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Henrik Kragh Sørensen and Line Edslev Andersen

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Centre for Science Studies, University of Aarhus, Denmark
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Mapping Social Aspects of Mathematical Knowledge Production*

Henrik Kragh Sørensen† Line Edslev Andersen‡

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Abstract

In this chapter, we propose to address the philosophy of science in practice from a rather extreme point, namely by bringing social epistemology and philosophy of mathematical practice to bear on each other. By discussing the role played by epistemic dependence in select cases from contemporary mathematical practice, we can begin to systemically map how social aspects are involved in proving, checking and using mathematical results.

1 Introduction

Contemporary mathematical research practice does not live up to the stereotypical folklore of isolated geniuses working on tractable but mindboggling problems producing results in the form of theorems with proofs that can be checked step-by-step by other members of the community.

Over the past decades, a practice-oriented approach to the philosophy of mathematics has developed which — among other things — is capable of questioning, modifying, and eventually empirically substantiating philosophical reflection on the actual practice of actual mathematicians when they produce mathematics (see e.g. Ferreirós, 2016; and the anthologies Larvor, 2016; Löwe and Müller, 2010). Such approaches are capable of, among many other things, showing that mathematical practice is prone with issues of complication to the point of making the process, product, and evaluation of mathematical research unsurveyable by any individual and thus dependent on a communal sense of trust. Mathematical research may for a number of reasons involve situations of unsurveyability, understood as situations where an individual mathematician cannot directly assess and verify steps necessary for the proof. In such situations, the mathematician would be epistemically dependent on others for assessing the proof.

In this chapter, we discuss various cases of unsurveyability that arise in mathematical practice and influence how mathematicians become epistemically dependent upon others when 1. proving, 2. checking, and 3. using mathematical results. Thus, we examine how

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†Section for History and Philosophy of Science, Department of Science Education, University of Copenhagen. Øster Voldgade 3, 1350 København K. henrik.kragh@ind.ku.dk.

‡Center for Science Studies, Department of Mathematics, Aarhus University.

trust is necessary in contemporary mathematical research. Our central argument is that epistemic dependence is integral in mathematical knowledge production, and although the cases are chosen from prominent and admittedly extreme examples, we believe that our analyses and conclusions apply to the broad range of mathematical practice.

First, we consider epistemic dependence relations involved in checking mathematical proofs (section 2). A mathematical result may arise on the borderline between different mathematical sub-disciplines or techniques that are mastered by no individual mathematician or only a few. This situation would often arise out of collaborations that share much with the division of epistemic labour in interdisciplinary settings (see also H. Andersen and Wagenknecht, 2013), albeit within mathematics. However, we shall consider two cases of this type where the proofs are not themselves the result of collaborations, namely Andrew Wiles' proof of Fermat's Last Theorem and Grisha Perelman's proof of the Poincaré Conjecture. Nonetheless, the complex character of the proofs required mathematicians with different types of expertise to work together to check them.

Second, we analyse epistemic dependence relations involved at the level of proving mathematical results (section 3). This topic is becoming increasingly relevant as more and more 'ordinary' mathematical research articles are written in collaborations. A result may arise from a massive collaborative project that has (more or less) explicitly applied a division of labour between its participants. Here, the matter is complicated even more as the entity which the mathematician is required to trust is not a given set of individuals but a changing collective which may involve many collaborators and span long periods of time and large numbers of publications. As such, this element also brings into play the issue of trust in the mathematical literature. We discuss the Classification of Finite Simple Groups, which is a case of this type.

Third, we analyse a very particular type of epistemic dependence relations also involved at the level of proving mathematical results (section 4). Sometimes mathematicians utilize (or, for lack of better word, collaborate with) computers to prove a result. A result may arise from the use of computer technology to run through steps too numerous to be verified by any human mathematician or group thereof. Here, the entity to be trusted is an even more complex one in that it not only includes mathematicians but also other technicians and, importantly, technologies. Kenneth Appel and Wolfgang Haken's proof of the Four Color Theorem is an example of a proof of this kind.

When epistemic dependence relations are necessary at the level of proving a mathematical result, such relations are also necessary at the level of checking the result. In addition, the epistemic dependence relations involved in proving and checking the result at least partly determines the epistemic dependence pattern that arises when a mathematician uses that result in a new proof. We end the chapter by addressing how the epistemic dependence relations involved in proving, checking, and using mathematical results are related (section 5; see also figure 1). Our philosophical analyses are, although grounded and presented from cases, principled and apply to wide ranges of contemporary mathematical practice.

Note that, from the individual mathematician's perspective, the mentioned modes of unsurveyability are characterized by the circumstance that she, when wanting to prove p (with others), or to check the proof of p (with others), or to use p in her own work, does not have the possibility of checking the entire proof directly. Deprived of the possibility

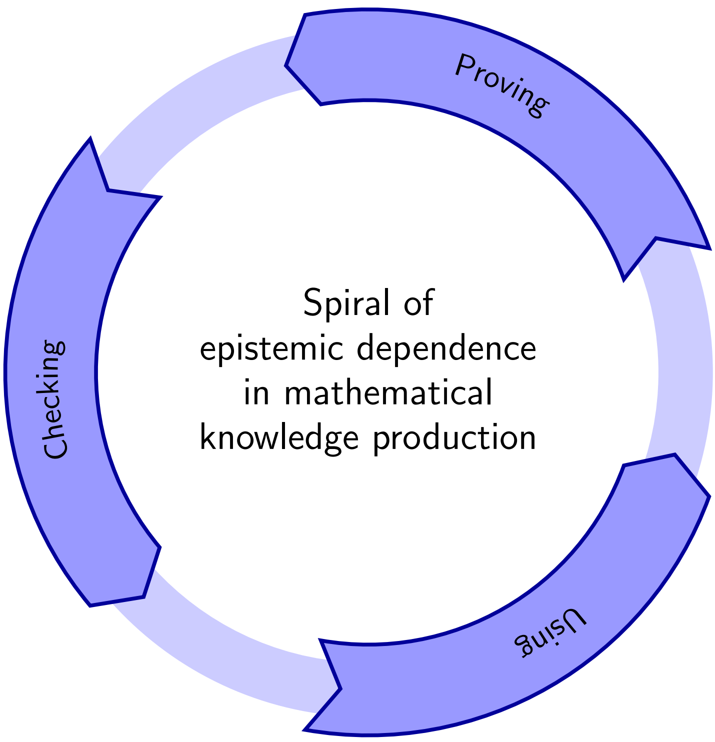

Figure 1: Epistemic dependence in mathematical knowledge production.

of direct calibration, of checking every step of the proof of p, she is faced with having to depend epistemically on the testimony of others.

2 Checking proofs with others

Some of the most publicized recent breakthroughs in mathematics have come on the seams that connect bordering sub-disciplines, and philosophers have begun to approach such disciplinary complexity as intrinsic properties of 'deep' proofs (see Ernst et al., 2015). Wiles' proof of Fermat's Last Theorem (1993, published 1995) and the resolution of the Poincaré Conjecture by Perelman (2003) are both complex proofs that combine methods developed in different branches of mathematics to bear on a celebrated mathematical problem. In both cases, the possibility of resolving the problem through answering a series of conjectures was well-known. Yet, the particular set of expertise combined with the personal tenacity and audacity required made the problems seem intractable if not impossible to the general opinion of the mathematical community.

Heralded as one of the greatest mathematical achievements of the twentieth century, Wiles' proof of Fermat's Last Theorem combined different branches of number theory to answer a conjecture developed by Goro Shimura and Yutaka Taniyama in the 1950's and 1960's which had been proved by Ken Ribet in 1986 to imply Fermat's Last Theorem (for an exposition for a general audience, see Singh, 1997). According to Wiles' own publication, "[t]he key development in the proof is a new and surprising link between two strong but distinct traditions in number theory, the relationship between Galois representations and modular forms on the one hand and the interpretation of special values of L-functions on the other" (Wiles, 1995, p. 444). The proof, which ran to 108 pages was densely packed with novel ideas and specialized notation. The first announcement by Wiles had come in 1993, but after the review process was started, a lacuna was identified by a series of questions raised by Nick Katz. With the help of his former student Richard Taylor, Wiles was able to repair the proof in 1994 and subsumed it to a new round of review by Gerd Faltings and others. The result was published in two conjoined papers in the *Annals of Mathematics* in May 1995, two years after Wiles' initial public announcement (Wiles, 1995; Taylor and Wiles, 1995).

The celebrated resolution of the century-old Poincaré Conjecture by Perelman proceeded from ideas of Richard Hamilton to use so-called Ricci flows in attacking the problem (for an exposition aimed at a general audience, see e.g. O'Shea, 2007). In three papers uploaded to the online repository arXiv in 2002–2003 (Perelman, 2002; Perelman, 2003b; Perelman, 2003a), Perelman then developed a method of 'surgery' for such Ricci flows, thereby showing how the techniques would apply to resolve the Poincaré Conjecture and the more general Thurston Geometrization Conjecture proposed by the mathematician William Thurston in 1982 (for an exposition that is itself part of the post-publication checking procedure, see Kleiner and Lott, 2013). Soon after Perelman's papers were made available, in the spring 2003, Perelman went on a lecture tour in the US where he was able to convince many experts that he possessed the details backing his claims for having a proof of the conjecture (see e.g. Woit, 2004). In 2003, John Milnor who is an expert in the field evaluated Perelman's contributions to be both "ingenious and highly technical", and Milnor found that Perelman "has introduced new methods that are both powerful and beautiful and made a substantial contribution to our understanding" (Milnor, 2003, p. 1231). Perelman's application of the theory of Ricci flows to topology came with great virtuosity and a wealth of new ideas, and his announcement was initially met with great surprise. Indeed, it took quite some effort on behalf of the mathematical community to digest and elaborate on Perelman's methods and proofs, and different groups pursued the task while Perelman, himself, retracted into self-imposed isolation back in Russia. In processing and validating Perelman's ideas, methods, and proofs, various groups provided elaborate, book-length expositions of the proof that led to the validation of the proof being accepted and eventually honoured with the Fields Medal (2006) and the first of the CMI Millennium Prizes (2010). With the emergence of more accessible expositions, the theory and Perelman's proof has also become the material for graduate courses and theses, variations in the approaches and development of new mathematical ideas in the mathematical community (see e.g. Tao, 2006, whose author also taught and blogged about Perelman's results from a variety of perspectives).

These two cases, prominent as they are, are exceptional for being the work of highly dedicated individuals working in relative isolation which necessitated the combined and concerted efforts of the mathematical community in checking their claims and providing the scaffolding upon which their results could be integrated into the collection of mathematical results. The original papers were — for reasons of complexity — beyond the epistemic horizon of most professional mathematicians, if not in principle then at least in practice. By necessity of the complex task and the need for mastery of newly developed techniques in a span of mathematical branches, the first peer assessment of such novel claims were only possible from a small set of experts who came to have a special function as vouchers for the results before the process could be completed (see also L. E. Andersen, 2017b). In a sense, for years the mathematical community believed in Wiles' proof because of the popularization and the testimony of experts such as Ribet. The formal peer review conducted by only a handful of individuals was supplemented by a series of other means that involved more perspectives such as, for instance, the more detailed expositions of the theory of Terence Tao's recasting of Perelman's proof from a different perspective. Thus, a process of checking was initiated which extended beyond the ordinary peer reviews required for assessing the possibility of publication and went well into the scrutiny of the results and methods by allocating summer schools, conferences, exposition papers and books to disseminating and evaluating the proofs and concepts. In the end, it is these efforts that provide the broader mathematical community the means to accept that the two results are now proved — and this social closure is celebrated by the community bestowing rewards and recognition upon the men behind the proofs, whether they like it or not.

Another recently claimed mathematical result, the resolution of the so-called ABC-conjecture by Shinichi Mochizuki in 2012 (see Hartnett, 2012/2014; Castelvecchi, 2015) points to the necessity and challenge involved in this appropriation process. When Mochizuki proposed his solution, it came in the form of a series of four documents totalling more than 500 pages filled with highly specialised, even idiosyncratic notation and techniques. Consequently, the immediate response of many mathematicians recorded on numerous social media was to wait and see before forming an opinion or investing in the new proof. And the technical challenges to understand Mochizuki's proof was supplemented by frustrations when Mochizuki proved to be less willing to explain himself and his proof than was expected by many (non-Japanese) mathematicians interested in the proof. Since these frustrations sometimes entered into public record on social media and in the press, they can be used to point to a code of compliance on behalf of the author of the proof to explain herself and assist in the validation and appropriation of the proof.

These discussions have served to illustrate how social factors are involved in checking and eventually integrating new mathematical proofs, results, and techniques in the corpus of established mathematical knowledge. Vetting published mathematical proofs in seminars and summer schools is an important, yet demanding, social process in which new proofs are scrutinized, tested, developed, and appropriated by the community. The original author is required to take part in this process, but it involves many actors beyond the mathematician who proposed the proof. Since this is a complicated and time-consuming matter, the reputation of the original author, the prominence of the result, and the initial evaluation and reception by experts will matter as to how much others are willing to invest in checking the proof.

We have now discussed how social processes are involved in validating and integrating new mathematical results and proofs into the corpus of established knowledge. Whereas peer-review can filter the mathematical literature, the processes discussed here are mainly post-publication and more directed at integrating new results in the established corpus of knowledge. In the case of Wiles' proof of Fermat's Last Theorem or Perelman's proof of the Riemann Hypothesis, the review efforts — both pre- and post-publication — were directed not only at checking the correctness of each step in the proof but also of explaining the proof against a background of concepts, techniques and aspirations of the mathematical community. This required careful scrutiny of the original proposed proof but resulted in more processes and publications that eventually integrated the proof in a richer web of mathematical results and techniques. Thus, the review-process can be seen as a concerted effort of a reasonably sized sub-set of the mathematical community to make new claims accessible to the community at large.

This points to a general issue to be raised in relation to the mathematical literature, namely that it is often the case that for a given mathematical result p, many mathematicians will have to rely on relatively few mathematicians to vouch for the result. This argument is obviously related to the epistemic division of labour in collaborations, yet it differs in that it points out that certain key experts — who become experts by virtue of being epistemically and morally trustworthy and are willing to vouch for an eventually explain important claims — serve a special role in the social fabric of mathematical practice.

3 Doing proofs with others

Reliance on trust is a necessity in any collaboration, and mathematics is becoming increasingly collaborative as modern means of communication continue to transform the discipline. Increasingly, mathematical research results are produced in collaborations that include mathematicians from different institutions, even different countries (Frame and Carpenter, 1979, pp. 483–484; Luukkonen, Persson, and Sivertsen, 1992, p. 118; Coccia and Wang, 2016). Obviously, traditional patterns of research collaborations such as supervisor-relations are also frequent in mathematics. But being a highly theoretical discipline not clustered around an apparatus nor heavily impeded by cultural or linguistic barriers, mathematics lends itself as a good example of a global epistemological community. Collaborations involving mathematicians that are not working in daily physical proximity clearly extends the analyses of the roles of trust in research and in publication. Collaborations between individuals who have never met in person are clearly possible, but in practice they will typically involve assessments of the potential for success that either depends on the track-record of the other part or on the testimony of a third part, or on both. This is particularly important where the collaboration brings together different sets of expertise and experience from different branches that prohibit any extensive direct calibration. Such mathematical research relies on a division of labour that extends the notion of the laboratory into geographically extended collaborations, and as such also calls for a nuanced view of the role of epistemic trust in mathematics.

Whereas the sociological study of experimental sciences may focus on the laboratory as a unit of collaboration, the similar situation in mathematics is quite different. Bibliometric studies have shown that it is only within the past 60 years that mathematics has become collaborative if measured by the number of authors listed in mathematical publications (Behrens and Luksch, 2011; Grossman, 2002). It has been said of David Hilbert (1862–1943) or Henri Poincaré (1854–1912) that they, in the early twentieth centuries, were the last mathematicians to have a comprehensive overview of the current state of the field. Since WWII, specialization and the sheer explosion in the number of contributing mathematicians and published mathematical papers have made it impossible for any individual to survey the field in its entirety. Instead, individual mathematicians have to specialize and can master only a smaller subset of the total mathematical literature, techniques, results, and proofs.

An important form of complexity in mathematical proofs that can lead to interesting forms of unsurveyability arises from large collaborative projects aimed at attacking a single result by carving it up into smaller, partially independent sub-projects. A prominent example of this kind of mathematical program can be found in the decade-long quest to classify all finite, simple groups initiated by Daniel Gorenstein in the 1970s (for a sociological analysis, see Steingart, 2012). Over the years, Gorenstein's program was pursued by more than 100 mathematicians, who published several hundred papers spanning in excess of 10.000 pages before the classification was completed. In the process, numerous local errors were detected and corrected, and Gorenstein even prematurely declared the proof to be complete in 1983. However, Gorenstein had been misinformed about one particular lacking case which was only completed in a paper of more than 1200 pages before Michael Aschbacher could declare the classification complete in 2004. By that time, Gorenstein had been dead for a decade. A simplified proof is still in the process of revising and publishing.

Whereas each of the contributions could, at least in principle, be checked and validated as a standard contribution to the mathematical edifice, the classification in its entirety was (and remains) a highly complex mathematical claim which cannot be surveyed by any epistemically unaided mathematician within a reasonable time. Therefore, acceptance of the classification relies on not only the usual critical stance towards published mathematical claims but also on the fact that the circumstance that the claim in its entirety and in its present form is epistemically warranted only by a collaboration of individuals spanning many years and working without the possibility of continuous calibrations of trust (some of the key people had died by the completion of the classification).

Instead, the classification is premised on reliance on a proof sketch (the program set out by Gorenstein in 1972) and the reliance of mathematicians on their literature. In this light, the two types of problems alluded to above — the correction of mistakes and the premature announcement of success — draw directly to these two observations: The individual mathematical proofs involved in the classification are obviously susceptible to the kinds of criticism raised against all mathematical proofs for minor imperfections or arguments not being spelled out to the expected degree (see also L. E. Andersen, 2018). And the premature announcement by Gorenstein, himself, was due to a miscommunication between collaborators about the completion of the last, particularly stubborn case of the so-called quasi-thin case.

Complex collaborations such as the classification of finite, simple groups feature individual results as stepping-stones towards a complex theorem, and the focus for epistemic analysis here is at the warrants for the overarching claim. As illustrated in the example, this depends on correctness and stability of the mathematical literature and as well as the set of methods and the framework under which the study is undertaken. And as in the first case, the validation eventually exercised to verify the claim extend beyond peer-review (see also L. E. Andersen, 2017a) and, in the particular case of the classification and characterization of finite simple groups, computer algebra systems have been developed and distributed to routinely verify and use the results.

4 Doing proofs with computers

The role of computers in mathematical proofs is one of the oldest discussions about the implications of unsurveyability for mathematical knowledge. In the wake of the proof of the Four Color Theorem by Appel, Haken and John A. Koch in 1976, a philosophical discussion about the implications of computer-assisted proofs erupted (for a general exposition, see e.g. Wilson, 2002). In a philosophically influential paper, Thomas Tymoczko developed an analogy with a Martian mathematical oracle called Simon, whose success in answering mathematical questions eventually led mathematicians on Earth to warrant their knowledge claims simply by reference to “Simon says!” (Tymoczko, 1979). Thus began a discussion about whether the computer was just another technological invention easing the work of the mathematician and thus similar to the introduction of a pen, or whether the computer rendered mathematical knowledge derived through its use a posteriori. Central to this discussion is the circumstance that by using a computer, cases can be verified that would exceed the combined capabilities of all living mathematicians through their combined lifetime. In the case of the Four Color Theorem, it was only a matter of checking some 1470 graphs for reducibility, but it still ran to 1200 CPU-hours on state-of-the-art computing machinery in 1975.

However, because highly disjoint proofs — such as the case-by-case verification involved in the proof of the Four Color Theorem — do not provide the kinds of explanation and insight craved by many mathematicians and are unsurveyable at the basic level to individual mathematicians, they are frequently met with reservations in the mathematical community (Montaño, 2012). Yet, it is important to notice that the level of unsurveyability is relevant to the discussion: It is, indeed, possible to give an explanation of the proof of e.g.the Four Color Theorem such that the epistemic warrant for the proof can be broken into the acceptance of this piece of ordinary, high-level mathematics, the acceptance of the correctness of the implementation on the computer, and the acceptance of the successful run of the computer program. Of these three steps, the first one is an ordinary mathematical step, and the last one is the step that spurned the debate over the a posteriori nature of computer proofs. Yet, the middle step points to the more complex epistemic dependence involved when bringing in complex programming tasks to a mathematical question: Though intimately linked, the practitioners of mathematics and computer programming need not overlap in expertise, and the communication of computer programs and reflections about them are not part of the standard mathematical discourse (see also Sørensen, 2016). Thus, the mathematical community as well as the individual mathematical researcher will have to rely on trust not directly calibrated towards the programmers and technicians who set up their essential computer runs. Today, as mathematical computing enters the desktop and everyday practice of mathematicians (see also Sørensen, 2010), some of the reservations towards this mode of epistemic labour division may be reduced for smaller runs or for standardized software, but for more extensive runs that require dedicated and optimized programming, they are likely to remain a source of epistemic trust for the immediate future.

Through the cases discussed in the previous section and the case of computer-assisted proofs, we have shown that research collaborations may induce unsurveyability in the result such that no individual mathematician will be in possession of a complete understanding of the entire proof. This serves to show that situations arise in mathematical practice where no individual mathematician can be said to know the complete proof of a theorem. Instead, trust in the form of epistemic dependence is required for collaborations that span multiple complex expertise or uses of complex techniques or technologies to function.

5 Conclusions

In this chapter, we have applied notions developed in the social epistemology of science to the particular case of mathematics, where a certain belief in the purely rational individual seemingly rules out the necessity for trust and epistemic dependence. Grounded in a bottom-up approach and illustrated by cases from mathematical practice, our analyses have mapped out distinct ways in which unsurveyability of mathematical proofs and results enter into the practice of producing, checking and relying on mathematical claims. Thus, we have systematically argued for ways in which (epistemic) trust is important in mathematical result.

Moreover, these processes are interconnected and dynamic in the form a three-way circular feedback loop: When the epistemic dependence pattern in the production of a mathematical proof is complex, the checking of the proof is often required to be more social. For example, when a proof is made by a large collaboration, is very long and involves different sub-disciplines, then the checking of the proof is required to be a highly social in order to integrate sufficient expertise. Next, this further influences the reliance on mathematical results in the sense that the more social the checking of the proof is, the more complex is the epistemic dependence pattern that arises when one relies on the proof. Finally, the epistemic dependence patterns involved in relying on proofs are part of the epistemic dependence patterns involved in the production of the new proofs that rely on them — the former patterns make the latter more complex. One could say that, when the social aspect of one of the three 'steps' is increased, the social aspect of the other two is increased as well.

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Centre for Science Studies
University of Aarhus
Ny Munkegade 120, building 1520
DK-8000 Aarhus C
DENMARK
www.css.au.dk

Faculty of Science
Department of Science Studies
AARHUS UNIVERSITET