Big mathematics – reflections on the history of the Classification of Finite Simple Groups, 1950s to 1980s

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The Classification of Finite Simple Groups (CFSG) is a highlight of 20th-century mathematics, both with respect to its mathematical content and to the complex process of proving the result. From a historical perspective, it offers an excellent opportunity to focus on more general developments in the history of 20th-century mathematics, such as changing perceptions of what a mathematical proof is, the character and the many contexts of mathematics as an intergenerational and international collaborative enterprise, the roles that trust and consensus play within this enterprise, and the impact of Cold War research policies on the CFSG/pure mathematics. We consider the CFSG as (possibly) the first instant of what we tentatively call "big mathematics" in our project. In this article we report on work in progress, first sketching out our project and then explaining what we mean by "big mathematics."

1 The project: history of the CFSG

Scope

We begin with Hans Zassenhaus (1912–1991), a student of Emil Artin, who briefly had a chair at Hamburg University before permanently leaving Germany in 1948. After an academic peregrination (Glasgow, Montreal, Notre Dame), he worked at Ohio State University from 1964. In his 1947 inaugural lecture in Hamburg, Zassenhaus presented several mathematical areas of great interest to him, starting with groups. He explicitly called for a classification of finite simple groups:

> The most interesting problem, to me, seems to be the search for all finite simple groups. We are still far away from a solution to this problem. [37, p. 5]

Zassenhaus was not the first mathematician to wish for a classification of finite simple groups, and he was not the last one to press the question, lamenting a lack of substantial progress. Richard Brauer (1901–1977), a German mathematician who lost his position in 1933, emigrated and from 1952 held a chair in Harvard, took the question up in his plenary talk at the ICM in Amsterdam in 1954:

> The theory of groups of finite order has been rather in a state of stagnation in recent years. This has certainly not been due to a lack of unsolved problems. As in the theory of numbers, it is easier to ask questions in the theory of groups than to answer them. If I present here some investigations on groups of finite order, it is with the hope of raising new interest in this field. [7, p. 209]

New interest rose, indeed, and around 1980 the community agreed that the CFSG had been accomplished. We state the final CFSG here exactly as it appears in [17, p. 6]:

Classification Theorem. *Every finite simple group is cyclic of prime order, an alternating group,*[1] *a finite simple group of Lie type,*[2] *or one of the twenty-six sporadic*[3] *finite simple groups.*

This statement, in its simplicity and elegance, might seem surprising, and the short list of possibilities for all finite simple groups is misleading in the sense that it does not reveal the complexity and scope of what is going on here. In the words of Michael Aschbacher (1944–), one of the main contributors to the proof of the CFSG theorem:

> The hypothesis of the theorem is simple and easily understood by anyone who has taken a decent undergraduate course in abstract algebra. The conclusion also appears at first glance to be at least moderately simple. However, when one looks more closely, one finds that it takes some effort and sophistication to define many of the examples. [4, p. 2403]

[1] We add for clarity that the alternating groups of degree 1, 2 and 4 are not simple, but that otherwise a finite group is simple if and only if it occurs in the list given in this theorem.

[2] Without giving a definition, we want to mention that a group of Lie type is not the same as a Lie group.

[3] A term coined by William Burnside in 1911 for finite simple groups that do not belong to an infinite series.

And what about the proof? According to [17, p. 3], we were originally dealing with:

> […] somewhere between 10,000 and 15,000 journal pages, spread across some 500 separate articles by more than 100 mathematicians, almost all written between 1950 and the early 1980s. Moreover, it was not until the 1970s that a global strategy was developed for attacking the complete classification problem. In addition, new simple groups were being discovered throughout the entire period […] so that it was not even possible to state the full theorem in precise form […]. Under such circumstances it is not surprising that the existing proof has an organizational structure that is rather inefficient and that its evolution was somewhat haphazard, including some duplications and false starts.

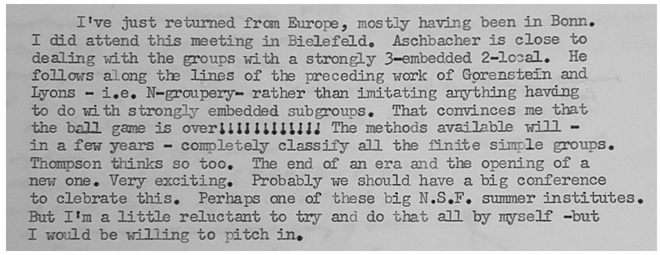
This illustrates that the CFSG is a piece of mathematics of enormous scope, both in terms of the depth and breadth of the mathematics involved and in terms of the duration of the project, the number of people involved and resources used. Towards the end, in the 1970s, there was a strategy for proving the CFSG, but still it is difficult to fully grasp the amount of communication and coordination that was needed. As we will see in more detail below, this leads to several layers of unsurveyability and many interesting questions from the perspective of the history of mathematics as well as the history of science.

Sources

There are easily available printed sources to study the history of the CFSG, such as survey articles by some of the main contributors (e.g., Aschbacher, Solomon), obituaries of key contributors (e.g., Feit, Gorenstein, Suzuki), research articles contributing to the proof, and conference proceedings. Moreover, there are also transcripts of interviews conducted by Joseph Gallian with some of the main contributors. These are available in the Archives of American Mathematics at the Briscoe Center for American History, Austin, Texas, and we have conducted many interviews ourselves over the last two years. Moreover, we have begun to collect personal papers of contributors to the CFSG. Where we have permission, such original material will be made available over the course of our research project. Given the large number of contributors, it is a challenge to decide who are the most important ones, and by what criteria. Such decisions depend, at least partly, on the timeline of the CFSG.

Timeline

Daniel Gorenstein (1923–1992), who played a crucial role in the CFSG and is often referred to as its "CEO" or "architect," as well as other contributors tend to put emphasis on a period of about 30 years. However, there are arguments for both a shorter and a longer time frame of this story. One might argue that just a few years in the late 1960s and 1970s are the crucial time period, starting with John Thompson's (1931–) so-called N-group paper ([34], first article of three), Daniel Gorenstein's programme to tackle the classification presented in Chicago in June 1972 [15, pp. 178–193], a number of crucial publications by Michael Aschbacher and others, and with a feeling just a few years later that the work was mostly done. Ron Solomon (1948–) remarked that "we could hear the bell toll" in 1976 at the Duluth conference [30, p. 340], and Jon Alperin (1937–2025) expressed the feeling "that the ball game is over" in a letter of 1976 to Walter Feit (1930–2004) (Figure 1).



I've just returned from Europe, mostly having been in Bonn. I did attend this meeting in Bielefeld. Aschbacher is close to dealing with the groups with a strongly 3-embedded 2-local. He follows along the lines of the preceding work of Gorenstein and Lyons - i.e. N-groupery- rather than imitating anything having to do with strongly embedded subgroups. That convinces me that the ball game is over!!!!!!!!!!!! The methods available will - in a few years - completely classify all the finite simple groups. Thompson thinks so too. The end of an era and the opening of a new one. Very exciting. Probably we should have a big conference to clebrate this. Perhaps one of these big N.S.F. summer institutes. But I'm a little reluctant to try and do that all by myself -but I would be willing to pitch in.

Figure 1. Jon Alperin to Walter Feit, 27 June 1976, from the Walter Feit Papers, Dolph Briscoe Center for American History, The University of Texas at Austin (4RM121).

However, our story might well begin with the first explicit call for a classification by Otto Hölder (1859–1937), who noted in 1892, that it "would be of the greatest interest, if a survey of all simple groups could be given whose number of operations is finite" [19, p. 55]. If we then take the last relevant publications for the so-called first-generation proof as the end point [35], the classification project would have spanned 114 years. One might begin even earlier with the Mathieu groups discovered in the 1860s and 1870s as highly transitive subgroups of the symmetric groups of degree 11, 12, 22, 23 and 24, respectively, counting among the 26 sporadic groups [28]. We will, however, follow [30] and consider [19] the beginning of the CFSG project. Towards the end, we can see how Gorenstein's strategy from the 1970s was successful and that the techniques did in fact lead to a proof of the CFSG theorem. It took until 2004 for all major cases to be completed and published. To shed some light on this delay, we briefly explain some background.

The analysis of finite simple groups along their local structure includes a case distinction along a parameter called $e(G)$,[4] which captures an aspect of the interplay between the prime 2 and odd primes dividing the group order. The case where $e(G) = 2$, famously called the "quasithin case" of the CFSG, had been partially treated, but without publication, and it was noticed that deep and difficult

[4] $e(G)$ denotes the maximum p-rank, for odd primes p, of 2-local subgroups of a finite group G.

work was necessary to close this gap. This work has been done by Aschbacher and Smith [5] long after the CFSG had been announced to be completed.

People

What is a contribution to the proof of the CFSG theorem? Does it matter whether a result was proven specifically with the CFSG in mind or just out of general interest, moving the theory forward? The definition of a mathematical or strategic contribution will influence whom we consider a major contributor, along with the definition of the timeline. With this in mind, we recall a few names: Jon Alperin, Michael Aschbacher, Richard Brauer, William Burnside, Walter Feit, Daniel Gorenstein, Stephen Smith, Ron Solomon, and John Thompson (in alphabetical order, concentrating on the 20th century and on direct contributions). Some more appear if we look at milestones on the way. Brauer and Fowler proved that, if H is a finite group with a central involution, then there are only finitely many possibilities for a finite simple group G with involution centraliser H [9]. Starting with an involution centraliser of structure $C_2 \times \mathrm{Alt}_5$, this approach led Zvonimir Janko (1932–2022) to the discovery of a new sporadic simple group, J_1, in 1965 [20]. The 1950s and 1960s brought new ideas and techniques. Some of them came from work of Michio Suzuki (1926–1998), a Japanese mathematician who for many years worked at the University of Illinois at Urbana-Champaign and discovered a new series of finite simple groups of Lie type as well as a sporadic simple group. His articles were influential in terms of concepts and strategies for the characterisation of groups with given properties. In the early 1960s, we see the fundamental work by Feit and Thompson, proving that groups of odd order are soluble. In other words, every non-abelian finite simple group contains an involution, and this is one of the reasons why the prime 2 plays this very special role among the prime divisors of group orders for finite simple groups.

Around the same time, we see that the general activity grows, with more young mathematicians entering the field and the culture changing by a rising number of conferences and special programmes. This is true for several, maybe most areas of mathematics and of science in general, but we mention it because the CFSG problem is so difficult and has so many sub-problems that need to be solved, that it is hard to imagine how the actual theorem could have been proven without the conferences and special programmes that brought a substantial number of researchers together, allowed them to discuss their progress, new methods and remaining open problems and that eventually led to a strategy towards a complete proof.

New sporadic groups were found in the late 1960s and the 1970s by John Conway (1937–2020), Bernd Fischer (1936–2020), and some of their students, to name just a few. We would have to list many more people if we took individual results and techniques into account (e.g., Helmut Bender, George Glauberman) or computational work (e.g., Charles Sims). At some point, it became necessary to collect the knowledge that was there and to create some kind of plan for how to proceed further. This is why we identify Daniel Gorenstein's programme from 1972 as a milestone, next to and building on John Thompson's N-group paper series from 1968 to 1973.

Once there was a list of finite simple groups that was thought of as (almost) complete, it was natural to begin with a minimal counterexample, i.e., a finite simple group G that is not on the list of known finite simple groups and of minimal order with this property. Then, Gorenstein suggested a case distinction with several layers, and he explained where existing publications already covered some of the cases, where cases were still wide open, where he had an idea about how to proceed further, and where he expected difficulties.

We have been told in interviews (and this is supported by self-historicising articles) that at the time, in 1972, this looked like a good, but completely unrealistic plan. Still, the mere existence of a strategy and evidence towards its feasibility made a difference in the research community, and the division into cases facilitated splitting up the work still to be done. We would like to better understand the dynamics in the research community exactly around that time, 1972–1976, because of the astonishing activity during these few years and the surprisingly quick progress in Gorenstein's programme. For such a complex project with hundreds of contributions, it is difficult to imagine how, without such a programme, the community would ever have felt certain that they were done classifying. (After all, with an incomplete list, the strategy has to make sure that any unknown finite simple group is discovered, or at least evidence for it, in the thorough case-by-case analysis. This was covered by Gorenstein's approach.)

Therefore, next to the strictly mathematical side and all the theorems that were proven treating cases of Gorenstein's programme, we are interested in the organisational side of the CFSG as well. According to the community, Gorenstein played a fundamental role in assigning tasks and keeping track of progress. After presenting his programme in 1972, Gorenstein became the key authority, trusted by the community (Figure 2). The many names that he has been given by the community point to his being seen as the coordinator of the project, at least for the final stage: CEO, godfather, architect of the CFSG. Jon Alperin explained in 1982 that, in 1968, Gorenstein had not yet been "captain of the team," the role that he would only grow into later, when he, in Alperin's words "was the one that was the chief optimist" [14, pp. 23, 15]. In our view such a complex, technically difficult piece of mathematical research needs, at some point, a person with energy and charisma, drive and chutzpah, motivating colleagues to tackle the difficult problems, to keep surveying the field, while also having expertise themselves. For a young academic entering the research area of finite simple groups, the whole project was probably overwhelming, but Gorenstein's strategy made it possible to pick up a case, work on that and

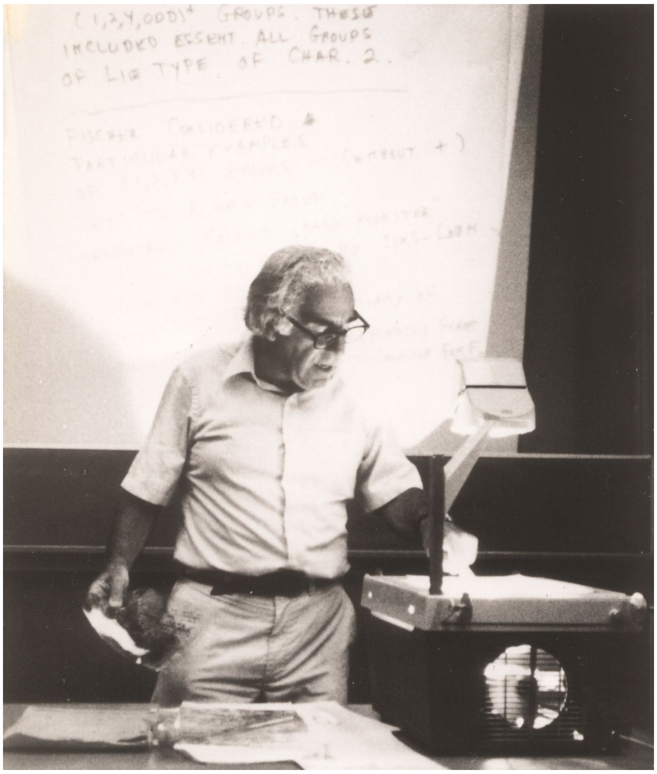


Figure 2. Daniel Gorenstein (1923–1992). (Photo: Konrad Jacobs; source: Archives of the Mathematisches Forschungsinstitut Oberwolfach)

contribute in this way. His energy and charisma helped to attract young academics and secure funding, and towards the end it certainly contributed to "communal energy," moving quickly through the remaining cases. We hope that, in our ongoing research, we can find more sources showing how exactly Gorenstein and others divided up the work and how communication on the progress worked, besides what we know about the key conferences.

Also, for clarity, we do not think that the CFSG would have been impossible without Gorenstein. There are many places in the CFSG where, if one person would not have proven a theorem or found a new group, then probably someone else would have done it, maybe a few years later. But there have been key contributions, key ideas, and their absence would have changed the course of the whole CFSG project dramatically. Maybe someone else would have stepped into Gorenstein's role, maybe a group of people, maybe decades would have gone by with more and more cases being solved, but without an end in sight. Maybe the internet and digital collaboration tools would eventually have led to some kind of coordination, and the CFSG would have happened some decades later. But even then, even with the actual work being done, we do not have a proof without a strategy for how the proof is organised and publications that follow this strategy. This needs at least one person with the whole picture in mind, and we have much evidence that Gorenstein was such a person for the CFSG from the 1970s to his death in 1992.

Geographic spread

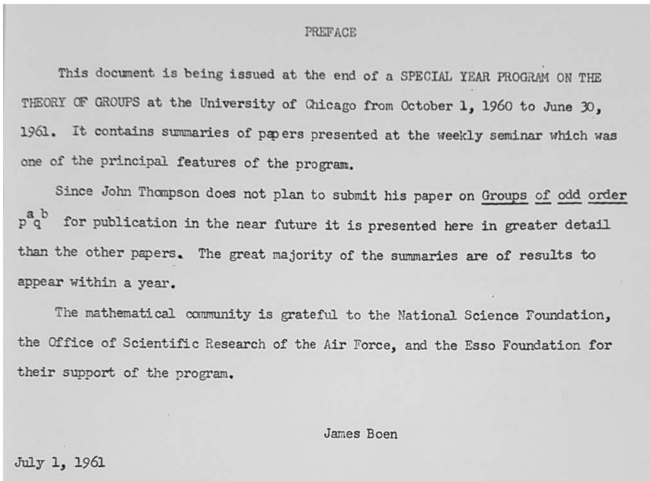
Our definition of the timeline and of a contribution to the CFSG will also influence where we think the work on the CFSG took place. Some locations are easy to identify. Chicago, because of the significant special programme in finite group theory in 1960/61, Rutgers and Princeton, again because of special programmes, Duluth, Durham and Santa Cruz because of famous conferences, Cambridge (UK) because of computing facilities and the influence of John Conway, John Thompson and their students, Oberwolfach because of the Mathematical Research Institute, Bielefeld because of Bernd Fischer and his students. The list goes on, and we identify several countries (e.g., France, Germany, Japan, UK, Australia) as well as, within the U.S., several universities with great distances between them.

One way to illustrate the geographic spread of the CFSG community would be to look at printed contributions between 1950 and 1985. Here a preliminary literature review shows a substantial number of CFSG-relevant publications from the USSR during that time. This was surprising to us because we could hardly find any evidence of references to CFSG work being done across the Iron Curtain. Except for the ICM in 1966 in Moscow and individual reports of personal correspondence between colleagues, we do not have much evidence of an exchange of research results in that area or of actual collaboration. We have started to investigate this further.

Teamwork

Getting mathematicians together in thematic (international) meetings was relatively new in the 1950s and 1960s, even though topology set a precedent from the mid-1930s [3]. From the 1950s such in-person meetings became increasingly instrumental for teamwork oriented research such as the CFSG turned out to be. The Oberwolfach Institute with its thematic workshops is a very good case in point – Gerhard Pazderski, a participant of one of these workshops in 1960 who at the time worked in East Germany, remembers vividly a series of lectures given by John Thompson about an early stage of his work on the odd order theorem. These lectures allowed the audience to witness an outstanding development in the theory of finite groups long before publication. Another prominent example is the Group Theory Year in Chicago 1960/61 (Figure 3), organised by Adrian A. Albert (1905–1972), where according to Solomon, the "first real signs appeared of the evolving sociology of the classification effort as a team project" [30, p. 330].

In 1960/61, the Chicago team included many of the central figures of the classification project, such as Jon Alperin, Richard Brauer, Walter Feit, Daniel Gorenstein, Graham Higman, Michio



PREFACE

This document is being issued at the end of a SPECIAL YEAR PROGRAM ON THE THEORY OF GROUPS at the University of Chicago from October 1, 1960 to June 30, 1961. It contains summaries of papers presented at the weekly seminar which was one of the principal features of the program.

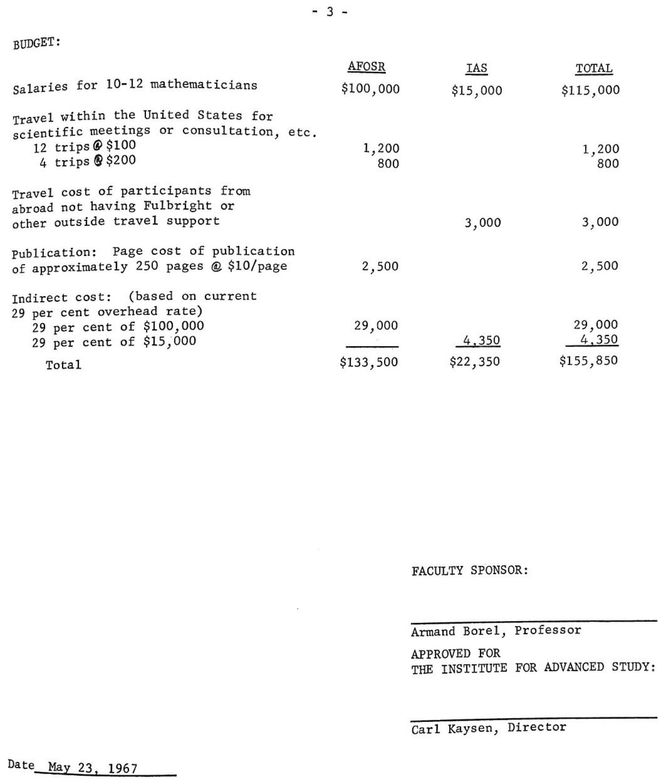
Since John Thompson does not plan to submit his paper on Groups of odd order $p^a q^b$ for publication in the near future it is presented here in greater detail than the other papers. The great majority of the summaries are of results to appear within a year.

The mathematical community is grateful to the National Science Foundation, the Office of Scientific Research of the Air Force, and the Esso Foundation for their support of the program.

James Boen

July 1, 1961

Figure 3. Preface of [6].



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BUDGET:

	AFOSR	IAS	TOTAL
Salaries for 10-12 mathematicians	\$100,000	\$15,000	\$115,000
Travel within the United States for scientific meetings or consultation, etc.			
12 trips @ \$100	1,200		1,200
4 trips @ \$200	800		800
Travel cost of participants from abroad not having Fulbright or other outside travel support		3,000	3,000
Publication: Page cost of publication of approximately 250 pages @ \$10/page	2,500		2,500
Indirect cost: (based on current 29 per cent overhead rate)			
29 per cent of \$100,000	29,000		29,000
29 per cent of \$15,000		4,350	4,350
Total	\$133,500	\$22,350	\$155,850

FACULTY SPONSOR:

Armand Borel, Professor

APPROVED FOR
THE INSTITUTE FOR ADVANCED STUDY:

Carl Kaysen, Director

Date May 23, 1967

Figure 4. IAS Princeton, (successful) grant proposal for the Group Theory Year 1968/69 to the Air Force Office of Scientific Research, 1967. (IAS archives, AFSOR 68-1468)

Suzuki, and John Thompson. In 1982, Alperin assessed the Chicago Group Theory Year as "still one of the best one-year-long programs I think there's ever been, in terms of what happened" [14, p. 6]. At the same time Gorenstein described the CFSG as an "unprecedented thirty-year team effort" [16, p. 58]. From this, as well as from recent interviews with some of those involved, it is clear that teamwork was in the DNA of the classification community from the late 1950s on, if not earlier.

This brings us to the question of logistics and funding.

Resources

Even now, organising big (or medium-sized) conferences takes a lot of time and money. It is hard for us to imagine how much more time-consuming and expensive it was in the 1960s and 1970s. The availability of public transport and the political situation often created unexpected obstacles, and long-distance travelling (airfares) was more cost intensive than today. The context of the Cold War offered new possibilities for science funding on a large scale, also for pure mathematics. New funding opportunities facilitated bringing ambitious plans into reality. For the Chicago Group Theory Year 1960/61 mentioned above, we have information about the sponsors, including the National Science Foundation, the Air Force Office of Scientific Research and the Esso Foundation, but not about the actual budget. However, for the Group Theory Year at the IAS Princeton in 1968/69, organised by Armand Borel (1923–2003) who at the time had a strong interest in the CFSG, we have a grant proposal (Figure 4) and know that it was sponsored by the Air Force Office of Scientific Research, a regular patron of the IAS at the time, with 133,500 USD (ca. 1.3 million USD in 2025).[5] Among the people invited from the CFSG community were Jon Alperin, Richard Brauer, George Glauberman, Daniel Gorenstein, Zvonimir Janko, Michio Suzuki, and John Thompson.

Budgets for the conferences the CFSG community attended and organised were much smaller, and some were sponsored by the AMS, supported by an NSF funding scheme. However, it was in trying to get an overview of the relevant and specialised conferences for the CFSG that we began to understand the scope of the resources necessary to facilitate the exchange of knowledge and to keep the CFSG team together. The budget for the crucial conference in Duluth in 1976, covered by the NSF, ran up to 8,700 USD (ca. 50,000 USD in 2025).[6] Considering the number of conferences with at least a partial focus on the CFSG or programmes like the Group Theory Years mentioned, this adds up to a substantial overall budget – at least for research in pure mathematics. We would like to specify this much more, following up on grant numbers (if they are published), searching in university archives (if documentation of a special programme has been kept) or in personal papers of conference organisers (if we have access and if they have kept the relevant documents). We do not claim that the CFSG would not

[5] Cf. https://www.bls.gov/data/inflation_calculator.htm.

[6] NSF to Joseph Gallian, 28 May 1976 (Joseph Gallian has kindly made this information available to us).

have been possible without the funding for the conferences and special programmes. However, given the large number of people involved, in particular in the 1970s, opportunities for face-to-face communication make a huge difference. Without them, the information would have reached much fewer people, and the process would have taken much longer.

Another perspective on the resources needed are the cost of computing time and the availability of computing facilities in the 1960s and 1970s. For the above-mentioned Janko group J_1, for instance, a group of order only 175,560, a computer was used to find two explicit matrices that generate the group as a matrix group. It was a recurring theme that evidence for a new sporadic simple group was found, and its existence was proven later, often supported by computer calculations. Another example illustrates this: the Monster group, the largest of the sporadic groups, of order $2^{46} \cdot 3^{20} \cdot 5^9 \cdot 7^6 \cdot 11^2 \cdot 13^3 \cdot 17 \cdot 19 \cdot 23 \cdot 29 \cdot 31 \cdot 41 \cdot 47 \cdot 59 \cdot 71$.[7] Charles C. Sims (1937–2017), one of the central players in the chase for sporadic groups, when looking back on his efforts to construct sporadic groups in 1978, also commented on the role of computers and the ensuing costs. With respect to the Monster group, he explained:

> These rough calculations indicate that the construction of the monster is technically feasible but not yet economically justifiable. I doubt that anyone would argue that the existence or non-existence of the monster is of such importance to the mathematical community that it warrants devoting the resources of a major computer center for the better part of a year to settle the matter. However, it should be possible to refine the techniques sufficiently so that the question can be answered at a price which can be justified. [29, p. 135]

A computer-free construction of the Monster group was later given by Robert Griess [18]. We would like to better understand the role of computers/computing facilities in the original proof of the CFSG theorem and how this was discussed within the community. After all, depending on how a computer is used, it changes the nature of proof of a mathematical result and therefore the culture of the discipline.

Coming back to the costs of computing time, Richard Brauer, also engaging with the question whether there were "finitely many or infinitely many sporadic groups," expresses concern about the cost of computational resources in 1976 (published after his death in 1979):

> If they succeed in finding an upper bound for the order of sporadic groups, then the whole problem can be handled by a computer, though the costs of the project may very well be prohibitive. [8, p. 22]

These examples give an idea of the resources that were relevant in the classification project, and we have already seen some of the sources of funding. In the context of the Cold War there was a wide array of funding opportunities. Of course, we would expect that in the U.S., pure mathematics would have been supported by the NSF and the AMS as well as private and industrial funding agencies. In the UK we see funding from the Science Research Council, and research visits were paid for by the German Research Foundation in (West) Germany. We still have to systematically investigate funding and budgets of the major CFSG conferences in Australia, Europe, the UK, etc. However, the scope of military funding that went into pure mathematics in general and the CFSG in particular is striking (e.g., in the U.S., the Air Force Office for Scientific Research, the Office for Naval Research, and the NSA), and we intend to specify this in much more detail and find out if the amount of funding for the CFSG was in any way unusual, compared to projects in other disciplines. It fits into this context that, in some of our interviews, it became clear that mathematicians, even when trained in areas that seem far away from applications, were deemed a crucial resource in both the civil and military arenas. This is supported by advertisements such as the in the Notices of the AMS shown in Figure 5.

2 Our framework: big mathematics

As we have seen, the CFSG is of enormous scope, and so is its historical analysis. To be better able to tackle it, we use the analytical framework of what we tentatively call "big mathematics," to be laid out in this section.

From a historical perspective, the history of the CFSG has not yet been systematically studied (but see [32]), even though it offers an excellent opportunity to focus on more general developments in the history of 20th-century mathematics, such as:

(1) changing perceptions of what a mathematical proof might be;
(2) the character of mathematics as an intergenerational and international collaborative enterprise;
(3) the impact of the Cold War on science and mathematics.

(1) The CFSG raises specific questions about the nature of proof in mathematics, mainly with respect to two epistemologically different types of surveyability. On the one hand, the surveyability of mathematical proofs in a sense very specific to the CFSG has been viewed critically, because the proof is scattered in a huge number of papers, each (in principle) individually checked or verified by mathematicians, but difficult or impossible to understand as a whole. On the other hand, the use of computers in proofs was often seen as entailing a certain kind of opacity in allowing access to the content of the proof, i.e., that the proof might not be verifiable in detail by individual mathematicians, a problem discussed since

[7] We display the group order because we want to make the magnitude explicit, also for computer calculations. To this day, it is a challenge to do computer calculations in the Monster group.

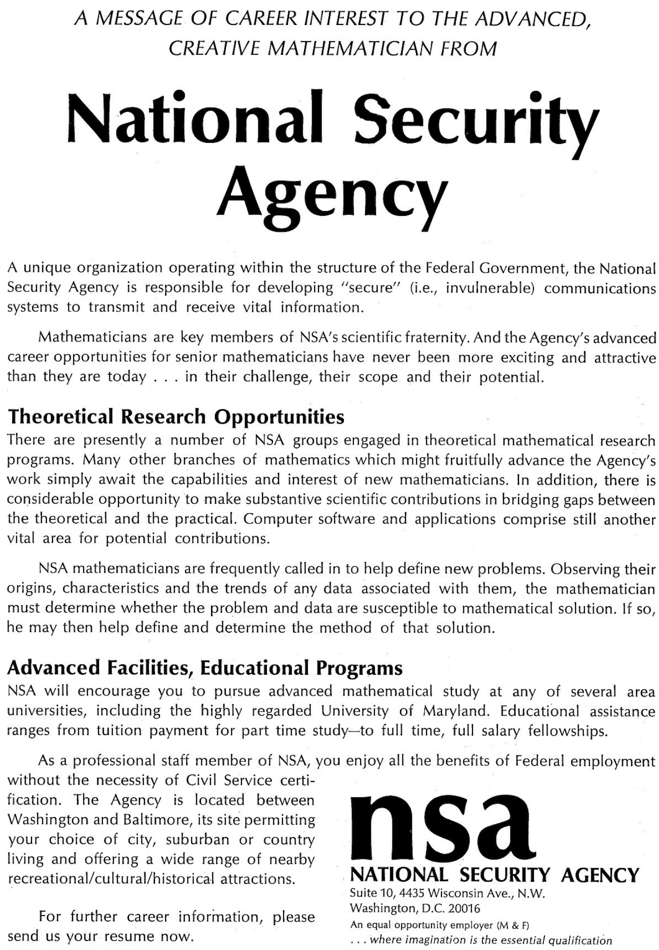

Figure 5. NSA advertisement in the *Notices of the AMS* **13**, p. 179 (1966).

1976 in the context of the proof of the four-colour theorem [22]. These aspects are deeply intertwined with the roles that trust and consensus played within the community.

(2) The CFSG is exemplary with respect to the character and the many contexts of mathematics as an intergenerational and international collaborative enterprise. Apart from international cooperation in the CFSG community, specific aspects such as the importance of German émigré mathematicians, e.g., Richard Brauer and Reinhold Baer (1902–1979) stand out. The example of Baer, who had left Germany in 1933, but returned to a professorship in Frankfurt in 1956, is interesting from an additional perspective. While he might not be seen as a contributor to the CFSG himself, he influenced a generation of mathematicians some of whom became contributors to the CFSG (for example Helmut Bender, Bernd Fischer, and Dieter Held).

(3) Finally, as we have explained, the history of the CFSG can be studied as a key example of the impact of Cold War politics on research in general and on mathematical research in particular, namely via new possibilities of funding through both civil and military agencies. With respect to historical detail about what fields in mathematics profited from this and how, this is a rather unexplored territory, and we have a feeling that it is crucial for the classification project. In this context, the concept of "big mathematics" can be useful.

The classification effort took decades, and more than 100 mathematicians were "enrolled" (many of them via PhD theses) [15, p. 44]. The sheer size of the group active in the CFSG between the 1950s and the 1980s is without precedent in the history of mathematics, and mathematician E. Brian Davies characterised the CFSG as "industrial scale pure mathematics" [11, p. 1353], while George Szpiro, journalist and mathematician, even compared the CFSG to the genome project [33, p. 94]. In this vein we want to tentatively consider the phenomenon of the CFSG as "big mathematics," in a sense adapted from characterizations of "big science" [10, 13]. Big mathematics would be a project that is:

(1) geographically widespread;
(2) teamwork oriented;
(3) resource intensive;
(4) relying on large equipment;
(5) at some point necessitating a central strategy and coordination.

Before we briefly discuss these characteristics, a little caveat is in order. Standard examples usually referred to when discussing big science include Stanford's Linear Accelerator (SLAC), CERN, or Los Alamos. Clearly, when proposing "big mathematics" as an analytical framework to study the history of the CFSG, we understand we need to scale down in terms of "big." Also, we are well aware that the proposed concept of big mathematics needs to be open to adaptation (cf. ideas developed in [1, 23]). We do not intend to use the framework of big mathematics to claim that the CFSG is the only instance of mathematical research possibly meeting these criteria. On the contrary, once we have worked with the concept, and clarified some of its strengths and weaknesses, we hope that it can be used to analyse other mathematical research projects that might meet all, or some, of the criteria. We chose the analytical framework of big mathematics because it helps us to study and analyse the history of the CFSG in its various contexts. We are thinking of the role of Cold War Science/Mathematics, the importance of German émigré mathematicians in the CFSG and, in general, the impact of politics on research in pure mathematics. During the Cold War, we see this in new possibilities of funding of research in general [12, 23, 31] and of mathematical research in particular [2, 27].

(1) The CFSG as a geographically widespread enterprise

Based on the material that we have about the people involved, their affiliations and the locations of conferences and special programmes, our approach shows that the U.S. and some European countries were in centre stage of the CFSG for a substantial amount of the work. As explained above, in the context of the time period we focus on and given the possibilities for travelling, we consider this to be a wide geographic spread.

(2) Teamwork in the CFSG
In the first half of the 20th century scientific research would have been predominantly conceived of as being individualistic, and surely this held for (pure) mathematics. Carl Ludwig Siegel made scathing remarks on teamwork in mathematics after having left Princeton. However, quite naturally, by the mid-20th-century teamwork was an essential ingredient of big science, and teamwork also became a characteristic feature of the CFSG. This can be very prominently seen at the Group Theory Year in Chicago 1960/61, and the proof of the CFSG in its entirety is clearly a product of teamwork. This is exactly in line with contemporary developments in the sciences (e.g., [21, 36] and [25, Chapter 6]).

(3) The CFSG as a resource intensive project
We already explained what "resource intensive" means in our context and that we do not intend to compare this to the resources of big science projects in physics or the earth sciences. Compared to building and operating a large hadron collider, most science funding can hardly be assessed as resource intensive. However, when compared with the costs of average activities in mathematics from the 1950s to 1970s – say library access, office space, pencils, pens and paper, typewriters and ribbons, chalk and blackboards –, then the budgets of some of the major conferences and year-long events in the CFSG are rather amazing. Jon Alperin put it succinctly when interviewed by Joe Gallian in 1982: "Of course, the amount of money is peanuts compared to outside mathematics. […] We're talking about a few million dollars maybe" [14, p. 151]. An open problem in our project is how to quantify these costs based on reliable data, often difficult to come by, and to identify the funding sources. Detailed information and historical work on the scope of military patronage/funding of mathematics in general is scarce (however cf. [24, 26, 27]) and this also pertains to the CFSG.

(4) Large equipment in the CFSG
In terms of large equipment, we have already mentioned computing facilities in the 1960s and 1970s. However, as we are today surrounded by powerful, small and relatively inexpensive computing devices, we tend to forget the sheer size and expense of powerful computers in the 1960s and 1970s. When proving the four-colour theorem in the mid-1970s, Kenneth Appel and Wolfgang Haken used "1,200 hours of time on 3 different computers," and in particular, after they had been granted permission, they used the "IBM 370-168 that had just been installed at the University of Illinois Administrative Data Processing Unit" [22, p. 134]. The same model was used in CERN, and a photo taken in 1977 gives an idea of why large equipment is an adequate description (Figure 6).[8]

[8] Cf. the wonderful photos of an IBM 370-168 being delivered to Newcastle University: https://blogs.ncl.ac.uk/cs-history/2019/09/08/a-moment-in-time-the-370-arrives/.

Figure 6. CERN, IBM 370-168, installed in 1977. (© CERN)

(5) Central strategy and coordination of the CFSG
In our discussion of the people involved, we have mentioned the specific role of Daniel Gorenstein. We have already spent a substantial amount of time and effort to critically discuss his impact, by looking at interviews, asking contributors and searching for evidence in personal correspondence and publications. While the signs are often subtle, we have as of now only evidence confirming his role as coordinator and chief enthusiast, and we intend to clarify and explain the nuances of this specific aspect of the CFSG in much more detail in the future.

Conclusion
We have argued that, at least tentatively, big mathematics can be used as an analytical framework to grasp and describe some of the specific and singular aspects of the CFSG, when adequately downscaled from big science. To analyse the CFSG as a major achievement of 20th-century mathematics from a historical perspective is a rather ambitious project, and we are only at the beginning. We are therefore aware that this article raises many more questions than it can answer. But, as we have sketched out, such a project has a multitude of fascinating dimensions, shedding light on the evolvement of the classification project as such as well as the various relevant contexts framing mathematics in the second half of the 20th century. We hope that big mathematics might be a useful framework to investigate other extensive mathematical projects, such as the Bourbaki group or the proof of Fermat's Last Theorem. Our project abounds with open questions. To tackle them, additional source material is needed (such as grant proposals and responses, final reports, material on the planning of specific conferences, correspondence with the main players, etc.).

Therefore, we close with an invitation: We are very grateful for all hints towards such material.

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