Mathematical Research in Context

Michael J. Barany

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Abstract

University mathematicians spend their careers producing theorems and proofs, but little is known about how they do this. This thesis elaborates a programme of ethnographic study aimed at resolving the intractable puzzle of how these scholars do what they do. I describe several approaches designed to follow mathematicians in their work. This allows me to characterize the products and activities of their research in terms of the observable features of mathematical writing and communication found in research seminars and individual researchers' notes. By casting light on the oft-unseen difficulties besetting research mathematicians, these investigations enable a re-framing of their scholarship in terms of topicalizing, temporalizing, and materializing elements of their research programmes. As a science of ideals, mathematics rests on the capacity of its producers to legitimize and manipulate particular representations of its phenomena in order to elucidate rigorous knowledge. An ethnographic treatment of these activities recasts mathematics as a science of a form of representation accountable to its own complex and socially reified knowledge-work.

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Chapter 1

Introduction

In most people's experience, mathematics is a static body of knowledge consisting of terms, concepts, and techniques that are the same now as they were when they were developed hundreds or thousands of years ago. Even professional users of advanced mathematical concepts in science, statistics, and other domains rarely see the corpus of mathematical knowledge change. Occasional news reports of the proof of a famous mathematical conjecture hardly shake the view that such developments leave the ultimate status of mathematics unchanged—perhaps scholars had just missed a conclusion that was, in some sense, already there in the tangle of established truths. After all, what is a proof but a simple recombination of already-existing mathematical truths to show something else that must have been true all along?

Nothing in most people's experience would suggest that there are corners of mathematics that are changing all the time, where as-yet unthinkable entities interact in a primordial soup of practices that compose the process of mathematical research. This primordial soup is the object of my study, which applies the techniques of ethnography to examine the routine activities of a group of university mathematicians. By following researchers in their work, I attempt to explain the relationship between mathematical knowledge and mathematical practice in those spheres where the former is constantly transformed by the latter.

There is something a little nonsensical (even quixotic) in the ethnographic study of mathematics. As a subject to be investigated, it is simultaneously too many things and too few. The mathematics of logicians, schoolteachers, engineers, and theoretical physicists have, in practical terms, very little in common. They use different techniques, invoke different objects, and frame those techniques and objects within vastly different systems. Their mathematical writings can be mutually unrecognizable, and their standards for what counts as valid mathematical reasoning can differ in more ways than

simply by degree of 'rigour' (a term which itself has many meanings). The mathematicians I followed practice a form of the subject common to very few, and even they will disagree from time to time about the things they study.

Nevertheless, there remains something on which all of these varied practitioners, if asked, would agree. It almost goes without saying for most people, most of the time, that mathematics, whatever it is, is the same everywhere and for everyone. One does not ask *where* $2+2$ equals 4 or *when* the sum of the angles of a triangle is equal to two right angles. Just because the engineer does not *know* Gödel's Theorem or the logician cannot calculate the stresses on a bridge, few would argue that the theorem is any less true for the engineer or that the calculations are any less valid for the logician. Somehow, the mathematics people envision always transcends the mathematics that people do.

This paradox—that mathematics has too many manifestations to study and is ultimately reducible to none of them—appears time and again in the ethnography that follows. It is a paradox whose very existence is a testament to the power and importance of mathematics, a discipline whose social existence defies ready-made explanations. Because of what mathematics is, it is impossible to study as such. One must find a thread and weave with it through the tangled web of those things that constitute the art—its peoples, principles, and practices, as well as the network that joins them to make the gestalt figure of mathematics itself.

The thread of my ethnography is the Analysis Group of the University of Edinburgh's School of Mathematics. From among the innumerable peoples who lay claim to mathematics, I single out a group whose claim is particularly authoritative, if also particularly unknown. Few people ever encounter the theorems to which my subjects will devote entire academic careers, but those theorems will do something rare and remarkable: they will alter the body of what counts as mathematical knowledge. My study asks how they do this, and what that says about the thing we call mathematics.

My project entails, of necessity, a methodological experiment. Chapter 2 sets its context in prior studies of mathematics and science, and chapter 3 presents the variety of methods I designed in light of that context. My methods allow me to view mathematical research in terms of mathematicians' interactions with each other (chapter 4) and with their individual media of research (chapter 5). I conclude by reconsidering the paradoxical relationship between mathematics and its users.

More than other users of mathematics, researchers like those I study offer to shed particular light on the above paradox. When mathematics is seen to exist in a Platonic realm of pure ideas, it is easy to countenance

a view of it that ignores the crucial social enunciations of its practitioners. Settled knowledge of any sort has a tendency to seem inevitable, and the mythos of mathematical methods makes its knowledge appear more final than most. The second half of this study's animating paradox—the part that separates mathematics from its users—is sustainable only insofar as mathematics appears as a single settled body that its users merely access but do not shape. I challenge this conceit by going to the elusive places where mathematical ideas are least pure—the crucibles of mathematical research where official knowledge is forged.

Chapter 2

The Ethnography of Mathematics

Mathematics has a special place in the sociology of knowledge, dating perhaps to Karl Mannheim's (e.g. 1936, 43) repeated exemption of its basic facts from sociological analysis (see also Bloor 1973). David Bloor (1976, 73) made a sociology of mathematics the holy grail of his Strong Programme in the sociology of scientific knowledge, a centrepiece of what has come to be known as the 'Edinburgh School' of science studies. Strong Programme accounts of mathematics, inspired by the later philosophical writings of Ludwig Wittgenstein (in particular, his *Philosophical Investigations* and *Remarks on the Foundations of Mathematics*), focused on social explanations of elementary mathematical knowledge such as basic arithmetic and how to count and measure. These investigations have sustained lively debates and spawned competing schools of analysis,[1] but have largely failed to furnish grounds for empirical studies of mathematical work or to support satisfying explanations of more complex phenomena.

More than thirty years on, few sociological studies have moved far beyond these safe (if provocative) grounds.[2] A famous early attempt was Livingston's (1986) treatment of Gödel's Theorem in mathematical logic, now read primarily for its methodological introduction, which discusses simpler examples from elementary geometry. The most obvious reason for the relative paucity of advanced mathematics among the subjects of sociology is simply that mathematics is difficult, both to understand as a researcher and to explain to other sociologists. For most mathematicians, two decades of

[1] E.g. Lynch (1992a,b), Bloor (1992).

[2] Substantially more work has been produced from historical perspectives, some of which explicitly engage the sociological literature on mathematics. See especially MacKenzie (2001), Pickering (1995), Porter (1995), Warwick (2003).

continuous training from primary through doctoral education barely qualifies them to contribute in piecemeal ways to their field, much less to characterize it synthetically. Livingston's (1999, 879–884) remarks notwithstanding, it remains to be seen whether it is possible to use elementary examples or simplified *post hoc* views of mathematical work in such a way as to offer definitive insight into the highly complex work of mathematical research—work that mathematicians themselves (including the ones I interviewed for this study) find hard to describe. The prospect is unlikely, especially because the modes and expectations for teaching, justifying, and occasionally disputing results in mathematics tend to differ markedly from those found in everyday mathematical research and discussion.

Rather than analyse the record of accomplished mathematical results, my project investigates whether it is possible to derive meaningful sociological observations from mathematics in progress. This setting brings its own barriers to the research process. As Merz and Knorr Cetina (1997, 74) discuss, the everyday work of mathematics is not confined to specific laboratories or field sites. Rather, ideas can strike at any instant, and any nearby piece of paper or slate can become a space for the working-out of new mathematics—indeed, mathematics sometimes seems to be done entirely in the head. Mathematicians collaborate in many venues—seminars, receptions, conferences, offices, computer networks—and these sites of mathematical production are both difficult to circumscribe and indispensable to a prospective account of mathematical innovation.

My work joins a slowly growing literature that draws on the methods and theories of so-called "laboratory studies" of the natural sciences to investigate theoretical disciplines which lack laboratories of their own. Section 2.1 describes the foundational laboratory studies literature and its recent adaptations to mathematics. Sections 2.2 and 2.3 then elaborate on two specific issues from laboratory studies methodology—namely, the respective places of researcher expertise and material contexts in the production and analysis of ethnographic data. These considerations lay the groundwork for my study's specific design, described in chapter 3.

2.1 Laboratory Studies

For much of their history, studies of scientific work have relied on a combination of first-hand and idealized accounts of scientific practice. Such accounts came from current or past practitioners, many of whom joined in public speculations about their disciplines, and were supplemented by historical case studies. Well-worn parables mixed with anecdotes and general observations

to provide a picture of scientific practice which was, if not orderly, at least rationalizable and comprehensible. The early 1980s saw a first major wave of critical attention toward a different approach to the data for sociologies of science, known as laboratory studies.[3] This approach encouraged researchers to consider scientific work as an anthropological field, and to describe it from an outsider's perspective.[4] Untethered from the peculiar form of rational accountability built into practitioners' ways of narrating science, laboratory studies quickly became a potent and controversial resource for sociologists.

United by a common goal of describing "science *as it happens*" (Woolgar 1982: 483; Latour and Woolgar 1986: 28) and seeing scientific activities the "first time through" (Garfinkel, Lynch, and Livingston 1981: 135), these studies came from a great variety of often quite divergent theoretical standpoints whose mutual interactions generated a glowing hoard of methodological challenges and opportunities. Their shared emphasis on quotidian aspects of scientific work fostered ethnomethodological, actor-network, and a range of other explanations (including from the Edinburgh School noted above), each of which accounted differently for the relationship between practices and knowledge. My ethnography begins with the laboratory studies principle of pursuing knowledge-work at its sites of practice.

The laboratory studies revolution came late to mathematics. Nor is that surprising. Mathematics is devoid not just of laboratories but of many of the analytically fruitful representational dilemmas exploited in laboratory studies of other sciences. The world mathematicians must tame is, regardless of the potential realities to which it might refer, one of their own creation. They are not accountable to nature, to experimental replication, or to particular sets of apparatus designed to transform experimentally regulated nature into mathematical plots and data. Where laboratory sociologists found early success unpacking the hidden representational work of scientific instruments (e.g. Latour and Woolgar 1986, Lynch 1985, Collins 1985), sociologists of mathematics have struggled mightily to locate the material media on which to found a comparable investigation of mathematical signification.[5]

Greiffenhagen (2008) describes three significant approaches to this problem. The first is Livingston's (1986, 1999, 2006) "demonstrative sociology," which is based on the attentive reconstruction of mathematical arguments derived from specialized training in the discipline. By doing mathematics

[3]Of particular importance were special issues of *Philosophy of the Social Sciences* 11(2), 1981, and *Social Studies of Science* 12(4), 1982; see also Pinch and Bijker (1984, n.10), Lynch (1985b, n.3), and Collins (1985, 169–174).

[4]Anthropologists of science differed on the question of what it meant to "learn the language" of their subjects; see section 2.2, below.

[5]But see Rotman's (1988, 1997) philosophical and semiotic analyses.

(whether producing simple model proofs or following difficult theorems), Livingston aims to recover aspects of the work that are typically left out of expert accounts. The second is what Greiffenhagen calls the "e-mail ethnography" of Merz and Knorr Cetina's (1997) work on theoretical physicists.[6] Their ethnography, and others like it, combine close reading with expert training to unpack the limited electronic debris of theoretical collaboration or dispute, making use of the relatively recent advent of easy-to-store and easy-to-transmit electronic communications between researchers. Finally, Greiffenhagen offers a preliminary view of his own "video ethnography," which takes an ethnomethodological approach to video-recorded advanced mathematical instruction in classroom and supervisory settings.[7]

Each of these approaches represents a different way of identifying the "laboratories" and "instruments" of mathematical work (table 2.1). Demonstrative sociology exhibits methods of thinking and writing in the mind of an individual mathematician; e-mail ethnography traces the public and private representations of mathematics and logic produced by researchers for each other; video ethnography examines both official and informal accounts of mathematical logic by initiates for students. The approaches depend on singling out particular sites of practice and their corresponding modes of instrumental or quasi-instrumental mediation between mathematicians and the putative objects of mathematics.

Method	Laboratory	Instruments
Demonstrative Sociology	Mathematicians' Minds	Thinking, writing
E-mail Ethnography	Computer Networks	E-mails, computers
Video Ethnography	Classrooms, Offices	Gestures, narratives, descriptions, boards

Table 2.1: 'Laboratories' and 'instruments' in three sociologies of mathematics.

The methods of chapter 3 are derived from a comparable attempt to find laboratories and instruments in mathematical research. In order to isolate mathematical research as a laboratory-like phenomenon, I explore a variety of settings where researchers enunciate mathematics. It is improbable that one

[6]Greiffenhagen omits Rosental's (e.g. 2003, 2004, 2008) studies of logicians, including an analysis of a dispute whose main venue was a Usenet electronic discussion forum (but see Greiffenhagen's 2010 review of Rosental's 2008 book). Barany (2010) applies principles from e-mail ethnography to the online records of a recent blog collaboration.

[7]In addition to Rosental's sociologies of logic, Heintz's (2000) ethnography of mathematics, whose theoretical approach differs substantially from those of laboratory studies, might have supplied a fourth methodological case study.

will ever be able to rely on "catching mathematicians in the act" of formulating proofs and theorems, especially because the historicity of such breakthroughs only becomes analytically available after the fact (cf. Garfinkel, Lynch, and Livingston 1981). Rather, the salient features of such mathematically significant moments must be accessed through the many different spheres of communication that construct and refract them.

2.2 Mathematical Expertise and the Ethnographer

The particular methods of my study depended on my own research experience in mathematical analysis in order to design, carry out, and interpret my ethnographic investigations. This is in keeping with laboratory studies, where researchers must be participant observers in order to access and comprehend their phenomena (Woolgar 1982, 482). Engaging the technical details of scientists' work can be compared to "learning the language" in an anthropological field. As with other languages, the expertise required to interpret scientific displays differs from that which is required to produce them (Collins 2004, 103–104). Along the spectrum of foreignness and familiarity, then, one must strike a balance between 'participant comprehension' (Collins 1984) and 'going native' (cf. Gale and Pinnick 1997, 114, 121; Latour 1986b). Access to the actors' meaning of practices and phenomena always engenders the concomitant risk of foreclosing alternative ways of sense-making.

There is no royal road to the ethnography of science, although many approaches to its expertise conundrum have been suggested. Latour and Woolgar (1986, 18, 29–30) embrace the problem's tensions and propose to emphasize the ultimate fallibility of ethnographic accounts by adopting conceptually alienating or ironic prose.[8] Merz and Knorr Cetina (1997, 74, 125–126) privilege access and comprehension by resolving specifically to use "native" terms and categories to elucidate actors' logic and meaning. Shapin and Schaffer (1985, 6) use their expertise in an opposite way, to "play the stranger" by strategically suspending their taken-for-granted knowledge in order to examine specific sense-making practices in their historical subjects. Haraway (1997, ch. 1) cautions that all of these approaches are susceptible to naturalizing in-built relations of power and authority and insists that these must be foregrounded deliberately in one's research and analysis. The mere assumption of anthropological strangeness can capture neither the details of expert understanding nor the politics and frameworks within which it is

[8] Woolgar (1982, 491, 494). See also Lynch (1982, 506–509).

staged.[9]

Within ethnomethodology, the paramount concern is for the "unique adequacy" of practitioners' knowledge.[10] Here, "native" comprehension is necessary to determine the particular instrumental registers and functions of routine activities, which can point to the distinctive features of a knowledge practice. While it is not possible to see through the eyes of one's subjects, one can aspire to see what it would be like to do so. The result is an account that is alienating in an entirely different way from those of Latour and Woolgar, with the latter two's conceptual strangeness replaced by an always-already-inaccessible conceptual intimacy (see Lynch 1985, 11). These competing approaches re-frame the expertise conundrum as a battle between analytic competence and analytic distance.

But it is also a mistake to assume that gaps in competence and struggles for participant comprehension are the sole province of ethnographers. Practitioners themselves have the "communicative competence" to discuss many more mathematical phenomena than they would be competent to research themselves (see Keating 2001, 287–289).[11] Shortcomings in my understanding of my subjects could help me identify evidence of mutual incomprehensions amongst them. At the same time, there remain important differences between my struggles and those of my subjects. Whereas my difficulties represented barriers to a unilateral effort to textualize and contextualize their work, their difficulties ought to be seen not as barriers but as part of their work itself. Only a stereotype of mathematics that posits research as an entirely unproblematic exchange of pure ideas would countenance the view that the unavoidable lacunae in mathematicians' mutual communications were not fully part of their research efforts.[12]

Ultimately, my task was to compile a picture of mathematics based on two partial, sometimes-overlapping views: that of the mathematician and that of the ethnographer. Each view entails a set of narration practices

[9]Nor, stepping back from laboratory studies, can the answer be found between the facile methodological positivisms of systematic ethnographers like Spradley (1980) or the impressionistic gestalt methodologies such as those of Agar (1980), although in combination they provide a valuable corrective.

[10]Lynch (1985, 9–14, 293; 1999). See also Garfinkel (1967).

[11]Chapter 4 develops the implications of this gap in the context of the mathematical research seminar.

[12]cf. Bourdieu's (1981, 309–310) critique of Sartre, who argues that the café waiter's activity is built around the activity of playing at being a café waiter, while Bourdieu counters that this interpretation ignores the extent to which the waiter must inhabit his role. The stereotype has mathematicians playing at doing mathematics (and failing most of the time) where, in fact, such apparent play must be seen as a genuine part of mathematics.

and conditions specific to its circumstances. For instance, ethnographic observations are beset by 'observer effects' where subjects' behaviours change because of the ethnographer's presence, as well as interpretive constraints from the 'perspective effects' of the mores of ethnographic description and the ethnographer's outsider status.[13] These were especially evident in smaller settings such as Tea Room conversations and mathematicians' offices. Similar constraints were apparent in my interviewees' narratives. Their competing depictions in interviews of the scenes I witnessed in observations served to temper both my interpretations of their words and my reliance on my own observer-derived understandings.

Lynch and Edgerton (1988, 195) observe that ethnographic subjects derive their own understandings of an ethnographer's work, and shape their interactions with the ethnographer accordingly. Reviewing transcripts of my interviews, I noted many points where interviewees noticeably adapted their explanations to fit narratives suggested by the arc of my questions or their image of my broader project. In many cases, this worked in my favour, prompting subjects to describe aspects of their undertakings in rich contextual detail. Armed with the appropriate caveats that come with taking any subject at her or his word, it is not inevitable that interviewer-introduced distortions impinge on the fruitfulness of interviewers' efforts. Famous accounts, such as those of Hardy (1967) or Polya (1945), by prominent mathematicians of their practices can provide invaluable clues about the relationships between mathematicians' self-image, reported practice, and actual endeavours, even when their particular analyses do not pass sociological muster. Subject-supplied problematics arising out of specialist concerns—such as De Millo, Lipton, and Perlis's (1979) discussion of formal verification in computer programming—have figured centrally in sociological investigations such as MacKenzie's (2001) study of mathematical proofs and trust.

2.3 Materials and Materializations

Where the explicit objects of mathematics are 'ideas' it is tempting to ignore the circumstantial apparatus on which such objects are reified and circulated. While I will not develop the potential connections to actor-network accounts of agency,[14] it is not hard to see that both the mundane objects of mathematics, from chalk to coffee cups, as well as its formal and informal conceptual ideals, like convergence and 'well-behavedness,' dramatically shape and con-

[13] On the hazards of observer-supplied problematics, see Bourdieu 2003, 288.

[14] See Callon and Latour (1981), Callon, Law, and Rip (1986), Callon (1986), Latour (1992).

strain mathematical research activities. My ethnography therefore attempts what Rosental (2004, 163) terms "a material approach to abstraction."

Chief among the material-semiotic elements I study are the texts and records produced through both mathematics-work and sociology-work in the context of the former.[15] I face the canonical challenge of reducing to textual descriptions the person-text interactions that give mathematical writing its force and effect.[16] The differences between the appearance of my notes (e.g. figure 2.1, explained in section 3.1.2) and those of a seminar speaker (e.g. figure 2.2) are striking. Specifically, the texts I study are mathematically instrumental in a way that relies on the indexical registers of board and paper-work in their particular contexts of use. Mathematical writing attains its instrumental meaning only in the context of the thoughts and actions that animate it.

The story I will tell is one of a mathematics manifested in the spaces between thought and action, concept and concretion. The stereotype of mathematics is as an internal, mental science of ideals. This work's emphasis on external and material dimensions of these putatively internal and ideal factors helps to explain the relationship between mathematical concepts and their tangible sedimentations in contexts of research. It is then possible to consider mathematical knowledge as a coproduction of individual and social agencies, acting with both mind and matter in a way that challenges their facile distinction.

[15]See Lynch 1985, 285 on the methodological problematics of texts and records "as a common ground of analysis between a scientific practice and a social science analysis." It is not inconsequential, for instance, that both my subjects' and my own experiences of the research seminars I observed depended integrally on the pen-and-paper notes we took while it was in progress.

[16]Tilley (2001, 260, 268) identifies the corresponding general problem in ethnography.

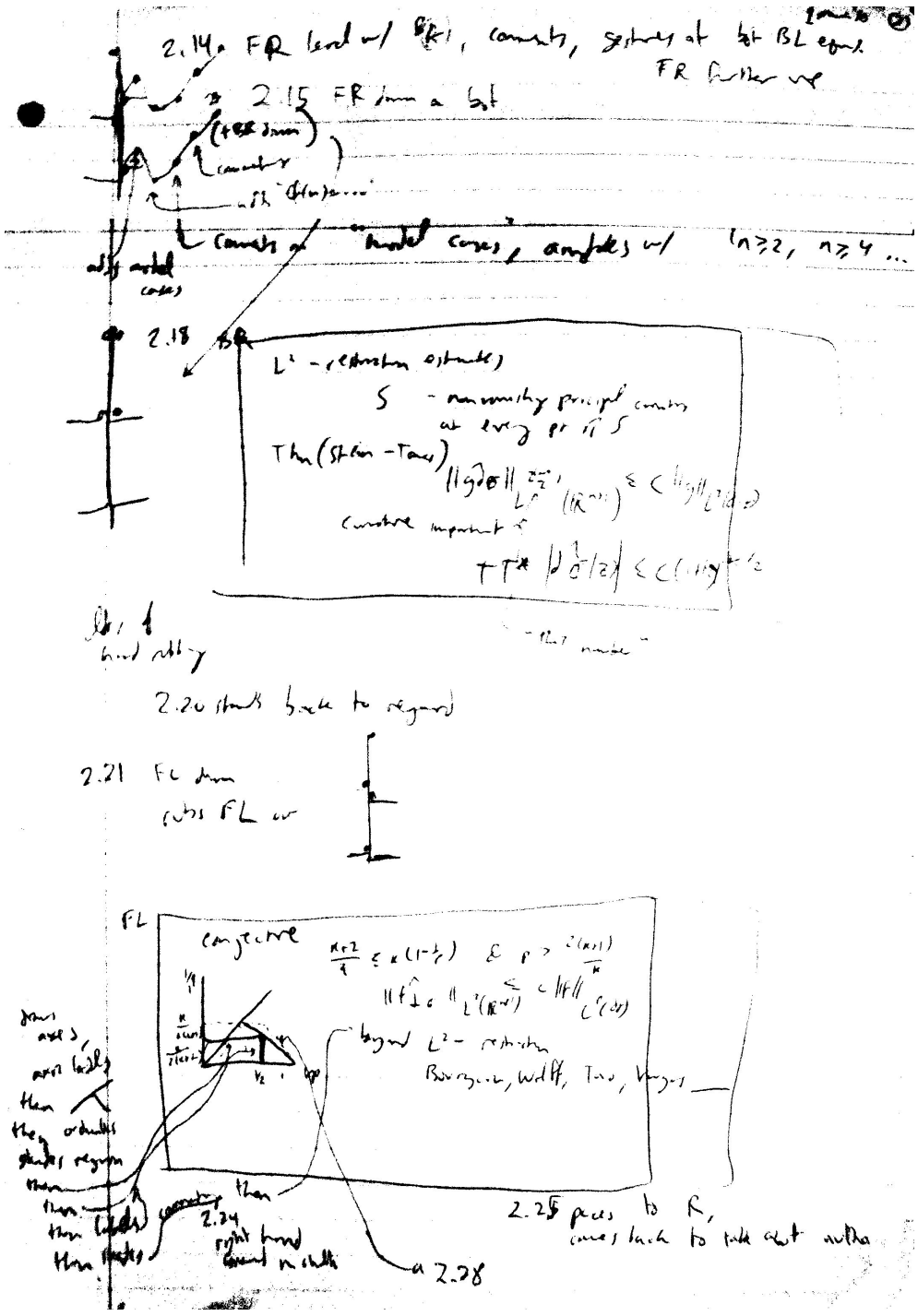


Figure 2.1: An excerpt from my lecture notes.

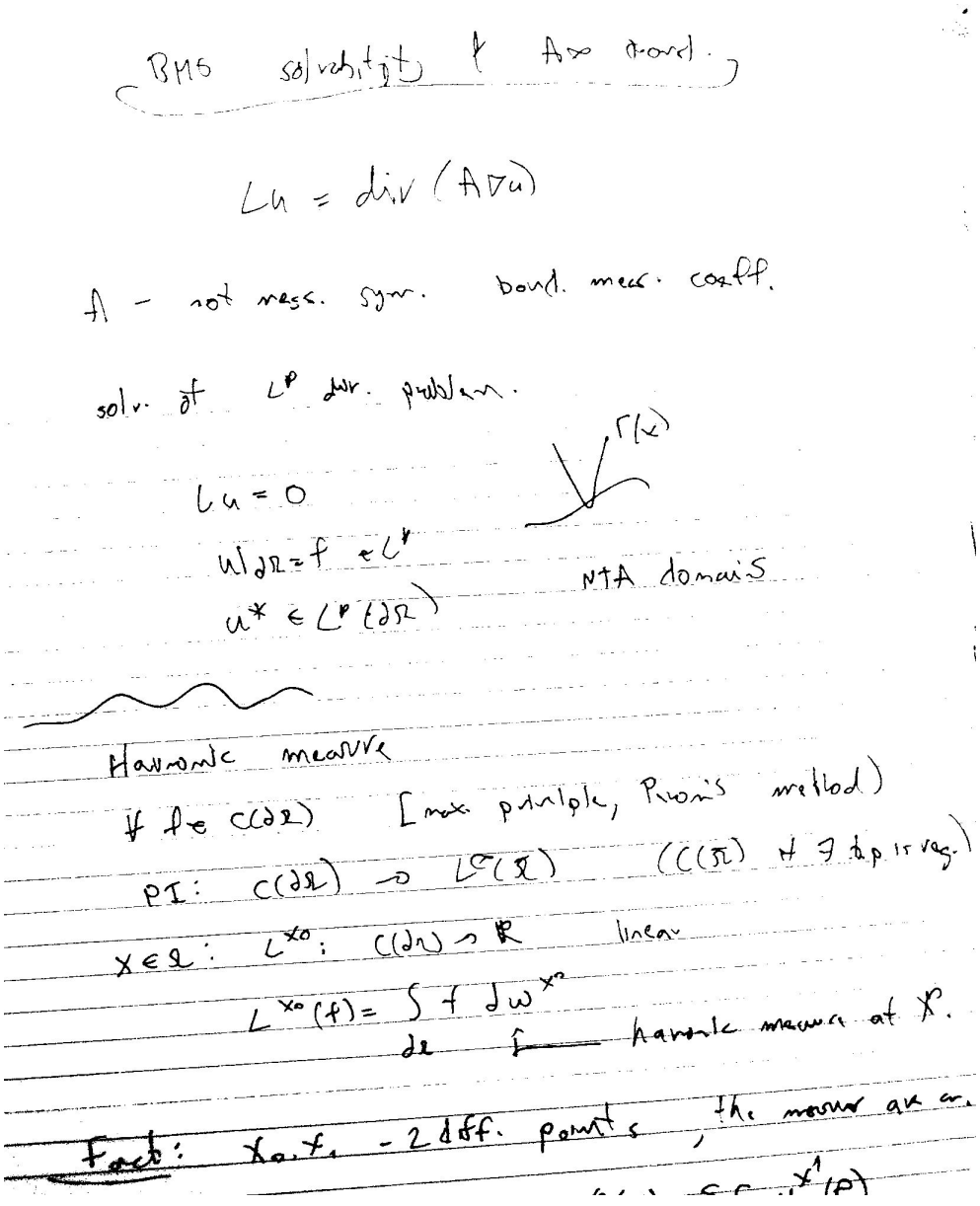


Figure 2.2: An excerpt from a speaker's lecture notes.

Chapter 3

Approaches and Adaptations

The ethnography reported in this study is the outcome of the systematic introduction and adaptation of a series of approaches during my involvement with the University of Edinburgh's School of Mathematics. My early contact with a member of the school's Analysis Group through my undergraduate research advisor gave me a means of inserting myself into a mathematics department that proved alternately amenable or resistant to my efforts to characterize them, in ways that had little to do with the action and inclinations of individual scholars. The greatest challenges to my work came not from the school's people, who were encouragingly eager to help (or at least not off-put by my project), but from its institutions and modes of practice—the very things my methods aimed most to tame. Section 3.1 describes my two primary observation sites, the departmental Tea Room (3.1.1) and the Analysis Group's weekly research seminar (3.1.2), which demanded two contrasting approaches to ethnographic investigation and documentation. Section 3.2 then details my two approaches to interviews with members of the Analysis Group. I conclude in section 3.3 with a summary of some strengths and weaknesses of my menagerie of methodological tools.

3.1 Observations

My program of observation grew out of preparations for the set of interviews described in section 3.2.2. Hoping to augment my expertise in my principle subject's area of study, as well as to signal to him the sincerity of my efforts, I followed through on his invitation to attend a lecture in the Analysis Seminar on a topic related to his recent work. Having researched the lecturer's topic briefly beforehand, I arrived early for the seminar and found myself waiting in what I later learned was the Staff Common Room, informally known as

the Tea Room—then populated by a mixture of faculty, administrative staff, and postgraduate students. I continued to attend seminars and to spend time beforehand in the Tea Room in order to gain contacts, credibility, and scholarly context, but soon found that with an appropriate ethnographic posture these spaces could be worthy sites of ethnography in their own right.

3.1.1 The Tea Room

Beginning with the lunch hour before my first visit to the Analysis Seminar, I spent the time between my arrival at the James Clerk Maxwell Building and the start of the seminar primarily in the Tea Room, and included summaries and discussions of these visits in observation diaries written after each visit to the department. On interview days when there was no seminar, I would also spend most of the time between interviews in the Tea Room and was thus able to observe its use at different points in the day, as well as during different phases of the academic calendar. Arriving one day to find the room empty, I photographed the room's configuration and several of its artifacts for later reference.

I did not make audio recordings in the Tea Room except when conducting an interview there, as the processes of obtaining adequate informed consent and setting up recording equipment would have overly disrupted the proceedings.[1] I included personal names in my notes when I knew them, but have not identified them in this report. No special effort was made to gather identifying information (except for my interviewees) or to reconcile the different spellings or identifications between different sections of my notes and transcripts. Moreover, while I did not announce my sociological intentions to all who entered, I introduced myself and my project to any who asked. In these cases, I legitimated my presence by mentioning either the faculty member who was my primary contact, my attendance at the Analysis Seminar, or both. By the end of my observations, most of the regular visitors to the Tea Room could recognize me and knew something of my reasons for being there, and some even felt confident to introduce me informally to visitors with whom they were acquainted.

My Tea Room observations extended in a limited capacity to other parts of the building, including the hallways outside the room and the busy common space two floors below, to broaden the context for my analysis. I expanded upon my observations with explicit discussions of offices and other

[1] This was among the only areas where my routine ethical self-assessments, according to Edinburgh's School of Social and Political Science guidelines, turned up potential difficulties—most of my work was in public or quasi-public settings with means of providing sufficient protections or assurances to subjects to be able to proceed ethically.

departmental spaces in some of my interviews, aiming at users' experiences of these locations. For all except individual offices, I relied on my status as a University student and my familiarity with mathematics departments to enter sites unannounced and to blend in.[2]

At first, I separated myself physically from others in the room so as to minimize my intrusion and foster analytic distance in my own experience. This allowed me to make notes discreetly on my laptop computer or notepad at an early stage of the research which required more documentation. As my study progressed and I became a more familiar presence, this practice was neither desirable nor sustainable. Instead, I integrated myself into a regular group of postgraduate lunch-goers, taking a quiet approach but not abstaining completely from the conversations. I attempted to note salient features of their use of the Tea Room as well as that of others nearby while limiting the distorting effect of my presence in the group. Accordingly, my diaries reflect my own actions and contributions as much as those of my subjects and interlocutors.

3.1.2 The Analysis Seminar

The Analysis Seminar offered a venue for glimpsing mathematicians' representations of their recent and ongoing work to each other. The participants in the seminar gather most weeks at the same time in a classroom not far from the Tea Room, where one or more local or invited speakers present their results in the area of mathematical analysis, with a focus on partial differential equations. As a group, the participants' combined intellectual and physical proximity during the hour of the seminar make their proceedings a potent model for the social elaboration of advanced and advancing mathematical knowledge. The structure and relative formality of the seminar environment, as well as the expectation of passive participation on the part of most seminar-goers,[3] gave me the chance to develop a range of strategies for documenting the proceedings. These included several forms of handwritten notation, as well as one set of photographs and two video recordings.

[2]On roles and strangers in group contexts and their sociological implications, see Goffman (1961, 7–14, 143–152). Many factors limited the strangeness of my presence, including my gender, appearance, and demeanour.

[3]Within the seemingly oxymoronic 'passive participation' I count a range of activities that, while not constituting direct interventions, nonetheless affect the seminar lecture in significant ways. Among these are the audience's interjections, presence, expertise, and non-verbal cues (including sleeping or ignoring the speaker). Goodwin (1984, 242–243) catalogues listener functions in story narration. Greiffenhagen (2008, n.14) gives a case for omitting audience data in accounts of mathematical lectures—one that does not seem to hold in my case.

I began with a crude two-column notation, taking conventional mathematical seminar notes in one column and making sociological observations in another, aligning co-temporal notes horizontally and connecting related observations with arrows. At different lectures, I noted the arrival of attendees, their relative placement around the room, their posture with respect to the speaker, their questions and note-taking habits, the speaker's mannerisms and gestures, his notation and wordings, and his use of the four large blackboards at the front of the room, all of which reflected aspects of the seminar's social and conceptual organization. Attempts to account for these different features of the lecture resulted in a refined two-column system represented in figure 3.1, with time-points for key events and a division of the notes into horizontal cells corresponding to the speaker's writing on a particular blackboard.

I singled out the four boards, in particular, for their role in shaping the speaker's performative possibilities and the audience's experience of the lecture. The boards stand at the front of the room in two ranks of two, separated by a vertical dividing post (figure 3.2). On either side, the two boards are mounted on tracks, one in front of the other, and can easily be moved up and down by speakers. This allows them to make marks on all four boards and to make different boards visible at different points in the lecture. But the system also constrains which markings can be visible at what times, due both to space limitations on the boards and the fact that one of the boards on each side can slide only behind its counterpart.

Drawing on an ethnomethodological interest in the specific ways different speakers use chalkboards, I incorporated increasingly precise marks in my seminar notes of where boards were placed and how they were being used (cf. Greiffenhagen and Sharrock 2005, Greiffenhagen 2008). The result was two types of diagrams designed to track chalkboard use on two different timescales in the seminar, excerpted in figure 3.3. The first diagram was produced to analyse the relative movement of the different boards during the lecture, with a focus on how and when speakers change chalkboards and whether that is affected by their position on the left or right of the room or in the front or back of their respective columns. Front boards are represented with dots and rear boards are represented with dashes. Configurations are placed chronologically, left to right. In the example here, it is dramatically clear that rear boards were substantially constrained in their ability to move and display the lecturer's writing. The speaker started at boards on the left side of the room; at the solid vertical line, he switched to boards on the right; at the dotted line, he returned to the left boards, and returned again to the right boards at the next solid line.

The second diagram tracks the sequential addition and spatial organiza-

	1:56 asked speaker for permission to photograph lecture 1:58 convenor enters 2:03 [name] moves overhead out of the way so that he can see better; [name] banters about this. Many people have tea at their desks, about half have note paper at the ready in front of them. 2:05 convenor gives up waiting for [name], closes the door, begins to introduce the speaker 2:05.30 [name] walks in during introduction
Restriction estimates for surfaces with vanishing [ins.: principal] curvatures A Fourier Restriction problem S. smooth [2:09 ins.: 'cpt' = compact] surface $\subset \mathbb{R}^{n+1}$, $n \geq 1$ dP : surface measure $\hat{f}(\xi) = \int e^{-2\pi i x_i \xi} f(x) dx$	after writing title, moves front left board level with back left board at '$\subset$': "just for convenience" 2:09 pushes front left chalkboard up then pulls back left down
Problem Determine (P, s) s.t. $\left(\int_S \lvert\hat{f}[\ldots]\rvert\right)^{1/s} \leq C\lVert f\rVert_{L^{p(n+1)}}$ [2:11 ins. '.'] Extension operator: $Rf = \hat{f}\lvert_S$ [hand rubbing] $R^* g(z) = \widehat{gd}\sigma(-z) =$ $\int e^{2\pi i z} f(\xi) d[?]$ $z = (x, t)$	2:09: Question from [name], speaker pulls down front left board, adds 'cpt', replaces it in previous position 2:12 goes to front right.
$S = \{(x, \phi(x)) :$ [hand rubbing] $\widehat{g}\lvert\sigma(-z) \cong$ [$\sim$ added after end of expression] $\int e^{2\pi i([\ldots])} d(s) dz \quad \phi(n) = \lvert n\rvert$	2:13 paces back to point at $\widehat{gd}$ integral

Figure 3.1: An anonymized excerpt from my two-column notes.

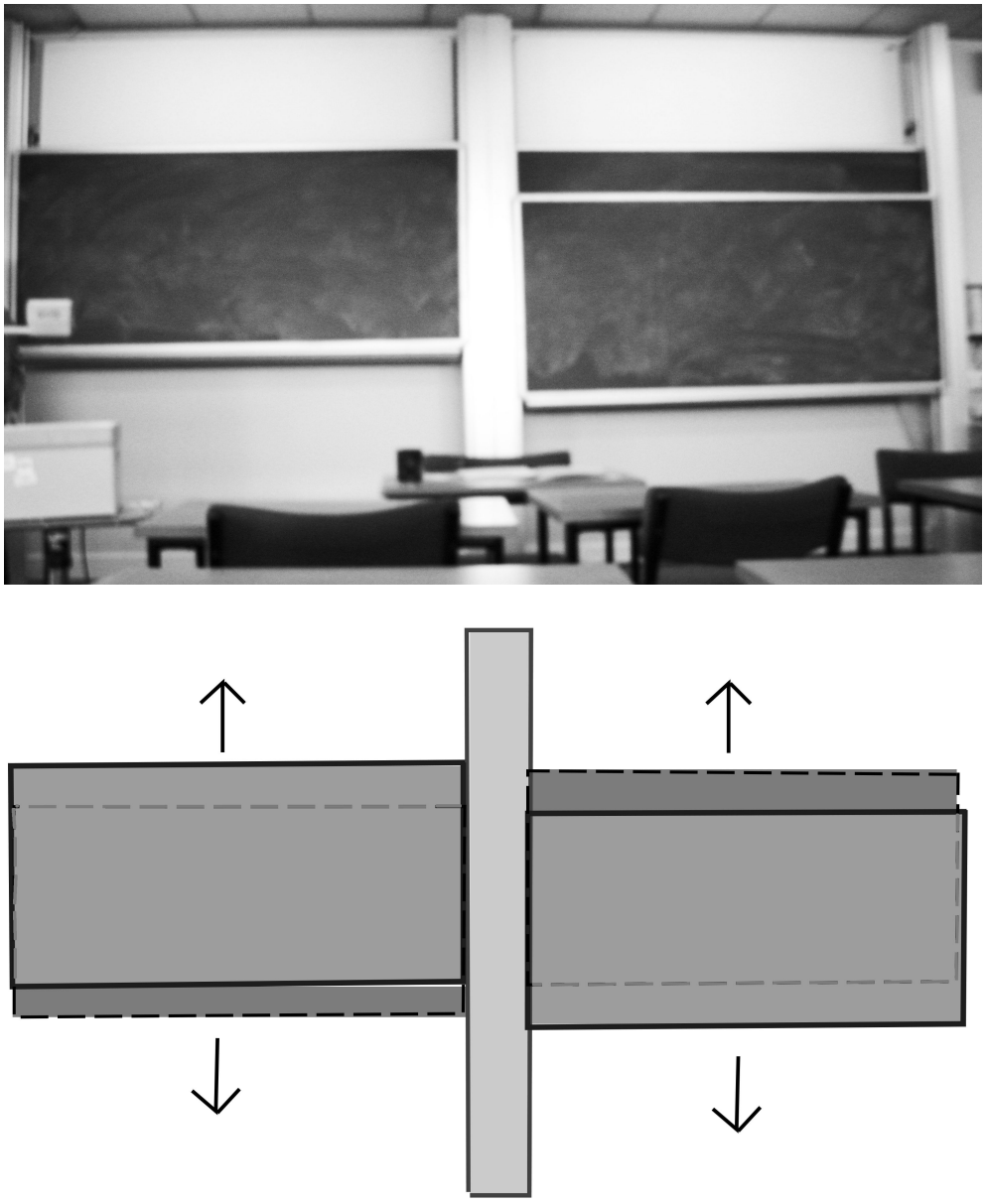


Figure 3.2: The four chalkboards at the front of the seminar room.

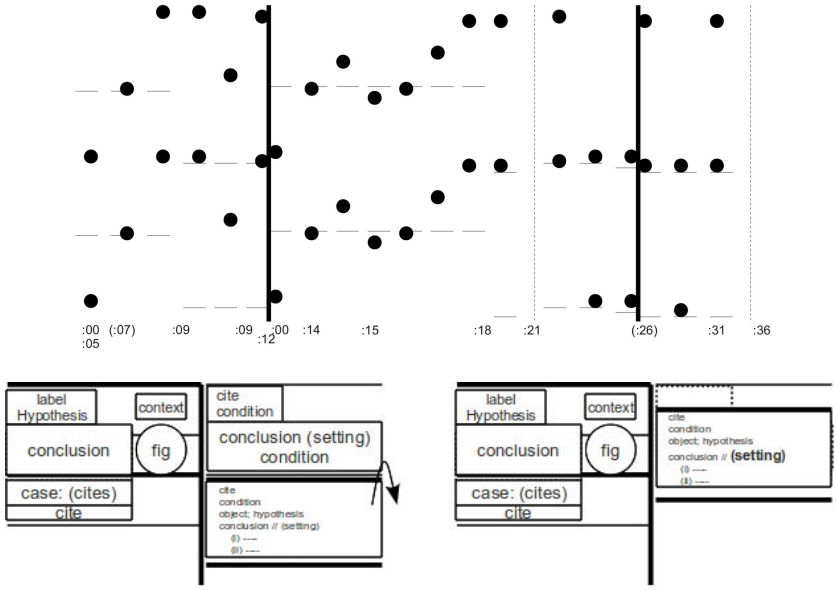


Figure 3.3: Two varieties of chalkboard diagram.

tion of specific kinds of writing over a smaller time-scale in the lecture. Successive images show glossed "snapshots" of the chalkboards as they would be visible from the audience. The example here depicts the relative position of boards at two points in the lecture (the arrow indicates that the board was moved up and then down before being moved to the position in the next snapshot); in the second snapshot, the speaker elaborates on the mathematical setting for an argument only after moving its board into a more prominent position, one which blocks a range of still-relevant information on the board behind it. The boards' relative contents and positions are thus shown to depend on each other in ways that demonstrably affect one's experience of the lecture.

In order to represent more detailed information in these diagrams, I employed yet another form of notation in which material from chalkboards was copied into rectangles on the page with commentary and sociological observations arranged around the outside of these rectangles (see figure 2.1). These observations focused on the sequential ordering of blackboard work in relation to other activities by the speaker, with the specific goal of treating ordering phenomena in seminar presentations.[4] Figure 3.4 compares the formats of conventional, two-column, and this third approach to notation.

I encountered several difficulties peculiar to the seminar setting. Speakers

[4]On the importance of diachronic orders and sequences as an analytic resource, as opposed to synchronic structural depictions, see Abbott 1995.

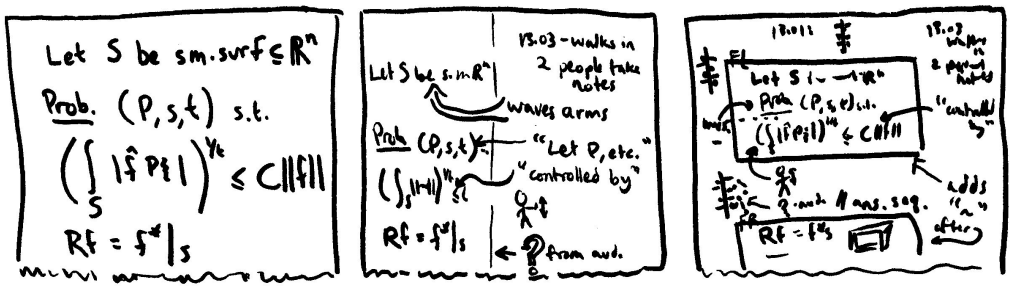


Figure 3.4: Conventional, two-column, and blackboard-oriented notation.

often arrived just before they were to begin speaking and were immediately occupied with follow-up questions after they finished, preventing me from asking whether they would be comfortable being photographed.[5] In order to reduce the disruptive effects of various recording technologies on the audience, I waited until I had become a "regular" at the seminar to introduce photography and, ultimately, video-recording. In these cases, recording equipment drew obvious notice from participants, but seemed to have a negligible effect on the duly forewarned speakers. During lectures, speakers moved across a large part of the front of the room, and often spoke into the chalkboard as they wrote, making audio recordings inconsistently reliable. In the absence of video, further notes had to be specially made to correlate speech to writing for analysis; with video, the equipment's demands prevented me from making thorough in-the-moment notes of the seminar.

Throughout, I faced the participant observer's paradox—that the more one participates in an event the less one is able to document that participation. In particular, attempting to contextualize the lectures' mathematical details made it impossible to follow those details as they unfolded, forcing me to reconstruct that experience from my notes. Struggles in documentation, however, also suggested otherwise unseen aspects of participation. They amplified difficulties from speakers' diction and handwriting, and indicated how the seminar's function differed from other classroom settings where copious note-taking is more the norm.

The frequency of my observations allowed me to explore many different means of documentation, each of which presented a different kind of image of the seminar and the settings surrounding it. Rather than forming a disconnected series of views, however, each approach persisted in shaping the context and possibilities for the others. Old techniques, such as two-column

[5]None expressed reservations about any form of recording when asked. Video recordings had to be arranged in advance with the speaker and convenor so that equipment could be reserved.

notes, suggested features that might deserve additional documentation, such as the spatial constraints of blackboard writing. Repetition also allowed me to identify the relative peculiarity or generality of phenomena sedimented by particular notations of particular performances, allowing constantly changing documentation techniques to record a substantially stable image of seminar work. This image was further enriched by participant images of the same seminars taken from interviews, themselves shaped in turn by considerations from my observations.

3.2 Interviews

Mathematicians are a remarkably self-reflective group. It is not hard to engage them in discussions about the nature of their discipline—a discipline that lends itself to many kinds of philosophy. The goal of my interviews was to get past the conversations mathematicians are eager to have in order to explore the much less talked-about matters of the day-to-day practice of mathematical research, but without discounting the role of those other conversations to their disciplinary identity and work. The quotidian details of mathematical work typically fall behind the veil of practitioners' modesty, and often escape their notice altogether. Asked after the fact to comment on notes they had made while working on a research problem, many expressed genuine bafflement at their own markings and fumbled for explanations. There was a clear gap between their eagerness to talk (in decreasing order of enthusiasm) about mathematics, about themselves, and about their mathematics.

Having met my principal informant through a mutual contact, I enlisted the others in a single afternoon after introducing myself following a seminar meeting in which two of them had given brief presentations. All of my interviewees were relatively young faculty or postdoctoral fellows in the Analysis Group; their age-bias came despite attempting to enlist a broader selection of faculty at other occasions. I am satisfied, however, that my subjects represent a sufficient diversity of career stages to allow suitably robust conclusions to be drawn from their experiences while being sufficiently similar to each other to permit meaningful comparisons.

Interviews were recorded either on my laptop computer or with a portable voice recorder. I brought along a digital camera to preserve sheets of paper or chalkboard work from the interview, and kept some sheets of mathematically annotated scrap paper when they were offered to me by the interviewee. During the interviews, I made rough notes on a pad of paper that also contained a pre-written agenda of topics or questions for discussion, and made

particular note of any phenomena which would be difficult to discern in the audio recording. Interviews were of two types. With all of my interviewees I conducted a pair of interviews, which I have called "document studies," built around their research practices and notes. The second type of interview, which I have called "research studies," was conducted on a regular basis with my primary informant and supplemented with further interviews or sections thereof with two of his junior collaborators from the department.

3.2.1 Document Studies

The first interview of my two-part document studies began with biographical questions about how each subject became a research mathematician. While this helped contextualize their future responses and provided different examples of the early career arc for University mathematicians, the main purpose of this section was to establish rapport with the interviewees and to have them describe their work in a way less connected to the routine descriptions they may be more accustomed to giving to students and colleagues. I also used these first interviews to inquire about work habits, collaborations, and major research projects, developing pertinent tangents as they emerged. To close, I asked respondents to save an example of their research notes from the upcoming week, stressing that the notes need not be an example of a "successful" or "complete" result. This last request led to a range of exhibited documents that challenged many of my assumptions about the place of such research notes in mathematical research.

The second interview, inspired by Lynch's (1985) pursuit of 'artifact accounts' to understand neuroscientific research, aimed to guide the respondent through a reconstruction of the meanings of the notations and writing approaches from his document in order to reveal how different inscription practices relate both to mathematical thinking within the project of the moment and to other features of his work.[6] While it would not be possible to capture knowledge work in its immediate self-presence, I could hope at least to understand the traces of that work as they appeared on paper after the fact.

Throughout the discussion, I had to balance the competing pressures of

[6]Discussing and producing documents within interviews are widely used techniques of social science, particularly in cases where the concepts being investigated are, like mathematical writing, not typically or primarily experienced or accessed through oral language. On inscription, see Latour 1986, 1987; Latour and Woolgar 1986; Callon, Law, and Rip 1986. Section 5.2 of this thesis describes some differences between my findings and this canonical account.

over-simplification and the over-reliance on technical scripts.[7] To ward off over-simplification, I periodically interjected with comments or questions on technical details of subjects' research to reinforce the expectation that these matters were relevant to the interview. The matter of technical scripts was more difficult—they make it difficult to access certain routine features of technical work, but are also part of practitioners' own arsenal for understanding and doing that everyday work. In order to explore the practical meanings of these scripts for the individual researchers, I allowed and even re-used interviewees' more formulaic assertions while insisting that they redefine or describe central ones after their use.

The unexpected challenges of this format were evident in my starkly differing experiences across the interviews. One subject's notes were meticulous and well-organized with several clear markers of his thought process and the temporal organization of his work. All the same, he struggled to reconstruct the in-the-moment indexical function of many of his marks, and backtracked and reconsidered many of his assertions about the document over the course of the interview. My questions could help him reconstruct, with no guarantee of reliability, individual parts of his research experience. But the "act" of mathematics reflected in his document was irrecoverable.

Another subject's notes were haphazard and scattered over the front and back of a reused piece of scrap paper. Perhaps, I hoped upon seeing it, its in-the-moment-ness would be better preserved in its absence of deliberation and its profusion of stray marks, scratches, and reconsiderations. Not so: the fits and starts of our effort to account for the paper in the interview revealed the notes' mathematical presence to be beyond lost. Their mathematical function was served purely in the moment—the paper was the trace of a text that was not co-presently a record.

Anticipating my interview later in the day, my third subject had begun that very morning to work out an idea on a fresh sheet of paper. His work was interrupted, however, and in the end he presented me, somewhat apologetically, with just the mathematical fragment (figure 5.3). His experience reminded me of the pervasive oriented-to-ness of mathematical notes (in this case, oriented to our scheduled meeting). The fact was, it was too simplistic of me to expect the notes and the research to ever manifest an unproblematic co-presence. Looking back, the deliberate record-making of the first subject and the ever-fleeting mathematical play of the second could be seen as orientations toward particular purposes in their own right, orientations which

[7] Technical scripts are standardized expressions that are routinely associated with particular phenomena of research, serving as both an explanatory resource and a mnemotechnic device. See section 4.1.1 of this dissertation.

needed to be accounted for in my analysis.

The final subject did the most to show the limitations of my design. He did not keep notes in his office, but volunteered to bring some from home for our interview. He forgot them the first week. And the second week. In the end, the best he could supply were his notes for a seminar presentation from more than a month prior (figure 2.2). Notes, I learned, were not such a big part of this researcher's work. He certainly wrote; indeed, quite frequently. He could readily speak in the abstract about his use of diagrams and other textualizations. But the particular artifacts I had in mind were not present in his mathematical endeavours in the way I had expected.

3.2.2 Research Studies

The final source of data for my project consisted of a series of interviews, primarily with my principal subject, to document further aspects of his research programme and to explore his work in undertakings at different stages of completion. I learned early in my meetings with him that he would be undertaking a new collaboration with a postdoctoral fellow in the department, whom I was also able to interview. Because of the limited term of that collaborator's appointment at Edinburgh, he expected most of the work to be carried out contemporaneously with my study. Their project gave me a glimpse of the difficult-to-document early stages of research. Their efforts faced several disruptions, and their problem remained unsolved in the period of my interaction with them, but I was able to learn about other phases of research through several other projects in which my principal subject and his collaborators were engaged during that time.

In each interview for my research studies I asked for an update on the progress of my subjects' research, which was often limited (especially when there were conflicting demands during the term), and designed follow-up questions to explore both particulars of the research and their relation to more general phenomena in the subjects' or other mathematicians' research experiences. It was important for my subjects to speak open-endedly about their work, as part of my focus was on how they experienced their own process of research, and I inquired about specific projects or touch-points only after they had completed their undirected accounts. I also prepared a set of further questions, differing from week to week, on topics ranging from from reading and writing habits in different phases of research to relations with graduate students, the department, and distant collaborators. Before one collaborator's upcoming seminar presentation, I devoted an interview primarily to his preparations and expectations for his talk.

The extended course of this set of interviews tested my ability to establish

an effective rapport with a subject who seemed initially quite ambivalent about participating. I cultivated his participation both within and outside of our interviews, and drew credibility from regular attendance at the Analysis Seminar. My experience here fit well with a model of the ethnographic interview as a social formation in itself, one necessitating suitably reflexive analyses (Briggs 1983, 363 et passim, Heyl 2001).

3.3 Prospects and Limitations

The research methods just described are deliberately document-intensive. As an attempt to adapt a methodological insight from instrument-focused laboratory studies developed in section 2.1, my approach relied on the exploitation of a narrow and often-elusive feature of mathematical work in order to explain broader processes of research. I identified marks on chalkboards and scraps of paper as important points of passage for mathematical communication, and hoped that they would offer insights into the knowledge-work that produces them.

In the putatively mental discipline of mathematics, it is both risky and necessary to base one's ethnography on the art's fleeting inscriptions: risky, because there is no guarantee that what is important in mathematics is ever quite written down; necessary, because there are few other aspects of work in the discipline that can be accessed without relying on just ethnographers' experiences or subjects' testimony. My efforts to capture the various inscriptions of research mathematics took many forms, most notably in the variety of attempts to capture the crucial (and crucially transient) marks of chalkboard mathematics from the Analysis Seminar. The adaptations between notations relied on an evolving reflexive engagement with the relationship between what I was studying and how I was studying it. They also required a constant interrogation of that relationship using expertise and experience gained both within and prior to my study.

If anything, these indispensable engagements drew attention to the impossibility of producing a fully satisfying record of the processes I witnessed. The plurality of partial accounts I produced was itself an aspect of my design. Adaptations—of which the above account is merely indicative, not exhaustive—were not aimed not at an unachievable ideal record that is somehow self-identical with the proceedings it documents. Rather they were targeted at constructing different ways of recording that might add to my understanding of the proceedings' possible contours.

From these partial perspectives, I draw generalizations that rely on the variety of views considered and the expertise I brought to their interpreta-

tion. My conclusions in the ensuing chapters touch on a great many themes that implicate the documents and documentable features of mathematics in important ways. But I do not pretend that these give definitive access to every important phenomenon in mathematical research. I could, for instance, barely scratch the surface of the complex issues of disciplinary identity suggested in several interviews; nor could I address at this stage of my work the question of how knowledge is certified and taught by practitioners—a matter of immense importance in the history and sociology of science and a particular focus of laboratory studies research.

The effect of my focus on writing, as the following chapters show, is to reveal a mathematics that defies many of the assumptions supporting its most visible extant sociological and philosophical explanations. In particular, this focus allows me to challenge the truism that mathematicians justify new theorems to themselves in the same way that they defend them in official texts. The writings I analyse furnish a potent image of the accountable features of day-to-day mathematical work.

Chapter 4

Time and Topic in Mathematical Research

On Mondays during term, members of the Analysis Group return from lunch and assemble at two o'clock in the afternoon in the seminar room at the end of a corridor along the north side of the James Clerk Maxwell Building. There, for one hour, mathematics researchers ranging from newly-minted postgraduates to seasoned professors emeriti share in the telling of one colleague's forays on the frontiers of mathematical knowledge. The weekly Analysis Seminar provides these researchers with a formal setting in which to join in doing mathematics.

The speaker may be one of the seminar's regular participants or a visiting researcher. Those from outside of the Department might be on the way to a nearby conference or a visit to another institution; some are past or present collaborators with Analysis Group members. In its most recent term, the group hosted presenters from three continents, each with a distinctive collection of personal, intellectual, and professional ties to the individuals who, week after week, populate the rigid blue-upholstered chairs of the seminar room. These presenters delivered talks composed specifically for the occasion or adapted from prior conference or seminar presentations, treating new results, past triumphs, and ongoing conundrums alike.

The mathematics of these lectures is represents the outcomes of extensive research and study. It is phrased in routinized terms designed to tap into the vast specialized vocabulary and experience of those in attendance. In one sense, there is very little that is arbitrary about what is said for that hallowed hour. One gets the impression, however, that the specific mathematics of the presentation is of at best marginal interest to most of the gathered audience. To be sure, some jot detailed notes from the speaker's board-work, others nod in agreement, furrow their brows, and interject with quibbles or queries that

seem to betray a keen regard for the lofty concepts at play. But it is striking how few of those present make more than the most cursory notes of the proceedings. Neither the technical sorcery arrayed on the chalkboards, nor the dense allusive web of citations, heuristics, and technical symbols seems to merit any more contemplation than what can be had in the lecture's own fleeting moments. Many weeks, one is just as likely to see someone nodding off to sleep as nodding knowingly at the speaker's clever remark. Most audience members regard the speaker with a reserved attentiveness that is easily mistaken for comprehension.

Lurking in the subtext of the numerous interviews and observations conducted for this project is the embarrassing open secret that mathematicians tend to have comparatively little idea of what each other does.[1] They have seen each others' talks, read each others' papers, and even contributed directly to each others' work. But all of these ties belie the vastly greater idiosyncrasies that pervade each individual's understanding of his or her subject of research. Mathematics is a discipline marked by a staggering degree of fragmentation. Even within the Analysis Group, a band of researchers joined by a particular interest in the methods and mathematics of partial differential equations, researchers confess at best to a broad familiarity with the work of many of their colleagues. Among collaborators, a shared interest in a specific problem can alternately both mask and amplify vast differences in approach, training, and thinking.

And yet, contrary to the stereotype of the lone mathematical genius, university practitioners seem persistently preoccupied with sharing their work with each other. On a more-than-weekly basis they boldly blind themselves to the petty incomensurabilities of their respective studies and join in displays, like the seminar, of mutual mathematical activity on scales ranging from local collaborations to international congresses. Far from empty gestures at some fantasy of common mathematics, these interpersonal contexts form an essential part of the search for problems, theorems, and proofs which defines the researcher's work. This chapter uses the seminar to develop observations about several spheres of interpersonal mathematics in order to begin to explain the crucial relationship between mathematics and mathematicians.

Of particular importance in my analysis are two activities at the heart of mathematical research. The first entails setting work in one or more programmes—that is, fields of problems, methods, and heuristics which enable researchers to structure approaches to their chosen material. The second involves placing work in a temporal structure comprising single encounters,

[1] Merz and Knorr Cetina (1997, 74) note a similar phenomenon among theoretical physicists.

entire careers, and many scales in between. As in any field of knowledge, these two ongoing processes help scholars make sense of their work in a way specific to the mores and demands of their discipline, and also account for many distinctive features of mathematical research.

4.1 Time and Topic in the Analysis Seminar

The seminar's order of service varies little from week to week. I made a habit of arriving a few minutes early and was often the first in the room. Occasionally, the day's speaker paces quietly in front of the room or murmurs to himself over his notes at an impromptu podium afforded by a single desk and chair set against the front row of desks. As the appointed hour comes and goes the room rapidly fills. The seminar-goers take their seats near to where they sat the week before: postgraduate students tend to cluster; established faculty hold court along the sides or at the front; junior and emeritus faculty tend toward the back, and the former sometimes fall in with the students.

A few minutes past two o'clock, the convenor asks after late-comers and closes the door—often just before a final participant or two sneak past him to their seats. Regardless of how well known the speaker is to those assembled, the convenor then states his name, affiliation, and a few familiarizing words. The speaker offers an informal remark, gives and then qualifies the lecture's title, and then embarks on an hour-long mathematical presentation. The talk starts by framing a problem in terms of other better-known ones, which allow the speaker to introduce key terms and notation. A prior result or special case might then demonstrate a method or identify an important feature of the speaker's new result or approach. Rare is the lecture that fits neatly into the prescribed hour—most feature impromptu leaps and cuts, hasty summaries and omissions. Few give detailed arguments for what they would consider their main result.

At points along the talk, the speaker asks if there are any questions, and at entirely different points the seminar-goers interrupt the speaker to ask them. Invitations for questions typically concern global conceptual features of the argument; the questions that are asked are most often about notation or conceptual particulars such as the domain of applicability for a specific statement. After the talk concludes, polite applause wakes any whose alertness proved unsustainable. A follow-up question is solicited and sometimes asked, depending on how acutely the audience feels the pressure of the clock. Another round of applause invites some to leave the room, others to gather, standing, in small clusters to discuss passing matters or upcoming events, and still a few more to approach the chalkboards at the head of the room

to petition the speaker for a final set of clarifications or elaborations. These latter questions tend to explore the conceptual robustness of ideas from the lecture, with a focus on their inter-applicability.

Seminar performances are conditioned on a form of understanding quite unsuited to acquiring or trading research competence for specific mathematical studies. Synthetic comprehension is out of the question for anyone for whom the talk has anything of substance to offer—that is, all but the speaker and any collaborators in the audience. Instead, seminar-goers comprehend the talk in the sense of *following the argument*. This register of understanding encompasses the technical manipulations and heuristic indications with which the talk's conceptual narrative is constructed, and forms the basis for the bulk of audience questions, notes, and other interactions with the speaker and presentation. It does not, however, include the other practical, theoretical, and technical knowledges necessary for a working understanding of the mathematics at issue—these can take years to acquire.

This *following* mode is reflected in how both speakers and participants prepare for the seminar. Which is to say, in large part, how they do not prepare. Audience-members did not "study up" for upcoming talks by looking into the speaker's topic or previous work. Seminar-goers, I was told, are primarily interested in the speaker's findings, rarely worrying after how they were attained. They are easily bored, my informant added, and prone to distraction by unimportant details. Speakers indicated that their preparations, depending on the formality and importance of the occasion, ranged from "exactly four minutes" to a week of sporadic effort. For a chalk lecture, a single draft of highly-condensed notes suffices. Slide lectures take more time to prepare, but the slides' equations can be lifted directly from the electronic text of draft articles; indeed, the seminar is often the pretext for setting such equations to text for later use in papers.

Speakers do not aim to give fully rigorous mathematical demonstrations, to present the full extent of their own findings, or to convey the details of their research process. Good speakers will isolate the most important and interoperable features of their work and combine them with the most accessible and intuitable rationalizations from the relevant literature. Seminar lectures provide the kind of narrative lacking in formal published writings, one rich in the kinds of circumstantial cues that make it possible to follow an argument over its macrological arc while foreclosing the possibility of verifying it rigorously as it goes along. Indeed, informants' characerizations of advanced mathematical arguments suggested, surprisingly, that in many cases the writing and thinking needed for *following* and *verifying* are mutually exclusive.

Occasionally, seminar narratives provoke a question or comment that al-

ters the course of a speaker's research, but not so reliably that the presenter plans for that possibility. More often, the speaker's and audience's expertise and interests align only superficially. As one speaker put it: "it's not clear that there's anything in the intersection of what this person's thinking of and what I know how to do." No active researcher listed seminars or conferences among their resources for staying abreast of the field. The seminar's material, while potentially helpful for orienting younger scholars in an area of study, lacked the scope and representativeness to do so reliably. Seminar-goers infrequently find a new question or approach from a talk, but this hope hardly begins to account for the persistent presence of researchers at talks further afield from their studies, nor does it explain the attendance of emeritus faculty who no longer publish actively. "It's a bit like a beehive," one speaker volunteered in an interview a few days before his talk: "Collecting nectar and pollen doesn't benefit the specific bee so well, but it's important for the community."

4.1.1 The Seminar's Programmes

The seminar's community-work takes place at many scales and in many registers. Its most obvious effect is to consecrate its participants as a community of researchers. Seminar-goers go to the seminar because they are members of the Analysis Group, a group whose chief manifestation is the attendance of its members at the Analysis Seminar.

During the talk, the speaker's claims and allusions join with audience questions to reinforce specific kinds of shared expertise that join them as a community. The speaker's introduction places the talk within what one participant described as "industries" of research in analysis. Industries, as he described them, are recognizable from their distinctive sets of standard problems, examples, and approaches, influencing both the questions researchers pose and the techniques they use to tackle them. They organize participation, funding, and prestige within a field. Younger researchers learn early to slot themselves into industries, typically ones associated with their supervisors and early-career mentors; one such researcher told me that he would not feel entitled to decide which problems were interesting until he was older.[2]

Within the rarely-upset order of a research industry, a handful of well-circulated results supply a common vocabulary and methodological framework for members' interactions. It was not difficult for respondents to reel off strings of standard questions to be asked of objects or ideas from theirs

[2]Such an attitude is not uncommon in the sciences, but is significant insofar as young mathematicians are less dependent than their laboratory-based peers on the material support of senior researchers.

or colleagues' work, and without prompting they gave remarkably uniform justifications for their specific inquiries with reference to these questions and their canonical motivations. Standard problems, such as the Dirichlet and Neumann boundary problems in partial differential equations, need not be solvable for all of the cases in which they are posed in order to direct and regulate research; some famous quandaries, such as the Navier-Stokes problem in the field of partial differential equations, motivate a great deal of research without promising to be solved themselves in the near term.[3]

Industries are also characterized by canonical case studies which can alternately confirm, shape, or overturn a researcher's intuitions, and which are the source of many commonly-used formulations.[4] Researchers are well-practised in applying a narrow range of familiar arguments from these case studies, and can develop expectations of those arguments' strengths and weaknesses. These expectations, in turn, help scholars focus their efforts and avoid tedious false trails. "If the argument is sort of well-established," I was told, "it can be the case that people know where it's going to break if it's going to break." Speakers borrow terms from these approaches even when the terms themselves no longer apply in the case under consideration, as where a speaker referred to a region of radius two as "the unit ball" in order to invoke a well-travelled method of subdividing a calculation in analysis.

These features of research industries structure and divide scholarly activities. In particular, the pursuit of common tokens in the form of problems, examples, and approaches within mathematical industries gives different groups of mathematicians the means and motivations to drive their work well beyond their respective spheres of mutual intelligibility. The relative disciplinary fragmentation noted above, then, can be seen as a direct consequence of mathematicians' industry-based approach to coordinating their work.

In the seminar this coordination appears in sub-discipline-specific ways of describing objects and operations and rendering them on chalkboards and other media. These practices are enculturated through attending and presenting lectures: "you somehow learn how to talk," explained an experienced speaker. Seminar presenters refer explicitly to industries' features, peppering their talks with remarks about "what everybody calls" certain objects or citations of "some standard assumptions" and noting standard approaches even when not using them. Speakers tap into other forms of (stereotypically) shared experience by mentioning, for example, the conduct of undergraduate

[3]One researcher's undergraduate advisor made him promise (unsuccessfully) never to work on the Navier-Stokes problem because he saw its industry as unfruitfully difficult.

[4]Lakatos (1976) illustrates this dramatically with his discussion of Euler's theorem about polyhedra.

tutorials or having an idea when "lying in bed or sitting in a pub one night."

Seminar discourse also establishes programmes through citations of historical authorities in relevant subject areas and references to colleagues (including some present at the seminar). These collections of names further connect the speaker's work to problems and industries relevant to seminar-goers, and engages mathematicians' pervasive system of eponymy. Moreover, they establish a standing reserve of expertise where researchers in related areas can be expected to assist colleagues in need—as, indeed, they frequently do. After the seminar, it is not unusual to see one participant ask another a quick question pertaining to his own work or to request a meeting to discuss a matter at greater length.

Often, the seminar's mores of reference for this reserve of expertise bleed into other contexts. In the seminar, theorems from half a century ago may be just as conceptually pertinent as those from the last half-decade, and chronological distinctions fade in the face of conceptual ones. The same dissolution of distinctions can be jarring outside of the seminar, however, as when one junior researcher seamlessly blended in an interview insights from a senior colleague gleaned, respectively, from a conversation the previous week and from a body of work more than two decades old.

4.1.2 The Seminar's Times

Mathematical results come about in many ways, and their origins and journeys are difficult to isolate, even for their creators and shepherds. As Lynch (1985) notes for neuroscientific researchers, however, mathematicians tend to organize their experience of research into multiple projects, each of which groups together specific sets of endeavours and places them in a narrative arc comprising both advances and setbacks. Those I consulted typically maintain three or so ongoing projects. The relevance and concurrency of such projects can wax and wane, and it is possible to have large numbers of both active and inactive projects in progress or "on the shelf." Projects have origin-stories, research questions, "tools" or methods, and goals. They can be undertaken in isolation or in collaboration with other researchers both near and far who might have complementary skills and expertise. Over a longer period, projects are viewed as part of research trajectories that define one or more areas in which a mathematician has worked.

New projects are seen to emerge from many sources. They can come from colleagues, previous projects, or standard questions within one's industry. Stalled or failed work can be as fecund as prior successes. One researcher estimated that every mathematician has in mind some five to ten problems. Whatever their source, mathematicians tend to describe projects in terms

of motivating ideas—ideas which often seem to have little to do with the project's historical origins. In this way, they join together the project's time and programme.

Middle stages of projects are often described in terms of play and experimentation. Researchers identify techniques from their prior work, peers, and the published literature, and attempt to adapt it to satisfy benchmark requirements in service of their ultimate aim. They see the project as consisting alternately of easy tasks and difficult conceptual barriers. The former may require several persistent attempts to complete, but are not seen as necessitating the significant breakthroughs of the latter. In either case, work consists in manipulating established results and techniques to match one's problem while simultaneously re-framing (or modifying) one's problem to fit established results and techniques. There are often long gaps between identifying a technique and either successfully adapting it or abandoning it—one project I tracked appeared to stall for a month between choosing an idea to apply and expressing confidence in a specific way to apply it, and months after that it still was not clear whether the attempt had borne fruit.

Projects rarely end decisively. Often, collaborators simply lose interest and move on to other work. A change of institution can disrupt or end local collaborations. It sometimes happens that one of a project's motivating problems is solved by someone else, or that the researchers had missed a solution buried in the existing literature. Struggles to resolve a question can make the barriers seem insurmountable, at which point an attempt might be made to salvage a partial result before shelving the larger effort to wait for a breakthrough. Errors sometimes emerge in settled projects that point to greater shortcomings in the work.

After a result is achieved, either within or at the end of a project, there is a "writing-up" stage that marks a distinctive change in the status of a project's efforts. In "writing-up," the volleys of trials and manipulations from earlier stages of the work become transformed into the formal mathematics of publications. Researchers differ as to when and how to begin writing-up, but few begin in earnest until they are confident in the final theoretical result for their exposition. This confidence is always a qualified one, as every researcher has stories about conclusions that had either come apart in the attempt to formalize them or been found in error even after the paper had been drafted, submitted, or accepted. Most see writing-up as a process of verification as much as of presentation, even though the mathematical effort of writing-up is viewed as predominantly "technical," and thus implicitly not an obstacle to the result's ultimate correctness. Collaborators often take turns writing sections or exchanging drafts.

Seminars have a special place in the temporal imaginations of mathe-

matics researchers. For presenters, presentations can drive the writing-up process by forcing the speaker to cast recent results in a narrative that can be used in both talks and papers, one that mobilizes both programme and project to construct an intelligible account of their work. Preparing a piece of work for public consumption requires the impartition of an explanatory public logic where ideas develop according to concrete and recognizable methods. Seminars force researchers to articulate their thinking in terms of a series of significant of each steps, unavoidably changing the thinking in the process by forcing it to conform to a publicly viable model or heuristic.

For regular seminar participants, the weekly event structures their presence in the department, shaping when and how they are present in different parts of the building. The repeated performance of the seminar's project logic helps participants to see their own work more clearly in those terms. Finally, the audience's presence allows the seminar to fulfil its roles for the speaker, insofar as a public logic requires a public.

4.2 Time and Topic in the Department

A member of the Analysis Group is, of course, a member of far more communities than that. At different points in time, mathematicians will interact with local and distant collaborators, students and mentors, department and university members, and regional and international colleagues. Within each of these larger groups, there are sub-groups organized according to projects, industries, and specializations, as well as common background or disciplinary service. Programmatic features of mathematical work like those described for the seminar help to organize interaction across all of these levels, establishing, in particular, a notion of disciplinary proximity which marks the degree to which researchers' programmes coincide. Two disciplinarily close mathematicians may meet and interact far more often than two geographically close ones, and will generally have much more in common professionally. Geographic and disciplinary proximities are often connected, with departments specializing in specific areas of research and mathematicians in related industries congregating at known centres. This was particularly felt in the Analysis Group, where many participants saw partial differential equations as a relatively uncommon area of research in Britain or other locales.

Interactions across geographical and disciplinary strata also structure the temporal experience of mathematical research. In collaborations, work was measured against timelines of meetings and appointments, as well as career, departmental, and academic timelines affecting the collaborators' mutual availability. The experience of theoretical features of research is intimately

tied to these exchanges. Emails and telephone conversations are among the many interpersonal contexts in which the ruminations of lone contemplation coalesce into an advance or a misstep. Conferences and guest seminar lectures allow distant collaborators to convene, and their respective coordination further conjoins researchers' sometimes-competing calendars of academic commitments and collaboratory opportunities. At the planning stages of a research project, in-person interaction is widely regarded as essential, even with collaborators at far-off institutions. These intimate exchanges can be marked by a great degree of anticipation and uncertainty, especially in newer collaborations that have not yet produced notable successes.

Geographic and disciplinary proximity reappear in the arrangement of researchers' offices in the Maxwell Building—many members of the Analysis Group share a single fourth-floor hallway; another pair of members shares an office. This facilitates the group-members' frequent shared lunches and occasional after-work social engagements.

At a departmental level, the Tea Room's mailboxes and kitchen facilities promote daily visits by a wide range of faculty, as do the computer and copy room nearby. Microwave cleaning rotations and cost-sharing arrangements for coffee and milk force students, faculty, and staff to interact routinely. Although the Tea Room's doors are equipped with locks, deadbolts are left out so that at least one door is not fully closed on most days. Clusters of tables and chairs host groups of departmental peers for lunch or tea. While mathematical questions do not occupy most of their routine discussions, the topics of conversation tend to mark common features of departmental experience and frequent puns and appropriations of technical language remind conversers of their mathematical setting—an informal recapitulation of the programmatic uses of common tropes, as discussed above. Writings on the Tea Room chalkboard show that mathematical discussions do take place from time to time, while mathematical periodicals and posters remind visitors of the department's place in the professional trajectories of its multifarious inhabitants.

As we have seen, these seemingly inconsequential details of interpersonal coordination in the mathematics department have important effects on researchers' work. They guide consequential aspects ranging from researchers' choice of problems and methods to their opportunities for collaboration. Mathematical research and the communal structures through which it takes place are inseparable and mutually supporting institutions.

Chapter 5

Chalkboards and Mathematical Inscription

One enters the seminar room through a door at the front. On one's left runs a wall bearing an immobile pair of green chalkboards blocked by a rank of desks. The room's four main chalkboards loom to one's right, and the two walls farthest from the door sport a series of portraits interspersed with narrow windows that, on a clear day, afford seminar-goers a view of the dormant volcano called Arthur's Seat that sits on the edge of Edinburgh's Old Town. Ahead, six rows of desks and chairs, neatly split by two long aisles, elaborate a classroom geography whose orientation is always toward the boards at the front and the door at their side.

The dust-covered oracles at the front of the room preside over a mathematics that scarcely resembles the elusive dance of pure ideas found in its stereotypes. In chapter 4, I described a seminar room overflowing with concepts, programmes, ambitions, and associations. But it would be a mistake to presume that the seminar is, by that virtue, secretly suffused with invisible thoughts. Such fancied constituents are a needless frill—a facile shorthand—in an account of a place so manifestly full of pointings, graspings, wavings, tappings, tellings, rubbings, and writings. Most of these latter are concentrated at the front of the room, massed under the agency of the seminar speaker.

This chapter begins by considering how the chalkboard at the front of the seminar room organizes the activity of the speaker and audience. It then focuses on the distinctive act of mathematical writing in relation to its various media. These considerations permit a re-framing of the previous chapter's concerns in terms of the material exigencies of mathematical work.

5.1 Chalkboard Mathematics

There is nothing about the chalkboard that is strictly necessary to the mathematician. There are other means of writing equations for personal or public display, other tokens on which to hang one's disciplinary hat. That chalkboards seem so profoundly to represent the exertions and ontologies of mathematics is in some senses a fortuitous accident. Outside of the seminar room, chalkboards play a relatively limited role in mathematicians' daily practical work. The stereotype of the chalk-encrusted mathematician is nearly as misbegotten as that of the mathematician lost in his own mental world.

Nevertheless, mathematicians return to the chalkboard. Its status as an iconic signifier for the discipline is no accident. Chalkboards dominate mathematics in two crucial spheres: the classroom and the seminar. It is with chalkboards that young mathematicians learn the ins and outs of their art, and it is on chalkboards that established scholars publicly ply their newly-minted innovations. These twin spaces enshrine chalkboard mathematics as exemplary in a way that pervades all of mathematical practice—whether marked in dust, ink, or electrical circuits. It matters little that the full measure of the chalkboard's glory is confined to the narrow environs that lend it its profound influence.

To consider the role of chalkboards in seminars is thus, in a sense, to consider the role of mathematical work in mathematics. Chalkboard work is bound up in mathematicians' routine practices routines, whether or not those practices take place on chalkboards themselves. In that pregnant space between chalk and slate there reposes a germ of the bursts of inspiration, triumphs of logic, and leaps of intuition that dominate mind-centred accounts of mathematics. By paying heed to the seminar chalkboard, one glimpses the material features of the most canonically mental discipline.

5.1.1 What Chalkboards Do

A chalkboard begins the seminar as a blank slate, but even blank slates are full of meaning. Before they harbour any text, chalkboards in the seminar room orient the audience toward the speaker and forecast the spatial outlay of the lecture by delimiting the space on which writing will take place. They guide seminar-goers in choosing seats so as to avoid glares, awkward angles, and obstructions—and dictate how the room might be modified to improve their visibility (for instance, by closing window blinds or relocating obstructive equipment). At the start of the lecture, they collect the talk's title with other information about the speaker's aims and affiliations in order to match the forthcoming mathematics to its professional context.

Chalkboards, I learned through my observations and interviews, are many things to many mathematicians.

Chalkboards are big. Speakers using chalk generally have a much greater surface area to fill than those using overhead or digital slide projectors, or, though this would seem an impossibility for mathematics lectures, using no visual aids at all. The chalkboard's great area magnifies the possibilities for and importance of the arrangement of words and images on its surface.

Chalkboards are also loud. The audible tapping and sliding of chalkboard writing confine the speech of speaker and audience alike, regulating the timing of both explanations and interjections. Even when speakers talk over their own writing, the audible pace and rhythm of the chalk modulates and moderates the speaker's voice.

They are slow—each inscription must be discharged manually by the speaker in a particular sequence. The travail of chalkboard writing, explained one presenter, helps speakers "to fight the urge to sort of rattle through" their exposition and "to focus [their] audience's attention on a specific thing" by forcing the sequential coordination of talk and chalk writing.

They are visibly present—even when one chalkboard is covered by another a simple, observable, mechanical operation will restore it to view.

They are public and common—seminar participants access chalkboard text at the same time and in largely the same way as the lecture unfolds.

And they are robust—though fabricated with varying materials, chalkboards tend to resist the stains, fading, surface erosion, and technical hiccups characteristic of other classroom media. Similarly, though they are quick to break and perish, sticks of chalk will not "run dry" or conceal their waning vitality.

Other media allow seminar presenters to mimic the chalkboard's expositional characteristics. The projected image of a slide affords a limited amount of the chalkboard's spatial organization and permits gestures either at the screen itself or at the projector in order to produce shadows. Text on slides is often sequentially unveiled to match the gradual exposition of chalk text. Many chalk marks can be replicated with pens on overhead transparencies or mimed without writing in front of the screen; the screen is sometimes tapped for emphasis, invoking the board's audible character. These activities reinforced the chalkboard's prominence even when it was not immediately in use.

Chalkboards, of course, also occasion difficulties. They rely on handwriting's sundry legibilities and threaten to mire the speaker in writing lengthy and intricate equations. Their slowness makes them ill-suited for the shorter, densely-packed talks of conferences. The necessity of regularly erasing boards to free up writing space placed a limit, only partially mitigable by strategic

allocations of board space during the lecture, on what text the speaker could return to later. And yet, even when these drawbacks were in obvious effect, only three of thirteen seminar talks I observed used anything other than chalk, one of which used overhead transparencies (inadvertently water-damaged on the way to his presentation) only in the talk's final minutes.

The seminar chalkboard stands in a curious relationship to the many other boards of the department. The Tea Room chalkboard rarely saw either use or consultation while I was present, though it was usually full of text. During conferences or workshops hosted at the department, the chalkboard sprang to life as a facilitator of informal shared mathematics for the room's churning occupants. One afternoon I encountered a lone mathematician puzzling over a cup of tea at a table; eventually, he stood and made his way to the chalkboard. In a blank region of the left side of the board, he carefully chalked a short series of equations, pausing to contemplate each expression before following it with an equal sign and a subsequent clause.

When asked, he explained that these jottings were an attempt to work out how to explain to his colleagues the mathematical concepts just written—the board was an intersubjective medium even with a single user. His contribution in chalk remained intact for nearly the entire term. In fleeting moments like these, the chalkboard becomes a medium for tangling with concepts and signs until they match a suitably familiar intuition. The rest of the time, the Tea Room chalkboard exists in a state of perpetually unrealized mutability, signifying its readiness for public acts of inscription.

Every office in the mathematics department has either a chalkboard or a whiteboard, a fact one respondent took as part of his identity as "on the mathematics side" of mathematical physics researchers. Though office boards are smaller than their seminar counterparts, their size relative to other surfaces manifestly matters: "if you have pages and pages of messy notes, that's almost impossible to navigate through." All use boards as a common surface of inscription for student tutorials and discussions with collaborators. Most also use boards as impromptu phone books or memo pads, often sectioning off a portion which is then rarely modified. Those in a shared office were rather shy about the ostentatiousness of solitary board use, and preferred to do such work when colleagues were not present—a phenomenon which perhaps accounts for the rarity of Tea Room writing as well. Even beyond the seminar room the board's use is shaped by its highly public and visible character, as well as its readiness for both mathematical and non-mathematical writing.

5.1.2 Gestures at the Board

Greiffenhagen's (2008, par. 29–66) analysis of chalkboard work in a logic classroom identifies a range of gestures tied to specific mathematical concepts in an instructor's exposition.[1] Some of the gestures I catalogued over the course of several seminar presentations likewise dramatized particular mathematical phenomena. As he explained the bounding of an expression from the board by a small quantity, one speaker framed the expression on the board between two of his fingers, then turned to face the audience and pinched his fingers together. His description of the fixed point of a function was joined by an iterated gesture that brought his hands incrementally closer to each other, performing the contractive mapping principle associated to fixed points without ever having to invoke it in words or writing. Other motions indicated spheres, absolute value markers, and other symbols and images that cropped up in the lectures' narratives.

While such gestures that exploit explicit semiotic conventions played a crucial role in producing meaning in seminar board-work, they formed a comparatively small part of the collection I noted. Most seminar gestures, by contrast, exploited the spatial configuration of the chalkboard to organize concepts and settings. Pointing at or tapping boards filled with detailed lists of mathematical conditions offers an indispensable shorthand for proofs and explanations that rely heavily on their initial assumptions and parameters. These mathematical contexts are often placed at the tops of boards, and speakers frequently move to a new board for a new set of conditions even when space remains on their current one. When an argument was invoked for the second time in a lecture, the speaker's hand could trace its earlier manifestation from top to bottom as a substitute for re-reading or re-writing it. Gestures can demobilize facts as well as mobilize them. Multiple speakers mimed erasing an expression or simply blocked it with their hands in order temporarily to exclude it from a consideration or to show that an explanation strategically ignores it. Emphatic gestures such as hand-underlining play an opposite role, foregrounding marks as they are used or assigned a new significance.

These latter, more common gestures deploy the board as an indexical medium for mathematical narratives, tying space to concept in a way that invokes more than the mere momentary position of a set of chalk marks. Boards, for instance, allow arguments to cycle on and off display. A question from the audience frequently prompts the speaker to return a previously

[1] See also Greiffenhagen and Sharrock (2005). On gestures' importance to and analytic availability within talk-in-action, see Schegloff (1984). For a contrasting approach to gestures in mathematics, see Núñez (2008).

worked board to its position at the time of its working in order to answer queries about the writing thereon, even if no additional marks are made. It is not uncommon to see the speaker's eyes casting about the board for an earlier statement before deciding how to proceed with the next. On multiple occasions, the speaker gestured at a particular statement's former place on the board even after it had been erased, rather than reproduce the statement in another part of the board for the purpose of referring to it. So important is the ability to refer to prior writing that the London Mathematical Society (2010) advises that "Ideally, most of what is written during a fifty-minute lecture will still be visible at the end: the lecturer will often be referring back to earlier material" and for this to be possible "the lecturer will require several boards, with a large total area."

Board locations also harbour specifically mathematical references. In analysis, one frequently breaks expressions into parts in order to bound and describe them, and these parts are often physically separated and delimited on the board. In one case, an expression was erased in order to be replaced by its corresponding estimate. The speaker resolutely packed the new expression into the same region as the old one, even when doing so left his writing cramped and comparatively difficult to read.

It is hard to argue that this particular predicament substantially affects mathematics beyond those seminar rooms where it applies, but its presence indicates a larger and more pervasive feature of the spatialization of mathematics. Mathematical writing and the mathematical thinking that goes with it are markedly dependent on the media available to the mathematician. Mathematical work traces the contours of its surfaces—there is little that is thinkable in mathematics that need not also be writeable, and chalkboards, paper, and other media make certain forms of writing, and hence certain kinds of arguments and approaches, more feasible than others. As De Millo, Lipton, and Perlis (1979, 274–275) put it, "... propositions that require five blackboards or a roll of paper towels to sketch—these are unlikely ever to be assimilated into the body of mathematics."

5.2 Mathematical Inscription

Inscriptions dominate early laboratory studies and the theoretical works they engendered.[2] Because scientific work is certified and circulated in papers, textbooks, and technical tracts, scholars hunted after the sources of these writings in the laboratory practices of their subjects. Their approach in-

[2]See section 2.1.

spired my methodological focus on writing, but what I found was hardly recognizable in the terms of laboratory studies.

To see how, consider Latour's (e.g. 1986a) notion of the immutable mobile. Textual inscriptions, he argues, make it possible for scientists to materialize and stabilize scientific objects in the form of data, plots, and other representations. These representations can then be mobilized and assimilated with other representations, all while maintaining their ties to the natural or laboratory phenomena from which they were derived.

But immutable mobiles are just a tiny part of the story in mathematics. Instead, the vast majority of mathematical inscriptions encountered in the process of research are given to particular forms of mutability and immobility. Mathematical ideas are shaped and accessed not through permanent records but through strikingly fleeting marks on paper and chalkboards. The vaunted publications at the heart of the immutable mobile account of science must be transformed and immobilized in order to be of use to other mathematicians. These features of mathematical inscription make it possible for a group of mathematicians or a single researcher to grapple with concepts in ways that form a constitutive matrix for mathematical creativity.

5.2.1 What Chalk Does

It is difficult to understand mathematical cognition without first examining what it means to write with chalk. First and foremost, chalk entails the ability to add and remove marks from a board almost at will—but only certain kinds of marks. As new symbols become necessary and as details become relevant or obsolete, chalkboard writers can adapt the visual record of their work to fit these changing circumstances. The chalk's shape, however, and in particular its lack of a sharp point and the angle at which it must be held to make an impression, makes small writing exceedingly difficult and thereby limits the sorts of details available to chalk inscription. It is hard to differentiate fonts in chalk text, and so board-users adopt typewriter conventions such as underlining or overlining as well as board-specific notations such as "blackboard bold" characters (e.g. $\mathbb{Z}$, $\mathbb{R}$, and $\mathbb{C}$) to expand representational possibilities.[3] Another constraint is illustrated by the challenge of 'dotting' symbols to signify derivatives or other operations. Chalk must be dragged along the board to make clear marks, not simply dotted, and so the writer must choose between using the recognizable dotting gesture and producing a visible dot above the desired symbol—something is lost in the presentation

[3]These chalk-writing adaptations are, in turn, routinely found in other settings such as textbooks, articles, and handwritten notes.

with either option.

The process of writing with chalk plays an important epistemological function, as well. The London Mathematics Society (2010) recommends that "traditional board-based lectures" are indispensable for the demonstration of the "strict logical deduction" on which mathematics is based. As the memorandum explains, "one needs to see someone else, the lecturer, working through and *creating* the results" (emphasis in original). That is, the sequential production of mathematical texts is part of the mathematics itself. Its mathematical character comes through its position in an argument whose manner of unfolding is an accountably relevant feature of the exercise.

The mutability of chalkboard writing, moreover, enacts a specifically Platonist ontology of mathematics. In this view, mathematical objects and systems have an independent existence that is separate from their descriptions, and the same entity can be described in a variety of ways. On a chalkboard, lecturers frequently amend statements and definitions about these mathematical entities as they are made relevant by the exposition or by audience interrogation. In such a medium, the fact that the once-written text does not tell the final story about a mathematical concept allows a potentially infinite variety of descriptions simultaneously to apply to an object or situation under consideration. Thus, when the speaker whose talk is the subject of figure 3.1 at first leaves out the 'compactness' of a function's domain in its written definition, it can be seen as an omission rather than an error in his argument—the space can be made to have been compact all along at any such point as that anteriorized conceptual vestment is required for the lecture to go forward.

Likewise, troublesome conditions can be removed or deferred through erasure or annotation. In the seminars I observed, this took two forms. When a statement was deemed false, the offending marks could be rubbed out without incident, preserving the veracity of the chalkboard record. The dusty traces of the statement's removal cue those few taking notes by pen or pencil in the audience as to which items have been removed so they can appropriately modify their own transcripts. Meanwhile, the mis-statements can be set aside or written over as the argument is ushered along its modified course. In other situations, a statement was not necessarily false but, usually after an audience inquiry, was judged to be either misleading or beyond the scope of the presentation. In these cases, the speaker could cross out or put a line through the statement, removing it from the accountable portion of the talk but preserving it among the lecture's mathematical residues.

Narrative as well as mathematical features of a seminar lecture rely on the chalkboard's different modes of erasure. Minor corrections can be made using the side of one's hand to erase small areas of the board while producing

an audible thud that preserves the ongoing sequence of words and board-sounds in the speaker's story. Larger erasures, however, must be made with a separate instrument whose use requires the interruption of such discursive sequences—a desirable effect at the end of a planned segment of a talk and an appropriate one where the speaker must "reset" an argument after a significant lapse. If a narrative sequence overruns its allotted board space, the speaker sometimes squeezes the remaining text in blanks on the current board in order to respect the board-sequence division.

A similar situation was observable in office boards (both black and white), which are frequently spaces for working out or translating complex, symbol-intensive ideas before committing them to paper for other kinds of manipulation. Figure 5.1 shows one such instance of office board use with what the researcher described as a complicated set of indices. The board shows evidence of insertion, annotation, and erasure in the process of working out the equation and its relevances. In an office setting, the board gives the extra advantage of allowing the researcher to synoptically array information in a different format from that of papers or computer screens.

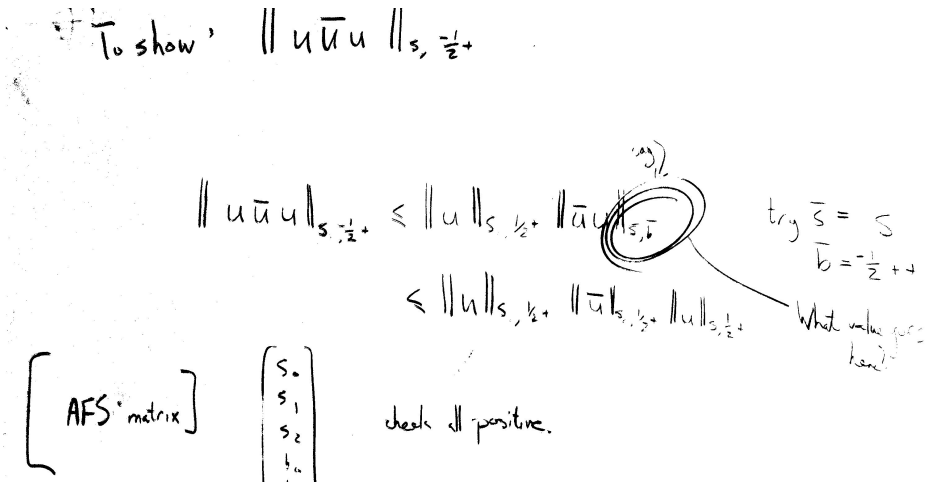


Figure 5.1: A example of blackboard work from a respondent's office (colours digitally inverted and contrasted).

5.2.2 Signs and Sedimentations

Asked while away from his office to describe his work space, one interviewee began with the piles and piles of paper covering his desk: a veritable mess (figure 5.2). Populating those piles are reprints of articles, teaching notes, and, most importantly, page after page of scrap paper. The inscriptions

of mathematical research, while using chalkboards, whiteboards, computers, and other inscription media, seems mostly to subsist in the sort of notes that suffuse the spare sheets of paper from my respondent's desk (the observations of section 3.2.2 notwithstanding). In my discussions with researchers it was clear that these are no ordinary pages. Their function in mathematical research owes to the particular way in which mathematicians access their phenomena of study, and owes also, of course, to the chalkboard.



Figure 5.2: One respondent's initial image of his office.

Merz and Knorr Cetina (1997, 87, 93) describe mathematical work as a process of 'deconstruction,' where equations from problems are successively transformed through a variety of techniques until they yield a new theoretical insight. One subject described the process perfectly:

> I'm going to keep doing the calculations again, only now trying to look for terms of this form.... I have an ocean of terms like this, and the problem in some sense is how do you put them together so that they make some sense.

It is this process of putting terms together that consumes the mathematical efforts of members of the Analysis Group.

Terms can be put together in many ways. Consider the the research notes excerpted in figure 5.3. This researcher's deconstruction begins with the operator L, whose effect on a function u is first written compactly on the left-hand side of the equation in his notes. (The brackets identifying this expression as Lu were added after the fact as he explained this inscription to me.) On the right there appears a nearly-identical expression, with a space opened up between the ∂_j and the rest of the expression's summand (that

is, $a_{jk}(x)\partial_k u)$. Brackets beneath the two sets of symbols identify them as members of specific families of mathematical objects, respectively $S^1_{1,0}$ and $L^\infty S^1_1$, and the latter identification merits a written-out speculation about a technique ("symbol smoothing") and a desired outcome (inversion). All the while, these textual tokens are experienced and described as ideas. In this example:

> We have some variable coefficient operator [i.e. L] that looks like the Laplacian, and so ... [we] split it up into a sum of pieces, I guess a product of two things. In my case, the first product ... is just a derivative, and ... the second factor, less is known about.

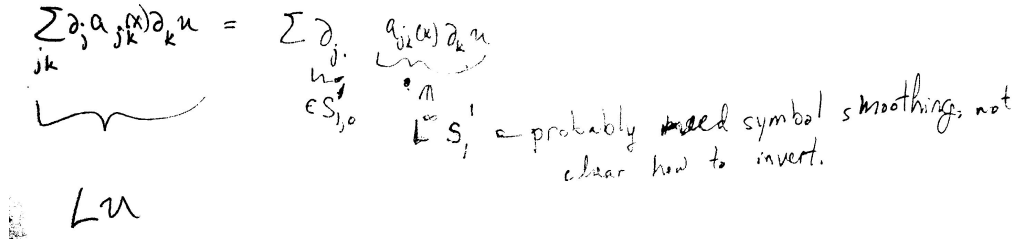


Figure 5.3: An equation from an interviewee's research notes.

In addition to regrouping, symbols can be transformed according mathematical principles and with the help of auxiliary equations and images. As an example of the latter, one researcher routinely uses interpolation diagrams like the one in figure 5.4 to re-frame problems in terms of different sets of parameters. Notations and framings are often adapted to particular approaches. "There's a lot of notation, and it does help to go back and forth between them," I was told. Moving between different variables and expressions can coax a troublesome formulation to resemble a familiar one or allow researchers to break a problem into smaller parts. Annotations can also declare aspects of a problem to be difficult, promising, or solved. In one particularly dramatic example of this, my interviewee recounted how

> I put that in a red box because I was very excited when I realized that.... In my mind it moved us closer to completion of the project.

As concepts are continually re-materialized, salient details are expanded or omitted, much as they would be on the chalkboard. One researcher's notes had the word "factor" in place of a positive constant whose particular value was not relevant at that stage of the investigation. He expected that

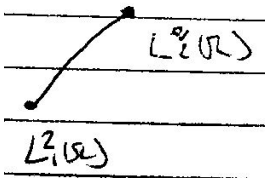


Figure 5.4: An interpolation diagram from an interviewee's research notes.

he might ultimately "see sort of which ones [i.e. factors] are ones that are helping you prove your result and which ones are the obstacles," and could then manage the obstacles separately. The process described to me by multiple respondents involved successive attempts to develop and refine a proof, with each attempt aimed at managing a new set of constraints after one is convinced of the proof's "main idea."

Such iterated efforts at proving, most of which are seen as unsuccessful, produce a long paper trail. One respondent explained, however, that "I don't tend to look back very much." Another has a policy of saving notes until he no longer recognizes the calculations, but confesses that he too rarely looks back at them. "I do a lot of stuff in my head," a third researcher informed me, and his research notes reflected this self-conception by rarely accumulating outside their immediate contexts of production. No researcher I interviewed ultimately worried about forgetting a good idea, once derived, in the absence of the particular texts through which it came about.

Like chalkboard writing, the research inscriptions rely on augmentations, annotations, and elisions alike as concepts are developed through iterated writing. Such inscriptions are closely tied to their times and places of production, rarely persisting in practice beyond their initial mobilization. For the purposes of research, the process of writing appears to matter more than the record it produces. Contrary to the image of scientific writing as an enterprise steeped in the mobilization and circulation of increasingly stable representations, mathematical writing seems built to pass away, leaving something it is tempting to call 'mathematics, itself' as its residue.

Section 4.1.2 described the "writing-up" process that turns achieved mathematical ideas into circulable prose. A large amount of space in mathematicians' offices is devoted to storing official records of such certified mathematics in the form of books and articles. But this prose does not circulate in a form that is operational for its would-be users. Instead, mathematicians' reading habits attest to a profound gap between official and operational knowledge in mathematics.

Mathematical papers, in contrast to scientific reports, are supposed to

contain the entirety of their findings. That is, where scientists must describe experiments and plot data, mathematicians are expected to reproduce in meticulous detail each of the novel rational steps behind their conclusions. These expositions appear to other researchers through preprint and citation databases, and in smaller subdisciplines researchers will simply send preprints to a regular list of colleagues. Accessing works on a database relies on successive filters: a scholar might read the titles of articles in relevant subject areas, the abstracts of articles with relevant titles, and so forth—but almost never reads papers in their entirety.[4]

When information from a paper is deemed relevant to an ongoing project, it is finally read for its technical detail. Rather than attempt to follow every claim, however, readers try to identify concepts, formulations, and conclusions that are recognizable in the context of their work. These identifications begin a process of re-rendering key passages of papers in terms readers hope may ultimately advance their instrumental research goals. Although I did not ask for them specifically, half of the texts my subjects offered for their document-focused interviews were aimed at reconstructing parts of articles they were attempting to use.

The formal expositions of articles are not typically useful for ongoing work. Instead, researchers must translate these texts into one of the several forms we have identified with seminar mathematics. To wit: programmatic understandings undergird what one subject identified as the intuitions at the heart of his work, consisting of networks of associated objects, systems, and properties whose kinships can suggest approaches to research problems. "In many cases," he claimed, "you just use these properties" and save their instantiation into equations for writing up. Other subjects engage with their material gesturally. One described an equation to me by placing invisible terms in the air, one by one, in front of him. "I've written it down so many times," he explained, that he instinctively saw "the first order terms appear here and the second order terms there." My interviewees described plots, limits, vectors, or six-dimensional polygons to me with hand motions in the air in front of them. One placed a box of tea on his desk to explain a source of consternation from a recent effort. Some characterized aspects of their work in terms of partial geometric pictures that captured the piecemeal features of their objects of study needed to permit successful research. Finally, it was clear that symbolic manipulations on scrap paper or notepads provided a crucial basis for research comprehension.

[4]An exception to this observation is when a paper is seen as particularly important for a group of scholars, in which case they might organize a seminar in order to attempt to understand it collectively.

Nor are these different modes of understanding disjoint or independent. A single page of one researcher's notepad turned out, in our interview, to manifest a series of translations from an article to penned equations to an email to gestures and further writings. Different modes of mathematical cognition must necessarily interact to produce the transformations that bring about original proofs and theorems—transformations that would not generally be possible within a single representative framework. Moreover, they must interact in a way that allows the coordination of mathematical understanding between different researchers in a variety of settings.[5]

This leaves mathematical ideas in a strange position. The entities mathematicians experience as ideas depend on inscriptions and other realizations that are inseparable from the ideal concepts they supposedly merely represent. Paradoxically, mathematical inscriptions (especially on chalkboards) work in ways that specifically assert the opposite—that ideas somehow do not depend on the ways in which they are mobilized. The flexibility of mathematical representations obscures the socio-material coordination necessary to move concepts so freely from one form to another. Mathematical work rests on self-effacing technologies of representation that seem to succeed in removing themselves entirely from the picture at the decisive junctures of mathematical understanding. It is only in this sense that one can be said to understand a concept itself rather than its particular manifestation.

Except when one cannot. Like scientific instruments, mathematical representations are subject to "troubles," flaws, and shortcomings (see Lynch 1985). The vast majority of attempts to use material proxies in one form or another to elucidate a concept are not counted as successes within a program of research. Seminars are among the rare displays of mathematical semiosis in a research setting where it is understood and expected that the signs will work. Mathematical research is marked by the constant struggle to create viable signs:

> It's largely having a model and trying to get the new thing to fit into the old model, and at certain points that simply fails, and at that point you sort of mess around and think about ... the old one a slightly different way, sometimes just calculating [and] seeing what comes out... and realizing that if you thought about it the right way in the first place it would have been immediate.

Even when a viable constellation of representations is found, the mathematician's work is not done. These multifarious semiotic entities must then be made accountable to the equations, syllogisms, and arguments found in

[5] cf. Hutchins's (1995) study of coordinated cognition surrounding navigational maps.

the published literature that compose the official corpus of mathematical knowledge—a project for which they are poorly adapted. A staggering portion of mathematicians' work goes into decoding published papers to create functional intuitions and understandings and, conversely, into encoding those intuitions in the accountable forms in which they will be credited as genuine. This is why seminars are so important. They give researchers partial access to what is so obviously missing from other accounts of completed work: namely, the experienced material performance of mathematics in action. The tension between circulation and application in mathematics is a real one. Mathematical ideas do not move well. The most important features of mathematics can be as ephemeral as the dust on the chalkboard.

Chapter 6

Conclusions

I began this thesis with a paradox: mathematics is locatable simultaneously in too many communities and too few. This view of the discipline rests, I claimed, on a view of mathematics that disconnects it from its users. My study examined one community of users for which this assumption is manifestly unsustainable. Within the Analysis Group, mathematics can be seen to depend in an essential way on the social and material figurations of its producers and their contexts of production.

It bears repeating that my efforts to follow how mathematicians represent their ideas to each other has implications for far more than the social status of those representations. Indeed, this study propounds a view in which mathematics and its representations prove inseparable and in many ways indistinguishable. Rather than work with stable incarnations of formal ideas, mathematicians must grapple with the always-fragmentary nature of their own understanding of their ongoing (and particularly nascent) projects, and their still more limited understandings of the work of their peers. Mathematicians engage in a specially-tailored form of social coordination whose effect is to permit a commerce in unsettled notions that sustains their professional enterprise.

Mathematics presents an imposing edifice. As a discipline, it appears to have a privileged access to truth and certainty. My ethnographic study confirms what many sociologists of science have perhaps long suspected, that this view of mathematics is a convenient and powerful fiction that masks substantial lacunae and incommensurabilities in mathematical knowledge. But I do not stop there. Research in mathematics persists in the face of real and monumental barriers to mutual understanding among researchers, barriers that seem to invalidate the very enterprise. Chapter 4 presents the socio-material grounds for explaining not just the massive fragmentation of such research, but also the discipline-specific institutions that make it whole.

These insights equip me to tackle, in chapter 5, the idealist assumptions underlying the paradox with which I began. By identifying the contexts and media of mathematical exchange, it becomes possible, again, to confirm what many have suspected—that mathematics is not so pure a realm of ideas as it appears to be—but also to indicate how that idealist assumption is maintained in the day-to-day work of mathematicians.

As a methodological experiment, my ethnography turns this study's animating paradox on its head to offer a template for future investigations. To understand mathematics, it would suggest, it is necessary to examine how each of its too-many communities manages, through that plurality, to make mathematics the dominion of too few.

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