FRACTALS AND CHAOS

The Mandelbrot Set and Beyond

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SELECTA VOLUME C

with a foreword by P.W. Jones
and texts co-authored by
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Foreword by Peter W. Jones, Yale University

IT IS ONLY TWENTY-THREE YEARS SINCE BENOIT MANDELBROT published his famous picture of what is now called the Mandelbrot set. The graphics available at that time seem primitive today, and Mandelbrot's working drafts were even harder to interpret. But how that picture has changed our views of the mathematical and physical universe! Fractals, a term coined by Mandelbrot, are now so ubiquitous in the scientific consciousness that it is difficult to remember the psychological shock of their arrival. A twenty-first-century researcher does not think twice about using a computer simulation to begin the investigation of a problem; indeed, it is now routine to use a desktop computer to search for new phenomena or seek hints about research problems. In 1980 this was very far from the case.

When a paradigm shift hits, it is rarely the old guard who ushers it in. New methods are required, and accepted orthodoxy is often turned on its head.

Thirty years ago, despite the appearance of an avant garde, there was a general feeling in the mathematics community that one should distrust pictures and any information they might carry. Computer experiments had already appeared in the undergraduate physics curriculum, but were almost nonexistent in mathematics. Perhaps this was due in part to the relatively weak computers then available, but there were other aspects of this attitude. Abstraction and generality were seen by many mathematicians as the guiding principles. There were cracks in this intellectual foundation, and the next twenty years were to see many of these prejudices disappear.

In my own field of analysis there had been overblown expectations in the 1950s and 1960s that abstract methods could be developed to solve a large range of very concrete problems. The correct axioms and clever theorems for abstract Banach spaces or algebras would conquer the day. By the late 1960s, groups in France and Sweden, along with the Chicago school in the U.S., had developed entirely new methods of a very concrete nature to solve old conjectures and open new frontiers. The hope of abstract salvation, at least in its most extreme forms, was revealed as naive. Especially for problems of a statistical nature, hard tools needed to be developed. (One should note that in other areas of mathematics, abstract methods have had spectacular success in solving even very concrete problems. What this means for the future of those fields is now a topic of broad speculation.)

How fascinating it is to look back on this period and observe Benoit Mandelbrot. He was looking at pictures, drawing conclusions in many fields, and being largely ignored by all. He was outside every orthodoxy imaginable.

To understand Mandelbrot's contributions to science, one must first give up the tendency to find a disciplinary pigeonhole for every scientist. What should one call someone who works simultaneously in mathematics, physics, economics, hydrology, geology, linguistics... ? And what should one think of someone whose method of entry into a field was often to find puzzling patterns, pictures, and statistics. The former could not be a scientist, and the latter could not be science! But Benoit Mandelbrot was really doing something very simple, at least at the entry point to a problem: He was looking at the pictures and letting them tell their own story.

In the mid 1500s, Galileo peered through telescopes to find astonishing celestial features imperceptible to the human eye. In very much the same spirit, Mandelbrot used the most modern computers available to investigate phenomena not well studied by closed formulas, and out popped strange and unexpected pictures. Furthermore, he worked with the idea that a feature observed in a mathematics problem might be related to "outliers" in financial data or the observed physics of some system. Perhaps these rare events or outliers were not actually so rare at all; perhaps they were even the main feature of the system!

After getting his foot in the mathematical door, Mandelbrot would start the next phase of research, erecting a mathematical framework and doing the hard estimates. Try today to explain to the scientifically literate high-school student that the beautiful fractal pictures on a computer screen are not interesting, at least not to be trusted, and try asserting that the fractals arising in wholly different problems are similar due just to chance.

While the aversion to looking at pictures has faded, there is still confusion as to why Mandelbrot's early works on fractals, e.g., his book *The Fractal Geometry of Nature,* generated such wild popularity in the general scientific community. One does not see on every page the "theorem–proof" methodology of a mathematics textbook. Furthermore, though one can easily find theorems and rigorous proofs in the book, the phenomena and pictures discussed may seem to a mathematician to be unrelated, because there is not necessarily an exact theorem to link any two of them.

What a poor world we would live in if this were the only permitted method to study the universe! Consider the plight facing a working biologist, where all data sets are dirty and causality difficult to determine. Should one demand a theorem in this situation? Should a geologist looking at rock strata search first for a theorem, when the formalism of multifractal measures might be more important? An old tradition in science is to seek first a description of the system at hand; this apparently simpler problem is usually much more difficult than is generally believed. Few doubt that Kepler's laws would have been formulated without his first seeking patterns by poring over reams of data.

Perhaps, however, the pictures studied by Mandelbrot arose randomly, and any connection to interesting science is just a coincidence. The Mandelbrot set M offers an instructive example. Despite twenty years of intensive research by the world's best analysts, we still do not know whether M is locally connected (the MLC conjecture), and progress on this problem has rather ground to a halt. This is now seen as one of the most central problems of complex dynamics, and the solution would have many deep consequences. The geometry of M is known to be devilishly complicated; M. Shishikura proved that the boundary has dimension equal to two.

We know today that the "Sullivan dictionary" provides many analogues between iteration of rational functions and the theory of Kleinian groups, but there is very much that remains open. For example, we do not know whether it is possible for either a Julia set or a limit set (of a Kleinian group) to have positive area unless it is the full sphere. If all Julia sets from quadratic polynomials have zero area, then the Fatou conjecture on density of hyperbolic systems would be proven for quadratics. It is also known that MLC implies both the Fatou conjecture for quadratics and the nonexistence of certain (but not all) Julia sets of positive area.

Another example is furnished by the Brownian boundary that is the subject of Plate 243 of *The Fractal Geometry of Nature.* Arguing by analogy and examination of simulations, Mandelbrot proposed that the Brownian boundary has dimension 4/3 and serves as a model for (continuous) self-avoiding random walks (SARW). The 4/3 conjecture was only recently solved by the spectacular work of G. Lawler, O. Schramm, and W. Werner. Their proof relied heavily on the new processes called SLE that Schramm invented. We now know that SLE (8/3) represents the Brownian boundary. This also proves another prediction of Mandelbrot that the two sides of the Brownian boundary are "statistically similar and independent." One of the major challenges in probability theory is to prove that SARW exists, and the new conjecture is that it can be identified with SLE (8/3).

The study of multifractals is another area where Mandelbrot played a leading role. Through multiplicative measures with singular support were known in certain areas of Fourier analysis and conformal mappings, their fine structure had not been examined, and they were virtually absent in discussions of physical problems until the work of Mandelbrot. He was also the first to write down $f(\alpha)$ in the form of normalized logarithms of large deviation probabilities.

The status of these problems may be open, but the beautiful pictures, now easily reproduced by the aforementioned high-school student, continue to fascinate and amaze. What we see in this book is a glimpse of how Mandelbrot helped change our way of looking at the world. It is not just a book about a particular class of problems; it also contains a view on how to approach the mathematical and physical universe. This view is certain not to fade, but to be part of the working philosophy of the next mathematical revolution, wherever it may take us.

Peter W. Jones, Professor of Mathematics, Yale University
New Haven, Connecticut, October 1, 2003

Preface

THE INTERCONNECTIONS BETWEEN FRACTALS AND CHAOTIC dynamical systems are numerous and varied. But this is neither a monograph on those interconnections, nor a textbook.

The core consists of reprints of the direct technical contributions I made in the 1980s to four great and enduring topics of mathematics: (A) Fatou–Julia iteration of the quadratic map $z^2 + c$, (B) Fatou-Julia iteration of other rational maps, (C) Poincaré's "Kleinian" limit sets, and (D) related singular measures. My contributions are not available at present in any single library. They were few in number, but several became influential, while others are perhaps more rarely quoted than they deserve.

To weave those topics together, new chapters were specially written, and many reprints are clarified by new forewords and annotations. There is a strange but widely held belief that science is a passionless and dull enterprise. This belief is certainly contradicted by the historical and biographical sketches in this book.

An eventful history and newly published pictures might well attract to this book some readers not concerned with mathematics *per se*. To help the pictures catch the interest of those readers, existing expository material that is comparatively "light" has been scattered throughout, especially in Chapters C23 and the first half of Chapter C17.

Sketches of the four main topics

Part I. Quadratic iteration and its Mandelbrot set. In the case of Fatou–Julia iteration, an object now denoted by M and called the "Mandelbrot set" has opened wide new vistas. For the quadratic map, I defined M in the plane of the complex variable c by the condition that the sequence c, $c^2 + c$,

$(c^2 + c)^2 + c, \ldots$ does *not* diverge. This definition, which may seem haphazard, will be seen to be deeply motivated. In contrast to its extreme simplicity, the complexity and beauty of M provoke wide fascination.

In 1980, paying close attention to computer-generated pictures led me to a number of striking observations that — either immediately or after a short delay — became mathematical conjectures concerning the quadratic Mandelbrot set. Though very simple to state, those conjectures were hard to prove. In fact, the most important of them—the Mandelbrot set is locally connected— remains open and has become notorious under the letters MLC.

My discovery of M consisted of those observations, and the deep contrast between merely seeing and discovering is discussed in Chapter C1.

Fractals and the Mandelbrot set in the classroom. A striking and important broad feature, not only of the Mandelbrot set but of all of fractal geometry, is that unknown territory lurks close to elementary considerations now taught in many high schools. The fact that the boundary of the unknown comes close to every known area has been of great help to many teachers. The bibliography lists two "waves" of material on fractals for the classroom. One was coauthored by Heinz-Otto Peitgen. Another is coauthored by M.L. Frame and me and includes Frame's course notes on the web and a DVD.

Part II: Nonquadratic iterations. Preparing this book brought a delightful surprise. Old archives preserved by my programming assistant in 1977–1979, Mark R. Laff, included my never-before-published illustrations (each imprinted by a date) concerning nonquadratic rational maps. Those pictures reveal that the discoveries I made in 1980 were preceded by a rich and subtle early period of fumbling and bumbling. Until now it could not be documented and therefore I mentioned it rarely. Today, with hindsight, everyone will recognize in Chapter C14 the overall shape and other features of the quadratic Julia and Mandelbrot sets studied in Part I. However, the nonquadratic environment of those early pictures was so complex that there was very little I could do with them in 1979.

The story of what happened in 1980 remains unaffected, but the events of 1979 and 1980 combined into an interesting case of scientific search and discovery that several of the chapters written especially for this book will discuss.

Part III: Kleinian groups' limit sets. I contributed a rapidly converging algorithm that filled a longstanding gap in an old theory. More specifically, my algorithm constructs by successive approximations a set that is self-inverse with respect to a given collection of circles.

Part IV: Exponentially vanishing multifractal measures. Chapter C20 arose when my IBM colleague Martin Gutzwiller and I, coming from thoroughly distinct areas of physics, realized that we were both investigating the same strange singular measure. Our results were easily combined in one paper. That measure then turned out to have been defined long ago by H. Minkowski, but further study was well-deserved.

Motivation and tools of investigation

A strong long-term motivation. To a large extent — in fact, surprisingly so, even to me — my thinking was triggered by being young and adventurous enough to become a master of the use of the computer and old enough to have been immersed in some ancient mathematical traditions. They had arisen in early twentieth-century but by the 1970s were unfashionable and slumbering. Those traditions caused me to begin the study of iteration with the complicated rational maps taken up in Part II.

My involvement with this book's topics was largely independent of "chaos theory," understood as the revival of nonlinearity in the 1970s. While chaos theory favored the real map $x^2 + c$, it was already said that my move to its complex counterpart came late and reluctantly.

The relative roles of primitive or refined pictures, and of the eye. It was near-universally believed among pure mathematicians around 1980 that a picture can lead only to another, and never to fresh mathematical thinking. A striking innovation that helped thoroughly destroy this belief resided in my work's heavy reliance on detailed pictures, in contrast to schematic diagrams. Incidentally, a picture is like a reading of a scientific instrument. One reading is never enough. Neither is one picture.

More precisely, my discoveries of new mathematical conjectures relied greatly on the quality of visual analysis and little on the quality of the pictures. Indeed, Chapter C1 will establish that for discovering the Mandelbrot set, high quality graphics was not necessary, while Chapter C12 will establish that it was not sufficient, either.

Altogether, my lifetime scientific work rescued the verb "to see" from the figurative meaning to which both common usage and hard quantitative science had reduced it, and restored its concrete meaning, whose instrument is the eye.

Some fractal pictures are realistic and proudly called "forgeries" of mountains, clouds, trees, or galaxy clusters. Other pictures are totally abstract, like those of the Mandelbrot set. Moreover, some fractal pictures are perceived as having high aesthetic quality. Enormous numbers of persons have posted pictures of fractals on the Web. But the black-and-white computer pictures in my old files continue to be very valuable. If a suitable environment can be found, I would love to extend the small art portfolio implicit in this book into a "permanent exhibit" on the Web.

Fractal geometry opens up a quantitative theory of roughness

Given the variety of its manifestations, fractal geometry continues to surprise both the technical and the nontechnical audiences. It remains hard to pigeonhole, to classify, and to compare with existing disciplines.

Mostly after the fact, I view fractal geometry as opening up a study of roughness that is parallel to—but distinct from—the studies of brightness/color, loudness/pitch, heaviness, and heat, each of which has long since developed into a science. Compared to the studies of those other basic sensations, the study of roughness came late because it is more complex. Its quantitative measurement demands Hölder exponents and Hausdorff dimension—concepts that arose far later than, for example, periodic oscillations; fractal geometry was first in recognizing that they concern anything "real."

While it is tightly bound by the tools it uses and the flavor of the problems it faces, fractal geometry retains an intrinsic diversity that is rare, amusing and—I think—important. It has survived the childhood diseases and crises that strike intellectual initiatives involving an ambitious synthesis has been described as having changed the view of nature held by many mathematicians, scientists, engineers, artists, other professionals, and even every man and woman.

Open and fortress mathematics. Starting at the latest in ancient Greece with Archimedes and Plato, the views of the nature of mathematics has ranged between two extremes. My self-explanatory words for them are *open* and *fortress mathematics*. The former involves a lively sprawling collection of buildings permanently under construction or reconstruction, with many doors and windows revealing beautiful and varied landscapes. The highest ambition of fortress mathematics, to the contrary, is to wall off all openings but one. Its dwellers believe that their endeavors can evolve on their own steady path and need not interact with society at large.

While mathematics and science are among the highest achievements of humanity, all evidence shows that their history and the history of human civilization have been indissolubly intertwined. The claim that fortress mathematics *has* become independent is wishful thinking, and the notion that it *can* become independent is gratuitous.

Other contributions of fractals to pure mathematics

Following *The Fractal Geometry of Nature* (M 1982F), a series of my *"Selecta"*—selected papers— began with M1997E, M1999N, and M2002H, and continues with this Volume C, M2004C. The style of the preceding references is explained on the first page of the bibliography. Denoting those *Selecta* by nonconsecutive mnemonic letters suggests that they can be examined in any sequence.

The previous three volumes all concern a "state" of randomness and variability that I call "wild." This volume C is unrelated to the previous three, with the following important exception. Not only do Chapters C20 and C21 involve the topic of M1999N, which is multifractals, but those chapters were motivated by statistical physics through diffusion-limited aggregation. This is why M and Evertsz 1991 is reprinted as Chapter C22.

Early plans called for additional *Selecta* volumes. But, the Internet having transformed our world, the further *Selecta* will be "Web books" on my Web site. Each will reduce to a title page, a foreword, a table of contents, and links to papers on my home site. Given the diversity of my work, the web's flexibility is a great asset.

The "Overview" Chapter HO of M2002H presents a partial but nearly-up-to-date status report on fractal geometry. More specifically, what has been its overall impact on mathematics? While fractal geometry was young, it was invidiously observed that it "has not solved any mathematical problems." This is no longer true and in any event was always irrelevant. Indeed, my role in mathematics has been to provide a mass of *new problems and conjectures*. Each opened a new field that continues to prosper as I move to other concerns. Most widely known are the examples discussed in this book, but a few other examples deserve brief mention now.

M1982F (p. 243) introduces and M2002H (Chapter H3) investigates the concepts of Brownian cluster and Brownian boundary, culminating with the conjecture that this boundary's Hausdorff dimension is 4/3. Combined with related conjectured dimensions for percolation and Ising clusters, the diverse occurrences of 4/3 grew into sharp challenges to the analysts and has led since 1998 to widely acclaimed proofs by Duplantier, Lawler, Schramm, Werner, and Smirnov. Earlier, a dozen or so scattered technical conjectures in analysis had been shown to be equivalent to that

4/3. All have now been proven as corollaries and together provide mathematics with a new element of unity that continues to be explored.

M1999N collects many early papers in which I introduced and investigated the random multiplicative singular measures, now called "multifractal," an example of which stars in Part IV. They were not intended as new esoterica but as a model of turbulence and finance. My conjectures created an active and prosperous subbranch of mathematics, they served to organize some features of DLA (as already mentioned), and they underlie the main current branch of statistical modeling of the variation of financial prices. Increasingly rich structures arose as I repeatedly weakened the constraints on the multifractal multiplicands. The papers collected in M1999N took the step from microcanonical to canonical multiplicands. Recent papers coauthored by J. Barral moved on to products of pulses and other functions.

The telling pictures I drew of old standbys like the Koch and Peano curves and the Cantor dust achieved a broader and deep change of perspective. Those sets used to be viewed as "pathological" or "monsters." Quite to the contrary, I turned them around into unavoidable rough models ("cartoons") of a reality that science had previously been powerless to tackle, namely, the overwhelming fact that most of raw nature is not smooth but very rough. For example, I reinterpreted Peano "curves" as nothing but motions following a plane-filling network of rivers.

Norbert Wiener once described his key contributions to science as bringing together — starting from widely opposite horizons — the fine mathematical points of Lebesgue integration and the vigorous physics of Gibbs and Perrin. Also (like Poincaré), Wiener was very committed (and successful) in making frontier science known to a wide public. On both counts, the theory of fractals is arguably a multiple second flowering of Wiener's Brownian motion.

Overall acknowledgments

Let me now proceed beyond the acknowledgments printed after each paper, which were preserved, and the further acknowledgments found in several introductory chapters. Firstly, warm thanks go to the coauthors of joint papers for permission to reprint them.

For over 35 years, the Thomas J. Watson Research Center of the International Business Machines Corporation, in Yorktown Heights, N.Y., provided a unique haven for mavericks and for various investigations that

science and society forcefully demanded, but academia and its funding agencies neither welcomed nor rewarded.

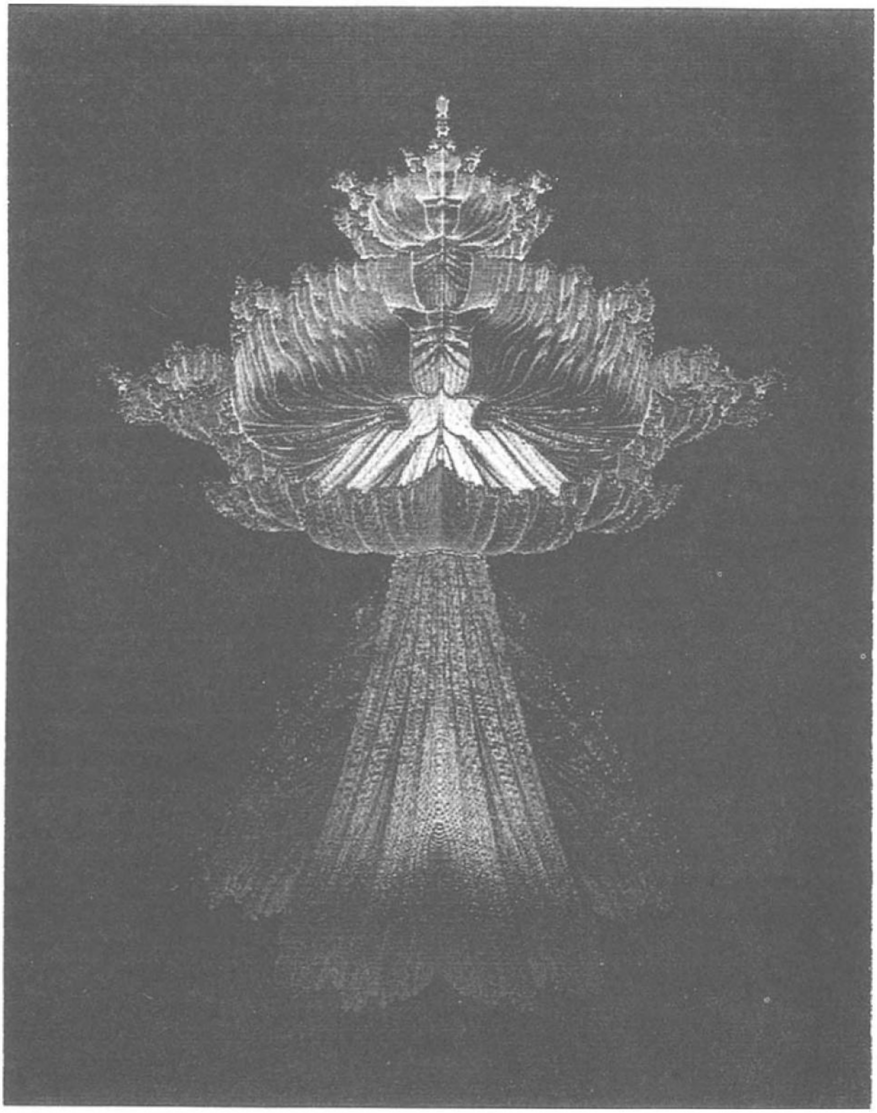
The originals of the old texts reprinted in this book were written at that haven. Invaluable programming and graphics support was provided there by Mark R. Laff, V. Alan Norton, and J.A. Given, and at Harvard in 1980 by Peter Moldave. The preparation of the original texts and the long–drawn–out preparation of this book were performed by several long–term secretaries, H. Catherine Dietrich (1933–2003), Janis Riznychok, Leslie Vasta, Premla Kumar, Kimberly Tetrault, Catherine McCarthy, and Barbara White. After retiring from IBM, I continued at Yorktown part time as IBM Fellow Emeritus, largely in order to prepare the *Selecta* books. Short-term assistants far too numerous to list were of great help. The clumsy English of some old papers was copy edited by Helen Muller-Landau, Noah Eisenkraft, and others. Of course, extreme care was taken never to modify the meaning. The originals are available in libraries and are gradually being posted on my Web site.

Never was IBM's pioneering *Script* word-processing language under VM expected to survive (unattended!) for ten years. But the clock is ticking, and this may be the last major project served by *Script*.

I am deeply indebted to the Yorktown of its heyday as a scientific powerhouse. Among long-term friends and colleagues, it will remain most closely associated with Richard F. Voss, Martin Gutzwiller, Rolf Landauer (1927–1999), and Philip E. Seiden (1934–2001). As to management, at a time when the old papers in this book were being written and were widely perceived as a wild gamble, my work received wholehearted support from Ralph E. Gomory, to whom I reported in his successive capacities as Group Manager, Department Director, and finally IBM Director of Research and Senior Vice-President. Gomory reminisced on the old times in a *Foreword* written for M 1997E.

As an adjunct in the Yale Mathematics Department before retiring from IBM, then as a tenured professor, I had the renewed great fortune of being invited, especially by R. R. Coifman and Peter W. Jones, to move on to another haven that also provided my life with welcome balance between industry and academia. The Yale postdocs I supervised include Carl J.G. Evertsz, coauthor of a paper that became Chapter C22.

Last but not least, this book is not solely dedicated to my uncle. As all my work, it is also dedicated to my wife, Aliette. The original papers would not have been written, assembled, and added to without her constant and extremely active participation and unfailingly enthusiastic support.



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PART I: QUADRATIC JULIA AND MANDELBROT SETS

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First publication

C1

Introduction to papers on quadratic dynamics: a progression from seeing to discovering

THIS CHAPTER DESCRIBES THE CIRCUMSTANCES under which I had the privilege of discovering in 1980 the set that is the main topic of this book. As will be meticulously documented in Chapters C12 and C14, I actually saw this set in 1979 but bumbled and fumbled for about a year.

M1980n{C3} ushered in the modern theory of complex dynamics, specifically of the quadratic dynamics of the maps $z^2 + c$ and $\lambda z(1-z)$. *The Fractal Geometry of Nature*, M 1982F, followed and was widely read. Very rapidly, interest in the Mandelbrot set became broad and extraordinarily intense. Many eminent mathematicians immediately took up its study and achieved spectacular results that provoked a historically significant and highly beneficial change in the mood of mathematics. Yesterday, "generality at all cost" was in the saddle. Today, "special" problems are more readily recognized as compelling.

However, my own research moved on to different topics. Therefore later developments of quadratic dynamics are little known to me, and few will be quoted; Urbanski 2003 is a recent survey.

The broad "popular infatuation" with the Mandelbrot set must be mentioned. This social phenomenon continues, astonishes everyone, and of course enchants me. It was largely spontaneous, no committee or organization being involved. It suggests that a strong interest for mathematics is widespread among humans, but only if its links with nature and the eye are not actively suppressed but, instead, brought out and celebrated.

This broad interest may bring to this book some readers from a "general public." For their sake, the introductory chapters interpret the term "historical circumstances" rather broadly and include facts already well known to many professional mathematicians. Earlier accounts such as M 1986p were very incomplete.

Section 1 is a sketch, and Sections 2 and 3 provide fuller discussions. Broader acknowledgments are postponed to the next chapter.

"Nice" illustrations are scattered throughout this book, but this chapter's main point is strengthened by returning to the sources. Figures 1, 2, 3, 4, and 5 are a small sample of the crude illustrations, many of them published for the first time, that led to the actual discovery of the Mandelbrot set during the Harvard spring term of 1980. Their number and variety, which I had forgotten, are significant. The computer did not automatically imprint a date, but they might be roughly ordered in time.

To print those old pictures, it was necessary to enhance them by repeated xeroxing. Since they are well-known today, and contain no fine detail that risks being lost, many were made small.

1. THE PROGRESSION FROM SEEING TO DISCOVERING

1.1 Definition and a key quotation from Adrien Douady

Everyone knows, or so it seems, that the set M is defined in the complex plane of the variable c by the condition that the sequence $c, c^2 + c$, $(c^2 + c)^2 + c, \ldots$ does not diverge. It is Adrien Douady who proposed the term "*Mandelbrot set M* because Benoit Mandelbrot was the first one to produce pictures of it, using a computer, and to start giving a description of it."

Verba volant, scripta manent. The words quoted above are found on the third and second lines from the bottom of page 161 of Douady 1986, which this chapter will quote again.

Those words and their date are important. Despite its brevity, Douady's statement subdivides into two clearly separate issues, one inconsequential, and the other broad and historically important. The fact that I was the first to produce pictures of the Mandelbrot set, in 1979, is nice. But in the context of mathematics, this is not much to be praised for. Section 2 will argue that the issue of the "first picture" is, by itself, unimportant. Section 3 will argue that the actual discovery occurred later in 1980, and consisted in my early description of many fundamental features of *M*. This discovery mattered a great deal, because it soon triggered important developments.

Between the unbeatable simplicity of the definition of *M* and its visual and mathematical complexity there is a profound contrast that marks an important discovery of the late twentieth century.

This book's core consists of reprints of papers in which my main observations were first presented in the form of mathematical challenges/conjectures.

1.2 Motivation for investigating the Mandelbrot set and a sketch of key observations

1.2.1 Orbits, their limit points or cycles, and the "filled-in" Julia sets. A rational function of a complex variable z is the ratio of two polynomials in z. Let $f(z, c)$ denote a rational function of z depending on a complex parameter c that can be onedimensional or multidimensional. For fixed c, the orbit of a starting point z_0 is defined as the infinite sequence z_0, $z_1(z_0, c) = f(z_0, c)$, $z_2(z_0, c) = f(z_1, c)$, and generally $z_k(z_0, c) = f\,[z_{k-1}(z_0, c), c]$.

In the late nineteenth century, the notation arose that such sequences provide idealized versions of dynamical systems of a discrete time k. Within that perspective, it is important to classify the points z_0 and c according to the limit behavior of the corresponding orbit. Where does it fail to converge and can be called "chaotic"? Where does it converge to one of several fixed points or finite cycles and can be called "orderly"?

Quadratic dynamics corresponds to the case where f is a second-order polynomial. Changing the variable z reduces f to either $z^2 + c$ or $\lambda z(1 - z)$. In either case, there is one complex parameter, c or λ. For every c, there is a fixed point at infinity to which an orbit converges if its starting point z_0 is far enough from the origin. But there also exist points z_0 such that the orbit starting at z_0 fails to converge to infinity. Those points taken together define the "filled-in Julia set" corresponding to c.

1.2.2 M^0 versus M. For some values of c, the orbits of some points z_0 converge not to infinity but to a finite stable cycle of size $N \geq 1$. In this dynamical-systems perspective, I became interested in 1979 in identifying the set M^0 of those values of c and classifying them according to N.

In all interesting cases, an analytic study of M^0 is impossible. Therefore, I attempted to study it numerically. But the task proved to be extremely hard computationally and the approximate M^0 it yielded was very blotchy. Making the task even harder was the fact that in 1979, I opted to start not with quadratic dynamics but with the far more complicated $f(z, c)$ to be discussed in Chapter C14. This part picks up the story at the point where I turned back to $f(z) = z^2 + c$.

It occurred to me that the existence of limit sets implied domains of convergence separated by curves. Hence the set M^0 I was seeking had to be a subset of the set of c's for which the Julia set is not a dust but connected. Fatou and Julia had given a criterion that is straightforward and particularly easy to program for the quadratic map: c belongs to the set M if and only if the orbit with the starting point $z_0 = 0$ (called "critical point") *fails* to converge to infinity. This set of values of c is identical to the set M as defined above.

I conjectured that M was the closure of M^0 but that in any event, M was relatively easy to investigate, hence well worth exploring. My conjecture is most often restated today as asserting that the Mandelbrot set is locally connected (MLC). Despite heroic efforts it has not yet been proven true or false. How fortunate that I did not try to settle this issue!

1.2.3 A structure made of "atoms" combined in a big "molecule." Douady 1986 continues (p. 162) his description of my key early observations on M as follows: "When you look at the Mandelbrot set, the first thing you see is a region limited by a cardioid, with a cusp at the point .25, and its round top at the point $-.75$. Then there is a disk centered at the point -1 with a radius .25, tangent to the cardioid. Then you see an infinity of smaller disk-like components, tangent to the cardioid, most of which are very small. Attached to each of those components, there is again an infinity of smaller disk-like components, and on each of these there is attached an infinity of smaller disk-like components, and so on."

Let me interrupt Douady to describe the circumstances of my discovery of those disc-like components for the quadratic map in the alternative form $z \rightarrow \lambda z(1-z)$. After a few iteration stages on a rough grid, we saw that the set M includes very crude outline of the disks $|\lambda| < 1$ and $|\lambda - 2| < 1$. Two lines of algebra confirmed that these disks were to be

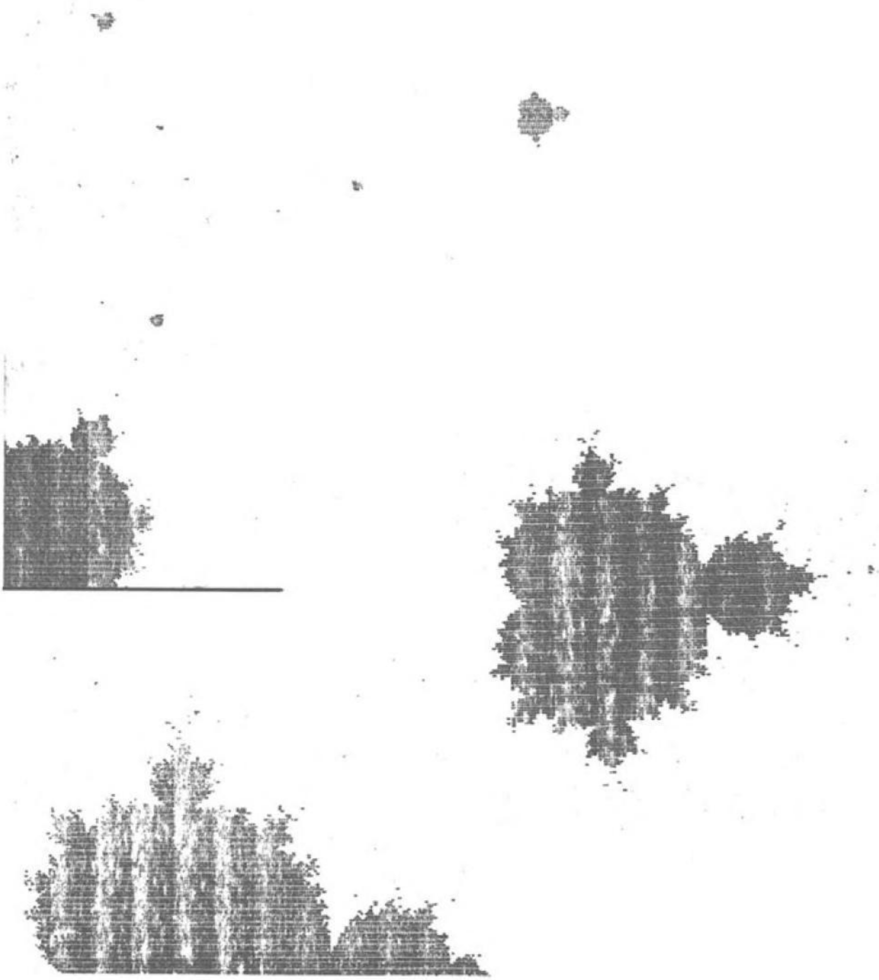

FIGURE C1-1. [Harvard, early 1980] My first picture of the whole M set is—unfortunately— either misfiled or lost. The first picture made at IBM in 1980 is reproduced in later chapters. The top panel here is a blow-up of the most conspicuously "messy" corner of M, near the bifurcation of order 3. The middle and bottom panels show the oldest preserved blow-ups of the two largest islands, one already seen in the top panel and the other intersected by the real axis (for reasons of economy, only half was computed).

Discovering such "pearls" in a pigsty motivated a passionate investigation that led to the first paper on the Mandelbrot set, M1980n{C3}; some of its pictures were prepared after I returned from Harvard to IBM. This text appeared very quickly in the *Annals of the New York Academy of Sciences,* in the widely read proceedings of a major meeting on nonlinearity. The title, "Fractal aspects of the iteration of $z \to \lambda z(1-z)$ for complex λ

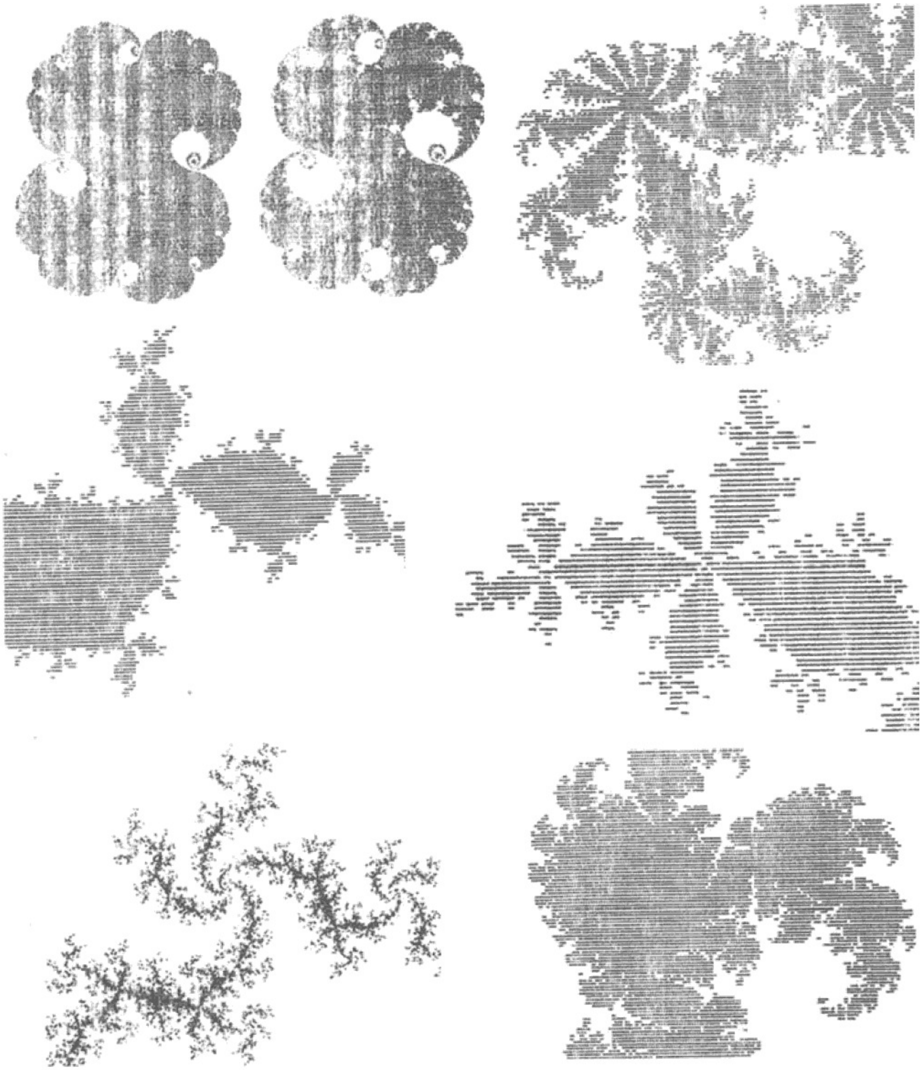

FIGURE C1-3. [Harvard, early 1980] Miscellaneous Julia sets for complex parameter values in the main continental molecule of the Mandelbrot set.

and z, " suffices to show that my goal was to revive experimental mathematics by reporting observations triggering new mathematics.

1.2.5 The web-like structures linking the islands to create a connected set M. The quotation from Douady 1986 continues as follows: "This is not all

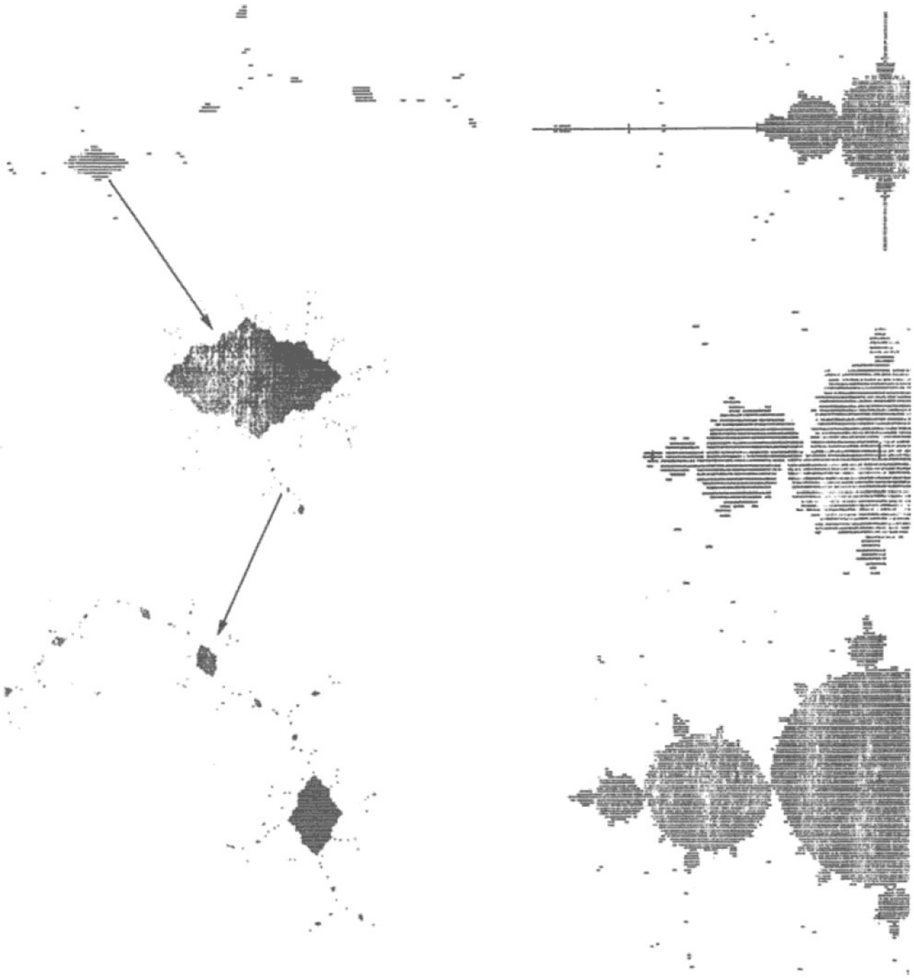

FIGURE C1-4. [Harvard, early 1980]. Miscellaneous Julia sets for parameter values in the "island molecules" of the Mandelbrot set. By a theorem Julia and Fatou, those Julia sets are connected. Therefore the broken-up appearances is necessarily due to the discrete variables used in computation. These graphs were important to my thinking because they sufficed to show that the broken-up early *M* set pictures were compatible with connectedness.

... All of these cardioid-like components are linked to the main cardioid by filaments, charged with small cardioid-like components, each of which is accompanied by its family of satellites. These filaments are branched according to a very sophisticated pattern."

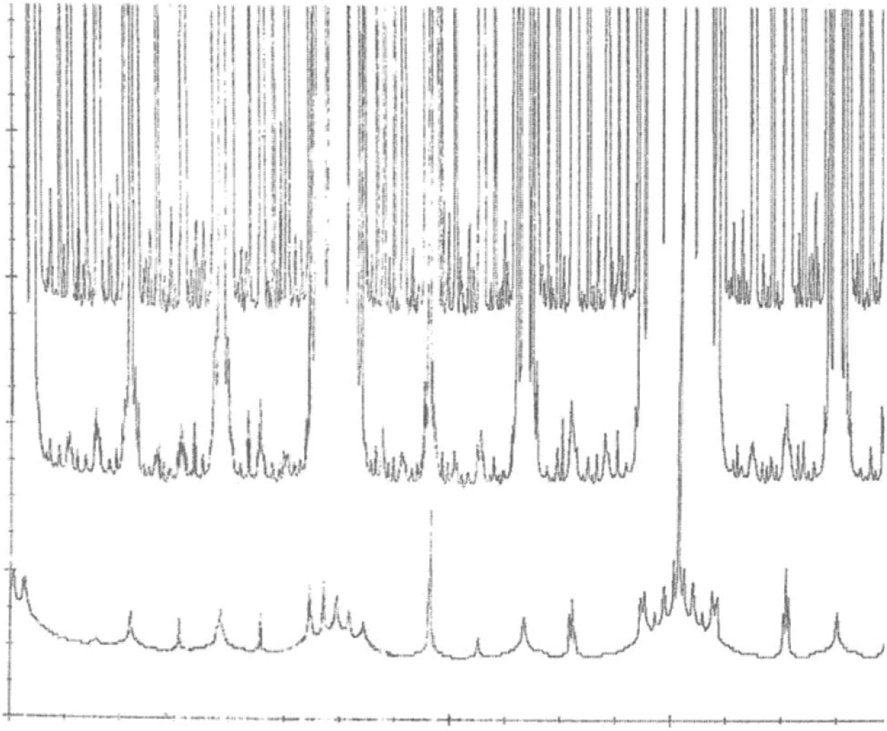

FIGURE C1-5. [Harvard, early 1980] Early on, I imagined the surface that is defined, for each λ, by the function " $H(\lambda)$ = the number of iterations needed to first achieve the inequality $|f_n(1/2)| > 2$. " The M set is defined by $H = \infty$. Plate 189 of M1982F (reproduced in Chapter C4) includes level surfaces of $H(\lambda)$ represented in shades of grey. Peitgen & Richter 1986 taught everybody how to represent isolines or perspective views of $H(\lambda)$ in bright colors. But in early 1980, all I could do is to examine vertical cuts of that surface along lines in the λ plane. The abscissa being denoted by t, this figure combines the (non-overlapping original) records of cuts truncated to $H \leq 300$. corresponding to the lines $\lambda = t + 0.00001i$, $\lambda = t + 0.0001i$, and $\lambda = t + 0.001i$. The range of t (not recorded on the originals) clearly corresponds to small islands along the real axis of λ. To make the overlap legible, the first and second cuts are moved up, respectively, by 100 and 50 iteration stages. The result confirmed what one could expect on the basis of real, as opposed to complex, iteration.

Again, let me elaborate. In parallel with blowups of the set *M*, we were also running pictures of the Julia sets for values of *c* that lie within the island molecules. They are exemplified by Figure 4. This was the "Eureka moment" for which decades of work seemed to have prepared me. The shapes we saw also appeared to split into many islands, each of these a reduced-scale version of the Julia set corresponding to a matching value of *c* in the continental molecule of the set *M*. However, we knew from old mathematics that this appearance had to be misleading. While island interiors cannot overlap, Julia's criterion implied—and closeups confirmed—that the gaps between islands had to be partly spanned by a peculiar geometry best illustrated by the following analogy. Imagine a stream so wide that only large beasts can jump from one side to the other. To accommodate smaller beasts, smaller stones are added half way through each gap. And to accommodate devilishly small beasts, the process must continue ad infinitum. Ultimately, the islands connect by their coastlines, adding up to "devil's" polymer, whose "strands" were invisible because actual computation is necessarily limited to a lattice.

Returning to the set *M*, it became very important that Myrberg had developed a theory of the iteration of the real map x^2+c. That theory implies that the real axis pierces a string of islands of the set *M*, and connects them in devil's fashion by their coastlines. This suggested the conjecture that the whole set *M* is a connected devil polymer.

However, I faced this issue with a degree of caution that was out of character and perhaps brought about by living during that year among pure mathematicians. I presented the connectedness of the set *M* as a question to be answered, not a conjecture to be verified. This was a distinction without a difference but it led me in M 1980n{C3} to define an awkward surrogate to *M*, whose properties could be described in mathematically more "firm" fashion. M 1982F snapped back into proper assertiveness. The issue soon became moot: also in 1982, A. Douady and J.H. Hubbard gave a proof of the connectedness of *M* and went on to study it in admirable detail.

1.2.6 "Stellate structures" and uncanny resemblances between Julia sets and the "corresponding" corners of the Mandelbrot set. Continual flipping between the set *M* and selected Julia sets led to another exciting discovery. I saw that the set *M* goes beyond being a numerical record of numbers of points in limit cycles. It also has an uncanny "hieroglyphic" character: It includes within itself a whole deformed collection of miniature versions of all the Julia sets.

My other early observations included "stellate structures" that are common to the Julia set of parameter c and the neighborhood of c in the Mandelbrot set.

1.2.7 The conjecture that the Hausdorff–Besicovitch dimension of the boundary of M is 2. This conjecture was gradually refined and is discussed in Chapter C8.

2. AN INCONSEQUENTIAL ISSUE EXAMINED CLOSELY: WHICH WERE THE "FIRST PICTURES" OF THE MANDELBROT SET?

Once again, Douady's statement in Section 1.1, despite its brevity, touched upon two clearly separate issues that this and the next section will examine separately and closely.

2.1 In mathematics, a picture cannot be interesting in itself, but only for the descriptions and investigations that it triggers

Douady calls me the "first to produce pictures" of M. This is indeed the case. Moreover, his use of the plural "pictures" is accurate and important, but the statement demands careful elaboration. Suppose a future historian finds that Julia and/or Fatou observed the following somewhere, in passing: for $|c| > 2$, the quadratic Julia set is totally disconnected. Such a hypothetical but conceivable finding would imply an "approximate picture" of M as a circle. No conclusion can be drawn from this approximation, hence no one would claim it as a first.

Instead of a hypothetical event, consider two well-documented ones concerning a different object, namely, the diffusion-limited aggregates, which will be the topic of Chapter C22. After "DLA" was discovered in Witten & Sander 1981, a search through the literature revealed many old pictures of DLA-like natural phenomena. But those pictures had led to no insight, because suitable tools became available only after the development of fractal geometry. Fractals were a prerequisite for the brilliant and very influential description provided by Witten and Sander. Those authors are the discoverers of DLA, and the early pictures deserve neglect.

Similarly, Figure 2 of Chapter C19 served in M and Evertsz 1991 to report some very peculiar properties of the Laplacian potential around DLA. A scientist then pointed out a related figure that was older. But that figure had appeared without comment, for the sole purpose of illustrating a computing technique. Maybe it was "first" but it was forgotten, as it deserved.

3. THE DISCOVERY OF THE MANDELBROT SET CONTRIBUTED TO THE REBIRTH OF EXPERIMENTAL MATHEMATICS

The demise of the presumed "beautiful IBM graphics" leads one to wonder why the Mandelbrot set was not discovered by someone with access to graphics better than what I had in 1980. The answer is part of the mystery of scientific discovery. A discovery is made when the tools—both intellectual and physical—are available to an individual with the motivation, acuity, and inspiration to use them, and I was motivated to sniff out the ramifications of those specks of dirt.

Later conversions to a belief in the power of observations were triggered by the inspiration that my conjectures about those dust spots awakened among many mathematicians—not by a discontinuity in the availability of computers. To quote from John F. Kennedy's 1962 Yale commencement address, "The great enemy of truth is very often not the lie— deliberate, contrived, and dishonest, but the myth—persistent, persuasive, and realistic." It was a myth—one all too persistent, persuasive, and long realistic—that experiment in mathematics had become useless.

3.1 A key fact that is perennially misunderstood and misinterpreted: many sciences, especially early on, encounter periods during which a well-trained and skillful eye is essential

It is worth recalling at this point some notable examples of the role played by properly interpreted messages from the eye. The telescope was invented, built, and marketed in Holland, as a toy. At least one person, Thomas Harriott (1560–1621), well regarded in his day as an astronomer and mathematician, had the idea of pointing it towards the Moon and making a drawing of what he saw. His drawing was preserved but shows nothing but blobs with no structure.

Galileo Galilei (1564–1642) was not the first to handle the telescope, but the first to change it from a toy to a vital tool. He was a trained painter, a negligibly minor one, to be sure, against stiff competition in Renaissance Tuscany, but equal to the new task of taming the telescope. Once directed to structures he had not discovered, Harriott instantly confirmed their existence. He might have uttered T.H. Huxley's exclamation upon reading Darwin's *The Origin of Species:* "How extremely stupid not to have thought of that."

There is a strong reason why Galileo performed (immensely) better than his contemporaries, invented physics, and became the first physicist.

Modern science arose in his hands from the notion that truth did not reside in ancient books written by men but in the "great book of Nature" opened in front of Man's eyes. In effect, Galileo argued against the New Testament assertion that "in the beginning was the word." Had he dared counter the Scriptures (unlikely, since he did not seek a conflict with the Church), Galileo might have proclaimed (as I do not fear to do) that "in the beginning were the picture and the eye." Even better, "In the beginning, the word joined the picture and the eye."

Let us move on. The microscope and the photographic camera were ancient tools in the time of Santiago Ramon y Cajal (1852–1934) but their availability did not suffice to resolve the complexity—nay, the utter messiness— of the human nervous system. Once again, the reason why "fate" chose Ramon y Cajal for this task is because the task was not "normal," and he—perhaps he alone in his day—was well prepared. Being a trained and infinitely patient artist, he overwhelmed the inadequacies of his miserably outdated microscope. He saw—and revealed through classic pictures—marvels that long remained unsurpassed. Around 1950, my neurologist friends were still relying on pictures first published in near-medieval Spain in the 1890s. That Cajal did not achieve and hold true fame is a disgrace.

A third notable "seer" worth mentioning was the meteorologist and geophysicist Alfred Wegener (1880–1930). The near match between the southwest coast of Africa and northeast coast of South America must have been noticed early. Wegener dared take the next step, from seeing to discovering. He compared fossils separated by the Atlantic and imagined a primordial continent that broke into parts that drifted away.

Galileo and Cajal loom high in my personal Pantheon. In kind, and without any claim concerning relative importance, my experience was like theirs. Quite explicitly, I thought of Cajal while discovering the M set and of Galileo while discovering the Brownian boundary dimension 4/3.

Major differences are obvious: Galileo's story marked a nearly absolute beginning, Ramon y Cajal's marked a deepening, and mine marked a renewal after a long lapse. The "fate" that drove me to revive the theory of iteration, first chose me to reinvent the role of the eye in a field, mathematics, where it and explicit computation had become anathema, about as unwelcome as they could possibly be.

3.2 The culture of mathematics during the 1960s and 1970s

Within that culture the Mandelbrot set could not have been discovered. Hence its discovery marked a historical departure. Today—but not yesterday— only a minority among mathematicians would agree with the opinion due to someone *who did not* discover that set, that the study of M reflects "a rather infantile and somewhat dull mathematical sensibility" (Brooks 1989).

The attitudes in which all mathematicians were trained not so long ago is witnessed by Stanislas Ulam (1909–1984). He might have been expected to speak to the power and utility of the pictures produced by his associates. Instead, Ulam 1974 (pages 378 and 490) informs us that "Mathematics is not really an observational science and not even an experimental one. Nevertheless ... computations... were useful in establishing some curious facts ... Fermi expressed ... a belief that it would be useful to attempt practice in the mathematics... of nonlinear systems. The results [described in a famous report by Fermi, Pasta, and Ulam reproduced in Ulam 1974] ... were interesting and quite surprising to Fermi."

Among pure mathematicians, Ulam's lukewarm advocacy of the computer strengthened an antagonism that was obvious well before 1979. For example, a sustained effort brought prominent youngish mathematicians to visit the IBM Research Center to lecture on diverse topics. The unexpressed hope was that the computer's promise would impress some of them. The failure was complete, and (much later) certain communities accepted my "anomalous" manners grudgingly.

How to respond to serious thinkers who do not wish to distinguish between seeing and discovering? Some still seek "pictures of M" earlier than either the trove of mine, or the single one found in Brooks & Metelski 1981 and often mentioned after 1988. That single picture is so indistinct that it could not—and did not—lead to any discovery.

To show how counterproductive such a search for "first sighting" can become, let me broaden it from iteration to all fractals. Who provided the first massive collection of many pictures clearly recognizable as fractal? Could it be the marvelous Hokusai Katsushika (1760–1849), to whom I pay homage in Plate C16 of M1982F and again in Chapter C13. His unforgettable pen drawings of *One hundred views of Mt. Fuji* depict clouds and trees admirably, and all those who can see and are familiar with fractal geometry recognize that Hokusai had a perfect "eye for fractals."

But neither he nor earlier or later landscape painters have any claim for fractals as a topic for either mathematics or science. Those credits belong, respectively, to contemporaries of Cantor and Peano, and to me. Yet, Hokusai holds a central role in my current view of fractals as a notion familiar to Man, in one form or another, since time immemorial.

The fractals' prehistory is a long story that must be reserved for a different forum, but it raises a question I commend to the specialists. The time when Cantor, Peano, et al. flourished was the heyday of Japanism, when Hokusai's direct influence on Western European art was widely acknowledged. Did this influence extend—with no acknowledgment — from the artists to the mathematicians?

3.3 A changed mood in mathematics?

An entirely different world is called for in Bourguignon 1999. Here is a free translation. "I think one must distinguish the future of mathematics from the future of those who claim to be mathematicians. The future of mathematics overflows with challenges and promise. But I fear that mathematicians may spoil it by failing to open up and dare, and by exhibiting a high propensity to exclude (I should say "excommunicate" because of religious overtones) from their community whole domains of knowledge. Without fear of fresh air, mathematicians must open up without shivering." I read this text with equal pleasure and surprise.

4 SUMMARY

One must heed the wise words of Whitehead 1974 (p. 127) that "To come very near a true theory and to grasp its precise application are two very different things, as the history of science teaches us. Everything of importance has been said before by somebody who did not discover it." The thought also applies when the word "said" is replaced by "seen."

A contribution to mathematics and/or science does not consist of a picture, but rather of a picture combined with a description. Without words and formulas, a picture can, at best, be praised for artistic quality. Without an interest in pictures and other aspects of "reality," pictures can play no role whatsoever.

This comment will be amplified in the next chapter by a discussion of several individuals and institutions. All affected my life when I was seeking my way.

Acknowledgments related to quadratic dynamics

THIS CHAPTER BEGAN AS A SINGLE PAGE TO ACKNOWLEDGE my indebtedness to three individuals: Mandelbrojt, Douady, and Hubbard. But it grew and — unavoidably — became increasingly autobiographical. It even extended the scope of the word "acknowledgment" by commenting about Bourbaki, my Nemesis.

When first mentioned, key names are set in bold, **roman** or ***italics***. Some background is provided for those unfamiliar with the history of mathematics, especially in France in the middle of the twentieth century. A more extensive background is found in Chapter 25.

1. Szolem Mandelbrojt (1899–1983)

Above all, to amplify this book's dedication, I am endlessly indebted to my uncle Szolem, a noted mathematician who reached the Collège de France, the top of French academia, when he was thirty-eight and I was thirteen. So I always knew that science was not just recorded in dusty tomes but was a flourishing enterprise, and the option of becoming a scientist was familiar to me as long as I can remember.

Brilliant, bold, and ambitious, he left his native Poland for France at age twenty, as an "ideological" refugee repelled by the excessively abstract "Polish mathematics" then being invented by Waclaw Sierpiński (1882–1969). He was, to the contrary, attracted to the mathematical school that ruled Paris in the 1920s, one linked with ***Henri Poincaré*** (1854–1912). He became close to ***Jacques Hadamard*** (1865–1963) and Vito Volterra (1860–1940), the period's most influential mathematicians in Paris and with the perception and analysis of broad mathematical patterns. Indeed, upon close examination we see that this new orientation, made possible only by the divorce of mathematics from its applications, has been the true source of its tremendous vitality and growth during the present century."

Once again, mentioning those persons and opinions gives the word "acknowledgment" an unusually broad meaning. Their influence on me has been enormous, since keeping away from them, and later assisting in their ideological demise, became an important aspect of my life and in particular of the work described in this book.

3. Two days at Normale and the continuing fallout

The quotation from M. Stone in Section 2 reminds me of an attractive and revealing autobiographical essay. In Hewitt 1990 we read that "From Stone and his fellow mathematicians at Harvard, I learned vital lessons about our wonderful subject: • Rule #1. Respect the profession. • Rule #2. In case of doubt, see Rule #1."

My uncle also had a deep respect toward his profession. His private reservations about Bourbaki did not in the least extend to École Normale. He passionately wanted me to go there. I did, but the next day walked out. He was bitterly disappointed, constantly worried about my future, and only when close to death did he stop asking me, "But why?"

Because, giddy as I was with surviving the war and passing those exams, prestige and authority did not affect me sufficiently. That strong–willed institution was absolutely the wrong place for a strong–willed person who dreamt of helping unscramble the messiness of nature. To me, the eye and geometry were not mere keys to acrobatic exam scores. I worshipped them, therefore turned down a golden opportunity to "outgrow" them so as to become a "true mathematician." Instead — or so it seems — I went on to prepare myself to sniff specks of dust on bad quality computer pictures.

In Paris, my leaving École Normale for École Polytechnique created a durable scandal. Had I never registered, my life might have been easier. On the other hand, the Directeur and another alumnus of École Normale attended my seventieth birthday celebration in Curaçao. As a joke, they "appointed" me as an "honorary freshman for life."

What I wanted to do in life was incomprehensible to others but surprisingly clear to me in 1945. Thus, as I near eighty, it is a deep privilege to observe the following. Having failed to prove any difficult theorems is not for me a source of any regret, and whatever I accomplished is roughly