact, something that is created through the interview, through the pressure the interview creates to rationalize and verbalize a behavior that most of the time goes without saying. Yet, given the way in which interviewees elaborate upon this uneasiness and the ways in which it surfaces in research practice, it stands to reason that there is, indeed, a sense in which epistemic trust is never quite fully warranted in scientific practice. The interviews do not lend support to the argument that epistemic trust among collaborating scientists is based upon an a priori entitlement. Rather, they suggest that it is a projection that is, over time, specified and adapted to the experiences of collaboration. The following quote from an interview with Laurits, the group's geologist and spokesperson, illustrates this point:

> Q: What do you look for if you work with someone? For example, if it's a new person. What do you look for to decide whether you really would like to collaborate with that person or it's maybe a bit risky?
> Laurits: Ah, that's something you find out when you start to collaborate. *You can never know it in advance.* So, it comes out of the collaboration, I would say. *(Laurits, senior scientist, group1)*

When the interview follows up on this statement, Laurits hints at some of the criteria he draws upon when he decides whether to trust and collaborate with a person in the first place:

> Q: What would signal to you that this person is maybe not good to collaborate with? What would be a signal of ahm…ah, maybe rather look for someone else?

> Laurits: That's the same when you start up with a new student that you want to have as a PhD student. You see what has the person done so far, and then you start with the collaboration. And it develops from that. And from that you get your opinion on the person. So, that you know if this is someone you would like to, ah, to have as a collaborator in the group in the future or not. *(Laurits, senior scientist, group1)*

As he sees it, "[o]ne way of getting to know people well is always if you have them as students" *(Laurits, senior scientist, group1)*. Student–teacher relationships are ideally relationships of trust as well as control. The teacher, at least in the initial stages of his or relationship, is able to assess the quality of the student's work and, in principle, need not make him or herself dependent upon the student. After all, the ultimate proof for the trustworthiness of a collaborator can only be obtained through control, by witnessing or at least tracing all the inferential steps deemed relevant to formulate a particular piece of scientific evidence. If, however, the trusting person were in a position to do so, trust would not be needed.

In the planetary science group, however, core members met relatively late in their careers. There were no teacher–student relationships between them, and so this way of trust-building was not available. In order to learn whether to trust each other, they had to invest some trust to begin with. As the interview with Victor, the planetary science group's senior biologist, suggests, trust can only be proven on the job:

> Now [in the group] I can have access to the [major experimental facility] and there is no way that I can run the [major experimental facility]. So, I would need Adam or Nikolaj or both to join to work with me on that, and for that of course I have to trust their expertise. So, if he tells me that the wind speed is ten meters a second, then—because he measures it—I have to believe that. Otherwise if, if I would doubt that, I could not collaborate with him. And that's a general problem or a challenge in that astrobiology field, because you need to collaborate with people from a different discipline. And ah well you know from your own research that you have, you have been through many years of training and I would be, yeah, of course I can question your expertise, but if you if we want to work together, I have to consider you

as an expert in your field. Otherwise it's not worthwhile. If I question the other person's expertise every time we did an experiment, we would never get started with an experiment. Of course, I can be disappointed and find out that he is maybe not the right person to team up with, but that's ah, *the only way you can find out is by giving it a try. (Victor, senior biologist, group1)*

Note that Victor focuses on expertise, trying to establish what exactly he can trust his collaborators for—gauging, in other words, the realm of their epistemic trustworthiness. Victor fine-tunes his expectations concerning collaborators' epistemic trustworthiness in the process of collaboration. He projects refined expectations of trustworthiness into the future while assessing past experiences (or, at least, hoping to be able to do so at a later point). After referee reports about a jointly authored paper are filed, after peers corroborate results independently or after having acquired more expertise, Victor explains, he may be able to asses whether or not it was right to trust. Trust, here, is built in iterative cycles in the course of which expectations of trustworthiness are met or disappointed and subsequently refined.

The difficulty of formulating adequate trust expectations is particularly prevalent in the domain of scientific collaboration. For scientists, as Adam, one of the group's physicists, points out, particular requirements apply in trust relations:

Q: […] When you work together with others who have a different expertise than you have, ahm to what extent do you at all try to check what they are doing? Find out whether what they are saying is actually …
Adam: No, we believe them, trust them.
Q: You just take it?
Adam: But then again, well, you don't as a scientist, you shouldn't. You should, you should—you should ask a question, you know, I don't understand this, explain this to me. And then they'll explain it and then if that sounds logical and reasonable, then you'll accept it obviously. But if it sounds really crazy and weird to you, then you don't accept it. And this is why sometimes you get into long discussion type arguments […]. *(Adam, senior scientist, group1)*

Adam has, as this interview fragment shows, developed strategies to probe the trustworthiness of collaborators, an aspect that I elaborate upon below. For the time being, let me note that the interviewees' perspective upon trust foregrounds their willingness to engage in epistemic trust, despite their awareness of the limited warrant that their trust attitudes have. All the scientists I interviewed are very aware of the fact that there is always risk attached to trust—the risk of being let down, either by dishonesty, incompetence or negligence. After all, even honest and competent collaborators can make mistakes without realizing their error. Even the trustworthiness of familiar collaborators is not a fact that could be established beyond reasonable doubt. Trustworthiness is a disposition, and how this disposition plays out given the contingency and difficulty of scientific practice, can never be established fully by a person that is epistemically dependent.

Since sufficient evidence for the trustworthiness of a collaborator cannot be established at the point of time when the trusting is done, trust always involves periods of uneasiness. And while epistemic trust does not seem to be something for which a satisfying warrant could be established experientially, it does not appear to be warranted by an a priori entitlement either. Rather, the trustworthiness of collaborators is assumed much in the same way as a promising hypothesis is assumed, only to be gradually corroborated.

This way of thinking about epistemic trust concurs well with the approach that Dan Sperber and collaborators (Sperber et al., 2010) have taken. In their approach, trust expectations are often provisional and the trustworthiness of others is not necessarily something one believes in or feels deeply about. Accordingly, knowledge *that p*, that one may or may not base upon others' trustworthiness, is accepted cautiously and with what Sperber and collaborators call "epistemic vigilance," a form of skepticism that does not eclipse trust but accompanies it: "Vigilance (unlike distrust) is not the opposite of trust; it is the opposite of blind trust" (Sperber et al., 2010, p. 363). Without vigilance, so their argument goes, epistemic trust would not be possible—it would lead people astray too many times.

There are various ways for scientists to address the fact that the trustworthiness of collaborators can never be completely evidenced while maintaining a vigilant yet cooperative attitude toward their collaborators. As I will argue, scientists can fine-tune expectations of trust towards collaborators, they can resort to impersonal trust or find ways to reduce the trust relations necessary through hierarchical co-authorship. Referring to fragments of the interviews I have conducted, I will elaborate on these strategies in the following sections. In doing so, I build upon the work of epistemologists such as Sperber and collaborators (2010) and Alvin Goldman (2001) who have discussed a number of ways in which epistemic trustworthiness can be empirically established. I refer to their work in the following whenever appropriate and show how it applies to collaboration in scientific practice.

8.3 Building Trust through Dialoging

With "dialoging" I seek to describe conversational practices that probe collaborators' abilities to explain and understand in order to fine-tune trust expectations and establish the realm of expertise for which a collaborator can be trusted. Such dialoging pervades collaborations of scientists among which differences of expertise, either due to interdisciplinarity or seniority, are palpable. When experimental processes, findings or interpretations seem dubious or incomprehensible, a scientist should ask questions—and when asked a question, he or she should provide an answer. Explaining and understanding, here, have to be conceived from a first-person/second-person relation; they are anchored in the exchange between me and you (Chang, 2011; Scriven, 1962).

In asking questions and having them explained, and in explaining questions and having them understood, scientists can evaluate each others' trustworthiness. In the conducted interviews, the theme of explaining/understanding is repeatedly brought up in the context of trust by different interviewees, yet most comprehensively by Adam. In the interview with him, the theme of explaining/understanding is interestingly referred to from two different angles. First, Adam talks about having a scientific question explained in discussions with other group members from different disciplines in order to "get the gist." Second, and a little later, he speaks about explaining a question himself to find out whether potential collaborators qualify as trustworthy enough:

> Adam: [. . .] I guess from, just from a conversation, I guess, you can tell whether somebody is technically competent by the way they talk about, ah you know, talk about things.
> Q: OK, so what would be an example for a signal that tells you this person is maybe not competent? Not competent enough?
> Adam: Ah, if they are missing *if they are missing fundamental lines of argument*, *if they are confusing cause and effect* for example, that would be bad. And if they are vague on something which is really quite important, and if, and if I explain something, something that is quite basic, and they still don't, still don't get it. *(Adam, senior scientist, group1)*

By talking to other scientists, Adam seeks to picture what the other person knows. But in addition to that, he gets an impression of whether or not the person he is talking to responds properly to his questions and his priorities, that is, whether this person picks up on "what is really quite important." I will call this ability "explanatory responsiveness."

With the notion of explanatory responsiveness I denote the willingness and the skill to tailor an explanation to an interlocutor's epistemic needs. Being explanatorily responsive means to possess a professional sensitivity for me–you dialoging, which provides an explanation that can be understood within the context of the epistemic background that the person receiving an explanation possesses. Explanatory responsiveness combines cognitive abilities and moral commitment, that is, like trustworthiness it has an epistemic and a moral facet. If a speaker is explanatorily responsive, she will commit herself to the listener's informational needs. She will tell her listener something that "makes sense." To do so, the speaker is required to have some knowledge about the capacity for understanding that the addressed person possesses.

Sperber et al. (2010) offer a noteworthy complement to my observations on understanding and explaining that helps to shed further light on the role of explanatory responsiveness in fostering trust. Their argument can be summarized as follows. What someone says is interpreted in light of his trustworthiness. If a hearer believes a speaker to be trustworthy right from the outset, she will tend to interpret his utterances in a way that make them acceptable to her, that is, in a way that creates coherence between his utterance and her prior beliefs. If the hearer does not hold any prior attitude of strong trust towards the speaker, her interpretation may not be guided by the wish to interpret what is said in an acceptable way. Rather, she interprets what is said in a way so as to emphasize its relevance to the communication between her and the speaker. Sperber et al. show that relevance-guided interpretation need not coincide with acceptability-guided interpretation—in fact, there might be a trade-off between the maximization of relevance and interpretative charity (Sperber et al., 2010, p. 368). Now, if the speaker shows explanatory responsiveness and addresses the listener's informational needs in the above-mentioned sense, such a trade-off is unlikely to arise and acceptance along with mutual understanding is alleviated. Mutual understanding, in turn, helps consolidate trust relations.

One might assume that it is difficult to provide explanations for scientists with different disciplinary backgrounds. But in Adam's experience, interdisciplinary dialoging is possible across disciplinary boundaries:

> Q: [...] I mean, if someone from another discipline tells you something, it necessarily sounds a bit weird, because this is a different field, they have different standards maybe and they ...
> Adam: Yeah, maybe, but but still we are normally talking about a very specific situation, a very specific thing like how do you get from this mineral to that mineral or how does this physical process work and there then although we come, we have different expertise, we have to come with an argument that is rational and logical and makes sense. *(Adam, senior scientist, group1)*

Because he is familiar with the subject matter, Adam is able to measure explanations against certain generalized expectations concerning the argumentative structure of the explanation offered (cf. Goldman, 2001, p. 93f.): does the explanation offered sound "rational and logical"? Formal reasoning is a strategy for epistemic vigilance (cf. Sperber et al., 2010, p. 376ff.), viable even when detailed, disciplinary expertise is lacking.

Formal reasoning is particularly effective because it can contribute both to second and first-order reasons, both helping to establish the trustworthiness of the speaker *and* the reliability of the content that the speaker conveys. Note, however, that formal reasoning alone provides only insufficient first-order reasons to justify a scientist in believing what his or her collaborator conveys if experimental results are at stake. In experimental scientific practice, formal reasoning cannot do away with epistemic dependence and trust.

8.4 Resorting to Impersonal Trust

Up to this point, I have focused on personal epistemic trust, that is, on trust relations between two individuals. However, a lack in personal trust can be compensated with impersonal trust, which concerns relations between a trusting person on the one hand and an "impersonal" entity, that is, a social structure, on the other (Shapiro, 1987). Impersonal epistemic trust, hence, enables an epistemically dependent person A to justify her belief *that p* with reference to the trustworthiness of a social structure.

What makes a social structure trustworthy are, for example, "a supporting social-control framework or procedural norms, organizational forms, and social-control specialists, which institutionalize distrust" (Shapiro, 1987, p. 635). An example of institutionalized distrust in scientific knowledge creation is the formalized peer-reviewing of scientific publications, which constitute a scientist's track record and are a measure of reputation. Peer review has been discussed in the context of epistemic trust and dependence before. Goldman (2001), for example, describes reliance upon peer-reviewing as a deference of judgment from a layperson, who is in no position to judge the quality of the publication in question, to an expert, who is in a better position to do so. (Yet, note that the peer-reviewing expert still faces what I have described in Chap. 7 as translucent epistemic dependence.) Despite social epistemology's concern for peer-reviewing, the distinction between personal and impersonal forms of epistemic trust has so far received only peripheral attention, a notable exception being Rolin (2002) who points out that Hardwig's account of trust is oblivious to the role communities play in establishing trustworthiness (see also Goldberg, 2010).

Impersonal trust in the peer-reviewing process can compensate for a lack of personal trust in the authors of a publication. Victor, senior biologist in the planetary science group, elaborates upon this possibility when talking about this reliance on Rasmus, one of the group's physicists:

> Q: […] Why do you think you can accept Rasmus's expertise? What is your, I mean you must have some criteria for …
> Victor: I consider him an expert in that field.
> Q: Umm. What makes him an expert?
> Victor: Yeah, the fact that he has published in the field, that he has, well, he is recognized among colleagues. That's my validation criteria.
> Q: That's the reputation part. Does it play also a role that you, you work together with Rasmus in this group? Is this …
> Victor: Yeah, of course I have confidence that he is a good person to …that I can rely on.
> Q: So, it would make a difference for you if it's someone from within the group or someone you barely know that has also a good reputation?
> Victor: No, not necessarily.
> Q: Ok, ok.
> Victor: And then the other thing is, you know when you, one thing is the work that you do here, but as soon as you submit a paper for publication, you get a review on that and if—people are usually quite picky. In particular, when it comes to multidisciplinary studies. So, ah if, if, you will get criticized if things don't stand. *(Victor, senior scientist, group1)*

In this interview fragment, Victor refers to impersonal trust in peer-reviewing mechanisms twice, regarding Rasmus's past track record (which establishes him as an expert) and regarding future feedback that Victor will receive about the quality of Rasmus's work (after having submitted a co-authored paper to a peer-reviewing publication venue).

Reliance upon peer-reviewing is one example of impersonal trust; trust in organizational membership is another. When I asked Laura, a physics post-doc in the planetary science group, about the choice of co-authors for a paper reporting on experiments conducted during a larger interdisciplinary project, she replied, not without a whiff of irony:

> If they have been associated with such a thing as a [prestigious international project], then—I could say that that actually gives you some sort of stamp saying OK, these people are not stupid, they know what they are talking about. 'Cause otherwise you don't get to do [project with renowned research organization]. That's only the best of the best. Or at least some of the good people. So you know the people associated with the [project] will be people who know what they are talking about. *(Laura, post-doc, group1)*

What underlies Laura's answer is the understanding that prestigious organizations function as gatekeepers and that organizational membership can be taken to indicate professional skill. Here again, (relative) laypeople, for example, junior scientists such as Laura, defer judgment to experts within the organization who are assumed to be in a better position to judge the trustworthiness of potential collaborators when admitting them into the organization.

8.5 Minimizing Trust in Co-authorship

Having elaborated on ways to increase personal epistemic trust and to complement it with impersonal trust, I will now discuss ways in which the need for epistemic trust can be minimized. In research group collaboration, epistemic trust can be minimized by reducing the number of trust relations that a collaboration involves. As I have observed, this can be achieved through adopting hierarchical co-authorship, making a "first author" the center of group collaboration.

In research groups that implement a hierarchy between first author and contributing authors, the number of relevant trust relations is significantly smaller than in groups where every member is accountable to everyone else. When first authors take the lead in collaborative co-authoring practices, what matters are the trust relations between first author and contributing authors. The first author will have to trust the contributing authors for the contributions they make; and contributing authors will trust the first author for his or her judgment of others' contributions and the way in which he or she integrates them into a coherent whole.

As interview data suggests, hierarchical co-authorship is the form of authorship that members of the planetary science group typically engage in. One or two first authors, "usually the driving force in the experiments" (Adam, senior scientist, group1), are accountable for creating a publishable text that is acceptable to all co-authors. Under their supervision, the paper to be written is split up into sections for which responsibility is distributed among co-authors. Each co-author is assigned particular sections, sub-sections or single important paragraphs. A co-author then either writes pieces of text him or herself or provides the first author with the information necessary to compose the respective section:

> [S]omebody writes the main part and then also comes with some ideas how the rest should look like and then he will send it to the different co-authors and they add and criticize and make adjustments, additions and so on. So, it's a collective type of work, but there is one person who has to come up with the major input. *(Victor, senior scientist, group1)*

> As a first author you start by typing up what you know and what you want in there and when you get to parts where you don't know, where you're unsure, you don't know how to conclude this, then you send what you have to other people and say: Hey, we'd like to talk something about this or that, and you ask them questions to this problem. So what do you think? Do you have an opinion on this? What should we write, what should we say? What do you think? Ahm, and they'll come with suggestions, and you'll use that. *(Laura, post-doc, group1)*

> Normally scientific papers are, can be split up quite easily into—in that you have ah you describe the experiment, so that's a section or different sections with a different experimental techniques. And then the results, there are different results from the different, different techniques. *(Adam, senior scientist, group1)*

When Victor calls co-authoring a "collective type of work," he stresses the involvement of authors beyond the contribution that they, individually, are able to make. Note that Adam and Laura, in contrast, emphasize the "modularity" of jointly authored publications, describing how one or two first authors solicit material from contributing authors who engage with the paper at large to a rather limited degree. When I ask Adam about cross-section commentaries, he replies that such commentaries can be "tricky" and he usually does not invite them.

A recurrent theme in interviewees' reflections upon the role of first authors was the necessity to "make things fit" when integrating different contributions—a first author is the "person responsible for making sure that everything fits together" *(Laura, post-doc, group1)*. First authors include additional contributions that "can be shown to be consistent" with the core of their experimental data *(Rasmus, senior scientists, group1)*. When during the writing process "something inconsistent comes up" *(Victor, senior scientist, group1)*, it is the first author's responsibility to clear such inconsistencies. Put differently, first authorship comes with the task of checking for coherence across individual contributions.

Coherence-checking, as Sperber and collaborators (2010) point out, is a way to exert epistemic vigilance; it is similar to formal reasoning and is a strategy available to epistemically dependent scientists to complement and bolster epistemic trust. The fact that a contribution is coherent with other contributions of a jointly authored paper does not in itself provide sufficient reason to justify it epistemically. Coherence alone is generally not considered to provide sufficient first-order reasons for the justification of a belief (Haack, 2009, p. 66; see also Angere 2008; Haack 2004; Meijs 2005). But the fact that a contribution is coherent with other contributions reflects positively upon the contributor and increases his or her trustworthiness.

8.6 By Comparison: The Molecular Biology Lab

Members of the molecular biology laboratory address issues of epistemic trust in ways similar to members of the planetary science group. In the lab, too, trust is inevitable for group collaboration and lab members fine-tune it through eliciting explanations and probing understanding, as well as complementing personal with impersonal trust and relying on hierarchical structures to reduce the trust relations that are necessary. Yet, given the laboratory's mono-disciplinary character and the high ratio of junior scientists among its members, lab members view issues of epistemic trust differently in some respects.

Most notably, the molecular biology laboratory leverages its hierarchical structure to address trust issues, making the group leader and subgroup leaders crucial knots in the group's trust relations. Their trustworthiness, and their ability to assess the trustworthiness of others, is crucial for the group's inner functioning and its recognition in the field. Usually, the laboratory leader "is guaranteeing the level of the group" *(Martin, PhD student, group2)*, but since the laboratory has grown so big, it is de facto the subgroup leaders who are overseeing group members' research activities. As elaborated in Chap. 7, subgroup leaders' oversight features elements of translucent epistemic dependence. Working side-by-side with subgroup members at the lab bench, they are in a good position to fine-tune their trust expectations to a person's abilities and commitment. In overseeing experimental practice, subgroup leaders and lab leaders enforce procedures and quality standards that help to establish impersonal trust in the laboratory, a resource that scientists outside the group draw upon when hiring lab members they not personally acquainted with.

Because the molecular biology laboratory is a mono-disciplinary research group, the realm of expertise of senior scientists, that is, what exactly one can trust them for, is not in question. In contrast to interdisciplinary collaboration, epistemic vigilance, here, focuses rather on the incomplete expertise of junior scientists and the moral facets of trustworthiness, such as honesty and the commitment to avoid errors of negligence. Alex, an advanced PhD student, elaborates when I ask him about trust:

> Alex: I think [trustworthiness] is very important, I think that's the most important thing in the whole field. Because I mean at the end of the day you have to convince other people that what you have done in your experiments is right, it's, you know, that the experiments have been done correctly and so on, and there is no sloppiness in there and so on. I think this is really, really important to me. And in general in the scientific field as well. The trust issue is one of the things you work everyday on by trying to convince

people of your you know ideas, of your logic, of how you do things, to really, that they understand how you work and how you see things, so.
Q: But on the other hand, on the other hand, you said like some minutes ago that you have this sort of paranoia, that you want to repeat things all the time.
Alex: Yes, that's true, it goes hand in hand a little bit, I mean the paranoia comes from the point that you wanna make sure that maybe other people are on the same page as you are as well and that you, that the science itself is like trustful in itself right, you wanna ask this question and have this kind of, build up this trust, also with a certain I would say *good sense of paranoia*. You shouldn't be you know too paranoid but you should, you should always *try to have a balance like between believing everything and follow it blindly* and, and having this kind of paranoia there, of also, sometimes just to see: can I actually trust all the people?*(Alex, PhD student, group2)*

Not all members of the molecular biology laboratory have developed such a pronounced skepticism. Martin, a less advanced PhD student, told me that he generally trusts "unless I have a particular reason not to" *(Martin, PhD student, group2)*. Still, he holds, it is necessary to have a vigilant attitude towards others—and particularly to oneself:

Even though of course it's always, *there is always some level of uncertainty* […], and that's both other people's results and also a little bit on your results, you always have to double-check yourself. But generally it is believed that the person easiest to fool is yourself. So, as I see it, you should also doubt your results. *(Martin, PhD student, group2)*

The uncertainty mentioned here has its source in the fact that experimental routines are complex and laborious. Mistakes are easily made. In fact, Martin mentions that he "burnt" himself twice when he was relying on experimental compounds that other group members had prepared, as it turned out, wrongly. In both cases, he had to repeat the laboratory work of several weeks.

In comparison to the planetary science group, epistemic trust in the molecular biology laboratory reaches more pervasively into the fabric of group relations. Because basic experimental procedures are often carried out collaboratively, trust is needed early on in collaborative research processes—not first when individually made contributions are combined in collaborative authorship. Yet, because the molecular biology laboratory carries out mono-disciplinary research, epistemic trust mostly addresses dependence that is "merely" translucent. This enables lab members to mobilize their disciplinary expertise for epistemic vigilance.

8.7 Conclusion

Epistemic trust is a necessity when epistemic labor is divided. Scientists have to trust their colleagues if they want to collaborate with them. And as much as trust comes with the risk of being let down, so does scientific collaboration put the trusting scientist in a position to probe the trustworthiness of his or her collaborators. Scientists find various ways to address the epistemic challenges associated with the necessity of trusting in collaborative scientific practice. First of all, they seek to increase trust and fine-tune their expectations of it toward collaborators through dialoging practices, eliciting explanations and probing understanding. Second, they supplement personal trust with impersonal trust. Third, they reduce the personal trust relations necessary through hierarchical modes of collaboration.

Nevertheless, epistemic trust in scientific practice remains tentative. The assumption of a collaborator's trustworthiness is a provisional hypothesis that is to be consolidated in iterative cycles of ongoing collaboration. In fact, my interview data support the notion that trust among collaborating scientists is accompanied by epistemic vigilance. In analogy to Chang's "epistemic iteration" in experimental science (Chang, 2004), moments of socio-epistemic iteration are key to understanding and managing the uncertainties of collaborative scientific practice. The rationale that underlies this iteration is that if trust is misplaced, it will show soon enough—in experimental failure, in inconsistent results, in critical referee reports.

In the midst of scientific practice, whether or not to trust is not a question that vexes collaborating scientists. Rather, for scientists, the pressing question that they confront continuously is how to make a research collaboration a scientific success—that is, how to meet the demands of scientific scrutiny, the scrutiny of collaborators, of peer-reviewing referees, and of those communities for which the epistemic fruits of this research collaboration are scientifically relevant. Against this backdrop, a philosophically most interesting question is how collaborators' individually held trust-based knowledge relates to collaboratively created *scientific* knowledge. As I elaborate in Chap. 9, scientific knowledge poses requirements that, in many instances of scientific practice, cannot be met by any one individual scientists.

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9

Collaboration and Collective Knowledge

For the two research groups studied, Chaps. 6, 7 and 8 have described how research efforts are divided among group members, how such division of labor creates relations of epistemic dependence and how such dependence relations are facilitated by trust. It was shown that it takes more than an individual effort to create scientific knowledge. Now, given that knowledge creation in much of today's natural sciences is the result of collaborative effort, philosophers need to explore whether or not scientific knowledge amounts to genuinely collective knowledge. And in fact, during recent years, diverse accounts of collective knowledge have been debated controversially (see, e.g., Andersen, 2010; Cheon, 2014; de Ridder, 2014; Fagan, 2011; Gilbert, 2000; Miller, 2015; Rolin, 2010; Wray, 2001).

In this chapter, I will focus on two exemplary accounts of collective knowledge (Sect. 9.1). I will discuss, on the grounds that previous chapters have laid, how far they apply to the previously observed scientific practice of collaborative knowledge creation (Sects. 9.2 and 9.3), a discussion on the basis of which I will formulate my own account of collective scientific knowledge.

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9.1 Approaches to Collective Knowledge

At this point, it should be clear that both the planetary science group and the molecular biology laboratory produce scientific knowledge collaboratively. But does that imply that the scientific knowledge they create is genuinely collective? Before I discuss this question, I want to mark the conceptual territory that my discussion seeks to cover.

First, I will limit myself to the question of whether collaboratively created *scientific* knowledge qualifies as "collective," leaving aside other forms of knowledge that research collaboration may produce. To distinguish analytically scientific knowledge from non-scientific knowledge is not a straightforward task, and it is not what I aim to do in this chapter. While I will make some remarks on such a distinction in Sect. 9.3, I suggest that we think of scientific knowledge in terms of the claims that collaborating co-authors make in research publications. This is not a refined analytic account of scientific knowledge, but it will help us to discuss the empirical case at hand.

Second, I will limit myself to accounts of *genuinely* collective knowledge, that is, knowledge that cannot be reduced to individual knowledge. Collective knowledge that can be reduced to individual knowledge can be, for example, shared, common knowledge that is held, in a more or less identical way, by several individual knowers. I will leave such kinds of collective knowledge aside and focus instead upon accounts of irreducibly collective knowledge, as suggested by Gilbert (1989, 1994, 2000, 2013) and de Ridder (2014). As Gilbert's and de Ridder's work exemplify two complementary approaches to collective knowledge and have, particularly in the case of Gilbert, been a major factor in the epistemological debate on the subject, I will elaborate upon their work in more detail below.

The debate on collective knowledge is strongly rooted in Gilbert's work. Gilbert (1989) formulated the concept of "group belief," a notion that has spilled over from social ontology into general epistemology and the social epistemology of science. Gilbert (2000) applied group belief to the analysis of science, explicating a sense in which scientific knowledge is genuinely collective and not attributable to individual scientists. When applying Gilbert's group belief to the analysis of scientific knowledge, the latter has typically been conceived of as a kind of belief or similar doxastic state such as an "accepted view" (e.g., in Andersen, 2010; Wray, 2001). The conception of scientific knowledge as a kind of doxastic state refers to the paradigm of knowledge as "justified (true) belief," a paradigm that underlies my reflections of collective knowledge.

When building upon Gilbert's group belief, notions of collective knowledge locate the collective quality of knowledge in its belief component. They "collectivize" *belief*. In contrast, de Ridder's (2014) notion of collective scientific knowledge locates the collective quality of knowledge in the *justification* that scientific knowledge requires. So, while Gilbert holds that group belief cannot be reduced to individual believing, de Ridder argues that the justification of collective scientific knowledge cannot be reduced to the justification that individuals have. In focusing either on the belief component or the component of justification, Gilbert and de Ridder's proposals are examples of complementary approaches to collective knowledge. In the following sections, I will discuss whether, and if so with what analytic benefit, Gilbert's and de Ridder's proposals of collective knowledge apply to the observed cases of collaborative knowledge creation.

9.2 Non-summative Belief and Joint Commitment

Following Gilbert's approach, collective knowledge is a form of group belief, characterized as a "non-summative" belief held by a "plural subject" that is constituted through the "joint commitment" of individuals (Gilbert, 1989, 1994, 2000, 2013). Let me elaborate on the conceptual elements of group belief one after another and provide a concise formulation of the condition to which Gilbert's group belief applies. My goal, here, is to probe the extent to which Gilbert's group belief applies and accounts for the two cases of collaborative knowledge creation in the research groups that I observed.

Gilbert's group belief is based on the distinction between summative and non-summative collective beliefs. A summative account holds that groups of individuals can have the belief *that p* insofar as all or most of their individual members have the belief *that p*. In other words, summative collective beliefs are reducible to individual beliefs. Non-summative beliefs, in contrast, are irreducibly collective beliefs. For these non-summative collective beliefs, Gilbert argues, it is neither a necessary nor a sufficient condition that all or most (or, in fact, any) group members individually hold the collective belief.

The observation that underlies Gilbert's non-summative collective belief is that there are cases in which individuals adopt beliefs only as a group. She argues that it can be plausible (and, for that matter, warranted) for two different groups with identical individual members to have conflicting group beliefs. She presents the example of a library committee and a food committee at a residential college which are identical in terms of membership. She argues that it is plausible for the two committees to differ in their group beliefs. All group members might individually have the opinion that college food is too high in carbohydrate; yet, while the food committee comes to adopt the group belief that college members are consuming too much starch, the library committee as a group need not hold this belief (Gilbert, 1989, p. 273; for an elaboration of this example see Schmitt, 1994, p. 261). These examples suggest that what is decisive for group belief are institutional constraints under which a group is forced to act collectively.

According to Gilbert, for a group to hold the non-summative, irreducibly collective belief *that p*, individual members must jointly express their individual willingness to let a proposition *p* stand as the belief of their collective—a condition that she describes as "joint commitment" of all group members (Gilbert, 1994, p. 245; Gilbert, 2000, p. 40). A joint commitment, then, constitutes a "plural subject" which holds the proposition in question as a collective belief.[1] Two or more individuals form a plural subject holding a collective belief if, and only if, they are jointly committed to holding this belief "as a body" (Gilbert, 1994, p. 244).

[1]The concept of a belief-holding plural subject has been met with reservation by social epistemologists. Therefore, collective belief is rephrased as a form of "collective acceptance" of a proposition. It is argued that to speak of collective acceptance does not necessitate a plural subject capable of holding mental states (cf. Schmitt, 1994, p. 262; see also Giere 2007; Hakli 2007; Matthiesen 2006; Wray 2001).

What a non-summative group belief thus demands from group members is not to adopt the group belief as individual belief, but to decide jointly with all other group members to let a certain proposition stand as the belief of their group. As members of a plural subject, individuals' actions are constrained. Their joint commitment imposes mutual obligations upon them. Once committed to others, individuals are entitled to expect other jointly committed individuals not to undermine their collective belief. Hence, a joint commitment prevents individual members from rescinding collective beliefs, since unilateral resignation will be negatively sanctioned by the collective (see, e.g., Gilbert, 1994, p. 238 or Gilbert, 2000, p. 44).

Drawing on the paradigm of knowledge as a form of belief, Gilbert's group belief can be reformulated as collective (scientific) knowledge, a line of thought which, in fact, a number of social epistemologists have pursued (Andersen, 2010; Bouvier, 2004, 2010; Rolin, 2010; Tollefsen, 2006). Let me label this notion of collective knowledge the *joint commitment account* (JC) and provide a concise formulation of the condition under which this account applies:

> According to the JC account, a necessary condition for collective knowledge to obtain is that a group of individuals is jointly committed to allowing a proposition to stand as the knowledge of their group.

The question that I will discuss in the remainder of this section is how far, and with what analytic benefit, the JC account can be applied to the observed cases of collaborative knowledge creation. This discussion will be rather critical, and I will argue that to conceive of collaborative knowledge creation on Gilbert's terms creates a misleading image of scientific practice.

Gilbert's notion of group belief and its reformulation as collective knowledge have done much to challenge epistemic individualism, reviving social epistemology's debate about the collective character of science. The conceptual appeal of her group belief is the "slack" that this notion cuts between individual and collective believing. Group beliefs, she argues, need not be reducible to individual beliefs. This notion of group beliefs seems to explain the socio-epistemic phenomena of scientific knowledge creation, such as an alleged conservatism in the adherence to scientific paradigms (Gilbert, 2000; Rolin, 2008; Wray, 2001). However, as Fagan (2011) critically points out, Gilbert's notion of group belief does not explain these phenomena particularly well. According to Fagan, accounts based on Gilbert's notion posit empirically unconfirmed explananda and fail to "identify positive counterparts (or extensions) […] which inferentially connect to features of collective belief stemming from joint commitment" (Fagan, 2011, p. 263). Joint commitment, Fagan holds, is an idle wheel (and what it seeks to explain, Fagan (2012) suggests, is better done through a web of interpersonal commitment).

My discussion of the JC account and its applicability bears parallels to Fagan's critique. Like Fagan, I have considerable doubts as to whether the JC account resonates with, let alone explains, empirical observations of scientific practice. To explicate these doubts in the following, I first revisit some of the—non-scientific—examples Gilbert proposes to illustrate regarding the plausibility of joint commitment. These examples, I argue, seem to suggest additional conditions for JC to obtain. Thereafter I spell out in more detail what the JC account of collective knowledge implies for collaborative scientific practice and discuss how far these implications relate to the empirical data I gathered.

It is important to note that when Gilbert develops her notion of group belief, she illustrates her conceptual discussion with a series of small, ad hoc examples—most of which refer to everyday situations in which a small number of people decide to endorse a statement or attitude jointly as a group. One example, the food and library committee, has been mentioned above. Other recurrent examples include a poetry discussion group which arrives at a group interpretation (e.g., in Gilbert, 2013, p. 169) and the spontaneous act of two people walking together (e.g., in Gilbert, 2013, p. 24). The range of Gilbert's examples suggests wide applicability. But in many of the examples she uses, there is an immediate pressure to decide and act on an issue as a group—while it is, at the same time, legitimate for group members to entertain private thoughts that do not coincide with the jointly accepted group view and that individual group members feel no pressure to act upon. It seems therefore as if her examples tacitly imply additional conditions necessary for JC to obtain. And it begs the question as to whether these conditions are met in collaborative scientific practice.

Gilbert and other philosophers, however, have in fact applied the notion of joint commitment to science. In the epistemological analysis of science, Gilbert's group belief, rephrased as collective knowledge, has been ascribed to different kinds of collectives in science, to specialist communities as well as to research groups. When Gilbert herself brings her account of collective belief to bear on the explanation of scientific change, she conceptualizes disciplinary peer communities as collectives bound by joint commitment to a scientific paradigm: "[…] I shall assume that, by and large, scientific communities do have scientific beliefs of their own" (Gilbert 2000; for a modified account of community-borne collective knowledge see Rolin 2008). According to Gilbert, the seemingly sudden character of scientific change and its absence over long periods of "conservative," normal science cannot be explained in terms of individual scientists. Rather, she suggests that members of a specialist community should be understood as being jointly committed to a scientific paradigm. This, she argues, explains the conservativeness of science, as individual members would face negative sanctions if they unilaterally dissented from the reigning paradigm. Moreover, it explains the sudden presence of rapid scientific change because if joint commitment is violated, then it breaks in its entirety. Peer communities, however, are not my focal interest.

In contrast to Gilbert, Wray (2007) argues that large peer communities cannot hold collective knowledge because they are unable to strike a joint commitment. According to Wray, only research groups, that is, small groups of interdependent collaborators, can do so. Yet with this argument, Wray posits an additional condition for JC to obtain—the condition of "organic solidarity," a feature of group members' relations with one another that reflects their state of mutual dependence, a state caused by division of labor. While Wray does not go very far in clarifying how organic solidarity should be linked to the collective possession of knowledge, I will seek to explicate a link between mutual interdependence and collective knowledge in the next section.

For my own discussion of whether the JC account of collective knowledge applies to research groups and their collaborative scientific practice, and particularly to the collaborative authoring of research publications, I confine myself to my comparative case study. To support the JC account of collective knowledge, I need to mobilize

(i) observations of empirical phenomena that are well accounted for by JC *and/or*

(ii) observations of empirical phenomena indicating that scientific practice complies with the implications of JC

Observations of the first type (i) should make it plausible that the JC account explains, in fact, a phenomenon of empirically observable practice. Observations of the second type (ii) should make it plausible that the JC account is compliant with actual scientific practice, that it aligns with the normative notions that are ingrained in scientific practice. Thus, while (i) reflects a concern for the explanatory value of JC, (ii) reflects a concern for its empirical applicability (see also Fagan, 2011).

Regarding (i), the data I have been able to gather do not testify to empirical phenomena that would be particularly well explained through JC. According to the JC account, a phenomenon that calls for joint commitment as an explanation would be a collectively held belief that need not be identical with those beliefs held individually by single members of the collective in question. Such a phenomenon, if it is a feature of scientific practice, should be especially observable in the case of interdisciplinary research, that is, research where individual scientists with different research backgrounds, perspectives and interests collaborate. However, in my fieldwork and my interviewing, I have not found evidence of such a phenomenon.

Instead, what the scientists I observed conveyed to me was an ethos of individual judgment and conviction—*if you are not personally convinced of something, you should not commit yourself to it.* Collaborative scientific practices should mobilize, not sideline, individual judgment so as to forestall the premature dissemination of badly corroborated knowledge claims. As Laura, junior physicist in the planetary science group, observes about collaborative authoring: "[As a contributing author] I might not be able to refer exactly to the details, but I've checked it through and there is nothing I am majorly unhappy about"—implying that if she or other authors were unhappy about a piece of research, she *should* bring it up with the first author who *should* "try to address their concerns and make sure that the article is written in such a way that people agree" *(Laura, interview, group1).*

If your scientific collaborators are not convinced of a research result, you should try to convince them, explaining your reasoning and helping them understand. As Laurits, senior geologist in the planetary science group puts it: "you have to convince other people about the stuff you know best" (*Laurits, interview, group1*). In their interviews, both Laurits and Adam, one of the group's physicists, describe an incident of disagreement which led an outside collaborator to opt out of a paper. "[I]t's sad when that happens," Adam says, "but it's kind of rare. It doesn't happen a lot—not to us, anyway. And it doesn't happen in-house." Because "[…] if we disagree with the conclusions of [collaborative experiments], then we would—we do some more measurements until we, we're sure" *(Adam, interview, group1)*. I have described this practice of collaboration as "dialoging," a practice that values "explanatory responsiveness," in Chap. 8.

Notwithstanding these dialoging practices, supporters of the JC account may still apply the notion of joint commitment to the collaborative creation of scientific knowledge if they declare individual understanding and conviction to be irrelevant, or secondary, to scientific practice. Supporters of the JC account could argue that while group members may have individual understanding and conviction, they are still bound by a non-summative group belief. But at least for the cases of collaborative scientific practice I observed, such an interpretation appears forced and the notion of joint commitment becomes, as Fagan puts it, "otiose" (Fagan, 2011, p. 269).

Regarding (ii), the data I have been able to gather do not indicate that scientific practice complies with implications of JC. According to the JC account, an indication of joint commitment would be the willingness of individual group members to endorse a view as their group view, a willingness that would have to be expressed by all involved individuals jointly. Another indication of joint commitment would be the negative sanctioning of group members who break a joint commitment. My data, however, do not lend support to any such things. In my fieldwork, I found that the professional freedom of individual group members to pursue their own interests and ideas plays a fundamental role in the functioning of the two research groups observed (see Chaps. 4 and 5). Supporters of the JC account might argue that jointly committed group members have deeply internalized the constraints that their joint commitment places upon their professional performance. But that is not what the joint commitment account, which emphasizes precisely the difference of collective belief and individual believing, is saying. Supporters of the JC account may furthermore point out that especially younger researchers are usually confined in their professional freedom and embedded in hierarchical relations. Yet Gilbert's notion of joint commitment does not capture hierarchical decision processes either. In hierarchical research groups, where senior scientists lead junior scientists, it stands to reason that the beliefs of senior scientists influence the individually held views that the junior scientists are endorsing (or are supposed to endorse). This, again, is not what a JC account is saying.

So despite the fruitful debate that Gilbert's group belief and the JC account of collective knowledge have spurred, their usefulness for an epistemological analysis of collaborative knowledge creation in scientific practice is not apparent. My empirical data provide no evidence suggesting that the JC account resonates with collaborative scientific practice, and that its application would generate explanatory benefits. Worse, in Gilbert's formulation, the JC account undercuts the practices of dialoging and explanatory responsiveness, blackening the processes that underlie, as my case study shows, the creation of scientific knowledge. As it fails to account for these processes, Gilbert's collective belief, rephrased as collective scientific knowledge, risks promoting forms of collaborative scientific knowledge creation that rely on intransparent and truncated collective deliberation and judgment (as, e.g., in Beatty, 2006; see also Beatty & Moore, 2010).

Given that the JC account of collective knowledge does not apply well to the practices of research collaboration I have studied, I will turn to the epistemic dependence (ED) account of collective knowledge in the following section. As I will show, there is, in fact, a robust sense in which the collaborative practices I have studied yield collective knowledge.

9.3 Irreducibly Collective Justification

When de Ridder (2014) formulates an account of collective scientific knowledge, he locates the collective quality of such knowledge in its justification. Scientific standards, he argues, demand a particularly qualified justification, a kind which individual scientists cannot always guarantee. If scientific justification for a piece of knowledge can only be guaranteed by a team of scientists, then scientific knowledge is collective—provided that the team in question consists of scientists who are mutually dependent upon one another and whose mutual dependence is cognitively necessary. Let me elaborate upon the details of this account.

Scientific justification, according to de Ridder, is a particularly demanding form of belief justification. The belief of a subject *S* has scientific justification "[…] only if it is properly based on a properly performed and objectively reliable process of scientific inquiry, the purpose of which was to gather evidence for the truth of *p*, and *S* understands this to be so" (de Ridder, 2014, p. 45). A scientific justification thus understood comprises a procedural requirement (i.e., the belief has to be properly based on an adequate research process) and a reflective requirement (i.e., *S* understands this to be so). While the procedural requirement need not be fulfilled by *S* and could be fulfilled by another epistemic subject, *S* has to fulfill the reflective requirement.

Both requirements are coupled to one another. For *S* to fulfill properly the reflective requirement, *S* has to be capable of obtaining first-hand information about the fulfillment of the procedural requirement. If *S* would necessarily have to rely upon the testimony of others for the information that the procedural requirement is fulfilled, de Ridder argues, *S* does not meet the reflective requirement: "[…] in relying on testimony-based justification, she [the subject] doesn't really understand in a direct way that the process of inquiry on which her belief is (ultimately) based is properly performed and objectively reliable, and that the evidence it produces indeed supports *p* [i.e., the belief whose scientific justification is in question]" (de Ridder, 2014, p. 48).

Because research is a collaborative endeavor, in many cases only a group of individual scientists is able to fulfill both procedural and reflective requirement. Yet not all collaboratively created scientific knowledge, de Ridder argues, is irreducibly collective. He confines irreducibly collective knowledge to cases of mutual and "cognitively necessary epistemic dependence" (de Ridder, 2014, p. 46). Epistemic dependence is cognitively necessary when the dependent individual is limited in expertise and when these limits are the motivation for engaging in a dependence relation. An individual that is cognitively dependent upon collaborators, de Ridder continues, is not able to fulfill the reflective requirement of scientific justification.

De Ridder distinguishes cognitively necessary epistemic dependence from dependence that is merely "practically necessary" (de Ridder, 2014, p. 46), a form of dependence that arises when scientists delegate parts of the research process to others, not because they lack the expertise, but because they lack the material resources to perform them. Sometimes, de Ridder observes, "the evidence needed to substantiate a conclusion is too much for any individual to collect and process. It is not that doing so requires skills or expertise that no single individual has; it is purely a matter of time" (de Ridder, 2014, p. 46). Yet in such cases, de Ridder argues, individual scientists can access the evidence after it has been created, provided it is properly conserved. Given the necessary expertise, practically necessary dependence does not hinder scientists in understanding how a piece of research is properly based upon an adequate process of inquiry. Hence, de Ridder argues, practically dependent scientists can fulfill the reflective requirement and have scientific justification individually.

When scientific justification can only be had collectively, that is, when collective scientific knowledge obtains, an individual can have collective scientific knowledge "derivatively" (de Ridder, 2014, p. 48). Individuals, de Ridder points out, can appropriate collective scientific knowledge into their individual knowing on the basis of testimony. Testimonial justification, however, falls short of scientific justification.

For the purpose of this chapter, let me label de Ridder's notion of collective scientific knowledge the *epistemic dependence account* (ED). On the ED account, in de Ridder's formulation, collective scientific knowledge applies under the following condition:

> According to the ED account, a necessary condition for collective scientific knowledge to obtain is mutual, cognitively necessary epistemic dependence among a group of individuals that allows only the group to satisfy the requirements of scientific justification for a proposition *p*.

This condition for collective scientific knowledge can be translated into the terminology that I introduced in Chap. 7. There, I developed the notions of opaque and translucent epistemic dependence. While opaque dependence is cognitively necessary, translucent dependence is not. I have also developed notions of first and second-order reasons, a distinction that can be linked to de Ridder's argument as well. De Ridder argues that for a subject to have scientific justification for *p* means to have sufficient "non-testimonial evidence" (de Ridder, 2014, p. 48) to assume that *p* is properly based on an adequate process of inquiry. Put differently, first-order reasons can constitute a scientific justification as they pertain to immediate, that is, non-testimonial, evidence for *p*. Second-order reasons, in contrast, concern the trustworthiness of a speaker and justify reliance upon a speaker's testimony *that p*. Second-order reasons may be admissible in the justification of individual knowing, but they cannot constitute a scientific justification. Interpreting de Ridder's ED account with this terminology, we can reformulate the condition for collective scientific knowledge as follows:

> According to the ED* account, a necessary condition for collective scientific knowledge to obtain is mutually opaque epistemic dependence among a group of individuals that allows only the group to provide those first-order reasons that the scientific justification for a proposition *p* requires.

For a discussion of whether the ED* account of collective knowledge applies to research groups and their collaborative scientific practice, and particularly to the collaborative authoring of research publications, I will again confine myself to my comparative case study. To support the ED* account of collective knowledge, I need to mobilize

(i) observations of empirical phenomena that are well accounted for by ED* *and/or*
(ii) observations of empirical phenomena indicating that scientific practice complies with implications of ED*

Drawing on analyses in previous chapters, I argue that the ED* account explains, in fact, an empirically observable phenomenon. It explains the predominance of collaborative authorship, itself a reflection of the division of labor in research groups. Group members collaborate with one another on a day-to-day basis, and I have characterized their collaborative relationships as help, delegation and deference, and I have argued that these relationships give rise to epistemic dependence, some of it opaque epistemic dependence. Yet, ED* implies that in the absence of mutually opaque epistemic dependence, collaboratively created scientific knowledge is not collective—an implication that I will take issue with in the remainder of this section.

The ED* account follows de Ridder's ED account in limiting collective knowledge to cases of mutually opaque (or, as de Ridder phrases it, "cognitively necessary") epistemic dependence. De Ridder maintains that:

> For cases where teamwork is a practical necessity, it could be argued that once the evidence is all collected, properly stored, and made accessible, individuals can have it non-testimonially. (de Ridder, 2014, p. 47)

I want to highlight two issues with this argument, one analytic and one concerning its relevance for the analysis of scientific practice. First, even when teamwork is a mere "practical necessity" and evidence is "made accessible," an individual scientist cannot have scientific justification entirely non-testimonially. In contrast to de Ridder, I maintain that even for a scientist with the necessary expertise, mere access to stored evidence is not sufficient to be certain enough about the way in which a piece of evidence is based on experimental inquiry. If a scientist has not eye-witnessed the experiment, she cannot be sure that the experiment has been performed correctly and its results reported truthfully. An unexpected result may catch her eye and raise skepticism, but an expected yet inferior result, accompanied by proper documentation, may not. Hence, if she has not eye-witnessed the experiments, even the scientist with the necessary expertise will have to rely upon the trustworthiness of her collaborator—that is, in justifying her belief in the scientific quality of the evidence in question, the scientist will have to rely upon second-order reasons. She alone cannot provide a scientific justification based on the experimental evidence provided by a collaborator. Therefore, contrary to de Ridder, I argue that collective knowledge does apply here.

Second, even when evidence *could* be had non-testimonially, my empirical observations suggest that, in actual practice, scientists often choose to let their epistemic dependence stand. They do not, whenever possible, eye-witness collaborators' experiments or reproduce their experimental results—time is too scarce, and experimental resources too expensive. This holds particularly for the molecular biology laboratory where single experimental steps can quickly cost several hundred, if not several thousand, Euros. So even though the laboratory is a mono-disciplinary research group where epistemic dependence is often translucent, scientists do not necessarily seek to minimize dependence and reliance upon testimony.

When I asked Johan, the lab leader, whether he controls or checks the scientists in his group, he replies: "I mean, as a mentor, right, you do have an influence on people as a mentor." In the interview, he also finds it important to assure me that for the research in his lab he knows exactly what the "criteria for good scientific conduct" are. He furthermore mentions that he has a "thorough" understanding of the journal articles that he co-authors as a lab leader and collaborator *(Johan, interview, group2)*. He does not claim, however, that he typically makes a point of eye-witnessing or reproducing experimental procedures. And neither do those lab members who allot more of their time to work at the lab bench. When I ask Alex, who has just finished his dissertation, whether he repeats experimental steps that a close collaborator of his performed, he answers:

> I mean for sure one is controlling each other as well within a group. I think that's also important, because there is always and is always gonna be people doing mistakes, if they are made just by chance or if it was really willing to do the mistake, it's always gonna happen, because there is so much pressure, right. So, it's important that you also internally have kind of a control system. However, this is more I would say on a base like *random control* and so on, yeah. And generally I mean it's not a big issue I would say within the group, because you know the experimental set-up, you follow them in group meetings and so on, so you know what they're doing. *(Alex, interview, group2)*

Collaborators, as Alex sees it, do control one another. But they do not do so all the time since, he continues, "you gonna easily figure out if they're constructing data" *(Alex, interview, group2)*. Much more difficult to detect than fraud and plain failure are experimental results that could have been better, for example, more precise or comprehensive, that maybe could have yielded serendipitous insights—if experimental routines had been handled with more skill, care or patience:

> […] at the end of the day you have to convince other people that what you have done in your experiments is right, it's – you know, that the experiments have been done correctly and so on, and there is *no sloppiness* in there and so on. *(Alex, interview, group2)*

Even adequately stored data need not show "sloppiness." And even scientists with the necessary expertise often choose to remain epistemically dependent upon their collaborators, not fulfilling the reflective requirement of scientific justification.

A robust notion of collective scientific knowledge should reflect these realities of scientific practice—the shortness of material resources and time, the pervasiveness of dependence and trust. Collective scientific knowledge should account for a broad range of de facto situations. It should not put too much emphasis on the available, yet unrealized possibility of epistemic independence. Therefore I suggest an account of collective scientific dependence, ED**, that is not limited to opaque epistemic dependence but encompasses cases of translucent epistemic dependence as well:

> According to the ED** account, a necessary condition for collective scientific knowledge to obtain is mutual epistemic dependence among a group of individuals that allows only the group to provide those first-order reasons that the scientific justification for a proposition p requires.

One may object to the scope of the ED** account, arguing that according to it most collaboratively created scientific knowledge should be considered collective—a conclusion that many social epistemologists are uneasy about. De Ridder, for example, anticipates the objection that his ED account would "overgeneralize," an objection he defends himself against by emphasizing how limited the conditions for collective scientific knowledge are if mutual cognitively necessary epistemic dependence is required (de Ridder, 2014, p. 51).

But why does a broad notion of collective scientific knowledge appear objectionable? Because it challenges individualism, a stance with a strong foothold in much of epistemology (as noted in, e.g., Fricker, 2006; Grasswick, 2004; Kusch, 2002). Epistemological individualism focuses on the individual as a primary epistemic subject. What is more, as a value, individualism transports the ideal of epistemic self-reliance, that is, the ideal of individual knowing that would not need to resort to the testimony of others. De Ridder defends this ideal, not sweepingly but discreetly, when he argues that scientists whose epistemic dependence is practically necessary could be epistemically independent—that they are, in fact, often not is an empirical insight about actual scientific practice that he is either not aware of or deems irrelevant.

With ED** I propose an account that recognizes the outcome of much of collaborative scientific practice as collective scientific knowledge, a position that I suggest calling "inter-individualism." While I provide a sense in which collaboratively created scientific knowledge should be considered collective, my approach foregrounds the individual epistemic efforts that scientists undertake in collaborative scientific practice, highlighting the inter-individual exchanges that scientific collaboration consists of.

9.4 Conclusion

In this chapter, I have considered the question as to whether, and in which sense, collaboratively created scientific knowledge can be considered irreducibly collective. I have based my consideration upon the empirical insights into the two research groups I have studied. On the basis of these insights, I have argued that the *joint commitment approach* to collective knowledge does not apply well to collaboratively created scientific knowledge. Instead, I have suggested pursuing the *epistemic dependence approach* to collective scientific knowledge. I have argued that scientific knowledge is collective when mutual epistemic dependence prevents individual scientists from providing a scientific justification for the piece of knowledge in question. In formulating this notion of collective scientific knowledge, I have built upon the work of de Ridder (2014).

In contrast to de Ridder, however, I do not confine collective scientific knowledge to cases of mutual epistemic dependence that are opaque. As a consequence, the notion of collective scientific knowledge that I propose (ED**) is a rather broad notion, applicable to much of collaboratively created scientific knowledge—only such a notion, I believe, acknowledges the collective dimension of actual scientific practice.

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10

Concluding Remarks

In this book, I have sought to provide a comprehensive, empirically grounded account of the collaborative creation of scientific knowledge in research groups. I hope to have contributed to a detailed understanding of the collective character of science, an understanding that reflects actual scientific practice and the perspectives of practicing scientists.

In Chap. 2, I outlined what I meant by "research groups." Having outlined the subject of the book, I then described my methodological approach to this subject in Chap. 3, reflecting on the role that qualitative empirical data can play in philosophical theorizing and in detailing the process of data collection and analysis upon which the empirical elements build. In Chaps. 4 and 5, I portrayed the two research groups that I studied empirically. In Chap. 6, I analyzed the ways in which these two groups divided scientific labor among their group members, and I argued why their division of labor is not well accounted for by existing community-focused approaches.

This book's most important conceptual contributions are in Chaps. 7, 8 and 9, where I explored how detailed qualitative data can contribute to ongoing epistemological debates about dependence, trust and collective

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knowledge. In Chap. 7, I provided two analytic distinctions—between first and second-order reasons, and between opaque and translucent epistemic dependence. First-order reasons are immediate, evidential reasons to accept a proposition *p*. Second-order reasons, instead, concern the trustworthiness of a speaker who testifies that *p* is the case. When a person has only second-order reasons at her disposal, her epistemic dependence upon the testifier is opaque. When she is in a position to acquire first-order reasons, then her epistemic dependence is translucent. In Chap. 8, I showed how epistemically dependent scientists gauge the trustworthiness of collaborators, that is, by which strategies they acquire and corroborate second-order reasons, and how they seek to minimize their reliance upon such reasons. In Chap. 9, I argued that mutual epistemic dependence allows only groups of scientists to provide a scientific justification for collaboratively formulated knowledge, rendering some scientific knowledge irreducibly collective.

When writing this book, something, that I at first perceived as a peculiar tension, was underpinning my reflections on research group collaboration—the relation between an ethos of individual freedom and collaborative commitment, between the pursuit of individual research interests and the necessities of collaboration. I perceived this relation as a tension because I looked upon it alternately through the lens of epistemic individualism and the lens of its critique (as presented, e.g., in Fricker, 2006; Grasswick, 2004; Hardwig, 1985, 1991; Kusch, 2002; Nelson, 1995).

Eventually, however, I came to see that collaborative scientific practice intertwines the individual and the collective. Professional autonomy, understood as the volitional autonomy to pursue individual research interests, often requires scientists to make themselves epistemically dependent upon collaborators. For example, in my interview with Claire, senior scientist in the molecular biology laboratory, she emphasizes that she feels "very independent" in the sense that she "can do what she wants." At the same time, she describes herself as highly dependent upon collaborators in daily laboratory practice *(Claire, interview, group2)*. Adam, senior scientist in the planetary science group, expresses a similar view when he underlines that all of his fellow group members have different research interests, but that they "[…] would miss out on a lot of research" if they worked alone *(Adam, interview, group1)*. For group members to collaborate and depend upon one another, they need to make themselves, their labor and their expertise available to one another—becoming a "knowledge base" for fellow group members *(Laura, interview, group1)*.

Which conclusions should epistemology draw from such observations? Do they undermine epistemic individualism, or do they vindicate it? Epistemic individualism, in its pure form, upholds the ideal of the autonomous, self-sufficient, individual knower. Its critics maintain that the ideal of individual epistemic self-sufficiency *should not* be realized, arguing that "[i]f rational at all, she [the autonomous knower] would not be ideal, but rather a paranoid sceptic about others' intentions and capacities" (Fricker, 2006, p. 243; cf. Foley, 1994). The inter-individual account of research group collaboration that I have developed in this book seeks to navigate past the ideal of the self-sufficient individual knower while avoiding the devaluation of individual knowing. For, what would science be without its competent, reflective practitioners? Do they not deserve to be called genuine "knowers"? Don't they have a responsibility to "know what they are doing"? I think so. Yet at the same time, my account of research group collaboration recognizes that the kind of scientific knowledge that research groups produce collaboratively often cannot be had by group members individually—not *as scientific knowledge*, that is, justified first-hand according to scientific standards. However, when its scientific justification cannot be had by any single scientist, scientific knowledge is irreducibly collective knowledge.

The question as to which conclusions philosophy should draw from empirical observations touches upon a more general issue: What is the role that descriptive accuracy can have for philosophical theorizing, normative efforts that typically aim at analyzing what "ought to be (done)"? After all, what "is," however accurately described, does not tell us what "ought to be." Still, descriptive accuracy is of value to philosophy, particularly to social epistemology and philosophy of science, which seeks to assist practicing scientists and help explicate what scientific practice actually is and what it means to practice science well.

Epistemology's core concern is for the acquisition of knowledge: How can we know? On the one hand, this concern is tied to conceptual challenges in defining what "knowledge" is (or in defining different types of knowledge and their respective characteristics). On the other hand, this concern is tied to questions that require prescriptive answers: What should we do to obtain knowledge? And, for the case of scientific knowledge, what ought good science be like?

Addressing some of social epistemology's conceptual challenges, I have suggested ways to differentiate, contextualize and modify some of the field's existing terminology in, for example, Chaps. 7, 8 and 9. And while I have refrained from formulating an outright prescriptive account of scientific practice, I do think that the book contributes to the question of what good science ought to be like, because any reasonable, practically relevant, philosophical account of what scientific practice "ought to be" requires a terminology appropriate to the subject matter—and it has to rely upon an understanding of actual scientific practice which is descriptively accurate and tailored to philosophical concerns.

How precisely prescriptive accounts relate to description is a difficult issue. At this point, let it suffice for us to focus on the relation between prescription and the description of capacity, a relation that can be phrased as "ought implies can." Whether or not ought does imply can is much debated, particularly among ethicists (for an overview, see Vranas, 2007). But it stands to reason that for the kind of "ought" that social epistemology and philosophy of science are interested in, "ought implies can" should hold true. If concerned with prescriptive questions, social epistemologists and philosophers of science typically investigate how the creation or acquisition of scientific knowledge can be *ensured*. For as this kind of agency-concerned "ought to ensure" (as distinguished from "ought to be"), Streumer (2003) shows, "ought" does indeed imply "can." In addition, more arguments have been made to support a relation between prescription and description. For example, Schleidgen and collaborators distinguish between abstract prescriptive principles and rules of practice that convey a "practical ought," arguing that such rules of practice need to take into account the various constraints—cognitive, financial, etc.—that human agents face in given situations (while abstract principles need not). Empirical insight in such constraints, the authors hold, can "help *adapting* basic principles to the capabilities of human agents" (Schleidgen, Jungert, & Bauer, 2010, p. 67). Empirical insight, the authors continue, also help to evaluate whether practice is in accordance with prescriptive rules. But for empirical insights to fulfill this function, descriptive accuracy is necessary. Intuition, imagination or eclectic first-hand experience won't do here; comprehensive data about actual scientific practice are necessary.

Therefore, I have collected qualitative empirical data through participant observation and interviewing. These data provide an account of what scientists actually do in day-to-day collaborations, and what they think they can and what they themselves believe they should do. The methodological approach I have chosen is, I believe, a step in avoiding prescriptive accounts of science that are too demanding, too idealized or too "embellished" (Soler et al., 2014, p. 14) to gain traction with actual scientific practice. It is also, I hope, a step toward avoiding an epistemology that is stylized as "science of science" and adopts "a theoretical position 'outside' and 'above' scientific practices" (Rouse, 2002, p. 180). Instead, I have sought to formulate an epistemology of science that acknowledges the experience and the professional reflection of practicing scientists.

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