mathematics
without
apologies

mathematics without apologies

portrait of a problematic vocation

with a new preface by the author

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preface to the paperback edition

The publication of a paperback edition of *Mathematics without Apologies* provides a welcome opportunity to correct some of the misunderstandings of my intentions. I've encountered these misunderstandings in published reviews and online comments, as well as in conversations. It's my responsibility as an author, of course, to be understood. Parts of the book were written in a deliberately elusive style, playing on the ambiguity of vocabulary and (for the most part) avoiding explicit value judgments. I rationalized this as an attempt to let the material speak for itself, rather than to impose my own necessarily partial perspective.

It is only to be expected, however, that when an author's intentions are obscure, readers attempt to fill in the gaps; and I should not have been surprised that some readers chose to do so with material from just the kinds of unexamined preconceptions I was hoping the book would challenge. So, for the space of a few pages, I will attempt to clarify my original intentions in writing this book. I have organized this new preface into four parts, with each one addressing one of the four most common misunderstandings.

Mathematics without Apologies is Not a Memoir

Although both Amazon and Wikipedia seem to think that *MWA* is autobiographical, Chapter 9 is the only part of the book that is really based on my life. It is also the only part framed as a conventional linear narrative—with a pair of significant flashbacks at the end. I included the story because I think it's a pretty good one. But it also happens to be my own story, and I'm the only one in a position to tell it properly. The purpose, however, was not to talk about myself—why should the reader care what happened to me?—but rather to make specific points about mathematical discovery.

Autobiographical fragments are included elsewhere in the book (all the champagne receptions, for example) in the service of journalistic authenticity, or just to provide a semblance of narrative structure. The events that altered the life of the ideal-typical protagonist of the sociological *bildungsroman* that is Chapter 2 were mainly taken from my own life, not because they are exceptionally interesting but precisely because they are typical. It would have made no difference if I had made them up.

Routinized Charisma and the Mathematical Hierarchy

The title of Chapter 2, "How I Acquired Charisma," is a deliberate provocation, chosen to grab the reader's attention. The chapter does contain a "how" narrative, constructed out of incidents from a typical career that just happens to be my own: a more accurate title for this material would have been "Stages in the Career of a Typical Pure Mathematician from the Awakening of Interest to Tenure." Such a title would have been less catchy, of course, but it would have made it immediately clear that the charisma of the title is *routinized charisma*. Having it is a source of satisfaction, but the point of the chapter is that it's not an unusual distinction; it belongs to everyone who is fortunate enough to make a career as a pure mathematician.

In retrospect, though, I do regret not having had more material about the importance of cooperation in mathematics. It's more fun to talk about the compulsive craving of some mathematicians for recognition, as in the Weil anecdotes early in the chapter, but the fact is that I know no other branch of academic scholarship that is more deeply cooperative—even though the actual work of mathematical research is generally carried out individually. Once, when I was seated next to an experimental biologist at an academic dinner. I explained to her the attention given to comprehensive bibliographic references in mathematical papers, some of them quite old, as well as the convention of acknowledging debts to earlier work. I asked whether there was anything similar in the literature of her field. Certainly not, she answered, they only cite their predecessors' work to point out what they got wrong. True or not, it's not the sort of thing a mathematician would ever say.

How Mathematics Is or Is Not Useful

This was the subject of the most serious misunderstandings, as expressed in at least two widely-read reviews, and in this case I don't think I'm entirely to blame. Any discussion of the utility of pure mathematics, especially of number theory, inevitably comes back to G. H. Hardy's declaration which is quoted at the beginning of Chapter 10, "I have never done anything 'useful.'"[1] Many readers seem to have decided that my unapologetic intention was to amplify Hardy's claim and to argue in favor of mathematics for its own sake.. Some worried in print that this was exactly the wrong message to send to elected officials and the general public, who would be all too happy to cut off our funding (the "external goods" to which I refer in the book).

The text anticipates this misunderstanding and addresses it repeatedly, but the message clearly didn't get through to everyone. So let me just say here that my goals were rather different. These three were perhaps the most important:

To draw attention to the increasing pressure on scholarship to prove its utility in narrowly economic terms. The humanities feel this pressure most strongly, for obvious reasons—Chapter 3 quotes an attempt at a response to this pressure by a British philosopher—but pure mathematics is not exempt. Paradoxically, precisely because much mathematics *is* conventionally useful—for creating gadgets, enhancing competitivity and the gross national product, and so on—mathematicians find it easy to justify our discipline by what the book, following Steven Shapin, calls the *Golden Goose Argument*. Examples of this argument from three European countries are analyzed in Chapter 10. And it's not wrong. However, it misses the second point—

Namely, that the desire to contribute to the GNP or to create a startup, laudable though it might be, is *almost never* the reason anyone chooses to go into pure mathematics in the first place. Some of the real reasons are introduced, haphazardly, throughout the book, but I may not have insisted strongly enough that I don't think keeping decision-makers and the public in the dark about our true motivations as pure mathematicians is a viable long-term strategy.

Most importantly of all, the utility argument is itself specious because there is no general agreement as to what is and is not useful. The problematic utility of financial mathematics is the main topic of Chapter 4, but other examples would have served just as well. The mathematical methods that protect privacy can be used to undermine privacy; the differential equations that maximize extraction of fossil fuels can be used to develop alternatives. Utility in mathematics, in other words, is of profoundly *political* import—as it is (perhaps more transparently) everywhere else. This leads me to my last topic:

Ethics

We choose to become mathematicians, of course, not for the sake of truth, beauty, or utility, but because it's *what we want to do*. If this strikes many people as unforgivable self-indulgence, it can only be because *most people don't get to do what they want to do*—or, not to put too fine a point on it, *what they get paid for is not something they particularly want to do*.[2] The freedom mathematicians enjoy to get paid to do what we want is indeed a rare privilege, and precisely because we can't claim to be more deserving of this privilege than anyone else, this position of freedom does confer a real responsibility, and not (or not exclusively) to the "powerful beings" who shadow Chapter 3.

I see this responsibility as an obligation to think through the ethical implications of all aspects of our work. I have in mind not only the implications of our work's potentially dangerous applications but also the compromises we make in order to enjoy our privileged professional situation, as well as the commitments we accept along with the "external goods" on which our professional freedom is based. If this book attempts to illuminate the circumstances of our freedom and the pressures our freedom faces, it is because I see these pressures as symptomatic of an attack on everyone's freedom, in the spirit of Margaret Thatcher's notorious and disempowering slogan TINA—There Is No Alternative. By the same token, the existence of pure mathematics—of its "relaxed field," in the vocabulary of Chapter 3—can serve as the beginning of a reminder that there is always an alternative.

preface

Wer das Buch schriebe, hätte die Vorrede Schritt für Schritt zurückzunehmen, aber sie ist das Beste daran, das Einzige was wir können, wir Modernen...

Ich will aus solchen Vorworten zu ungeschriebenen Büchern ein Buch machen, ein modernes Buch. Und ich schrieb eben—das Vorwort dazu.

—Paul Mongré, *Sant' Ilario**

When this book was nearly done and my colleagues started asking me what it is about, I found it simplest to answer that it's about how hard it is to write a book about mathematics. That's the short answer; the unabridged version involves a few pages of explanation. Here are those pages.

Of course people are writing books about mathematics all the time—and not only for expert audiences. The most effective of these books strip away the technical jargon to convey the magical sense that pure thought can conjure a second life, a virtual world of shapes and numbers and order and rules where not only do we know that everything is as it should be but we are also satisfied that we know *why*. Knowing why is the specialty of mathematical reasoning, but the virtual world of *pure* mathematics, not designed for any practical application, is remote from our first and authentic life; those of us dedicated to that world feel (or are made to feel) obliged to justify our indulging in an activity that is charming and engrossing but that appears to bring no benefit beyond the pleasure of knowing why.

These attempts at justification are the "apologies" of the title. They usually take one of three forms. Pure research in mathematics as in other fields is *good* because it often leads to useful practical conse-

* "Whoever would write the book would have to undo its preface step by step, but it's the best, the only thing we can do, we Moderns. . . . I want to make a book of such prefaces to unwritten books, a modern book. And thus I wrote—the preface to this book" (Hausdorff 2004).

quences (Steven Shapin calls this the *Golden Goose* argument); it is *true* because it offers a privileged access to certain truths; it is *beautiful*, an art form. To claim that these virtues are present in mathematics is not wrong, but it sheds little light on what is distinctively *mathematical* and even less about pure mathematicians' *intentions*. Intentions lie at the core of this book. I want to give the reader a sense of the mathematical life—what it feels like to be a mathematician in a society of mathematicians where first and second lives overlap. But during this guided tour of what I want to call the *pathos* of mathematics, we will repeatedly see our intentions misrepresented, and we will be reminded how hard it is to explain what impels us along this peculiar path.

Rather than rely on apologies, this book pieces together fragments found in libraries, in the arts, in popular culture, and in the media, to create a composite portrait of the mathematical vocation. The sequence of chapters very roughly follows the trajectory from the vocation's awakening, through struggles with various kinds of temptation, to its consolidation, followed by a conclusion consisting of inconclusive reflections on what we know when we "know why" and what it all means. Although I have consulted actual transcripts or recordings of mathematicians talking, my sources consist mainly of writings about mathematics, especially by participants—so the portrait is largely a self-portrait, though not of the author, of course!—but also by (usually, but not always, sympathetic) observers. Preconceptions and misrepresentations are fair game but are usually identified as such. I have paid special attention to writing or speaking by mathematicians whose manifest content may concern truth or utility or beauty, but which exhibit an aiming at something else, the values and emotional investment—the pathos, in other words—involved in pursuing the mathematical life.

Writing the book was a process of assembling and organizing this material in connection with selected themes and unifying perspectives. The process of assembling suggested virtues rather different from those usually invoked. Alternatives I explore in this book include the sense of contributing to a coherent and meaningful *tradition*, which entails both an attention to past achievements and an orientation to the future that is particularly pronounced in the areas of number theory to which my work is devoted; the participation in what has been described, in other settings, as a *relaxed field*, not subject to the pressures of material gain and productivity; and the pursuit of *pleasure* of an elusive, but nevertheless specific, kind.

The alert reader will have noted, correctly, that these alternatives are no more distinctively mathematical than *good, true,* and *beautiful*. I certainly don't think they offer definitive solutions to the riddles of mathematical pathos; but they did make it possible for me to hint at a vision of the mathematical good life that I find more reliable than the standard account. Another author, presented with the same material, would assemble it in a different way and would likely reveal a different set of habits, virtues, and goals. This is only natural; I try to make the diversity of the community of pure mathematicians visible by recording their distinctive opinions, and there's no reason to assume they come to the field sharing identical motivations. At most, I hope that the reader will see coherence in my personal assemblage.

The "problematic" subtitle alludes to the problems that define the intellectual landscape where the mathematical life makes its home. It's conventional to classify mathematicians as "problem solvers" or "theory builders," depending on temperament. My experience and the sources I consulted in writing this book convince me that curiosity about problems guides the growth of theories, rather than the other way around. Alexander Grothendieck and Robert Langlands, two elusive costars of the present book's narrative, count among the most ambitious of all builders of mathematical theories, but everything they built was addressed to specific problems with ancient roots. Entering the mathematical life is largely a matter of seeking an orientation among such outstanding problems. In this way, as in every other way, the mathematical life is a running dialogue with human history.

The mathematical life is problematic in other ways. Trade books about mathematics typically follow a quest narrative. They share with the currently dominant model of science writing an attachment to a simple moral economy in which the forces of light and darkness are clearly delineated. The quest may be embodied in the protagonist's need to overcome external obstacles or to meet an intellectual challenge; its happy ending takes the form of a triumph over a hostile or unpromising environment or the rewarding of the protagonist's unique talents—or both at once. The reality is not so simple. The most interesting obstacles are less straightforward and more problematic. These can never be overcome: they are inseparable from the practice of mathematics itself. There is the need to guarantee a constant supply of the material underpinnings of our practice, what the moral philosopher Alasdair MacIntyre calls "external goods." We might feel deeply ambivalent about what we provide in exchange. We set aside our ethical compass and contract Faustian bargains (Faust is a recurring figure in this book). We promise Golden Geese, immutable truths, ineffable beauty. We collude in the misrepresentation of our values and our intentions, in "the alienation from oneself that is experienced by those who are forced to describe their activities in misleading terms."*

But any burdens left on our conscience when we contract bargains, Faustian or otherwise, can be separated from mathematics, at least conceptually. It's not the least of the paradoxes explored in this book that the pathos of mathematics grows darkest and most problematic at its moments of greatest success. Satisfaction in solving a problem can be intense, but it is short-lived; our pathos is driven by what we have not yet understood. André Weil, one of the twentieth century's dominant mathematicians, described this as "achiev[ing] knowledge and indifference at the same time." We never understand more than a finite amount of the limitlessness of what mathematics potentially offers to the understanding. If anything, the situation is even more frustrating: the more we learn, the more we realize how much more we have yet to understand. This is also a kind of Faustian bargain—Goethe's Faust got to keep his soul until he reported to the Devil that he was satisfied with what he had seen. The mathematical soul, embodied in a historical tradition oriented to a limitless future, can rest secure in the knowledge that its dissatisfaction is guaranteed.

And yet there are enough of us who find the attractions of the mathematical vocation irresistible to fill any number of books this size. Is this because the vocation is so problematic, or in spite of that? In fact, this book narrates a quest after all, an abstract quest, to explain how being a pure mathematician is possible and, no less importantly, to explain why the reader should wonder why an explanation is needed—all

* The quotation is from (Collini 2013), about the subordination of higher education to market priorities—thus primarily concerned with the "good"; but it applies equally well to the two other ways of describing mathematics to which my title refers.

of which is just a roundabout way of saying that the book is about how hard it is to write a book about mathematics.

Apologies Nonetheless

The title is a transparent allusion to G. H. Hardy's *A Mathematician's Apology*, published in 1940, the closest thing pure mathematicians have to a moral manifesto. As such it tends to be read as a timeless declaration of principles, especially by outsiders writing about the ethos of mathematics. In chapter 10, I restore it to its historical setting and examine how the ethical obligations to which its author subscribes measure up against contemporary circumstances. "Apology" for Hardy meant an attempt to justify the choices he made in the course of his life according to the ethical standards of antiquity, as updated by G. E. Moore's *Principia Ethica*. Hardy's essay spurns useful Golden Geese and instead formulates the classic justification of mathematics on the grounds that it is *beautiful*.

This book does not offer an alternative apology (hence the title), nor does it claim that society owes pure mathematicians a comfortable (though hardly extravagant) living in our relaxed field. The point is rather that apologizing for the practice of pure mathematics is a way of postponing coming to terms with what its practitioners are really up to—what society gets in exchange for allowing pure mathematicians to thrive in its midst. But apologies are due, nonetheless, to my peers in other disciplines, as well as to the reader. In writing this book, I have drawn extensively, if not systematically, on the work of certain historians, philosophers, and sociologists, for whom mathematics is a matter of professional concern, and the reader may suspect me of making unjustified claims of disciplinary expertise. Separate apologies, or explanations, are therefore due to each of these professions.

To historians, I apologize if I have not been sufficiently diligent in insisting that what counts as mathematics—and what it means to be a mathematician—are in constant flux.* When the text alludes to mathe-

* The professional experience of mathematicians who teach at universities, which is where most research in pure mathematics takes place, has evolved considerably during my lifetime. Extended postdoctoral periods are now the norm in much of the world, for example, and attendance

trends in history of mathematics is the comparative study across cultures, especially between European (and Near Eastern) mathematics and the mathematics of East Asia. These studies, which are occasionally (too rarely) accompanied by no less exciting comparative philosophy, is necessarily cautious and painstaking, because its authors are trying to establish a reliable basis for future comparisons. I have no such obligation because I am not trying to establish anything; my knowledge of the relevant literature is secondhand and extremely limited in any event; and the allusions in the middle chapters do nothing more than provide an excuse to suggest that an exclusive reliance on the western metaphysical tradition (including its antimetaphysical versions) invariably leads to a stunted account of what mathematics is about. In the same way, the occasional references to sociology of religion or to religious texts are NOT to be taken as symptomatic of a belief that mathematics is a form of religion, even metaphorically. It rather expresses my sense that the way we talk about value in mathematics borrows heavily from the discourses associated with religion.

And to all my colleagues in other disciplines, I want to apologize again for the introduction of idiosyncratic terminology (like mathematics versus Mathematics) or my use out of context of terms (such as *charisma* or *relaxed field*) with well-defined meanings. This is not intended as a sign of disrespect. I chose these words to give names to conceptual gaps I find particularly problematic, and for this very reason I would be disappointed if anyone else used them; that would be a failure to recognize that the gaps deserve rigorous analysis.

The remaining apologies are addressed to readers. First and foremost, I need to explain my own first-person appearances in the book. My mathematical life, while by no means exemplary, does offer some illustrative examples; but the main function of the "I" who occasionally surfaces to interrupt a general or abstract discussion is to provide narrative unity. I drew on my personal experience when it served to illustrate a larger point (when it was *ideal-typical* in the Weberian sense I try to mobilize in chapter 2). This is especially true in chapter 9: the story is as literally accurate as my memory allowed but the reader should recognize that in dramatizing the creative process the chapter makes use of familiar narrative conventions, including the quest narrative. For such conventions a protagonist is indispensable. In chapter 9 and elsewhere

I occasionally fill that role, but other protagonists make brief appearances. They are taken from the rosters of prominent mathematicians past and present, real or fictional—or both at once, as is notoriously the case of Evariste Galois, prototype of romantic idealism in mathematics as well as forerunner of many of the themes that dominate contemporary number theory.

Although they were inspired by a (very brief) true incident, the series of chapters entitled "How to Explain Number Theory at a Dinner Party" are the least autobiographical as well as the most technical in the book. Each one, labeled by a Greek letter, is divided into two parts, the first an exposition of a basic notion in number theory, presented in approximate order of complexity, and the second a fictional dialogue designed to address questions that might have occurred to the reader while watching the mathematical material flow heedlessly to its conclusion. The thought was that there ought to be some number theory in a book about mathematics written by a number theorist, but since number theory has largely to do with equations, which lay readers reportedly find repellent, the equations and the surrounding number theory have been cordoned off in an alternative route, a separate tunnel from one end of the book to the other that can easily be visited but no less easily skipped.

One of the author's alter egos in the dialogues repeatedly invites the reader to think of a number neither as a thing nor as a platonic idea but rather as *an answer to a question*. Outside the fictional dialogues, I wouldn't put a lot of energy into defending this way of thinking—not because it's wrong but because the kind of number theory with which the book and its author are concerned cares less about the numbers themselves than about the kinds of questions that can be asked about them. The habit of questioning questions, rather than answering them, pervades contemporary pure mathematics. In this book, and especially in chapter 3, the habit spills over to question some of the questions that come to us from outside, the ones that seem to compel us to answer along the lines of "good," "true," or "beautiful." Chapter 3 is written in a more academic style than most of the others and is easily the most superficially "serious" part of the book. Readers who want to know what mathematics *is* and who don't need to be convinced that the subject is not exhausted by the good-true-beautiful trinity can safely skim or even skip chapter 3.

The expositions of mathematical material in the "Dinner Party" sequence are as scholarly as anything in the book, but it seemed to me that to maintain a serious tone in the dialogues that follow would make them impossibly stilted. Why this is the case inevitably raises the question of just how seriously mathematics is to be taken. My confusion on this point is such that I sincerely can't tell whether or not the question itself is a serious one. The position of this book is that what we understand as pure mathematics is a necessarily human activity and, as such, is mixed up with all the other activities, serious or otherwise, that we usually associate with human beings. The three chapters in part 2 try to make this apparent by depicting mathematicians in unfamiliar but recognizably human roles: the lover (or theorist of love), the visionary, and the trickster. Modes of mathematical seriousness come in for extensive examination throughout the book, especially in the final chapter of part 2. I can assure the reader who finds this unconvincing that my intentions, at least, are serious.

The contrast between seriousness and frivolity overlaps thematically with the opposition of high culture to popular culture. The presumption that mathematics is a high cultural form is one of those unexamined preconceptions that can stand to be examined; much of chapter 8 is devoted to precisely this question, but it comes up in other connections as well. I draw on popular culture because I find that it serves as a reliable mirror of the cultural status of high culture, and the representations of mathematics in popular culture have as much to do with sustaining the field's high cultural pretensions as do its (often-clichéd) representations by professionals.

Finally, although I have made an effort to list the many sources I consulted during the writing of this book and, although I have double-checked translations (sometimes with the help of native speakers) when they looked like cultural projections, the reader should not mistake this book for a work of scholarship. I tried to make sense of the material I did find, reconciling it as far as possible with mathematicians' accounts of their experience and with my own experience, but another author could well have consulted other sources and other experiences and come to very different, but no less valid, conclusions.

acknowledgments

The idea of writing a book of this kind came to me gradually, during the period between the Delphi meeting on *Mathematics and Narrative*, organized in 2007 by Apostolos Doxiadis and Barry Mazur, and the 2009 conference in Hatfield, England, on *Two Streams in the Philosophy of Mathematics*, organized by David Corfield and Brendan Larvor. All participants in both conferences encouraged me, directly or indirectly, consciously or not, to write this book, but without the consistent support in the early stages of the project of the four organizers of the two conferences and of Peter Galison, who was also at Delphi, this book would surely never have existed.

Several chapters are expanded versions of texts that have been published or presented elsewhere. An earlier and shorter version of chapter 5 was scheduled for presentation at the 2009 meeting of the Society for Literature, Science, and the Arts in Atlanta; I thank the organizers for accepting my proposal and Arkady Plotnitsky for presenting the paper in my place when a schedule conflict forced me to cancel. Chapter 7 is the third incarnation, much expanded, of a paper that was first presented at the 2009 Hatfield conference and again as a twenty-minute "After Hours Conversation" at the Institute for Advanced Study in 2011. I am grateful to Helmut Hofer and Piet Hut for making the 2011 version possible and to all the participants in both meetings for their generous attention and their challenging questions. The Hardy thread in chapter 10 originally appeared, in streamlined form, as a stand-alone essay in Issue 50 of *Tin House*, under the title *Unexpected, Economical, Inevitable*. Working with *Tin House* editor Cheston Knapp to get the article into shape, overcoming surprisingly deep differences in habits and expectations, was an immensely enjoyable and instructive experience, and it taught me lessons that I have tried to put to good use while writing this book.

The book began to take its present form began in the spring of 2011, while I was a Member of the Institute for Advanced Study in Princeton. The IAS has been one of the world's capitals of mathematical research since its founding in the 1930s, and it should inevitably figure prominently in any account of the culture of contemporary mathematics. If the IAS gets more than its strictly mathematical share of attention in this book, it's because I took advantage of (some of) the time I spent there to talk to scholars outside the School of Mathematics. I am grateful to Richard Taylor for inviting me to spend a term at the IAS and to then-director Peter Goddard for maintaining an environment conducive to this sort of interdisciplinary interaction.

I was fortunate to make use of the libraries at Princeton University and the IAS at the time and, later, the rich but scattered holdings in Paris in conjunction with preliminary searches on the Internet. Readers with access to the NSA's PRISM data center can probably reconstruct my Google searches, but even with the proper security clearance, current technology won't allow you to reconstruct the deep satisfaction I felt when, after finding a reference to an interesting item that had not yet been published or that was hidden behind a paywall, I was able to obtain the text in question from the author. There were many such authors, and practically all of them quickly responded to my requests. Invariably, the initial contact was continued with extended exchanges. Some authors were kind enough to look over the relevant chapters and make suggestions. Their names are listed below, alongside those of old friends and a great number of colleagues. Any mistakes that remain, of course, are my responsibility alone.

I thank Don Blasius for providing photographs of the array of mathematicians on the walls of the UCLA mathematics department, one of which was used in figure 2.1. Catherine Goldstein was kind enough to allow me to reproduce her father's drawing of the *Formule de l'amour prodigieux*, which appears here as figure 6.4. Alberto Arabia gave generously of his time to help me contact the officials of the Museo Nacional de Bellas Artes in Buenos Aires responsible for authorizing the reproduction of Luca Giordano's *Un matemático* (figure 8.2). I also thank Monica Petracci for permitting the inclusion of stills from *Riducimi in forma canonica* (figure 6.6) and William Stein for creating the Sato-Tate graph (figure δ.3).

Because the choice of wording or of individual words has been crucial in orienting the perception of mathematics by outsiders as well as practictioners, I owe special thanks to Alexey Nikolayevich Parshin, Marwan Rashed, Yuri Tschinkel, and Yang Xiao, who helped me decide

PART I

chapter 1

Introduction: The Veil

Who of us would not be glad to lift the veil behind which the future lies hidden; to cast a glance at the next advances of our science and at the secrets of its development during future centuries? What particular goals will there be toward which the leading mathematical spirits of coming generations will strive? What new methods and new facts in the wide and rich field of mathematical thought will the new centuries disclose?

—David Hilbert, Paris 1900

The next sentence of Hilbert's famous lecture at the Paris International Congress of Mathematicians (ICM), in which he proposed twenty-three problems to guide research in the dawning century, claims that "History teaches the continuity of the development of science."[1] We would still be glad to lift the veil, but we no longer believe in continuity. And we may no longer be sure that it's enough to lift a veil to make our goals clear to ourselves, much less to outsiders.

The standard wisdom is now that sciences undergo periodic ruptures so thorough that the generations of scientists on either side of the break express themselves in mutually incomprehensible languages. In the most familiar version of this thesis, outlined in T. S. Kuhn's *Structure of Scientific Revolutions*, the languages are called *paradigms*. Historians of science have puzzled over the relevance of Kuhn's framework to mathematics.[2] It's not as though mathematicians were unfamiliar with change. Kuhn had already pointed out that "Even in the mathematical sciences there are also theoretical problems of paradigm articulation."[3] Writing in 1891, shortly before the paradoxes in Cantor's set theory provoked a *Foundations Crisis* that took several decades to sort out, Leopold Kronecker insisted that "with the richer development of a science the need arises to alter its underlying concepts and principles. In this respect mathematics is no different from the natural sciences: new phenomena [*neue Erscheinungen*] overturn the old hypotheses and put others in their place."[4] And the new concepts often meet with resistance: the great Carl Ludwig Siegel thought he saw "a *pig* broken into a beautiful *garden* and rooting up all flowers and trees"[5] when a subject he had done so much to create in the 1920s was reworked in the 1960s.

Nevertheless, one might suppose pure mathematics to be relatively immune to revolutionary paradigm shift because, unlike the natural sciences, mathematics is not *about* anything and, therefore, does not really have to adjust to accommodate new discoveries. Kronecker's *neue Erscheinungen* are the unforeseen implications of our hypotheses, and if we don't like them, we are free to alter either our hypotheses or our sense of the acceptable. This is one way to understand Cantor's famous dictum that "the essence of mathematics lies in its freedom."

It's a matter of personal philosophy whether one sees the result of this freedom as evolution or revolution. For historian Jeremy Gray, it's part of the *professional autonomy* that characterizes what he calls *modernism* in mathematics; the imaginations of premodern mathematicians were constrained by preconceptions about the relations between mathematics and philosophy or the physical sciences:

> Without . . . professional autonomy the modernist shift could not have taken place. Modernism in mathematics is the appropriate ideology, the appropriate rationalization or overview of the enterprise. . . . it became the mainstream view because it articulated very well the new situation that mathematicians found themselves in.[6]

This "new situation" involved both the incorporation of mathematics within the structure of the modern research university—the creation of an international community of professional mathematicians—and new attitudes to the subject matter and objectives of mathematics. The new form and the new content appeared at roughly the same time and have persisted with little change, in spite of the dramatic expansion of mathematics and of universities in general in the second half of the twentieth century.

Insofar as the present book is about anything, it is about how it feels to live a mathematician's double life: one life within this framework of professional autonomy, answerable only to our colleagues, and the other life in the world at large. It's so hard to explain *what* we do—as David Mumford, one of my former teachers, put it, "I am accustomed, as a professional mathematician, to living in a sort of vacuum, surrounded by people who declare with an odd sort of pride that they are mathematically illiterate"[7]—that when, on rare occasions, we make the attempt, we wind up so frustrated at having left our interlocutor unconvinced, or at the gross misrepresentations to which we have resorted, or usually both at once, that we leave the next questions unasked: What *are* our goals? *Why* do we do it?

But sometimes we do get to the "why" question, and the reasons we usually advance are of three sorts. Two of them are obviously wrong. Mathematics is routinely justified either because of its fruitfulness for practical applications or because of its unique capacity to demonstrate truths not subject to doubt, *apodictically* certain (to revive a word Kant borrowed from Aristotle). Whatever the merits of these arguments, they are not credible as motivations for what's called *pure mathematics*—mathematics, that is, not designed to solve a specific range of practical problems—since the motivations come from outside mathematics and the justifications proposed imply that (pure) mathematicians are either failed engineers or failed philosophers. Instead, the motivation usually acknowledged is *aesthetic*, that mathematicians are seekers of beauty, that mathematics is in fact art as much as science, or that it is even more art than science. The classic statement of this motivation, due to G. H. Hardy, will be reviewed in the final chapter. Mathematics defended in this way is obviously open to the charge of sterility and self-indulgence, tolerated only because of those practical applications (such as radar, electronic computing, cryptography for e-commerce, and image compression, not to mention control of guided missiles, data mining, or options pricing) and because, for the time being at least, universities still need mathematicians to train authentically useful citizens.

There are new strains on this situation of tolerance. The economic crisis that began in 2008 arrived against the background of a global trend of importing methods of corporate governance into university administration and of attempting to foster an "entreprenurial mindset" among researchers in all potentially useful academic fields. The markets for apodictically certain truths or for inputs to the so-called knowledge

G. H. Hardy's successor at Cambridge to the Sadleirian Chair of Pure Mathematics; near the University of Pennsylvania mathematics library, where I did research for a high school project; and through all the steps of my undergraduate and graduate education. The poster was ubiquitous and certainly seemed timeless to my adolescent mind but had, in fact, been created only two years earlier by IBM. Its title alludes to Eric Temple Bell's *Men of Mathematics*, the lively but unreliable collection of biographies that served as motivational reading that summer at Temple. You will have noticed at least one problem with the title, and it's not only that one of the "men" in Bell's book and (a different) one on the IBM poster are, in fact, women. Whole books can and should be devoted to this problem, but for now let's just be grateful that something (though hardly enough) has been learned since 1968 and move on to the topic of this chapter: the contours and the hierarchical structure of what I did not yet know would be my chosen profession when I first saw that poster.

It was designed, according to Wikipedia, by the "famous California designer team of Charles Eames and his wife Ray Eames," with the "mathematical items" prepared by UCLA Professor Raymond Redheffer."[2] Each "man" is framed by a rectangle, with a portrait occupying the left-hand side, a black band running along the top with name and dates and places of scientific activity, and Redheffer's capsule scientific biography filling the rest of the space, stretched to the length of "his" lifespan. As my education progressed, I began to understand the biographies, but at the time most of the names were unfamiliar to me. With E. T. Bell's help, we learned some of the more entertaining or pathetic stories attached to these names. That's when I first heard not only about the work of Nils Henrik Abel and Evariste Galois (see figure 2.1) in connection with the impossibility of trisecting the angle and with the problem of solving polynomial equations of degree 5—they both showed there is no *formula* for the roots—but also how they both died at tragically young ages, ostensibly[3] through the neglect of Augustin-Louis Cauchy, acting as referee for the French *Académie des Sciences*. What surprised me was that Abel and Galois both had portraits and biographies of standard size, while Cauchy, of whose work I knew nothing at the time, belonged to the very select company of nine Men of Mathematics entitled to supersized entries. The other eight were (I recite from memory)

Figure 2.1. Portraits of eminent mathematicians on the wall of the UCLA mathematics department, with Galois in front. (Photo Don Blasius)

Pierre de Fermat, Sir Isaac Newton, Leonhard Euler, Joseph-Louis Lagrange, Carl Friedrich Gauss (the "Prince of Mathematicians"), Bernhard Riemann, Henri Poincaré, and David Hilbert.

Maybe Bell's book and the IBM poster should have been entitled *Giants of Mathematics*,[4] with a special category of *Supergiants*, including the nine just mentioned plus Archimedes and a few others from antiquity (the poster's timeline starts in AD 1000). The hierarchy admitted additional refinements, the Temple professors told us. It was generally agreed—the judgment goes back at least to Felix Klein, if not to Gauss himself—that Archimedes, Newton, and Gauss were the three greatest mathematicians of all time. And who among those three was the very greatest, we asked? One of our professors voted for Newton; the others invited us to make up our own minds.

> The field of mathematics has a natural hierarchy. Mathematicians generally work on research problems. There are problems and then there are hard problems. Mathematicians look to publish their work in journals. There are good journals and there are great journals. Mathematicians look to get academic jobs. There are good jobs and great jobs. . . . It is hard to do mathematics and not care about what your standing is.
>
> In Wall Street every year bonus numbers come out, promotions are made and people are reminded of where they stand. In mathematics, it is no different. . . . Even in graduate school, I found that everyone was trying to see where they stood.[5]

That's hedge-fund manager Neil Chriss, explaining why he quit mathematics for Wall Street. But his analogy between finance and mathematics doesn't quite hold up. For mathematicians, the fundamental comparisons are with those pictures on the wall. "To enter into a practice," according to moral philosopher Alasdair MacIntyre, "is to enter into a relationship not only with its contemporary practitioners, but also with those who have preceded us. . . ."[6] Adam Smith, writing in the eighteenth century, found these relations harmonious:

> Mathematicians and natural philosophers . . . live in good harmony with one another, are the friends of one another's reputation, enter into no intrigue in order to secure the public applause, but are pleased when their works are approved of, without being either much vexed or very angry when they are neglected.[7]

Two centuries later, one meets a more varied range of personality types:

> In the 1950's there was a math department Christmas party at the University of Chicago. Many distinguished mathematicians were present, including André Weil. . . . For amusement, the gathered company endeavored to . . . list . . . the ten greatest living mathematicians, present company excluded. Weil, however, insisted on being included in the consideration.
>
> The company then turned to the . . . list of the ten greatest mathematicians of all time. Weil again insisted on being included.

Weil soon moved to the Institute for Advanced Study (IAS) in Princeton, and when, in the mid-1970s, a Princeton University graduate student asked him to name the greatest twentieth-century mathematician, "the answer (without hesitation) was 'Carl Ludwig Siegel.' " Asked next to name the century's second-greatest mathematician, he "just smiled and proceeded to polish his fingernails on his lapel."[8] Fifteen years later my colleagues in Moscow proposed a different ranking: A. N. Kolmogorov was by consensus the greatest mathematician of the twentieth century, with a plurality supporting Alexander Grothendieck for the second spot.

Hierarchy and snobbery are, naturally, not specific to mathematics. "Democracy should be used only where it is in place," wrote Max Weber in the 1920s. "Scientific training . . . is the affair of an intellectual aristocracy, and we should not hide this from ourselves."[9] In the nineteenth century, Harvard professor Benjamin Peirce, perhaps the first American

Figure 2.2. "Word Cloud" of twentieth-century mathematicians. Generated by José Figueroa-O'Farrill for the StackExchange Web site MathOverflow, based on frequency of references in (Gowers et al. 2008). See http://mathoverflow net/questions/10103/great-mathematicians-born-1850-1920-et-bells-book-x-fields-medalists/10105#10105. Weil makes a respectable showing on the left, but Kolmogorov is barely visible on the right between Minkowski and Banach. Note the prominence of Einstein, who was not a mathematician.

mathematician to enjoy an international reputation, could "cast himself . . . as the enemy of sentimental egalitarianism . . . a pure meritocrat with no democracy about him."[10] Nowadays, of course, mathematicians are no less committed to democracy than the rest of our university colleagues. But we do seem peculiarly obsessed with ordered lists.[11] A lively exchange in 2009 on the collective blog MathOverflow aimed at filling in the gap between the last Giants of Bell's book and the winners in 1950 of the first postwar *Fields Medal*s, awarded every four years to distinguished mathematicians under 40 and still the most prestigious of mathematical honors. The discussion generated several overlapping lists of "great mathematicians born 1850–1920"[12] and at least one novel graphic representation, as shown in figure 2.2.

The MathOverflowers—who mainly treat the blog as a forum for exchanging technical questions and answers—favored the word *romanticizable* to qualify candidates for inclusion in the list. You read that right: mathematicians are not only individually fit subjects for romantic idealization—we'll see a lot of that in chapter 6—but romanticizable *collectively*, as befits the theme of this chapter. This may seem odd if you haven't read Bell's book and even odder if you have run through the list of Fields medalists—or winners of prizes created more recently, such as the *Wolf Prize* (since 1978), the *Abel Prize* (administered since 2003 by the Norwegian Academy of Science and Letters as an explicit substitute for the missing Nobel Prize in mathematics), or the *Shaw Prize* (the "Nobel of the East," awarded in Hong Kong since 2004).[13] Few prizewinners are obvious candidates for biographical treatment in Bell's romantic vein, but the same could have been said of the majority of Bell's subjects before he got hold of them. Leaving aside the tragic cases of Galois and Abel (after whom the prize is named), perhaps the most authentically romantic figure in Bell's book was Sofia Kovalevskaia—"scientist, writer, revolutionary" is the subtitle of one of her biographies—the first woman to receive a PhD in mathematics; she traveled to Paris to witness the Commune of 1871 and devoted herself as energetically to prose and political activism as to her work on differential equations. Among prizewinners, Fields medalist Laurent Schwartz (1950) surely qualifies as romantic for his long and courageous commitment to human rights; so do Fields medalists Grothendieck (1966) and Grigori Perelman (2006) and (Wolf Prize winner) Paul Erdős, for reasons we will explore at length.[14] And Cédric Villani, with his "romantic" dress code, has been a fixture of Paris talk shows since he won the Fields Medal in 2010 (he's on the radio again as I write this, talking about ideal gases and how most efficiently to transport croissants to cafés). As for the others, their native romanticism has found no public outlet. This is not an accident.

Weber, writing a few years before Bell, had a romantic vision of science: "Without this strange intoxication, ridiculed by every outsider; without this passion . . . you have no calling for science and you should do something else." Did this change after the war? Not for impressionable teenagers, in any case. The romance of a vocation and the mystique of hierarchy intertwined when the undergraduate adviser received me, a first-year student still under the spell of a recent encounter with Cauchy's residue formula, in one of the alcoves of the Princeton mathematics department common room. His deep voice and distant demeanor made me think of fate incarnate.[15] We talked about course requirements and such, but my only distinct memory of the conversation is that at one point he said, "You want to be the world's greatest mathematician"; my

The word *charisma* colloquially means a kind of personal magnetism, often mixed with glamour, but Weber chose the word to designate the quality endowing its bearer with *authority* (*Herrschaft*, also translated *domination*) that is neither *traditional* nor *rational* (legally prescribed). Charisma is (in the first place) "a certain quality of an individual personality, by virtue of which he is set apart from ordinary men and treated as endowed with supernatural, superhuman, or at least specifically exceptional powers or qualities," whose legitimacy is based on "the conception that it is the duty of those subject to charismatic authority to recognize its genuineness and to act accordingly."[21]

For mathematicians "acting accordingly" consists in participation in a *research program*. For example, the *Langlands program*, established by Robert P. Langlands—one of the great research programs of our time, very much in the way this term was understood by Imre Lakatos[22]—benefited from its founder's meticulous elaboration of the program's ultimate goals, too distant to serve as more than motivation, as well as a remarkably precise vision of accessible intermediate goals and the steps needed to attain them. In both these respects, the Langlands program, developed in several stages during the 1970s and thus part of my generation's collective memory, resembled the program promoted by Alexander Grothendieck in the 1960s, whose ultimate goal was to realize the implications of Weil's ideas about fixed point theorems.

It's for its subjective, romantic, and not altogether rational associations that I prefer charisma to words like *prestige*, *status*, *standing*, or *visibility* currently in use in the sociology of science.[23] Weber's primary target was religious leaders—Jesus, Mohammed, or the Buddha were extreme cases—but even mathematical charisma does not derive from "objective" external considerations. After Weil made his "topological" insight work for algebraic curves—a ten-year undertaking that required a complete rethinking of what he called the *Foundations of Algebraic Geometry*, the title of the most elaborate of the three books he wrote for the project—he formulated in three conjectures the outlines of a new geometry that would place topology at the center of number theory. For the next twenty-five years, the Weil conjectures served as a focus of charisma, what I would like to call a *guiding problem* in number theory and algebraic geometry, a challenge to specialists and a test of the rightness of their perspective. Grothendieck, who called them his "principal source of inspiration"[24] during his most active period of research, was the first to find a geometry that met Weil's specifications, proving all but one of Weil's conjectures and reorienting much of mathematics along the way, number theory in particular, before withdrawing from active mathematical research.

> Charisma . . . is imputed to persons, actions, roles, institutions, symbols, and material objects because of their presumed connection with "ultimate," "fundamental," "vital," order-determining powers. This presumed connection with the ultimately "serious" elements in the universe and in human life is seen as a quality or a state of being, manifested in the bearing or demeanor and in the actions of individual persons; it is also seen as inhering in certain roles and collectivities.
>
> What is alone important is how the individual is actually regarded by those subject to charismatic authority, by his "followers" or "disciples."[25]

Of course some mathematicians possess charisma in the colloquial sense as well as charismatic authority within the field.[26] Grothendieck was by all accounts such an individual. His close colleague, the French mathematician Jean-Pierre Serre, who received the Fields Medal in 1954, has for more than sixty years been one of the world's most influential mathematicians. In addition to his original research that literally reshaped at least four central branches of mathematics, he is an exceptionally gifted lecturer and an incomparable clarifier, whose books have been required reading since before I was a graduate student. The *Matthew Effect*, sociologist Robert K. Merton's name for the familiar tendency of prizes and honors to accrue to those who have already been honored,[27] works both ways: by awarding him the first Abel Prize in 2003, the Norwegian Academy borrowed Serre's charisma to secure their own legitimacy and to confirm the new prize's compatibility with community norms.

Serre is known for his effortless personal charm,[28] and in France, where politicians routinely point to the many French Fields Medalists as a mark of national glory, he could easily have become a media favorite. But he has limited his field of action to mathematics, apparently agreeing with Weber that the scientist who "steps upon the stage and seeks to legitimate himself through 'experience,' asking: How can I prove that I am something other than a mere 'specialist' . . . always makes a petty impression." It "debases the one who is thus concerned. Instead of this, an inner devotion to the task, and that alone, should lift the scientist to the height and dignity of the subject he pretends to serve."[29]

Cambridge professor Sir Timothy Gowers (Fields Medal 1998, knighted in 2012) is not the sort to "step upon the stage," but unlike Serre, he has chosen to devote time and energy to exploring mathematical communication in a variety of forms. For professional mathematicians and the mathematically informed, he edited the encyclopedic *Princeton Companion to Mathematics* and runs a popular blog that covers topics of broad interest; for specialists he has pioneered "massively collaborative mathematics" online; and for the general public he has written a *Very Short Introduction to Mathematics*. He's very good at communication—he was chosen to give one of the talks in Paris at the meeting organized by the Clay Mathematics Institute for the 100th anniversary of Hilbert's 1900 Paris lecture—and he has also shown considerable courage, since it's commonly felt, at least in English-speaking countries, that a mathematician who willingly spends time on anything other than research must be short of ideas.[30]

"In every age," according to Northrop Frye, "the ruling social or intellectual class tends to project its ideals in some form of Romance." Class struggle is a poor guide to the history of mathematics;[31] it's more accurate to say that the romantic ideal is projected onto charismatic figures and any "ruling" is as likely as not to be posthumous. What Weber called the *routinization of charisma* helps explain why mathematical romanticism is mainly to be sought in a legendary (nineteenth-century) past. A professional mathematician will regularly enjoy the privilege of consorting, consulting, lunching, and even partying with Giants of Mathematics. This is consistent with the dynamics of the routinization of charisma, which does not necessarily divide mathematics socially into segregated Weberian "status groups" (*Stände*): "The very effort of a charismatic elite to stabilize its position and to impose a charismatic order on the society or institution it controls entails . . . spreading the particular charismatic sensitivity to persons who did not share it previously. This means a considerable extension of the circle of charisma."[32] If we ignore charged verbs such as impose and controls that are misleading in the mathematical context, this sentence helps to understand how charisma can propagate from the "specifically exceptional" individuals to those (like the author of these lines) who occupy "certain roles" or participate in "collectivities."

"The charismatic leader," writes Pierre Bourdieu, "manages to be for the group what he is for himself, instead of being for himself, like those dominated in the symbolic struggle, what he is for others. He 'makes' the opinion which makes him; he constitutes himself as an absolute by a manipulation of symbolic power which is constitutive of his power since it enables him to produce and impose his own objectification." It may sound as if Bourdieu is saying that the charismatic leader, and the charismatic academic, in particular, exercises political power over the group. Whether or not this applies to mathematicians, it's not what I have in mind.[33] The bearer of mathematical charisma, consistently with Bourdieu's model, contributes to producing the objectification—the reality—of the discipline, in the process producing or imposing the objectification of his or her own position within the discipline. The reader can judge whether or not this is compatible with democracy—more material will be provided as the chapter progresses—but I want to stress that there is nothing deliberately misleading or sinister or even mysterious about this process, which is manifested in practice as well as in the perception of the field as a whole:

> In mathematics, many details of a proof are omitted because they are considered obvious. But what is "obvious" in a given subject evolves through time. It is the result of an implicit agreement between the reseachers based on their knowledge and experience. A mathematical theory is a social construction.[34]

The symbolic infrastructure of mathematical charisma is, likewise, a social construction, the result of an implicit agreement. But it is also the "objectification" of mathematics: the common object to which researchers refer, which in turn drives the evolution of aspects of mathematical discourse like the "obviousness" just cited. Does this mean that mathematicians can share only an understanding of mathematical theories and discoveries associated with charismatic individuals? No, but insofar as a mathematical theory is a social construction, one measures the importance of one's own contributions in terms of an accepted scale of values, which in turn is how charisma is qualified.

In other words, it's not just a theory's contents that are defined by a social understanding: so are the value judgments that organize these contents. Caroline Ehrhardt captures this process well in her account of the construction of *Galois theory*, decades after Galois' tragic and romantic death (my emphasis added to highlight effects of charisma):

> [W]riters of university textbooks not only played the passive role of collectors of research ideas . . . they also **created** mathematical knowledge in that when they introduced students to Galois theory, they offered **an organisation and a hierarchy of its constitutive elements** which were anything but established within the initial, fragmented landscape of local memories. In this way, these authors **structured the mathematical field**; they **redistributed symbolic capital between the authors**, they **defined which objects are legitimate**, which orientations took precedence, and they enabled the constitution of a community of specialists who had received the same kind of training. Mathematical content and practice thus **defined the social space** corresponding to Galois theory at the end of the 19th century within the mathematical field.[35]

Galois, you'll recall, showed that there is no formula for finding the root of a polynomial equation of degree 5 or more, like the one shown in figure 2.4. So did Abel. But Galois, before he died in a duel at the age of 20, did much more for equations: he invented a *method* for understanding their roots, even in the absence of a formula—the *Galois group*, which governs what, for want of a more precise term, one would now call the *structure* of the roots of an equation. Together with his successors who "defined the social space" of Galois theory, he also created a new *point of view*: that what's interesting is no longer the centuries-old goal of finding a root of the equation, but rather to understand the structure of all the roots—the Galois group. This is a stage in an ongoing process of abstraction; it is also a change in perspective. Today's mathematicians, especially number theorists, have taken this process one step further: rather than focus on the Galois group of a single equation, number theorists look at all the Galois groups of all equations simultaneously as a single structure. Is this a paradigm shift or a priority shift?

brothers argued that "there is substantial overlap in the groups having power and those having prestige" (charisma in my sense). The two overlapping groups are the "scientists who have earned recognition for their outstanding contributions to knowledge and those who hold key administrative positions." The first group "extends the circle of charisma" by distributing what Bourdieu calls "symbolic capital": Chriss's "great jobs," publication in "great journals". . . ; the second group distributes material resources ("[t]hey can determine what specific research areas are to receive priority, and what individuals are to receive support for their research programs").[42]

Let's leave the second group to professional sociologists and focus on the first group. A mathematician derives *authority* from being an *author*—the two words have the same root—but if one asks, with Michel Foucault, "What is an author?" one begins to get a sense of how peculiar the constellation of power around a charismatic mathematician looks, compared to the authorial aura of a charismatic scientist. Unlike articles in particle physics or biomedicine that can be signed by a thousand authors or contributors, it's unusual for articles in pure mathematics to have more than two or three authors, and single-author articles are common. Mathematics research, as opposed to mathematics publishing, is, nevertheless, intensely collaborative, and breakthroughs are always prepared by years of preliminary work, whose authors are usually given full credit by specialists and are quickly forgotten by nonspecialists. This *communalism*, in Merton's sense, is especially relevant in highly visible research programs like those of Langlands and Grothendieck. Langlands himself has observed that

> when the theorem in which the solution is formulated is a result of cumulative efforts by several mathematicians over decades, even over centuries . . . and when there may have been considerable effort—the more famous the problem the more intense—in the last stages, it is not easy, even for those with considerable understanding of the topic, to determine whose imagination and whose mathematical power were critical.

Langlands contrasts those responsible for "the novelty and insight of the solution" to mathematicians "whose contributions were presented with more aplomb and at a more auspicious moment."[43] So the question of how credit—and, therefore, authority—are apportioned has real consequences, and it is a shame that its sociological and philosophical underpinnings are so poorly understood. Foucault has left a hint. Alongside a mathematical treatise's historically determinate author and the "I" who serves as the subject of the proofs, with whom the reader identifies by accepting the rules in force, Foucault alludes to a "third self, one that speaks to tell the work's meaning, the obstacles encountered, the results obtained, and the remaining problems . . . situated in the field of already existing or yet-to-appear mathematical discourses."[44] This authorial self places each new work in one or more of the discourses instituted by those figures peering down from the (metaphorical) wall. Highly charismatic mathematical authors have a more fundamental responsibility: not only are they in dialogue with the portraits on the wall, whether or not they have a scholarly interest in the details of history; they—and not "nature" —mediate this dialogue for the rest of us, including those who contribute to their research programs.

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The way a graduate student, barely past 20, chooses a thesis adviser, and, in so doing, nearly always determines a permanent career orientation, has always fascinated me. Is the choice based on an ineffable preexisting harmony or is it true, as Pascal thought, that "chance decides"? Did I ask Barry Mazur to supervise my Harvard PhD because I admired his research style that unites methods and insights in novel and unexpected combinations or because he was (and still is) one of the few people I've met who can without hesitation speak engagingly and cogently on practically any topic—because of his personal charisma in the familiar sense? Does it even make sense to separate his personal style from his research style? However I made my choice, I soon found myself caught up in the thrill of the first encounter between two research programs, each of a scope and precision that would have been inconceivable to previous generations, each based on radically new heuristics, each experienced by my teachers' generation as a paradigm shift. Hilbert, whose quotation opens the previous chapter, pioneered the conception of mathematics as a practice oriented to the future, whose meaning is defined less by what we can prove than by what we expect to be able to prove.[45] But nothing in Hilbert's list of twenty-three problems could compare with the detailed predictions of Robert Langlands's program for number theory in the light of *automorphic forms* or of the conjectures deriving from Alexander Grothendieck's hypothetical theory of *motives*, both on the brink of spectacular expansion when I entered the field, both too complicated to do more than shadow my narrative.

The IHÉS (Institut des Hautes-Études Scientifiques) was created outside Paris in conscious imitation, on a smaller scale, of Princeton's IAS; Grothendieck was one of the first permanent professors. His IHES seminars in the 1960s, collected in a series of volumes known as SGA—*Séminaire de géométrie algébrique*, or Seminar of Algebraic Geometry[46]—were both symbolically and practically the founding acts of a school of experts, mainly Grothendieck's own students, who wrote most of the text and continued the research program after the leader's hermetic withdrawal from active research in 1972. Langlands, who joined the IAS as Hermann Weyl Professor that same year, promoted his unifying vision for automorphic forms and number theory for two decades, serving as the focus of research for widening circles of specialists before taking a break to think about physics; over the past fifteen years, he has returned to his guiding role with a "reckless" new approach to his conjectures.

Steven Shapin points out in *Scientific Life* that the word *charisma* is "the word used by both participants and commentators" to describe the "personally embodied leadership" exercised by scientific entrepreneurs. Quoting the economist Richard Langlois, Shapin argues that "charismatic authority 'solves a coordination problem' . . . in circumstances of radical normative uncertainty" and that the use of the word *charisma* in this way "is a consequential, reality-making usage."[47] Leaders of mathematical research programs, such as Grothendieck and Langlands, exercise their charismatic authority in a very similar way, even in the absence of entrepreneurial incentives.

Langlands and Grothendieck are both (at least) Giants by any measure, and both were consciously successors of Galois. In his attempt to prove the last of Weil's conjectures—solved instead by his most brilliant student, 1978 Fields medalist and IAS Professor Pierre Deligne—Grothendieck imagined a theory of elementary particles of algebraic geometry, called *motives*. The theory, still far from complete, represents one vision of a Galois theory of algebraic geometry, the geometry of equations. The Langlands program is another vision of Galois theory: his

Figure 2.5. Grothendieck lecturing at the Séminaire de Géométrie Algébrique (SGA), IHES, 1962–1964.

unexpected insight was that the structure of the theory of *automorphic forms*, rooted in a different geometry, the geometry of mechanics, is in large part determined by Galois' theory of polynomial equations. In a famous article entitled *Ein Märchen*—a fairy tale—Langlands speculates about a reformulation of number theory, also still incomplete, in terms of elementary particles (*automorphic representations*) that mirror Grothendieck's (hypothetical) motives but include additional mysterious particles that—unlike motives—the Galois group is not equipped to detect.[48]

Grothendieck, whose relations to the material world (and to his colleagues) are far from straightforward (see chapter 7 and his underground memoirs, *Récoltes et Sémailles*), may well qualify as the last century's most romantic mathematician; his life story begs for fictional treatment. Langlands' life has been by no means as extravagant as Grothendieck's, but his romanticism is evident to anyone who reads his prose; the audacity of his program, one of the most elaborate syntheses of conjectures and theorems ever undertaken, has few equivalents in any field of scholarship. While neither of them has ever "ruled" in Frye's sense, their research programs, like Hilbert's problems, typify charisma by focusing attention and establishing standards of value for specialists. They also provide a way for colleagues in other branches of mathematics to judge the importance of someone's research: proving a central conjecture of a named research program often suffices to establish one's charisma in the eyes of such colleagues, who may only dimly apprehend the program as a whole. Three of the seven *Clay Millennium Problems*, announced at the Clay meeting in Paris in 2000 by an all-star roster of speakers, including Gowers as well as John Tate (Abel Prize 2010) and Sir Michael Atiyah (Fields Medal 1966, Abel Prize 2004), in a conscious echo of Hilbert's problems—but each offering a million-dollar reward for its solution—predate but are at least loosely connected with one or both of these research programs.

Number theorists began building bridges back from the Langlands program to motives in connection with problems like the *Conjecture of Birch and Swinnerton-Dyer* (BSD), one of the three Clay problems related to number theory,[49] which served as the guiding problem for my own entry into research. Just as Weil's conjectures were about counting solutions to equations in a situation where the number of solutions is known to be finite, the BSD conjecture concerns the simplest class of polynomial equations—*elliptic curves*—for which there is no simple way to decide whether the number of solutions is finite or infinite. Here are the equations of two elliptic curves:

$$y^2 = x^3 - x, \tag{E1}$$

$$y^2 = x^3 - 25x. \tag{E5}$$

We will see these curves again in chapters γ and δ, along with a whole family labeled EN (one for every number N). It can be shown (but not in a simple way) that equation (E1) has only finitely many solutions, while (E5) has infinitely many solutions.

to function effectively within the routine at the level of the hierarchy to which I was soon successfully appointed.

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The Cole brothers' use of citation indices to quantify *visibility* and *influence* is characteristic of the Mertonian school that dominates what, taking a hint from philosopher David Corfield, one might call the *encyclopedic* tendency in sociology of science. Corfield borrowed the term to adapt Alasdair MacIntyre's tripartite division of "moral inquiry" to the philosophy of mathematics. Alongside the encyclopedic approach, associated with various strains of positivism, MacIntyre places the *genealogical* (inspired by Nietzche or Foucault but often conflated with historical relativism) and the *tradition-based*, which he and Corfield favor in their respective fields. Sociology of science can be similarly divided. Encyclopedic sociologists of science (to oversimplify) follow their philosophical counterparts in leaving unexamined the assumptions of practitioners—or the assumptions philosophers attribute to the latter—and concentrate on factors they are prepared to consider objective, which in practice means quantifiable. For the genealogists, on the other hand, specifically those associated with the *Sociology of Scientific Knowledge* (SSK) program, everything practitioners say is suspect.[54] David Bloor, for example, has written that "[t]he unique, compelling character of mathematics is part of the phenomenology of that subject. An account of the nature of mathematics is not duty-bound to affirm these appearances as truths, but it is bound to explain them as appearances." Bloor continues, "It is a notable characteristic of some philosophies of mathematics"—he has the encyclopedic approach in mind—"that they uncritically take over the phenomenological data and turn them into a metaphysics." This implies that "there can be no sociology of mathematics in the sense of the strong programme. What is required is a more critical and naturalistic approach."[55]

Appearances notwithstanding, my intention in this chapter is not to indulge in amateur sociologizing; but it's true that the questions I've raised are among the topics I'd like to see covered in a sociology of mathematics—a sociology of *meaning* as opposed to encyclopedists' exclusive emphasis on the implementation of preexisting consensual norms or the genealogists' focus on effects of power. Questions of meaning fit best with the tradition-based perspective—or so a participant in the tradition might be expected to say—but the sociology (as opposed to philosophy) of mathematics doesn't seem to have a tradition-based wing. I find it striking that among the philosophers and the (rare) sociologists who write about the subject, neither encyclopedists nor genealogists actually have much to say about mathematics. Instead, mathematics tends to serve as an excuse either to promote—or, as in the previous Bloor quotation, to strike a blow against—a questionable ideology, promoted by certain philosophers, most long dead.

(Controversies among philosophers, especially encyclopedist philosophers, surrounding the word *meaning*, which has just made its third appearance in this chapter and which will reappear repeatedly throughout the book, are as lively as they get. My intention in introducing the word is not to enter into these controversies, but rather to give the reader a sense of how it is used by pure mathematicians, even if the reader happens to be a philosopher. One theme of chapter 7 is that this use is fraught with paradox—which doesn't make it less meaningful.)

The sociological texts I've seen specifically devoted to mathematics mainly belong to the genealogical tendency,[56] but with few exceptions the encyclopedists and genealogists are united by their grounding in what I will be calling philosophy of Mathematics.[57] Capital-*M* Mathematics is a purely hypothetical subject invented by philosophers to address (for example) problems of truth and reference, which these days presupposes an outsized concern for logical formalization, with very little attention paid to what matters to mathematicians. The encyclopedic and genealogical tendencies are for the most part variants of the philosophy of Mathematics, the main difference being their differing degree of credulity versus skepticism regarding truth claims.[58] Philosophy of (small-*m*) mathematics, on the other hand, takes as its subject of inquiry the activity of mathematicians and other mathematically motivated humans—the basis, one would have thought, of what Bloor calls the "phenomenological data."[59]

Genealogists are especially concerned with the achievement of *epistemic consensus*, how mathematicians come to agree on the truth despite the paradoxes and antinomies that plague reason, but ignore *charismatic consensus*, whereas the encyclopedists treat the latter as a functional question, without looking into content. Christian Greiffenhagen, a sociologist trained in the SSK perspective, observes in a literature review that, thirty years after Bloor wrote the words quoted earlier, "There is almost a complete absence of anthropological or sociological studies of professional mathematics." By this he means there have been few "laboratory studies" of "science in the making" of the sort pioneered by SSK, and he proposes explanations as to why this might be the case. His own "video ethnography" of "situations in which mathematical competence is accountably visible," concerned as it is with mathematical communication, necessarily addresses questions of meaning.[60]

As for the third tendency, when Corfield and MacIntyre use the word *tradition* they mean it to be read descriptively; MacIntyre makes a point of divesting it of its associations with (traditional) conservatism. Within the tradition-based perspective, Corfield emphasizes the meaning attached to *value* rather than *truth*, to ethics rather than epistemology: "[I]f we wish to treat the vital decisions of mathematicians as to how to direct their own and others' research as more than mere preferences," he writes—in other words, if we want to treat these decisions as following from "rational considerations"—then value has to be admitted as a matter for rational analysis. "[I]t is the notion of progress toward a *telos*"—a goal—"that distinguishes genealogy and tradition."[61]

An example may help illustrate how a sociology of meaning can illuminate what Corfield means by *telos*. If Langlands was alone with a few of his friends and associates during his program's Act 1, extending, refining, and revising the outlines of his conjectures through calculations and a series of letters, Act 2 mobilized an international cast to verify these conjectures in a restricted situation called *endoscopy*, where Langlands thought his preferred technique would suffice if elaborated with sufficient diligence and attention.[62] Twenty years of intense work completed most of the steps required to bring Act 2 to the conclusion Langlands had marked out as an intermediate *telos*. But one vexing bottleneck remained, a series of calculations of the form

$$\text{quantity } G = \text{quantity } H \qquad \text{(FL1)}$$

conjectured by Langlands and Diana Shelstad and called the *fundamental lemma*. The earliest examples of (FL1) could be checked by fairly simple calculations of both sides of the equation, but the calculations quickly became tedious and then intractable, and by the year 2000 experts were convinced that the only sensible way to prove (FL1) was by deriving it from an identification of the form

$$\text{object } \mathscr{G} = \text{object } \mathscr{H} \qquad \text{(FL2)}$$

where $\mathscr{G}$ and $\mathscr{H}$ are objects in Grothendieck's geometry. Since Grothendieck's and Langlands' programs both emanate from Galois theory, it's not surprising that they overlap. Pierre Deligne, the Russian Vladimir Drinfel'd, and Langlands himself had been applying Grothendieck's methods to the questions raised in Act 1 since the 1970s, and two Fields Medals had been awarded for such work (to Drinfel'd in 1990 and to Laurent Lafforgue in 2002) by the time the Vietnamese mathematician Ngô Bảo Châu realized that the geometric objects needed for (FL2) could be built out of the repertoire of mathematical physics.[63] Here, by the way, Weil's and Grothendieck's original priorities are reversed: instead of using the results of counting to say something about the geometric objects—to pin them down as *motives*, for example—Grothendieck's methods provide the quantities of interest from the objects, and the identification (FL2) is used to avoid calculation of the complicated quantities in (FL1)—which brings Act 2 to a close, since in the applications all one needs to know is the equality in (FL1) and not the quantities themselves.

To explain why it was at this point generally agreed that Ngô's work was worthy of the Fields Medal—which he received in 2010—requires a vocabulary of value, of meaning, of *telos*. Had he devised the objects $\mathscr{G}$ and $\mathscr{H}$ and proved (FL2) without reference to the *telos* of the Langlands program—without the goal of deducing (FL1)—it would have been deep and difficult but unlikely to have attracted much notice; the Grothendieck tradition provides the means for proving any number of identifications like (FL2) but would not have recognized this one as furthering its specific goals. On the other hand, if Ngô had simply proved the experts wrong by displaying enough stamina to calculate the two sides of (FL1) directly—and before you set out to flaunt your own stamina I should warn you that (FL1) is shorthand for infinitely many separate calculations that would somehow have to be carried out simultaneously—he would have completed the *telos* of Act 2. Rapturous applause and offers of "great jobs" would have greeted the news. But Ngô would probably not have won the Fields Medal. This is because such a calculation has no *meaning* outside Langlands' endoscopy project, whereas (FL2)—precisely when it is seen against the background of the Langlands program—is the starting point for a new and open-ended research program (overlapping with Act 3 of the Langlands program, the "reckless" new initiative mentioned previously).

Ngô's prize-winning achievement was to create a *synoptic* proof, a proof that made the result "obvious" to those trained to see it, that served a specific *telos* within the overlapping traditions of the Grothendieck and Langlands research programs, and these programs link him both historically and conceptually to that poster on the wall and specifically to the figure of Galois. The danger of a tradition-based ethic is that it limits both understanding and participation to what David Pimm and Nathalie Sinclair call an "oligarchy" (Weber's "aristocracy"), whose rule, exercised "through the explicit notion of 'taste,'" excludes anyone outside the tradition. Asking "[I]n what sense . . . can mathematics be considered a democratic regime . . . " open to all, Pimm and Sinclair quote (Supergiant) Henri Poincaré to the effect that "only mathematicians are privy to the aesthetic sensibilities that enable" the decision of "what is worth studying."[64] The article, published in a journal for educators, is motivated by the "view that mathematics can do something for me in a humanistic sense that repays the careful attention and deep engagement it may require; one that may expose students to a fundamental sense and experience of equality . . . and provide them with another sense of human commonality."

The concern that mathematics is structured as a gated community to which only professional mathematicians are admitted calls to mind William Stanley Jevons's advocacy of "the better class of dance music, old English melodies, popular classical songs" as a vehicle for the moral improvement of nineteenth-century Britain's untutored working classes, who lacked the "long musical training" needed to appreciate "great musical structures," or the more recent defenders of elite standards against the encroachment of mass culture.[65] This is a debate mathematicians cannot afford to ignore, but this chapter is concerned only with a more specialized question: whether the hierarchy of values is compatible with democracy within the profession.

Rational authority in mathematics is vested not only in national (and in Europe supranational) administrative bodies reponsible for research "disruption" article in one of the discussions spawned by the boycott, Terence Tao—like Gowers, Tao hosts a wide-ranging and popular blog—suggested that the journal form remained necessary only for "validation (certifying correctness and significance of a paper) and designation (providing evidence of research achievement for the purposes of career advancement)." With regard to attempts to incorporate these functions into what some have called a "Math 2.0" framework, Tao wrote that "so far I don't see how to scale these efforts so that a typical maths research paper gets vetted at a comparable level to what a typical maths journal currently provides, without basically having the functional equivalent of a journal."[69]

The still unresolved case of Elsevier raises fascinating questions about the possibility of reconciling the goals of science with the material organization of society. A mathematical tradition unified around charismatic norms maintained by overlapping research programs hardly fits comfortably in the intellectual property regime monitored by the supervisors of globalization. What I find intriguing is the apparent consensus that publishing retains one indispensable function that can't be automated—the maintenance of the charismatic structure of the field. Given the preponderant role of informal communication even in the 1970s, long before the Internet made delivery of preprints instantaneous, the sociologist Bernard Gustin is convincing when he calls scientific publication a form of "Traditional Ritual."[70] "Validation"—the process of peer review—has two aspects in mathematics, as indicated before: "certifying correctness," the painstaking reading of an article, line by line if possible, to make sure all the arguments are valid; and certifying "significance," irrelevant to the "Mathematics" of logical empiricist philosophy, but the very life of the "mathematics" of mathematicians. The "good journals" of Chriss' comment differ from "great journals" by the relative "significance" of what they choose to publish; "designation" in turn relies on the "significance," as validated by more or less great journals, to assign candidates to the "good" or "great" (or lousy) jobs they are deemed to merit.

The commercial publishing issue points up the arbitrariness or absurdity of inherited social forms: there is no reason to assume traditional forms (like the research journal) are rational. I find it striking that most mathematicians take the logic of the current system for granted. The SoP acknowledges that junior colleagues place themselves at risk when boycotting expensive commercial publishers that hold a monopoly on the crucial fountains—“great journals”[71]—that dispense the charisma hiring committees (and those omnipotent Deans) need to evaluate before agreeing to award tenure; the SoP encourages senior mathematicians supporting the Elsevier boycott to “do their best to help minimize any negative career consequences.”

“In his later writings,” writes Robert Bellah, “Durkheim identified ‘society’ not with its existing reality but with the ideals that gave it coherence and purpose.”[72] Some colleagues judge that participation in such networked collective activities as blog discussions and MathOverflow represents a Sociability 2.0, close to mathematical ideals. MathOverflow’s constantly updated register lists the most active contributors, and those who take the time to answer questions are rewarded by a system of “badges” and reputation points, as on amazon.com or Facebook, but much more elaborate (that’s the 2.0 part); so are those who manage to find the right ways to frame questions that are or should be on everyone’s minds. Modesty regarding one’s own standing while displaying one’s expertise is an unwritten rule of good manners on this and similar blogs (there are also written rules of conduct). Gowers leads the gold-badge competition (he has nine) and clocks in with a very impressive 16,608 reputation points (as of November 3, 2012), which still places him well behind logician Joel David Hamkins of CUNY, the current reputation champion (64,313 points!).

“Reputation,” explains the MathOverflow FAQ page, “is earned by convincing other users that you know what you’re talking about.” A few participants in the Math 2.0 blog, launched soon after Gowers announced his Elsevier boycott, wonder why publishing can’t be reorganized along similar lines, eliminating the profit motive and replacing the less democratic features of the charismatic hierarchy by a permanent plebiscite.[73] One blogger wrote “some people like ‘elite communities,’ [others] prefer more democratic communities,” suggesting that the latter are in the majority. In contrast, SoP signatories were for the most part explicitly committed to maintaining the “élitist” functions of journals—validation and designation—in what they hoped would be a post-Elsevier era of mathematical publication; some were actively working to create new vehicles for this very purpose. If the content of mathematics is bound up, as I've argued, with a hierarchical charismatic structure, something of this sort is inevitable; if journals relinquish these functions, other institutions will take them up.

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By granting me tenure at the age of twenty-seven, Brandeis University ratified my permanent admission to the community of mathematicians. Thus I was endowed with the routinized charisma symbolized by my institutional position and fulfilled the (rational-bureaucratic) obligations[74] incumbent upon one enjoying the privileges befitting my charismatic status. These privileges included and still include regular invitations to research centers like the IAS (where I began writing this book in 2011), the IHES, or the Tata Institute of Fundamental Research (TIFR) in Mumbai (Bombay); to specialized mathematics institutes like the Fields Institute in Toronto, the Mathematical Sciences Research Institute (MSRI) in Berkeley, or the Institut Henri Poincaré (IHP) in Paris, where Cédric Villani is now director; or to speak about my work at picturesque locations like the *Mathematisches Forschungsinstitut Oberwolfach*, a conference center nestled high in the hills of the Black Forest in Germany, that for nearly seven decades has hosted weekly mathematics meetings.[75]

I could, therefore, end this chapter right now and consider that I have respected the bargain announced in its title. Eleven years after obtaining tenure, however, an unexpected event—a mystic vision, no less—started me on the path to being bumped up a few rungs on the charisma ladder. The incident, recounted in chapter 9, resulted directly in item 34 on my publication list, a premonitory sign of my impending symbolic promotion. The process continued with item 37 and was clinched with the much more substantial item 43, a collaboration with Richard Taylor in the form of a 276-page book published in the highly visible *Annals of Mathematics Studies* series edited by the mathematics departments of Princeton University and the IAS (where Taylor is now a professor).

My book with Taylor concluded with the proof of a conjecture in a named research program—the *local Langlands conjecture*, a step in Langlands' program. Like every mathematical breakthrough, this one was prepared by the extensive work of numerous colleagues, especially my Paris colleague Guy Henniart, who turned a qualitative prediction by Langlands into a precise and optimal quantitative conjecture. Henniart, whose office was across the hall from mine when I began thinking about the question in 1992, was also instrumental in making sense of my first results in the field. So it's only fitting that his name should be attached to the solution of the problem, and so it is, but—because the rules of charisma almost always severely discount work that stops short of the perceived *telos*—it's there on the grounds of the separate proof he found shortly after my announcement with Taylor.[76]

Ideal-typical honors—the details are unimportant, and anyway some of them are listed on the book jacket—flowed from this and subsequent collaborations with Taylor. Consistently with the Matthew Effect, the prize the French *Académie des Sciences* awarded my research group, including a substantial research grant, is routinely but inaccurately described as a prize for me alone. I'm sometimes invited to put an ordinary face on the Langlands program's charisma for the sake of mathematicians in other fields—Langlands can't be everywhere, and while none of his surrogates can really represent his perspective, he has been a good sport about it. Students with only the vaguest idea of my motivations think they want to work with me; indefatigable Deans solicit my opinions before proceeding with hiring decisions; colleagues I visit often hasten to meet even my tentative requests, as if the Matthew Effect had outfitted me with a built-in megaphone.

And, to cap it off, this book. A friend whose bluntness I cherish told me, "Of course if you're able to publish this book, it's because of the kind of mathematician you are." This is misleading. We'll see throughout the book quotations by Giants and Supergiants in which they conflate their own private opinions and feelings with the norms and values of mathematical research, seemingly unaware that the latter might benefit from more systematic examination. One of the premises of this chapter is that the generous license granted hieratic figures is of epistemological as well as ethical import.

My own experiments with the expression of what appear to be my private opinions resemble this model only superficially and only because they conform to the prevailing model for writing about mathematics. My friend's point was that even my modest level of charisma entitles me not only to say in public whatever nonsense comes into my head—at a philosophy meeting, for example, like the one for which I wrote the first version of chapter 7—but even to get it published. Or, to quote Pierre Bourdieu and Jean-Claude Passeron, "There is nothing upon which [the charismatic professor] cannot hold forth . . . because his situation, his person, and his rank 'neutralize' his remarks."[77] This book's ideal (-typical) reader, on the other hand, will know how to neutralize this kind of charisma, will have already recognized its ostensible narrator as little more than a convenient focal point around which to organize the text, and will be attentive to what is being organized.

First Session: Primes

One theorem found in nearly every popular book on mathematics, whether or not it is really relevant to the topic at hand, is the irrationality of the square root of 2. This can be stated as a theorem:

Theorem: There is no fraction p/q, where p and q are whole numbers, with the property that

$$\left(\frac{p}{q}\right)^2 = 2. \qquad \text{(Q)}$$

In other words, equation (Q) in the unknown quantities p and q is impossible to solve if p and q are required to be whole numbers. The theorem is easy to state, its proof fits in a few lines, and it's accompanied by a story about the crisis the discovery of such numbers provoked among the ancient Greeks who concerned themselves with such questions.

There is also a picture: figure α.1.

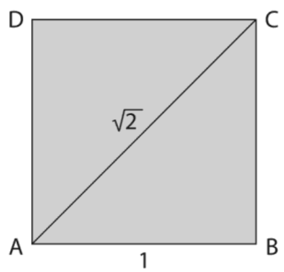

Figure α.1.

If $ABCD$ is a square with sides of length 1, then the Pythagorean theorem tells us that

$$AC^2 = AB^2 + BC^2 = 1 + 1 = 2.$$

Thus the diagonal AC has length $\sqrt{2}$, which the theorem tells us is an irrational number, that is, a number that is not a fraction. Here, therefore. is a quantity that one can perceive, or believe one perceives, but that cannot be apprehended rationally, or so the argument goes. The idea is that the rational number p/q can be understood in terms of acts that can be completed in finite time—cutting a unit length into q equal parts and then stringing p of these parts together; maybe this requires cutting several (or several billion) unit lengths in the first step. But $\sqrt{2}$ cannot be understood in such terms. It can be understood in other terms, say as the diagonal AC of the unit square in figure α.1, or as the number x that satisfies an equation of degree 2 in one variable:

$$x^2 = 2. \quad \text{(I)}$$

Comparing the ways one can understand this number to the way that the theorem tells us is off limits helps to attenuate the mystery that may be attached to $\sqrt{2}$. Figure α.1 suggests that $\sqrt{2}$ can be embedded in what Ludwig Wittgenstein called a "language game" that can be used to talk about geometry, whereas equation (I) tells us that $\sqrt{2}$ can be embedded in a language game that can be used to talk about algebraic equations. The theorem tells us that the language game of rational fractions has to be expanded in order to accommodate $\sqrt{2}$. The square root of 2, and the many numbers like it, thus serve to establish the relations among various language games that arise from different aspects of our experience with mathematics—measuring, counting, dividing up space into geometric figures, and comparing numbers with one another. The square root of 2 is old news, literally ancient history, but it remains true that mathematicians and those drawn to mathematics pay special attention to notions that can serve to link distinct language games—the more distinct, the better.

I borrow Wittgenstein's metaphor "language games" not because I believe experience is neatly divided up into such games (Wittgenstein certainly believed no such thing) but because it draws attention to the habit mathematicians have of using the word *exist* in regard to the objects we study when they become the subjects of the stories we tell about them. I have already begun to tell three stories about the square root of 2. The natural continuation of the first of the three stories is the proof of the theorem. Here's how it goes. We can write the fraction p/q in lowest terms. That means that we can assume that any number that divides p doesn't also divide q; otherwise, we can remove this factor from both numerator and denominator and simplify the fraction. "Simplifying" fractions is one of the skills we learn as part of learning to use fractions. There is an implicit value judgment that 2/3 is simpler than 10/15 even though they are two ways of writing the same number. Learning that a