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A group theory of group theory: Collaborative mathematics and the 'uninvention' of a 1000-page proof

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Abstract

Over a period of more than 30 years, more than 100 mathematicians worked on a project to classify mathematical objects known as finite simple groups. The Classification, when officially declared completed in 1981, ranged between 300 and 500 articles and ran somewhere between 5,000 and 10,000 journal pages. Mathematicians have hailed the project as one of the greatest mathematical achievements of the 20th century, and it surpasses, both in scale and scope, any other mathematical proof of the 20th century. The history of the Classification points to the importance of face-to-face interaction and close teaching relationships in the production and transformation of theoretical knowledge. The techniques and methods that governed much of the work in finite simple group theory circulated via personal, often informal, communication, rather than in published proofs. Consequently, the printed proofs that would constitute the Classification Theorem functioned as a sort of shorthand for and formalization of proofs that had already been established during personal interactions among mathematicians. The proof of the Classification was at once both a material artifact and a crystallization of one community's shared practices, values, histories, and expertise. However, beginning in the 1980s, the original proof of the Classification faced the threat of 'uninvention'. The papers that constituted it could still be found scattered throughout the mathematical literature, but no one other than the dwindling community of group theorists would know how to find them or how to piece them together. Faced with this problem, finite group theorists resolved to produce a 'second-generation proof' to streamline and centralize the Classification. This project highlights that the proof and the community of finite simple groups theorists who produced it were co-constitutive–one formed and reformed by the other.

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On 7 August 2010, Vinay Deolalika, a computer scientist working at the Hewlett Packard laboratories in India, emailed some top researchers in the field with what he claimed was a solution to the Clay Mathematics Institute's 'Millennium Problem' known as P vs NP.[1] The announcement sparked intense discussion in blogs, especially that of Richard Lipton, a computer scientist at the Georgia Institute of Technology. In his blog, 'Gödel's Last Letter', mathematicians, logicians, and computer scientists debated the merits of the proposed proof, posting more than 1000 comments over a few days. In less than a week, researchers had found major flaws in Deolalika's paper, rendering the proof incomplete.[2] Reporters continued to cover the event, but instead of focusing on Deolalika or the million-dollar prize the Clay Institute attaches to its Millennium Problems, they now focused their attention on the mathematics community's online response. The *New Scientist* announced: 'Flawed proof ushers in era of Wikimath', claiming that the 'flurry of online activity … points to a new way of doing mathematics' (Anonymous, 2010). *Science News* suggested that Deolalika 'helped spur on a new model of research' (Rehmeyer, 2010). John Markoff of the *New York Times* commented that the breakthrough was not 'in the science, but rather in the way science [was] practiced'. Markoff suggested that 'This kind of collaboration has emerged only in recent years in the math and computer science communities', enabled by online forums in which unprecedentedly large numbers of researchers could work together without being 'physically present at an appointed time' (Markoff, 2010).[3]

Large-scale mathematical collaboration, despite Markoff's claim, is not new, nor is it solely enabled by the Internet. Despite stereotypes to the contrary, little mathematical work is done in isolation, and 20th century mathematics offers multiple examples of large-scale collaborative efforts. This paper reports on the largest and most unwieldy mathematical collaboration in recent history, known as the 'Classification theorem', asking what this example reveals about mathematicians' ways of working, thinking, and proving.

Over more than 30 years, more than 100 mathematicians worked on a project to classify mathematical objects known as finite simple groups. The completed classification, when officially declared in 1981, ranged between 300 and 500 articles and ran somewhere between 5000 and 10,000 journal pages (Gorenstein, 1982).[4] That even the mathematicians who worked closely on the project could only give a rough approximation of the number of pages that constitute the proof is evidence of the unruly nature of the Classification, and is what ultimately earned it its unofficial title: the 'Enormous Theorem'.[5] Nonetheless, mathematicians have hailed the project as one of the greatest mathematical achievements of the 20th century, and it surpasses, both in scale and scope, any other mathematical proof of the 20th century. In the early 1950s, the field of finite group theory was not an active research front. It attracted few mathematicians. Yet by the end of the decade, a handful of mathematicians turned their attention to finite group theory, publishing results and developing new techniques. By the early 1960s, a pioneering community of finite group theorists bearing a coherent body of results and techniques had staked a claim to the American mathematics landscape. In the early

1970s, Daniel Gorenstein, who would soon become the closest thing to a leader the classification effort had, proposed the first comprehensive approach to the problem. Though many in the community initially met his enthusiasm with skepticism, by the end of the decade the tide had changed: a complete classification was no longer a fiction, but an impending reality. In the early 1980s, even before all the papers constituting the proof appeared in print, several mathematicians involved in the program had embarked on a project to revise the entire proof. At least in part, the purpose of the 'second generation' proof, as will be discussed at the end of this paper, was to protect the unwieldy proof from the danger of unintentional 'uninvention'.[6]

The history of the Classification points to the importance of face-to-face interaction and close teaching relationships in the production and transformation of theoretical knowledge. I suggest that mathematical sense-making is always an act of attribution and interpretation that cannot be divorced from a larger body of literature and a community of practitioners. Historians of science use the notion of 'paper tools' to talk about the place of tacit knowledge in theoretical practice, opening theoreticians' work to the same modes of analysis that were previously brought to bear solely on experimental science (Kaiser, 2005; Klein, 2003; Pickering and Stephanides, 1992; Warwick, 2003). Insisting that theoretical work does not lie exclusively in the realm of disembodied cognition, these studies show that theorizing always entails the 'deployment of practical and embodied skills that have to be learned, developed, and actively communicated' (Warwick, 2003: 16). Pure mathematics, as the Classification theorem shows, is no exception. However, it does have unique implications for mathematical epistemology. By following the history of the Classification, I suggest that a mathematical proof is not necessarily the product of one or a few mathematicians, but can be a collective effort in which no single mathematician has a global view of the proof or can attest to its veracity. This fact suggests that proofs are never isolated, but instead are embedded in social networks of attribution, trust, and credit.

The techniques and methods that governed much of the work in finite simple group theory circulated via personal, often informal, communication rather than in published proofs. Consequently, the printed proofs that would constitute the Classification Theorem functioned as a sort of shorthand for and formalization of proofs that already had been established during personal interactions among mathematicians. The decentralized nature of the Classification effort made the close-knit network that grew around it crucial to the dissemination of knowledge, adjudication of proofs, and circulation of shared norms and values. The proof of the Classification was at once both a material artifact and a crystallization of one community's shared practices, values, histories, and expertise.[7]

The story of the Classification mirrors many features of 20th century American history. In it, one can discern events and trends that marked and shaped the US in the early decades of that century: many members of the American mathematical community immigrated to the US during the 1930s to escape Nazism, bringing with them a Germanic tradition of mathematical abstraction, and challenging Depression-era American mathematicians for limited faculty positions (Sanford, 2002; Siegmund-Schultze, 2009). In the wake of the Second World War and at the dawn of the Cold War, military interest in cryptology stimulated support for the pure mathematical research that enabled applied research into code writing and breaking. Infusions of government funding from the National Science Foundation gave mathematicians the luxury to travel, organize working years, and attend conferences together. The American 'manpower shortage' occasioned a precipitous rise in the ranks of American mathematics PhDs in the 1960s (Kaiser, 2002, forthcoming). By the time the Classification theorem was well underway, the community, sustained by four decades of American history, was flush with money, institutional support, and growing ranks of enthusiastic group theorists (Figure 1).[8]

Before delving into the story of the Classification, I begin with a short introduction to the mathematical study of finite simple groups. I introduce the story of the Classification with the 1960–1961 Group Theory Year at the University of Chicago, during which mathematicians not only achieved significant mathematical results, but also established the form of collaboration that structured later work on the Classification. This section points to the significant role of personal communication not only for the dissemination of techniques and methods, but also for the circulation of the community's values and norms. It aims to articulate the gap between how mathematics is published and how it is shared through networks of communication. In the next section, I ask: If mathematical practice is fundamentally rooted in personal

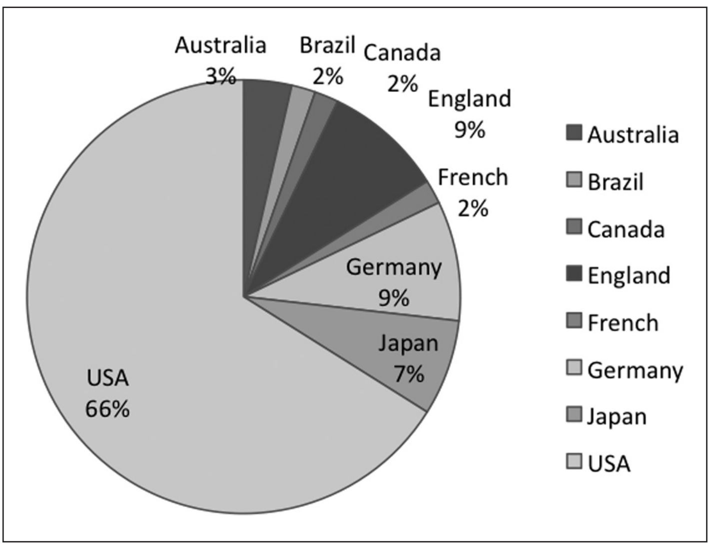

Figure 1. The distribution of 56 authors by country.
The chart lists the distribution of 56 authors by country. It demonstrates that the majority of work on the Classification was done in the USA: 66% of group theorists were based in the US. The second-most prolific countries, lagging far behind the US, are England and Germany, which each constitute 9% of finite group theorists. Both the British and German group theory communities can be traced to a single mathematician who worked closely with American group theorists for an extended period of time (Graham Higman and Reinhold Baer, respectively). Furthermore, if one restricts the set of authors to only those who published 10 or more articles on the Classification (see Table 1), then 14 of those 16 authors worked primarily in the US.

communication, how does that affect how proofs are defined and agreed upon? Looking to the community of finite simple group theorists, I suggest that the adjudication and circulation of knowledge within the community are two enterprises that cannot be divorced. As articles on finite simple groups continued to grow in length and number, the community had to continuously readjust its system of arbitration. The role of personal familiarity turned out to be crucial in the process. In the last section, I turn to some of the major events in the Classification during the 1970s and early 1980s, describing how work on the Classification ended. Finally, by way of conclusion, I point to the 'revisionist program', an attempt by several finite group theorists to stabilize and streamline the proof. Because the proof was long and scattered across multiple journals, as well as preprints, dissertations, and other 'gray literature', very few mathematicians could claim a comprehensive grasp of it. By clearly presenting the various techniques and methods that went into the proof, the mathematicians behind the second-generation proof wished to produce an explicit record of the community's shared knowledge and simultaneously to distance the proof from its producers. This project highlights once more the co-constitutive relationship between the proof and the community of finite simple group theorists who produced it.

Identity

> The theory of groups is a branch of mathematics in which one does something to something and then compares the results with the result of doing the same thing to something else, or something else to the same thing. (Newman, 1956: 1534)

Before I acquaint you with the Enormous Theorem, a few preliminaries are in order. The theorem classifies all *finite simple groups*. An abstract mathematical *group* is a set of elements and a binary operation satisfying four basic axioms: identity, inverse, associativity, and closure. This point is best relayed by example. From a geometrical perspective, a symmetry group of a figure such as a square can be thought of as all the possible permutations of the square that leave it unchanged (for example, rotation by 180º or a flip across a diagonal). The group consists of the group's elements, which here include each possible symmetry permutation of the square, plus the binary operation performed on those elements, such as combining two possible symmetry permutations of the square (for example, first flip and then rotate it). Not all groups are geometrical in nature, however. The set of all integers – negative, positive, and zero – also form a group with addition as the binary operation. The important thing is that together, the operation and the elements satisfy the axioms of the group. That is, there exists an *identity* element (leave the square unmoved in the first case, or add zero in the second case), the operation is *associative* (2 + [3+5] = [2+3] + 5), each element has an *inverse* (each positive number added to the negative with the same absolute value will equal zero), and *closure* (when performing the binary operation on two elements in the set, the resulting element will itself be a member of the group).

As these two examples show, groups can have either a finite number of elements (symmetric permutation of the square) or an infinite number (the integers). In finite groups, the number of elements in the group is called the group's *order* (the order of the group symmetry of the square is eight). Another concept mathematicians use in group theory is that of a *subgroup*. If a subset of the group's elements satisfies the four group axioms under the same binary operation, then the subset forms a subgroup (for example, all even integers form a subgroup of the integers). Finally, a subgroup is called a *normal subgroup* if its elements commute (that is, the binary operation is not order-specific: $a \times b = b \times a$) with all the other elements of the group. Leaving aside the precise mathematical definition of a normal subgroup, what is significant here is that, in a sense, a *normal subgroup* can 'divide' the group. This brings us to *simple groups,* which are defined as groups that do not have any proper normal subgroups.[9] Simple groups are therefore analogous to prime numbers. In the same way that every prime number can only be divided by itself and the number one, the only normal subgroups of a simple group are itself and the identity group. And just as prime numbers are considered the building blocks of all integers, simple groups are the building blocks of all other finite groups.

Which brings us to the Classification theorem itself. The Classification theorem states that *every finite simple group is either a cyclic of prime order, an alternating group, a finite simple group of Lie type, or one of the 26 sporadic finite simple groups*. Without going too deeply into the algebra behind this theorem, for our purposes it is sufficient to know the following: the cyclic groups of prime order and the alternating groups were known to be simple years before work on the Classification began. The groups of Lie Type are divided into 16 classes. By 1905, mathematicians had discovered seven classes of finite simple groups of classical linear type, which later became known as Lie type. Fifty years later, Claude Chevalley discovered an additional family of simple groups of Lie type. His discovery, as well as two other discoveries made by other mathematicians, helped spark renewed interest in finite simple groups in the late 1950s. The last category, known as sporadic groups, refers to the 26 finite simple groups that do not belong to any of the three infinite families of known type listed above. Five of these sporadic groups were discovered by Émile Léonard Mathieu around 1861; the other 21 sporadic groups remained uncovered for another century.[10] In 1961, Croatian mathematician Zvonimir Janko shocked the mathematical world by announcing his discovery of a new sporadic group of order 175,560.[11] In the following two decades, 20 more sporadic groups were discovered at a pace of around one per year.

For a classification to be considered complete, group theorists had to accomplish two goals. First, they had to find and describe all the known finite simple groups. Second, they had to show that any random finite simple group must necessarily belong to the list of already-known groups. This paper primarily focuses on the latter task, only briefly mentioning the discovery of new sporadic simple groups. In the early 1980s, when group theorists considered the Classification to be a theorem, the mathematical community had to accept that no more sporadic groups were lurking beyond the confines of the Classification, waiting to be discovered.[12]

Associativity

> The classification of finite simple groups is an exercise in taxonomy. This is obvious to the expert and to the uninitiated alike. To be sure, the exercise is of colossal length, but length is a concomitant of taxonomy. Those of us who have been engaged in this work are the intellectual

> confreres of Linnaeus. Not surprisingly, I wonder if a future Darwin will conceptualize and unify our hard won theorems. ... *The Origin of Groups* remains to be written, along lines foreign to those of Linnean outlook. (Thompson, 1984: 2)

In the 1960–1961 academic year, the Department of Mathematics at the University of Chicago gathered leading group theorists and junior mathematicians for a 9-month special program dedicated to the theory of groups. The mathematicians who attended the program arrived at the University of Chicago on 1 October and stayed until June, taking a yearlong paid leave or a sabbatical from their home institutions. To this day, group theorists consider the year an important turning point in the history of the Classification. Most notably, they extol John Thompson's and Walter Feit's work on what has since become known as the Odd Order Theorem as crucial to the success of the Classification. In his history of the Classification, mathematician Ronald Solomon goes so far as to write that the Odd Order Theorem and its proof were 'a moment in the evolution of finite group theory analogous to the emergence of fish onto dry land' (Solomon, 200l: 330). The theorem established that except for the known cyclic groups of prime order, every simple group must have even order. The proof spanned 255 pages and filled an entire issue of the *Pacific Journal of Mathematics*. The Odd Order paper holds a prominent place in the Classification history not only because of the implications of its result, but also because of the novel techniques it introduced to the study of finite groups. The Chicago Group Theory Year not only enabled Thompson and Feit to collaborate every day for a year to produce such an intricate proof, but also provided group theorists with an environment in which these results could be simultaneity communicated, taught, adopted, and later dispersed. It was also the first time one could speak of a 'community' of finite simple group theorists as such.

The Chicago Group Theory Year was organized by Chicago algebraist Adrian Albert and supported in part by the Air Force Office of Scientific Research and the National Science Foundation (NSF). After the Second World War, Albert became a strong advocate for governmental and military support for mathematical research.[13] He became an expert in cryptography, eventually serving as the first director of the Communications Research Division at the Institute of Defense Analysis.[14] Beginning in 1953, Albert worked as a researcher at, and later as project manager of, several Air Force research summer projects that brought together research mathematicians to work on questions arising in cryptography. These new funding opportunities were crucial to the early development of the field, both for directing attention to various group theoretic problems and providing opportunities for group theorists to meet and work on shared problems. Though Albert was not directly involved in the Classification effort, his advocacy for mathematical research in general, and group theory in particular, was essential to the early development of the field and helped enable its growth over the next three decades.

Daniel Gorenstein first heard about finite simple groups in the parking lot of the Air Force Research Center at Hanscom Field in Bedford, Massachusetts (Gorenstein, 1989: 447). Gorenstein had recently received his PhD from Harvard in algebraic geometry, and was working at Clark University, supplementing his income with summer projects on cryptography for the Air Force.[15] In a 1981 interview, Gorenstein recalled that 'the general way things worked is you sort of worked 50 percent of your time on their stuff and 50 percent of your time on your own stuff'.[16] It was during a 1957 summer project at Bowdoin College, directed by Albert, that Gorenstein became interested in finite group theory and befriended Cornell algebraist I.N. Herstein. By 1960, when Gorenstein heard about Albert's Group Theory Year, his conversion to finite group theory was complete. He arranged to spend a sabbatical year at the University of Chicago.

Albert's idea to invite both leading and young mathematicians to Chicago to work on shared topics came from a sabbatical he spent at Yale University setting up an NSF-sponsored 'special year' on algebra. Albert's faith in collaborative mathematics paid off: by the end of the year, not only had participating mathematicians proved the Odd Order theorem, but somewhat more astonishingly, they helped establish an independent field of study with a coherent body of techniques and a community of researchers dedicated to characterizing the properties of finite simple groups. Among the older mathematicians who participated that year and whose work was later associated with the Classification were Richard Brauer (Harvard University), Michio Suzuki (University of Illinois at Urbana-Champaign), and Graham Higman (Oxford University). The younger mathematicians included Walter Feit (Cornell University), John Thompson (University of Chicago), John Walter (University of Illinois at Urbana-Champaign), Daniel Gorenstein (Clark University), Paul Fong (Postdoctoral Fellow at Cambridge University), and Jonathan L. Alperin (University of Chicago).[17] At Albert's request, they were all housed in an old apartment building, furnished by the University of Chicago, which they referred to as 'the compound'. Though research in finite and simple group theory predates the Chicago year, the techniques, habits, and practices that would characterize research in the field over the next two decades began coalescing that year.

At Albert's suggestion, John Thompson arrived in Chicago after a year as a research fellow at the Institute for Defense Analysis (Ronan, 2006: 123). At the time, he was already famous among group theorists for proving in his PhD dissertation the Frobenius conjecture, a 50-year-old open problem in group theory,[18] which earned him a mention in the *New York Times* (Osmundsen, 1959). While proving the Frobenius conjecture, Thompson began developing a new method of analyzing finite groups, which would later be known as 'local analysis'.[19] In 1959, Thompson was invited to Cornell to give a series of lectures on his proof. Gorenstein traveled to Cornell to hear Thompson's lectures, and both stayed at Herstein's apartment. At the time, Herstein and Gorenstein were collaborating on a paper, and one evening they mentioned to Thompson a particular part of the proof that they had been having trouble solving. Gorenstein recalled that as he was going to sleep, Thompson sat in the living room working on the problem. Upon waking around 5.30 am, Gorenstein looked over and realized that the bed beside him was still empty. 'I walk into the living room. And Thompson is still sitting in the living room working on this problem. So I say, John, you know, it's five o'clock in the morning; you've got to give a lecture at ten o'clock. And so he looks up at me and he says, Oh, yeah, I guess I'd better go to sleep. And he goes to sleep.'[20] During the 1960s, Thompson achieved somewhat of a celebrity status within the community of group theorists, winning numerous prestigious awards, including the Fields Medal in 1970.

Walter Feit received a sabbatical from Cornell to join the Chicago Year. Feit, who was born in Vienna in 1930, was placed on the last train carrying Jewish children out of

Table 1. Core classification authors

Most published authors	Total articles	Authors with most co-authored articles	Total articles	Authors with most co-authors	Total co-authors
Gorenstein, Daniel	25	Gorenstein, Daniel	15	Solomon, Ronald	7
Aschbacher, Michael	21	Harada, Koichiro	7	Brauer, Richard	6
Thompson, John	19	Solomon, Ronald	7	Gorenstein, Daniel	6
Harada, Koichiro	14	Wong, Warren	7	Seitz, Gary	6
Solomon, Ronald	14	Brauer, Richard		Wong, Warren	6
Suzuki, Michio	13	Seitz, Gary	6	Fong, Paul	5
Glauberman, George	12	Fong, Paul	5	Higman, Graham	4
Seitz, Gary	12	Thompson, John	5	Wales, David	4
Wong, Warren	12	Wales, David	5	Feit, Walter	3
Bender, Helmut	11	Walter, John	5	Griess, Robert	3
Griess, Robert	11			Suzuki, Michio	3
Harris, Morton	11			Thompson, John	3
Janko, Zvonimir	11				
Wales, David	11				
Walter, John	11				
Brauer, Richard	10				
TOTAL	218				

Note: The leftmost columns of this table list those finite group theory authors who published most prolifically within the set of authors represented in note 8. Constituting 13% of the total set of 124 authors, these core 16 contributors together published 45% of the 416 articles included in Gorenstein's two bibliographies (not including duplicated articles in column 2). The distribution of the number of articles per author does not follow a normal distribution. Rather, it more closely reflects an exponential curve, in which one-third of the authors only wrote one article included in the set.

Austria when he was 9 years old. Feit lived in England for 7 years, emigrating to the US to live with his aunt and uncle in Miami (Reingold, 1981; Rider, 1984). As an undergraduate at the University of Chicago, Feit had shown interest in group theory and was advised to pursue his graduate studies at the University of Michigan, which was quickly becoming a hub for algebra research following the appointment of Richard Brauer to its faculty. At Michigan, Feit met fellow graduate student John Walter, and the two attended Brauer's seminar on modular representation. They rented an apartment together with another student, and attended lectures by Michio Suzuki, whom Brauer invited to present his latest results in finite group theory (Aschbacher et al., 1999: 543). In addition to the seminar, Feit took an independent reading course with Brauer, and although in 1954, Brauer announced he was accepting a position at Harvard University, he remained, albeit unofficially, Feit's principal advisor.

The Department of Mathematics at Harvard was the last stop in a string of institutional wanderings for Brauer, who had fled from Germany to the US in 1933 after losing his faculty position at Königsberg . After 2 years, he transferred to the University of Toronto, where he stayed for 13 years before moving to the University of Michigan in 1948. In 1954, upon his move to Harvard, Brauer addressed the International Congress of Mathematicians, urging his audience to turn to the study of finite groups, saying, 'if I present here some investigations on groups of finite order, it is with the hope of raising new interest in this field' (Brauer, 1957: 209). Brauer was already famous for his many contributions to algebra, but it was his work on character theory and his invention of modular character theory that would play a significant role in the Classification. Brauer remained interested in the Classification effort up until his death in 1977. He was at least a generation older than most finite simple group theorists, and while his mastery of character theory played a key role in the classification, he did not take part in the developments of new techniques in the 1960s. According to Gorenstein (1989: 454), Brauer 'never studied the local group-theoretic developments that were springing up around him. I was always surprised when he would refer to some comment of mine as "my methods." From my perspective local analysis *was* finite group theory.' Brauer's students were the ones who took active roles in the classification.

Foremost among them was Walter Feit. The Odd Order paper, which he co-authored with Thompson, depended on the integration of Thompson's local analysis with Feit's expertise in modular characters. Commenting on their work decades later, Thompson concluded, 'I was lucky to team up with Walter. He had completed his PhD with Brauer; I had studied Hall's work; we both benefited from Suzuki; and we both had the confidence to attack the odd order problem, which still looks to me like granite' (Scott et al., 2005: 731). Both Thompson and Feit acknowledge a 1957 paper published by Suzuki as an important impetus behind their work. Feit referred to it as 'an essential ingredient' (Aschbacher et al., 1999: 544) and Thompson claimed it was the 'opening wedge' (Raussen and Skau, 2009: 473) for their work. However, it was only through the collaboration of the two mathematicians – their daily interaction – that they were able to complete their work on the proof. As Thompson remarked, 'I think there are only a few who understood the precision and subtlety with which Walter handled a variety of character-theoretic situations. Suzuki and, of course, Brauer appreciated Walter's strength. But only Walter and I knew *just how intertwined our thinking* was over a period of more than a year' (Scott et al., 2005: 731, emphasis added).[21] While each theoretician came to the collaboration with his own toolbox of techniques and methods, Thompson's remark suggests that the product of their joint work was greater than the sum of their prior skills and knowledge. Rather, their work had an emergent quality to it, which arose from their prolonged personal interaction.

Thompson and Feit's work was not the only long-lasting collaboration that began during the Chicago Year. When he arrived in Chicago, Gorenstein began working on a problem in finite simple groups suggested to him by Feit. When he realized that the problem required working with certain groups known as dihedral Sylow 2-groups, Feit advised him to go talk to his friend from graduate school, John Walter, who was considering these groups in his own work. Soon, Gorenstein and Walter began collaborating, and wrote four joint papers over the next decade. Work on the classification was characterized by such collaborations. The 40 to 50 mathematicians who worked primarily on finite simple groups during these decades formed a close-knit network. During the 1960s, when only 52% of all authors of mathematical papers across all subfields had collaborated on one or more paper, nearly 100% of finite simple group theorists collaborated on at least one paper.[22]

The 255 pages of the Odd Order paper were notoriously difficult to understand. Graham Higman, who was among the senior mathematicians present at the Chicago Year, organized a 1-year seminar in Oxford that met weekly to read the proof (Neumann, 1996: 113). Higman fared better than Claude Chevalley, who, according to Fields medalist Jean-Pierre Serre, 'once tried to take this [the Odd Order paper] as the topic of a seminar, with the idea of giving a complete account of the proof', but 'had to give up' after 2 years (Chong et al., 1986: 11). For the mathematicians present at the Chicago Year, comprehending Thompson's and Feit's proof was a much easier task, as they were able to incorporate and make sense of its details as the work progressed. John Walter recalled that 'at the afternoon teas there were stimulating conversations; in particular, one could follow the latest about the status of Feit and Thompson's work' (Scott et al., 2005: 730). Gorenstein remembered that 'Thompson was so excited about everything. We would just stand around and kibbitz.'[23]

Donald MacKenzie (2001: 320) speculates that 'mathematics may be the most social of the non-experimental disciplines, the one in which the intensity of professional interaction is the greatest'. He suggests that proving is 'intrinsically a public, social act', but limits his definition of 'proving' to the work of convincing one's peers of the veracity of a written proof. The history of the Classification demonstrates that even working on a proof, long before publicly 'proving' the proof, is also an intensely 'social act'.

The group theory years, and other less formal extended visits that followed, were crucial to the success of the Classification because they gave finite group theorists access to proofs *in the making*. For example, in addition to the numerous conferences dedicated to finite group theory that took place starting in the 1960s and ranged to anywhere from a few days to several months, some finite simple group theorists spent the 1969–1970 academic year at the Institute for Advanced Study (IAS), and then 1974–1975 at Rutgers University. Over afternoon tea, when they stood around 'kibbitzing', group theorists were able to follow each other's work as it developed, not as a final product, but as a work in progress. When group theorists read a published proof, they could discern the characters, the discussions, and the years of work they remembered going into it.[24] The written document, for them, was easily legible, because it crystallized work in which they had shared.

A remark by Gorenstein illustrates this point. Gorenstein lamented that although he lived next door to Suzuki in the 'compound' and they later became good friends, Suzuki never engaged 'in the kind of back-and-forth discussion from which I learned the best' (a fact that Gorenstein attributed to Suzuki not being a fluent English speaker). Gorenstein explained, 'I was never able to get a *feeling* for Suzuki's approach to mathematics through conversation, only from his published papers' (Gorenstein, 1989: 452, emphasis added). Here, Gorenstein drew a clear distinction between mathematical ideas as they are presented in publication and a mathematician's approach as it is transferred through personal interaction. Beginning in the 1960s, the ability to communicate in person became especially important to finite simple group theorists, as papers in the field became so long that no one individual could read them in their entirety.

Close social interactions enabled mathematicians to share their ideas and approaches easily. Even more crucially, such interactions helped disseminate new techniques. Here, once more, Gorenstein's recollections are instructive:

> I was there. I'm assimilating the stuff hot off the press, as it were, just as these guys are doing it … . If you weren't there – I mean, the paper was not so easy to read. I was assimilating it just very easily. Lo and behold, exactly what Thompson and Feit were doing, John Walter and I needed to do in the dihedral paper. So not only were we learning the techniques as they were doing them, we were trying to generalize them slightly to apply them over in this problem … . People had a lot of difficulty learning those techniques, but being there in Chicago during the period that they were evolving … .[25]

During the 1960s, the methods pioneered by Thompson, known as local analysis, dominated much of the work classifying finite simple groups. Given that the methods could not be extracted easily from the Odd Order paper, nor from Thompson's subsequent six-part, 410-page proof of minimal simple groups,[26] how exactly did these methods travel? As published proofs grew increasingly unwieldy, group theorists relied on 'non-textual means of transmission', such as mentorship and pedagogy, to coordinate and disseminate their work (Kaiser, 2005: 13).

Thompson and Feit's Odd Order paper, as well as Thompson's subsequent work, attracted young mathematicians to the field. Solomon (200l: 332) writes that 'a crowd of new PhDs entered the fray in the late 1960s and the classification project entered high gear'. Many young mathematicians got involved in the Classification because their advisors were already active in the field. Gorenstein distilled Thompson's local analysis techniques into a user-friendly textbook, which became a bible for graduate students breaking into the field.[27] However, this textbook was not published until 1968, before which any attempt to learn the theory from books and papers was nearly impossible. Thompson at Chicago, Higman at Oxford, and Brauer at Harvard were the most prolific advisors, each mentoring more than a dozen students over the next two decades. Ronald Solomon, who became a graduate student at Yale in 1968 under the supervision of Walter Feit, recalled:

> In a tradition of which I was doubly a beneficiary, John Thompson sent his best recent PhD students to Yale as postdocs, and Walter reciprocated by sending his to Chicago. (I profited from this arrangement upon graduation in 1972.) Thus Goldschmidt came to Yale in 1969, and Lyons in 1970. Also, Len Scott returned from his Chicago postdoc to spend the 1970–71 year at Yale. And so I was blessed to study with four of the finest group theorists and four of the finest human beings anyone could ever hope to encounter. (Scott et al., 2005: 733)

This sort of informal arrangement helped initiate newcomers into the field, both its technical methods and its culture.

Such interactions fostered a sense of community among group theorists, which was especially fundamental to the success of the Classification in its final years. At any given point in time, the majority of information about the Classification circulated in the form of preprints rather than publications. For example, while Thompson's *N*-component paper appeared serially in print between 1968 and 1972, it was already available in preprint in 1964. Still, some preprints never appeared in (published) print at all, making them only available to those within the community. Michael Aschbacher, who joined the Classification in the early 1970s, and who had access to several preprints that circulated among finite group theorists, nonetheless reproduced in his early papers proofs that had already been established by other researchers. Commenting on this situation, Aschbacher explained, 'I wasn't plugged into the system at that point in time.'[28] Being 'plugged into the system' was the only way one could keep track of developments in the field.

According to both Alperin and Gorenstein, this also explained why most of the research in the field was conducted in the US, Germany, and England. Asked why there were no Russian or French mathematicians working on finite simple groups, Alperin replied, 'they tried, but you couldn't do it in isolation. You had to be in the in group the way things were moving so rapidly.'[29] Gorenstein concurred, suggesting that 'the reason the Russians could never make contributions to simple group theory is because they couldn't communicate with the rest of the world what's going on … . They weren't plugged into the system.'[30] 'The system', I suggest, was the set of social and mathematical practices maintained by the community of finite simple group theorists through both formal and informal interactions. That is, the role of face-to-face interaction was crucial not only for the dissemination of technical knowledge, but also for the formation of a set of community norms (cf. Kaiser, 2005: Chapter 4).

In 1982, Alperin suggested that Thompson's greatest contribution was merging multiple technical methods in finite group theory. 'What the Odd Order Paper and the N-group did was bring it all together … the character theory, the permutation groups, the local group theory, the new field of local group theory; it all came together.'[31] But not only did the mathematical techniques come together during these years, so did the mathematicians. Feit remarked that by living together during the Chicago Year, many of the mathematicians involved got to know one another (Aschbacher et al., 1999: 545). As will become clear in the next section, this interaction between knowing the mathematics and knowing the mathematicians behind it was instrumental to the success of the Classification as a group effort.

Inverse

> The only people who can work in simple groups are the people on the inside because it's a very fast moving, cooperative effort and other people are just left out. You're either in or you're out. (Jonathan Alperin)[32]

The Odd Order theorem not only introduced many of the techniques used in the Classification effort. Running to 255 pages, it also became a benchmark for the length of later Classification proofs. As the literature in finite simple group theory grew during the 1960s and 1970s, 100-page proofs became increasingly common, culminating in Aschbacher and Smith's hulking 1000-page monograph.[33] These increasingly long papers presented a challenge to the mathematics community, whose members had to read, referee, comprehend, and review them, and ultimately attest to their accuracy.

Not only would the complete proof of the Classification theorem be impossible for any one person to read, let alone understand or evaluate, but many of the papers that constituted the proof were highly complex, if not impenetrable. Finite simple group theorists adapted accordingly.

It has long been noted that scientific papers are crystallizations and abstractions of the daily manual and intellectual labor that laboratory experimentation demands. They are post hoc constructions of theoretical or exploratory work made to appear logical, methodical, even inevitable, and they obscure the meandering, provisional, and often improvizational nature of actual scientific inquiry (Latour, 1987; Latour and Woolgar, 1986 [1979]). The same has been said for mathematical papers, which appear to be inevitable successions of logical reasoning that hide the work that went into a proof's production – the hours spent working through specific examples, following false leads, pursuing techniques that may or may not pan out (Hersch, 1991). In this case, however, the volume and complexity of *published* work could not be navigated by reading alone. Here, tacit knowledge and personal communication were not glossed over in publications, but instead were indispensable components of how mathematicians were able to approach, apprehend, and evaluate an unwieldy body of literature.

While the previous section appraised how personal communication among finite group theorists was necessary for the circulation of mathematical knowledge and techniques, rendering the community and the content of the theorem mutually constitutive, this section will evaluate the consequences of that entanglement for the way the proof was evaluated and adjudicated. I first focus on how mathematicians distinguished 'local' – that is, insignificant – errors from consequential ones. A few experts, among them John Thompson, were able to make nuanced distinctions between those errors that could be 'patched' by some other local argument or technique and those that could not. Mathematical proof, at least in the case of the Classification theorem, was not an all-or-nothing category, but instead something with degrees of veracity and corroboration. Next, I turn to the fact that as the proof snowballed, papers became increasingly incomprehensible to other mathematicians *within* the group theory community. Whereas previous sections explored how a community of mathematicians collaboratively constructed the proof, here I demonstrate the inverse, and show how the unique nature of the proof in turn shaped the group of researchers devoted to it. I explore how mathematicians came to rely on personal trust and authors' reputations, evaluating papers not on their own weight, but in light of previous conversations and communications they had had with their authors. Finally, I suggest that 'proof', for the community of finite simple group theorists, was not static once established, but rather a work in progress whose standards evolved alongside the community.

Between 1962 and 1972, John Walter and Daniel Gorenstein published six joint papers, the longest of which, a three-part, 160-page manuscript, appeared in the *Journal of Algebra* in 1965 (Gorenstein and Walter, 1965). Thompson, whom the authors acknowledge for his helpful comments and remarks, wrote a two-part review of the paper in the Mathematical Reviews.[34] Thompson begins by summarizing the main results of the first two parts of the paper, pointing out the structure of the argument and the relevant literature to which the paper contributes. Thompson then chastises the authors for 'several mathematical malapropisms' contained in the paper. Among the various malapropisms he identifies are factual mistakes (‘the alternating group on four letters is a counterexample to this assertion’), misprints (‘the displayed equation has a superfluous λ, which is nicely balanced by an omitted λ in line 3’), lack of clarity (‘there is a murkiness even in the statement of this lemma … and the ensuing proof continues the obscurity’), lack of knowledge (‘we would infer from page 100, line 9, that the authors were unaware of this fact’), logical mistakes (‘it does not follow from $X_1 > 0$, $X_2 \leq X_3 < 0$ that $h_{0\mu} < 2(2-1/X_2)^2$’), and bad notation (‘this is not simply a misprint, but rather indicates a poor choice of notation’) (MR0177032).[35] In the second part of his review, which covered the last part of Gorenstein and Walter’s paper, Thompson turns from scolding to praise. Writing about the ‘delicate and difficult arguments’ one can find in these papers, Thompson concludes, ‘the techniques of these three papers cover the spectrum of finite group theory more thoroughly than any single paper known to the reviewer. No simpler proof seems available in the near future, though one would be welcome’ (MR0190220). Thompson’s ability to both repudiate the authors for their many mistakes and celebrate the proof as crucial to the Classification is indicative of the way the group theory community came to terms with increasingly lengthy papers. They distinguished between the logical structure of a proof and the details of its written components.

Commenting on Thompson’s review nearly 20 years later, Gorenstein said that when people read the first part of the review, they got the impression that ‘there was no proof there’, so when Thompson published the second half of his review, he was trying ‘to counteract the negative impact that he had given’.[36] Gorenstein’s assertion that despite the numerous ‘mathematical malapropisms’ Thompson identified in his review, the proof was nonetheless ‘there’, illustrates how finite simple group theorists redefined the notion of proof within the community. Expanding on Eric Livingston’s (1999) notion of ‘cultures of proving’, MacKenzie (2001: 306–313) argues that at least as far as mechanized proving is concerned, it is more accurate to speak of cultures, plural, rather than a singular culture of proving. The Classification suggests that pure mathematicians also establish what constitutes a proof, keying their definition to the needs and purposes of their own specialized subculture.[37] The notion of ‘local errors’ became integral to the development of the finite simple group community, allowing mathematicians to adapt to the thousands of pages of inscrutable proofs that accumulated in mathematical journals and preprints.

Over the years, John Walter developed a particular reputation for making numerous ‘local errors’ in his papers. Commenting on his work, Jonathan Alperin remarked, ‘John Walter can’t do things accurately; he just cannot. He comes to grips with the main problems and he’s had tremendous ideas, but when it comes to actually writing down a correct proof … he makes stupid errors. Now, the main point he usually has and the routine things that anybody can do, he makes a mess of … .’[38] In assessing Walter’s work, Alperin points out an important characteristic of what counts as a ‘local error’ by drawing a distinction between the ‘main point’ of a proof and the ‘routine things’ involved in its production. That is, claiming an error to be ‘local’ assumes that the mistake may be fixed without altering the statement of the theorem. Thompson, in reviewing Gorenstein and Walter’s paper, assured the reader that ‘when these [errors] are rectified, as with some care they may be, these papers provide an important contribution to our knowledge of finite groups'. Such a claim requires both expertise and a value judgment, as only a mathematician that is already a member of the community and who is familiar with its techniques and methods could identify, let alone 'rectify', such errors.[39]

In the early 1980s, when the proof of the entire Classification was considered complete, Feit explained the situation:

> Most experts are convinced that the proof is essentially correct; any errors which occur are expected to be minor oversights or local errors which can be corrected by the methods that have been developed in the process of completing the classification. More importantly, no error is expected to change the end result, that is, to lead to new simple groups. (Feit, 1983: 120)[40]

What counted as a proof was not only defined according to the standards of the community, but also depended on those 'experts' who could attest to its validity. The assertion that experts could correct any mistakes points to the place of tacit knowledge in mathematical practice. There was not an explicitly stated prescription of steps that the expert could appeal to when encountering an error in a given proof. Rather, a certain degree of 'know how' was assumed that enabled individual mathematicians to correct given mistakes. In the final section, I turn to the implications of this situation for the status of the proof in the following decade. However, here I suggest that even within the dedicated community of finite simple group theorists, the definition of what *counted* as a proof was unstable. Rather, it evolved as the community did.

In 1970, Gorenstein, Alperin, and Brauer published a 260-page paper in the *Transactions of the American Mathematical Society.* Paul Fong, a student of Brauer's, reviewed the paper. After asserting the 'unquestioned importance' of the result, Fong commented, 'as may be expected from the present state of finite group theory, the proof is long, difficult and complicated (and the reviewer readily admits he has neither read nor followed all the details)' (MR0284499). The Classification proof had mushroomed to the point that even its reviewers readily admitted to neither reading nor following it. In 1969, Gorenstein, reviewing a paper by Walter, began by vouching for the 'major contribution' the paper made to the Classification effort. He then conceded that 'the amount of notation involved and the author's expository style are such that the reviewer must admit that he has not read the argument in complete detail' (MR0249504). A few years later, when asked about his inability to follow certain sections of the proof, Gorenstein added, 'I said I couldn't. Neither could the referee.'[41] If in Thompson's review, the underlying assumption was that the various mistakes could be amended without altering the original proof, the same could not be said of Gorenstein's review. Instead, personal trust, familiarity, and reputation became integral to the adjudication process.[42]

Though Michael Aschbacher became interested in finite simple groups only as a postdoc (having written his dissertation on combinatorics), his early contribution to the field took the community by surprise. Gorenstein described Aschbacher's entrance into the field as 'dramatic' (Gorenstein, 1989: 469), and Solomon remarked that following Aschbacher's work in 1974 and 1975, 'belief mounted that nothing could stand between him and the completion of the Classification' (Solomon, 2001: 338). In recognition of his many accomplishments, Aschbacher won the prestigious Frank Nelson Cole Prize in

Algebra in 1980. However, as Aschbacher's esteem within the community grew, his papers became notoriously difficult to comprehend. When Aschbacher's first important paper in the field appeared in print, Solomon was an instructor in the Department of Mathematics at the University of Chicago. Together with George Glauberman, he tried to read the proof, but found it incredibly difficult to follow Aschbacher's arguments. Specifically, Solomon complained that 'there were these clever counting arguments, but he never bothered to write it [them] down'.[43] Aschbacher's style made it notoriously difficult for other mathematicians to follow his proofs, and as his papers grew longer, the difficulties were exacerbated.

Group theorists no longer judged a proof as a self-contained document, but took into account its author, his previous work, and his standing within the community. When asked about the process of refereeing in the finite simple group community, Gorenstein characterized Aschbacher's papers as 'extremely difficult'. He recalled, 'Aschbacher is so smart … . Richard Lyons and I and Aschbacher had just written a joint paper … . I wrote a draft of this paper, Aschbacher rewrote it – I had enormous difficulty reading my own goddamn paper … . He can see things so much better than anybody else.' He added, 'he's so quick that he misses things'.[44] The impenetrability of Aschbacher's papers was taken as testimony of his mathematical prowess, rather than a challenge to the veracity of his proofs.

In 1982, when he published *Finite Simple Groups: An Introduction to Their Classification*, the first textbook to offer an overview of the Classification, Gorenstein commented,

> Indeed, many of the papers in simple groups are known to contain a considerable number of 'local' errors … . Most of these errors, when uncovered, can be fixed up 'on the spot'. But many of the arguments are ad hoc, so how can one be certain that the 'sieve' has not let slip a configuration leading to yet another simple group? The prevailing opinion among finite group theorists is that the overall proof is accurate and that with so many individuals working on simple groups during the past fifteen years, often from such differing perspectives, every significant configuration has loomed into view sufficiently often and could not have been overlooked. (Gorenstein, 1982: 7–8)

No single person was responsible for checking and double-checking the Classification; it had become the responsibility of the entire community to function as a 'sieve' filtering local errors from serious ones. Further, a published proof could never be judged separately from its creator(s), who could only be evaluated within the network of finite group theorists, making every proof at once a material document and a testimony to the community's belief in it. When Aschbacher and Smith's manuscript dealing with the last known hole in the Classification appeared in print, Solomon reviewed it in *Mathematical Reviews*. He ended by stating that the first volume 'has been read carefully by John Thompson', while the second 'has been checked by a team of referees' (MR2097623 and MR2097624). It is significant that while the 'team of referees' remains nameless, John Thompson is singled out. Having a Fields medalist and Cole Prize winner attest to the correctness of the manuscript helped garner confidence in the proof, which at 12,000 pages was too long for any one individual to verify.

Starting with the Odd Order theorem, it became apparent that work in the field would involve long proofs stretching over hundreds of pages. Adapting to this situation, the community incorporated the notion of 'local errors' into its definition of proof, accepting mistakes that did not alter the final result of the proof. This modification of the traditional notion of a rigorous proof was maintained within the community, which depended on a body of 'experts' able to rectify these mistakes using known techniques and tools. However, by the late 1960s and early 1970s, as the number of long proofs ballooned, the verification of long proofs became the collective responsibility of the community. As the Classification neared its end, the multiplicity of approaches and the sheer number of individuals working in the field were taken as being in and of themselves a guarantee of the validity of Classification papers. If there were a serious error in the Classification, so the logic went, surely *someone* would have noticed it by now. Finite simple group theorists, first among them Daniel Gorenstein, recognized that the lengthy, cumbersome, and distributed state of the proof would not pass muster so easily among mathematicians outside the field. Even before all the pieces of the original proof appeared in print, they set out to revise the proof.

Closure

> A situation that Goldschmidt put to Thompson at one point: he said, 'imagine that you've died and the classification essentially has been finished. You die; you go up to heaven and St. Peter says, you missed one group and you won't be allowed in until you find it. Do you think you'd ever find it?' Thompson said, 'absolutely not. No chance.' (Ronald Solomon)[45]

In the summer of 1972, 70 mathematicians arrived at the University of Chicago for a month-long conference on finite groups organized by Jonathan Alperin. Participants were housed in the dormitories of the Theological Seminary and listened to lectures by some of the leading mathematicians in the field. Among the mathematicians Alperin invited to speak was Gorenstein, who used the opportunity to announce his '16 step program', the first proposal for a global strategy for a complete proof of the Classification Theorem. Gorenstein listed the obstacles standing in the way of complete classification and outlined the steps that had yet to be accomplished. In later years, Gorenstein acknowledged that while new results convinced him that a full Classification was possible, his program was nonetheless 'built on sheer speculation, supported more by my own enthusiasm than by any mathematical argument' (Gorenstein, 1989: 474). At first, the rest of the group theory community did not share his enthusiasm. However, only 4 years later, on a warm summer Friday afternoon, participants in a group theory conference in Duluth, Minnesota could 'hear the bell toll for the Classification' (Solomon, 2001: 340).

During these years, Gorenstein, who had made significant contributions to the Classification since the early 1960s, coordinated theorists' efforts. Solomon described Gorenstein's role during those final years as the 'central clearinghouse of information', not only for outlining his program, but also for 'keeping everybody informed about what had been done and what everybody was doing at any given moment and what needed to be done'.[46] His position as team leader and his charisma made Gorenstein the de facto spokesman for the community. In 1974, Gorenstein organized a Group Theory Year at Rutgers University, where he was then working. He gathered several finite simple group theorists to work on the remaining problems in the Classification. Gorenstein, who considered the 1960–1961 Chicago Year the most important year of his professional life, strongly believed that the opportunities ‘to interact on a day-to-day basis’ for an extended period of time were of ‘fantastic importance’ to the success of the Classification. According to Gorenstein, several of the results achieved that year were hashed out during daily interactions in which one mathematician would suggest a problem, offer a new line of approach, or even just ask the right kind of question.[47]

If Gorenstein took on a leadership role as far as organization was concerned, Michael Aschbacher was the driving force behind completing the Classification. Beginning in 1973, Aschbacher proved one result after another, at least one of which had previously resisted sustained assaults by other mathematicians, including Thompson.[48] When Aschbacher announced in Duluth that he had solved yet another problem, the group theorists in attendance were convinced that the end was in sight. The Duluth conference had one more unexpected outcome. At the height of their excitement, group theorists began to realize that as their quest was coming to an end, so too was the field that had grown around it and the community that had sustained it. Mathematicians, especially the younger ones, started reevaluating whether they should stay in the field or move on to a new one. Duluth represented a turning point in the growth of the field. The sort of excitement that had characterized the field since the early 1960s was dampened as the end loomed on the horizon. Students were no longer drawn to the field, and while a complete Classification required several more years of sustained work, mathematicians had already laid claim to the major remaining problems (Solomon, 1995).

It is unclear when the theorem was officially considered complete. Gorenstein marks February 1981, but even at that point many of the manuscripts that constituted the proof circulated as preprints, making them available only to community insiders. However, even before the proof was completed, finite group theorists began to reevaluate the state of the field in the wake of the Classification. When they met in Santa Cruz, California in 1979, finite group theorists began planning to revise the Classification and discussed the applicability of the methods, techniques, and results that they had developed over the years to other subfields in mathematics and the sciences. The preface to the conference proceedings concludes, ‘far from being dead, group theory has only just come of age’ (Cooperstein and Mason, 1980: xiii).

By way of conclusion, I turn to the revisionist program, an enterprise that is ongoing today. The aim of the project is to write a ‘second-generation proof’ in an expository style, making it accessible to more mathematicians while correcting the errors in existing papers. This work felt especially pressing to those who realized that when the leading mathematicians involved in the Classification died or retired, the proof would virtually disappear with them. It would not only be impossible for one individual to read the proof in toto, but even to grasp its main structure. Some form of revisionism was present in the Classification effort as early as the late 1960s. Helmut Bender developed new techniques that enabled him to significantly shorten some of the earlier papers, initiating what became known as the ‘Bender method’.[49] However, Daniel

Gorenstein centralized the effort to revise the existing proof. Gorenstein (1982: 8) suggested that a second-generation proof was necessary in order to eliminate existing local errors, revise earlier papers in light of later developments, and make the various parts of the proof self-contained.

Soon after he proposed his revisionist program, Ronald Solomon and Richard Lyons, who were among the crop of young PhDs that inundated the field in the late 1960s, joined Gorenstein in his endeavor to rewrite a large portion of the proof. The first book in the series came out in 1994, 2 years after Gorenstein's death, and the sixth and most recent book in the series was published in 2005. Throughout the late 1970s, Gorenstein gave numerous talks on the status of the Classification, and wrote articles and books aimed at familiarizing mathematicians outside the community with the proof's main structure. He was undoubtedly cognizant that the proof could not circulate on its own, and that, because of its uniqueness in the history of mathematics, it would require convincing for the mathematical community to accept the Classification as a theorem. Furthermore, as he so clearly articulated in the introduction to one of his books, Gorenstein realized that the 'survival' of the proof depended on this revisionist attempt. 'Without the existence of a coherent exposition of the total proof', he wrote, 'there is a very real danger that it will gradually become lost to the living world of mathematics, buried within the dusty pages of forgotten journals.'

Donald MacKenzie and Graham Spinardi (1995) argue that the relevance of tacit knowledge to nuclear weapons design means that, at least in some restricted sense, nuclear weapons can be 'uninvented'. Since nuclear weapon design cannot be acquired solely through the circulation of 'explicit knowledge' and since tacit knowledge is lost if not practiced, the ability to design new weapons could be lost with the death of a core group of nuclear weapons scientists. I suggest that a similar scenario faced finite group theorists in the 1980s. The techniques and methods developed by participants in the Classification did not travel on their own as self-contained documents. Rather, personal, often informal, communication was crucial to their circulation. The announcement that a complete Classification was near, and the lack of new mathematicians entering the field did not bode well for the continued propagation of the Classification and its methods. Beginning in the 1980s, the original proof of the Classification was faced with the threat of uninvention. The papers that constituted it could still be found scattered throughout the mathematical literature, but no one other than the dwindling community of group theorists would know how to find them or how to piece them together. This should not have been a pressing problem for the Classification, because, unlike nuclear weapons, the theorem's future use should not require the reconstruction of its proof. Nonetheless, the use of the theorem did require a faith in the proof's existence.

In the early 1980s, since no one individual could read and carefully check the thousands of pages that constituted the proof, trust in the community's expertise was just as important to the status of the proof as were the numerous papers scattered across the mathematical literature. As long as mathematicians believed that experts in the field could amend local errors, and that at least a few people (such as Gorenstein, Thompson, and Aschbacher) could claim a comprehensive understanding of the proof, mathematicians both within the community and outside of it might trust that the Classification had been proven. But what would happen when these mathematicians were no longer around to solve local errors or vouch for the proof?[50] The revisionist program was necessary to stabilize the proof *as a proof*, to separate, at least symbolically, the experts' trust in the proof and the papers that constitute it. If the project were ever completed, it would likely be incomprehensible to any one individual. However, it would be an artifact that binds the various mathematical arguments, techniques, and methods that went into its production with the community of researchers who produced it.

Philosophers, sociologists, and historians of mathematics have long problematized the notion of proof in mathematics. Starting with Lakatos's (1976) *Proof and Refutation*, scholars have challenged the view of theorems as universal and timeless, showing that what counts as a proof (and subsequently, what counts as rigor) is always contingent on social and historical factors.[51] The Classification is certainly an extreme case – a single proof rarely occupies the attention of so many mathematicians, fills so many pages, or extends for so many decades. However, if recent examples, most notably the proof of the Poincaré conjecture, are any indication, theoretical mathematics may increasingly entail long-term, complex, and collaborative labor. The history of the Classification provides a concrete example of how standards of proof and rigor are established by a dispersed community focused on a shared goal. It suggests that mathematical proofs should *always* be understood as positioned within networks of significance, as well as shared practices, values, and techniques.

Notes

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1. The problem known as 'P vs. NP' belongs to the set of seven Millennium problems. It asks whether a class of problems exists whose solution can be computationally easily verified, but extremely hard to find. The term 'easy' is understood as the time it takes a computer program to reach a solution in terms of the input it receives, where P stands for polynomial time and NP for non-polynomial time (see Carlson et al., 2006).
2. A chronological list of articles and blogs reporting on Deolalika's paper is available at http://michaelnielsen.org/polymath1/index.php?title=Deolalikar_P_vs_NP_paper (accessed 28 December 2011).
3. See Lewenstein (1995) for a comparable example.
4. Gorenstein begins his 1982 introductory book stating that the full proof 'covered something between 5,000 and 10,000 journal pages, spread over 300 to 500 individual papers' (Gorenstein, 1982: 1). However, 14 years later, when the first volume of the 'second generation proof' was published, the authors write that 'the existing proof of the classification of the finite simple groups runs to somewhere between 10,000 and 15,000 journal pages, spread across some 500 separate articles' (Gorenstein et al., 1994: 40). The 50 percent increase in the number of pages constituting the proof points to the uncertainty that surrounded the proof in the first two decades following its announcement. As I will show, what counted as a 'proof' for group theorists was a matter of degree rather than of kind.

5. Henceforth, I use ‘Classification’ as a shorthand for the project of classifying simple groups. Although it was not until the 1970s that one could discern a coherent project to classify all finite simple groups, I use the ‘Classification’ more broadly to refer to the coalescence starting in the 1960s of both group theorists and group theoretical techniques dedicated to the study of finite simple groups.
6. It was not until 2004 that the last known missing piece of the proof, which dealt with the case of the so-called ‘quasi-thin groups’, was written by Michael Aschbacher and Stephen Smith. It was published as a 1200-page monograph by the American Mathematical Society (Aschbacher and Smith, 2004).
7. Here I follow Lorraine Daston, who writes in her introduction to *Things That Talk*: ‘like seeds around which an elaborate crystal can suddenly congeal, things in a supersaturated cultural solution can crystallize ways of thinking, feeling, and acting. These thicknesses of significance are one way that things can be made to talk.’ By following the history of the Classification, I hope to unpack some of the ‘thicknesses of significance’ of these mathematical proofs (Daston, 2003: 20).
8. I have compiled a list of 416 articles based on the bibliographies of Gorenstein’s 1982 and 1983 books, which together give an introduction to the Classification proof. I only include articles published between 1958 and 1983. I limited my list to this period because I am interested in how the Classification community formed around the proof in the mid-20th century. This is certainly not an exhaustive list. Gorenstein (1983: 1) recognized more than 2000 papers published within the field, noting that ‘it is almost impossible for the uninitiated to find the way through the tangled proof without an experienced guide; even the 500 papers themselves require careful selection from among some 2000 articles on simple group theory’. However, in the early 1980s, this set of papers was considered to comprise the body of the proof. The proof, as it is construed today, would not map perfectly onto its 1983 incarnation; some articles would be omitted and replaced by others. Together, these 416 papers are authored by 125 mathematicians. To map the core group, Figure 1 includes only those mathematicians who published three or more articles on the Classification. Each author is associated with the country in which he published his work on the Classification. For those few authors who changed country of residence during the course of their career, I list them under the country in which they published most of their articles in finite group theory. For example, although Thompson moved to England in 1968, for the purposes of this analysis I count him as an American because he began publishing in the US and did most of his Classification work while living in the US.
9. By definition, the entire group and the group consisting of the identity element alone are always normal subgroups of the original group.
10. For non-technical surveys of the history of finite simple groups, see, for example, Gallian (1976), Hurley and Rudvalis (1977), and Neumann (1996). For more technical surveys, see Gorenstein (1982) and Solomon (1995, 2001).
11. Janko’s path to the Classification is unique among finite simple group theorists. That he was able to contribute to the Classification while working in relative isolation in Australia does seem to challenge the significance of personal interactions to the success of the Classification. However, according to Mark Ronan (2006: 137–138), before Janko moved to Australia, he received a fellowship in Germany and spent some time in the early 1960s in Frankfurt, where Reinhold Baer built a center for finite group theory. Moreover, before Janko submitted his paper announcing his construction of a new simple group, he wrote John Thompson a letter to discuss the problem. In 1966, Thompson and Janko co-authored a paper, and in 1968 Janko moved to the US, where he stayed for 4 years before moving to Germany.

12. Readers may note that in my description of group theory, I adopt a Platonist language common among mathematicians who believe, at least sometimes, that mathematical concepts and objects preexist their discovery. I use this trope as a poetic kind of shorthand, and do not mean to convey my own philosophical commitments.
13. Albert served on the Office of Naval Research mathematics advisory committee from 1948, as chairman of the section on mathematics for the National Academy of Science from 1958 to 1961, on the National Security Agency advisory board from 1953, and as consultant for RAND in 1951 (Albert, 2005).
14. ‘Cryptography’ designates writing codes; ‘cryptology’ encompasses both cryptography and cryptanalysis, which is the breaking of codes (see Singh, 2000).
15. In an interview in 1981, Gorenstein recalled: ‘I became an expert in cryptanalysis to make money, since I was making starvation wages as an assistant professor’ (Daniel Gorenstein, interview by Joseph Gallian, 20 August 1981. Finite Simple Group Theory Oral History Collection, 1980–1985, Archives of American Mathematics, The Dolph Briscoe Center for American History, The University of Texas at Austin). Whereas the influence of military funding on physical and biological sciences during the Cold War has been documented by historians of science, the mathematical sciences, specifically theoretical mathematics, has mostly been neglected in this literature (Forman, 1987; Mendelsohn et al., 1989; compare with Dahan-Dalmédico, 1996). As Gorenstein’s professional trajectory demonstrates, research in theoretical and pure mathematics also felt the influence of military patronage. In fact, many of the group theorists who published during the 1960s were supported at least in part by military funding.
16. Daniel Gorenstein, Interview by Joseph Gallian, 17 August 1981. Finite Simple Group Theory Oral History Collection, 1980–1985, Archives of American Mathematics, The Dolph Briscoe Center for American History, The University of Texas at Austin.
17. As this list makes apparent, finite simple group theorists were overwhelmingly male. As far as I know, only four women worked on the Classification: Thompson’s PhD students, Anne MacWilliams and Pamela Ferguson, Betty Stark, a student of Graham Higman, and Nita Bryce, Zvonimir Janko’s graduate student. The near-absence of women from the Classification effort is characteristic of gender disparities in American mathematics at the time. Between 1960 and 1964, only 5.5% of American mathematics PhDs were women, a threefold decline from the decade before the Second World War (Murray, 2001).
18. The theorem that Thompson proved in his PhD thesis states that if a finite group G permits an automorphism α of prime order such that only the identity element in G remains fixed, then G must be a nilpotent group (Solomon, 2001).
19. The designation ‘local group-theoretic analysis’ refers to the study of the structure of the local subgroups of a group G. A *local subgroup* (or a ‘p-local’) of G is the normalizer (that is, the set of elements g in G such that $gHg^{-1}=H$) in G of a non-identity subgroup of prime power p^n. By studying the structure of these local groups, one can obtain information on the structure of the group G (Gorenstein, 1979; Solomon, 2001: 320).
20. Gorenstein, Interview by Joseph Gallian, 17 August 1981. A similar sentiment was expressed by Jonathan Alperin, who commented that over 4 years in which he and Thompson met daily for lunch, only 5 minutes were spent discussing topics unrelated to mathematics. Alperin, interview by Joseph Gallian, 29 June 1982. Box 4RM210. Finite Simple Group Theory Oral History Collection, 1980–1985, Archives of American Mathematics, The Dolph Briscoe Center for American History, The University of Texas at Austin.
21. While the significance of personal interactions and familiarity is emphasized in accounts of the Classification, the available record (letters, interviews, articles, personal histories) does

not reveal the content of these collaborations. The above quotation by Thompson suggests that the whole of his and Feit's work was greater than the sum of its parts. Yet, an analysis of the ways in which collaborations were structured would require different source material (e.g., participant observation).

22. During the 20th century, collaboration became increasingly common in mathematics. In the 1970s, the fraction of mathematicians who collaborated on some papers increased to 64%, and by the 1980s it reached 81%. Similarly, the percentage of papers with only one author decreased from 81% during the 1960s to 54% during the 1990s (Grossman, 2002, 2005).
23. Daniel Gorenstein, interview by Joseph Gallian, 20 August 1981. Finite Simple Group Theory Oral History Collection, 1980–1985, Archives of American Mathematics, The Dolph Briscoe Center for American History, The University of Texas at Austin.
24. David Rowe describes the rise of 'oral culture' (by which he denotes all aspects of mathematical practice that are not textual) in the Göttingen seminar when led by Hilbert and Klein. Rowe argues that over time, formalized mathematical knowledge presented in texts was eclipsed by oral communication in Göttingen. Rowe calls upon historians of mathematics to study the leading mathematical centers of the 19th and 20th centuries, in order to analyze the role of 'local cultures' in mathematical research. The Classification suggests that in studying the history of 20th century mathematics, an emphasis on 'local cultures' should be supplemented by histories of communities of researchers that are centered on shared research agendas rather than location. One can still track the formation of 'local cultures' by following the emergence of new research fields.
25. Gorenstein, interview by Joseph Gallian, 20 August 1981.
26. Minimal groups are simple groups in which all proper subgroups are solvable.
27. Mathematician Michael Collins (1992) noted that the book was 'indispensable reading to those moving into the subject'.
28. Michael Aschbacher, interview by Joseph Gallian, 9 January 1981. Box 4RM210. Finite Simple Group Theory Oral History Collection, 1980–1985, Archives of American Mathematics, The Dolph Briscoe Center for American History, The University of Texas at Austin.
29. Alperin, interview by Joseph Gallian, 29 June 1982. Box 4RM210. Finite Simple Group Theory Oral History Collection, 1980–1985, Archives of American Mathematics, The Dolph Briscoe Center for American History, The University of Texas at Austin.
30. Gorenstein, interview by Joseph Gallian, 20 August 1981.
31. Alperin, interview by Joseph Gallian, 29 June 1982.
32. Alperin, interview by Joseph Gallian, 29 June 1982.
33. In the introduction to *Finite Groups*, Gorenstein commented on this situation, giving the following examples: Thompson's classification of minimal simple groups was 410 pages and published in six parts; Walter's classification of simple groups with abelian Sylow 2-subgroups was a 109-page paper; Alperin, Brauer, and Gorenstein's classification of simple groups with quasi-dihedral or wreathed Sylow 2-subgroups was 261 pages; Gorenstein and Harada's classification of simple groups whose 2-subgroups are generated by at most four elements was 461 pages; and Aschbacher's 'classical involution' theorem was 115 pages long.
34. The American Mathematical Society (AMS) established *Mathematical Reviews* in 1940 as an abstracting journal for the mathematics community (Siegmund-Schultze, 1994). The main aim of *Mathematical Reviews* is to enable mathematicians to stay informed of the growing literature in the field by consolidating expert reviews of current articles and books in mathematics. The reviewer's job, therefore, is quite distinct from that of the referee, who must check the validity of the proof in question and decide whether to recommend the paper for publication. Moreover, while the referee remains anonymous, most reviewers are not.
35. *Mathematical Reviews* is available online through MathSciNet: www.ams.org/mathscinet/ (accessed 28 December 2011). Each review is given a unique seven-digit MR identification

number. The MR numbers of Thompson's reviews of Gorenstein and Walter's paper are MR0177032 and MR0190220.

36. Gorenstein, interview by Joseph Gallian, 20 August 1981.
37. By 'proof', I here refer to both singular proofs and proof as an epistemic standard.
38. Jonathan Alperin, interview by Joseph Gallian, 29 June 1982. Box 4RM210. Finite Simple Group Theory Oral History Collection, 1980–1985, Archives of American Mathematics, The Dolph Briscoe Center for American History, The University of Texas at Austin.
39. Thompson was not immune to making errors in papers. Most notably, in the first part of his six-part N-group paper, published in 1968, Thompson set out to classify all N-groups (i.e. all finite simple groups whose local subgroups are all solvable). However, because of a calculation mistake he made in his notes, he did not include in the list the sporadic group $F_4(2)$, which is an N-group (see Thompson, 1974: 573).
40. As Aschbacher described the situation: 'as the length of a proof increases, so does the possibility for error. The probability of an error in the proof of the Classification Theorem is virtually 1. On the other hand the probability that any single error cannot be easily corrected is virtually zero, and as the proof is finite, the probability that the theorem is incorrect is close to zero. As time passes and we have an opportunity to assimilate the proof, that confidence level can only increase' (Aschbacher, 1980: 59).
41. Daniel Gorenstein, interview by Joseph Gallian, 21 August 1981. Finite Simple Group Theory Oral History Collection, 1980–1985, Archives of American Mathematics, The Dolph Briscoe Center for American History, The University of Texas at Austin.
42. Steven Shapin has written extensively on the central role of trust in scientific enterprise. In *A Social History of Truth,* Shapin (1994: 415) writes that 'one story about the modern condition points to the anonymity and system-trust in abstract capacities, while the other identifies persisting patterns of traditional familiarity and trust in known persons'. Following Harry Collins, Shapin emphasizes the significance of a core set as a way of maintaining trust through personal face-to-face communication. As the Classification project expanded, the existence of a self-identified core group was instrumental to the success of the project. The dispersed nature of this network forced group theorists to reconvene often and for extended periods of time to maintain the face-to-face interaction that was crucial to the success of their work. Moreover, as the individual proof involved in the Classification became increasingly unwieldy, the dependence on personal communication and trust increased.
43. Ronald Solomon, interview by Joseph A. Gallian, 12 January 1982. Finite Simple Group Theory Oral History Collection, 1980–1985, Archives of American Mathematics, The Dolph Briscoe Center for American History, The University of Texas at Austin.
44. Daniel Gorenstein, interview by Joseph Gallian, 21 August 1981.
45. Solomon, interview by Joseph A. Gallian, 12 January 1982.
46. Solomon, interview by Joseph A. Gallian, 12 January 1982.
47. In 1975, David Goldschmidt (1975) echoed Gorenstein's claims: 'it is a pleasure to report that the 1974–1975 "group theory year" at Rutgers University and the participants therein provided the creative and very stimulating atmosphere in which this paper originated.'
48. The problem was known as the 'thin group' case. Solomon recalled:

    Sometime around spring in '75, Janko wrote to Danny and said, I give up. I can't do the thin group problem … . Thompson who was visiting at Yale at the time started working on the thin group problem after Janko threw in the towel. He worked on it for several months around that spring and got stuck … . And I think Danny was seriously worried that if Janko couldn't do it and if Thompson didn't see at least after a few months of thinking about it how to finish it off, maybe there was some major intractable problem that the methods that we had developed just couldn't handle. And Aschbacher picked up that problem over the summer and by the end of the summer finished it. And I think that was the psychological turning point of the classification. (Solomon, interview by Joseph A. Gallian, 12 January 1982)

49. Helmut Bender was among several of Reinhold Baer's students in Germany (others were Bernd Fischer and Franz Georg Timmesfeld). Between 1935 and 1956, Baer lived in the US, serving as chair of the Department of Mathematics at the University of Illinois at Urbana-Champaign. He also became a good friend of Richard Brauer. In 1956, Baer returned to Germany and continued teaching in Frankfurt, establishing a center for algebraists.
50. In 2005, British mathematician Brian Davies voiced this concern:

    The completion of the classification project (in the sense of the publication of a connected account of the entire calculation) is threatened by the attrition of the leading players by death and retirement. Within ten years most of them may have stopped working, and there may well be too few left with the necessary deep understanding of the subject to complete the task.

    Davies's article angered Aschbacher, Smith, Solomon, and Lyons, who wrote a letter to the editor, pointing out several errors in Davies's article. The four, however, did not respond directly to this claim (Davies, 2005).

51. For philosophical literature on proof, see Kitcher (1985), Lakatos (1976), and Tymoczko (1998). For sociological studies, see Barnes et al. (1996), Bloor (1978, 1994), Livingston (1999), MacKenzie (1995, 1999, 2001), and Rosental (2008). For historical accounts, see Grabiner (1974), Kleiner (1991), and Richards (1991).

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