Scientific Collaboration and Collective Knowledge

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Introduction

In 2001, nearly three thousand scientists coauthored a single paper for the journal *Nature*. Although the sheer number of coauthors was unusual, the collaborative nature of the project from which the paper stemmed was not: over the last century, the size, complexity, and frequency of collaboration in the sciences have increased rapidly. For example, hundreds of scientists contribute to the intricate climate models from which environmental policy is derived. No single researcher has the empirical knowledge, mathematical training, and programming abilities to construct the models of clouds, precipitation, glacier movement, etc. that are employed in such climate studies. Similar remarks apply to large-scale projects in particle physics and the biological sciences including, for instance, the human genome project, which produced the above-mentioned paper in *Nature*. Not less importantly, collaboration is not instantiated in very large groups only: small collaborations are ubiquitous in the sciences. Overall, single-author papers have become uncommon nowadays [Wuchty et al., 2007].

Despite its growing prevalence and importance for decades, only in the last few years has there been a growth in philosophical work analyzing collaborative research in the sciences.[1] What are the benefits and costs of such collaborations, and are current practices for encouraging collaborations optimal? How should credit for discovery and responsibility for error be attributed to members of a lab or co-authored paper? How ought collaborating scientists summarize their findings if they disagree about the interpretation of their results? The papers in this volume attack these difficult questions, among others.

Although some contributors employ empirical research,[2] the ensuing papers contain no new data from journal databases, no new surveys of scientists, and no novel analyses of current trends in memberships to scientific societies. Instead, contributions attack *conceptual* questions about the ways in which scientific collaborations might take place, and *normative* questions about how scientific institutions ought to be organized in light of such possibilities. For this reason, this volume is a natural extension of research in philosophy of science and the growing field called "social epistemology." Although primarily philosophical, the questions addressed in this volume are important not only for philosophers, but for scientific policy-makers and for social scientists who are interested in what data *ought* to be collected so as to advance our understanding of scientific practice and how it ought to be improved.

[1] Some recent works include: [Andersen, 2014a,b, Andersen and Wagenknecht, 2013], [Boyer-Kassem and Imbert, 2015], [Fallis, 2006], [Frost-Arnold, 2013], [Galison et al., 2003], [Rolin, 2015], [Thagard, 2005], [Winsberg et al., 2014], [Wray, 2002, 2006], [Wylie, 2014, 2015].

[2] See Bruner and O'Connor's contribution, for instance.

How do contributors address these conceptual and normative questions? Several authors represent scientific communities by mathematical models, and they study these models either analytically (Bonnay and Strevens) or through computer simulations (Angere and Olsson, Bruner and O'Connor, and Zollman). Other authors (Huebner, Kukla and Winsberg, and Wray) provide detailed analysis of case studies of past and current scientific collaborations, thereby highlighting the complexities of contemporary scientific practice. Many contributors (especially Bruner and O'Connor, Muldoon, and Martini and Sprenger) employ a somewhat hybrid methodology of analyzing large-scale, statistical trends in scientific practice and then proposing abstract models to explain those trends. All contributors, however, share the method of employing normative theories from epistemology and philosophy of science to draw morals about existing collaborative research, possible patterns of collaboration, and how scientific institutions can be improved.

The virtues and vices of case-study analysis, formal modelling, and philosophical analysis are well-known.[3] Here, we briefly discuss the virtues of these methods as they are employed by authors in this volume. One virtue of the formal models in this volume is their generality: they apply to many types of collaborations, even ones among non-scientists. For instance, Strevens presents a "waiting-time model of discovery" that assumes very little about how often scientists make discoveries. In principle, the model could be applied to any group engaged in inquiry, whether empirical or not. As a second example, Bruner and O'Connor's model explains why minorities might receive less credit for their contributions to co-authored scientific papers, but there is nothing in principle that prohibits their model from being applied to cases of credit-sharing in collaborations in the humanities.

Because these formal models are quite general, they do not represent many important features of scientific communities. The case-studies investigated in the Part III of the book, in contrast, are extremely rich and highlight the difficulties of assigning credit and blame in "radically collaborative" projects in science. Huebner, Kukla and Winsberg consider the case of modular computer simulations in contemporary climate science, as well as multi-site clinical trials in biomedical research, in which no single scientist can vouch for the reliability of all of the group's findings. Wray studies policies of medical journals and of the National Science Foundation. A central virtue of these case studies is that, by illustrating the complexity of large-scale scientific collaborations, they indicate the inadequacy of existing concepts for describing scientific research and the limitations of existing norms for assigning credit for discovery and blame for misconduct. These contributions, therefore, force us to extend our current vocabulary

[3]On case study analysis, see [Burian, 2001], [Chang, 2011], [Currie, 2015], [Kinzel, 2015], [Morgan, 2012], and [Pitt, 2001]. On the use of formal models, see [Salmon, 1978, Salmon and Salmon, 1979]'s criticisms of formal models in the social sciences, for example. For a defense of mathematical philosophy, see for instance [Leitgeb, 2013].

and norms to understand and improve contemporary scientific practice.

Regardless of methodology, contributors to this volume study scientific groups of various scales and natures: a "group" might refer to the whole scientific community, a research program, a university department, a peer-review board, or a specific collaboration of a few scientists. Huebner, Kukla and Winsberg even discuss cases in which non-academics are involved in scientific collaborations.[4] Thus, the contributions to the volume take a first step towards understanding the rich philosophical issues surrounding collaboration that arise at all levels of scientific practice, from data collection to abstract theorizing.

This volume is part of the larger "social turn" in philosophy of science and epistemology.[5] Throughout much of the twentieth century, many philosophers of science studied properties of scientific theories and neglected the social nature of science. Central research topics included explanation, confirmation, probability, and causation, for instance. Similarly, although epistemologists developed thought experiments with human protagonists, these philosophers were interested in evidence, justification, and knowledge in the abstract, potentially divorced from any human subject. In both epistemology and philosophy of science, epistemic norms, either for the everyday individual or for the working scientist, were often understood as descriptions of the mental life of an isolated, stoic, ideally rational agent.

Starting with Kuhn, the focus on the individual, ideally rational scientists started to erode. In the ensuing decades, philosophers of science and epistemologists began to investigate the serious philosophical issues that arise from the social nature of science. Collective knowledge [Nelson, 1990], trust within large scientific communities [Hardwig, 1985, 1991], and testimony [Lackey and Sosa, 2006] became central issues of philosophical concern. As the contributions to this volume show, investigating the practices of collaboration in sciences raises a new rich set of philosophical questions about communication, collective responsibility, authorship, and more.

The chapters of the volume tackle various problems in relation to scientific collaboration and group knowledge. We summarize the key points for each part of the volume.

[4] Collaboration among scientists and non-academics is becoming more common in archaeology, where participation of indigenous communities has been tremendously fruitful. See [Wylie, 2014, 2015].

[5] Longino [2006] recognizes, in fact, that it is better to call this movement a "social *re*turn" to emphasize that the early logical positivists had likewise emphasized the social nature of science. For example, [Hahn et al., 1929] write: "The scientific world conception is characterized not so much by theses of its own, but rather by its basic attitude, its points of view and direction of research ... The endeavor is to link and harmonize the achievements of individual investigators in their various fields of science. From this aim follows the emphasis on collective efforts ..."

Part I: Sharing Knowledge

What should scientists communicate to which others? This is the central question of the part one of this volume. Strevens invites us to consider a radical strategy that stems from the "communist" norm of the sociologist of science Merton: share everything with everyone. Angere and Olsson consider the alternative proposal that scientists ought to limit their communications to information that is above some threshold in quality.

Strevens' surprising conclusion is that Hobbesian scientists, who only care for their individual benefit, should be willing to sign a contract for total sharing of "all pre-publication information unconditionally", and that this contract is the best possible one on information sharing. Thus, he provides a "Hobbesian vindication" of Merton's communist norm, which partially explains why the norm does and should prevail. Angere and Olsson, in contrast, vindicate the intuition that it is in general better for inquirers to refrain from sharing low-quality information, which "pulls the entire research network away from truth." On the other hand, their finding that there are cases in which not communicating at all prior to voting is the best option may strike the reader as surprising.[6]

These chapters raise a number of questions for future research. Let us mention a few ones, in addition to the ones that the authors already indicate. Strevens assumes that sharing information is free. Thus, it would be interesting to investigate whether Strevens' results still hold if information retrieval were costly, as the models of Angere, Olsson, and Zollman suppose. Another question for Strevens' important result is how the Hobbesian contract can be implemented in practice. How should we respond to free-riders, who benefit from the sharing of others, but do not share themselves? How could or should the contract be enforced?

Angere and Olsson assume that each scientist's sources (i.e., the other individuals in the research network) are independent. This is obviously an idealization. Scientists within a common research community share methods and knowledge — sometimes even raw data or material. Consequently, sources of error and evidence are likely to be correlated in some sense. So the next step coul be to study how Angere and Olsson's model extends to cases in which informational sources are dependent.

[6] Arguments for such a conclusion are not new, however. In Book III (Chapter 2) of *Of Social Contract*, Rousseau argues, "If, when the people deliberates and is adequately informed, the citizens were to have no private communication among themselves, the general will would always result from the large number of small differences and the deliberation would always be good" [Rousseau, 2012]. Similar conclusions have been defended on the basis of Condorcet's jury theorem.

Part II: Forming Collaborations

Why do scientists collaborate and how can we encourage more fruitful collaborations? This is the central question of the papers by Muldoon and Zollman.

Muldoon begins by discussing a limitation of some existing mathematical models of the "division of cognitive labor" in science: paradoxically, some models fail to recognize that scientists have different skill sets and that researchers often choose projects based upon what skills they currently have. Muldoon argues that, by combining this simple fact with the observation that acquiring skills is costly, we can better explain the rapid rise of collaboration in the sciences, the social structure of evolving collaborative networks, and new trends in science, such as disciplinary "colonization." As an example of "colonization", Muldoon explores how physicists are in increasing demand in biology, where the mathematical training of physicists is in high-demand.

Zollman takes a first step in addressing Muldoon's challenge to develop a formal model in which scientists have different skill sets and different information. He assumes that scientists possess different "conceptual schemes", and in order to solve a given problem, scientists may need to employ the schemes of others. For simplicity, Zollman models "one-way" collaboration, in which one scientist borrows another's conceptual scheme at a cost. Given this setting, Zollman characterizes the optimal structure for a scientific community and argues that, surprisingly, it can be counter-productive to encourage scientists to seek out new collaborators, as doing so may lead to failure of certain conceptual schemes to be transferred widely.

Muldoon and Zollman's arguments raise interesting issues for philosophers and scientific policy-makers alike. For example, in Zollman's model of collaboration, scientists share their own way of doing research voluntarily. But as Strevens discusses, there are sometimes strong incentives against sharing information in the sciences. So how would Zollman's results be changed if a system of incentives were created for the scientists to share their conceptual schemes? And if Muldoon is right that disciplinary "colonization" is unavoidable, how could scientific institutions best foster fruitful collaborations for researchers who move into scientific fields with very different conceptual schemes?

Part III: Authorship and refereeing in collaborative research

The third part of the volume investigates authorship, refereeing, credit, and responsibility in collaborative scientific research. When several scientists sign a paper, is each of them an author in the same sense they would be for a single-authored paper? Do each of the authors, or at least one, have to agree with each claim made in the paper? Or can some claims be endorsed only collectively, and by no one individually? If credit is shared for co-authored papers, what would a fair sharing between collaborators be? Or, how would other credit sharing rules give incentives for other patterns of collaboration? These are the types of questions addressed by Huebner, Kukla and Winsberg, Wray, and Bruner and O'Connor,

Huebner, Kukla and Winsberg distinguish between collaborative research and multiple authorship, and put forth the case of radically collaborative research, "that is distributed widely over disciplinary expertise, time, and space", like in some climate modeling projects that are modular in their development. They argue that this distributed collaboration threatens our common understanding of authorship because many of the methodological choices made by scientists cannot be checked by their collaborators, and yet those play an indispensable role in the final result. The work by Huebner, Kukla, and Winsberg suggests to consider the roles of values in shaping these choices, making a link with classical discussions on this topic.[7]

Like Huebner, Kukla and Winsberg, Wray considers the question of the adequacy of the traditional notion of authorship in collaborative research, for instance in cases where hundreds of scientists co-author a paper. After analyzing the authorship criteria that some institutions and journals have recently adopted, he criticizes the requirement that authors identify their contributions. Finally, Wray investigates whether current refereeing practice is well-suited for (large) collaborative works. If the skills of hundreds of scientists were necessary to produce some piece of research, and if no individual scientist can justify the group's conclusions, what kind of assessment can an individual referee provide? Wray suggests that one might consider referee-teams, composed of individuals with various skills that enable them to *collectively* evaluate a paper or a grant proposal.

Bruner and O'Connor investigate authorship and credit in increasingly diverse scientific research teams. Substantial research has defended the value of *methodological* and *ideological* diversity.[8] That is, there is ample empirical and theoretical evidence that, when scientists address the same questions using different methods, background theories, and values, their results are more accurate and serve the interests of a greater number of scientists in their communities. Diverse ideas and methods, of course, often originate from diverse people; such is the feminist insight that the inclusion of minorities in research improves the products of science.[9] Nonetheless, few philosophers have studied how diversity might affect the attribution of credit in scientific communities with power inequalities. Bruner and O'Connor tackle this difficult issue. Using several evolutionary game-theoretic models, they show why minorities might receive systematically less credit for

[7]See [Douglas, 2009], [Rudner, 1953].

[8]See [Hong and Page, 2001], [Kitcher, 1990], [Longino, 1990, 2001], [Mayo-Wilson et al., 2011], [Weisberg and Muldoon, 2009], [Wylie, 2012], and [Zollman, 2010].

[9]See [Intemann, 2009, 2010], [Longino, 1990, 2001], and [Wylie, 2003, 2012].

their contributions if they must negotiate with collaborators for first-author or principal-investigator status. They find similar results for collaborations within existing power hierarchies, like those between a senior researcher and her graduate students. Their results explain existing empirical findings about inequality in first-authorship status, and perhaps equally importantly, their results point the way to rectifying the problem.

All three chapters in Part III raise important worries that the traditional notion of authorship faces in the age of collaboration. They consider possible remedies, but problems remain. Thus, journals and editorial boards would benefit from further investigation of the best refereeing practices for collaborative works. More generally, given the role credit and citation has acquired in contemporary science (see e.g., [Strevens, 2003]), a wider critical appraisal of the reward system of science seems unavoidable as collaboration increases. Regarding Bruner's and O'Connor's contribution, a question that could be posed is to what extent the proposed models are realistic and represent actual features of academic research. The authors rightly note that the models exhibit *possible* mechanisms that could be looked for by sociologists. But even if these mechanisms were not found, the models could actually be of some explanatory value, as it is well-known from philosophy of science debates that idealized or unrealistic models are not valueless. More generally, and this holds for the models proposed in other chapters too, an analysis of the explanatory power of the simplified models that are used would be a welcome future addition.

Part IV: From Individual to Collective Opinion

The last two papers in the volume address the question: how are and ought a scientific community's judgments be related to those of individual scientists? Martini and Sprenger focus on the particular problem of characterizing the best methods for amalgamating an expert community's estimates of a single quantity. For example, imagine a panel of nuclear scientists and medical researchers is asked to estimate the maximum amount of radiation to which one can be safely exposed. Or suppose climate scientists are asked how many additional hurricanes there will be in fifty years due to climate change. In these cases, individual scientists' estimates will likely differ. How ought we combine their estimates in order to decide what to believe about the quantity, or to make a policy?

Martini and Sprenger consider two types of strategies: (1) treat all scientists as equally reliable and take a straight average of their guesses, and (2) try to weight expert estimates by perceived reliability. Employing mathematical results of [Klein and Sprenger, 2015], Martini and Sprenger discuss the conditions under which treating experts as differently reliable leads to better estimates, even if we are not certain how exactly reliable different experts are. Nonetheless, the mathematical results they discuss require that we can *compare* expert reliability, and so Martini and Sprenger discuss the empirical prospects for accurate comparisons. They find modest support for the claim that, in limited circumstances, comparisons of expert reliability are possible, but often, equal weighting is just effective in practice.

Martini and Sprenger largely ignore the question of identifying the *reasons* expert estimates differ. Suppose that instead of aggregating expert judgments, a scientific institution or grant-giving body (e.g., the National Science Foundation in the United States) wishes to identify different scientific camps so as to discern why there is disagreement about some hypothesis. How might the institution identify *clusters* of scientists who share similar theoretical commitments? Bonnay's paper addresses this important question.

Bonnay first discusses axioms that any procedure for dividing scientists into clusters ought to satisfy. For example, cloning a scientist might increase the size of a cluster in an existing scientific community (namely, the cluster with her clone), but it should not result in the creation of new clusters; the clone does not add any ideological diversity to the existing community. Bonnay then proves that all clustering methods satisfying a few axioms have a similar functional form. Bonnay's work, therefore, not only provides a principled method for identifying like-minded groups of scientists, but it also provides insight into difficult metaphysical questions about when a group has a shared view or purpose.

Bonnay's formal framework differs from that of Martini and Sprenger in an important way. Whereas Martini and Sprenger represent scientists' beliefs using numbers (which represent a scientist's estimate of some unknown quantity), Bonnay represents beliefs using sets of propositions. This difference raises interesting prospects for future research for their respective projects. For example, can Bonnay's principles concerning propositional beliefs be integrated with existing axioms for dividing groups into clusters based on their quantitative beliefs [Ben-David and Ackerman, 2009]?

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