Preface

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Holomorphic dynamics is a subject with an ancient history: Fatou, Julia, Schroeder, Koenigs, Böttcher, Lattès, which then went into hibernation for about 60 years, and came back to explosive life in the 1980's.

This rebirth is in part due to the introduction of a new theoretical tool: Sullivan's use of quasi-conformal mappings allowed him to prove Fatou's no-wandering domains conjecture, thus solving the main problem Fatou had left open.

But it is also due to a genuinely new phenomenon: the use of computers as an experimental mathematical tool. Until the advent of the computer, the notion that there might be an "experimental component" to mathematics was completely alien. Several early computer experiments showed great promise: the Fermi-Pasta-Ulam experiment, the number-theoretic computations of Birch and Swinnerton-Dyer, and Lorenz's experiment in theoretical meteorology stand out. But the unwieldiness of mainframes prevented their widespread use.

The microcomputer and improved computer graphics changed that: now a mathematical field was behaving like a field of physics, with brisk interactions between experiment and theory.

I mention computer graphics because faster and cheaper computers alone would not have had the same impact; without pictures, the information pouring out of mathematical computations would have remained hidden in a flood of numbers, difficult if not impossible to interpret. For people who doubt this, I have a story to relate. Lars Ahlfors, then in his seventies, told me in 1984 that in his youth, his adviser Lindelöf had made him read the memoirs of Fatou and Julia, the prize essays from the *Académie des Sciences* in Paris. Ahlfors told me that they struck him at the time as "the pits of complex analysis": he only understood what Fatou and Julia had in mind when he saw the pictures Mandelbrot and I were producing. If Ahlfors, the creator of one of the main tools in the subject and the inspirer of Sullivan's no-wandering domains theorem, needed pictures to come to terms with the subject, what can one say of lesser mortals?

In this preface, I will mainly describe the events from 1976 to 1982, as I saw them.

The first pictures

For me at least, holomorphic dynamics started as an experiment. During the academic year 1976-77, I was teaching DEUG (first and second year calculus) at the University of Paris at Orsay. At the time it was clear that willy-nilly, applied mathematics would never be the same again after computers. So I tried to introduce some computing into the curriculum.

This was not so easy. For one thing, I was no computer whiz: at the time I was a complex analyst with a strong topological bent, and no knowledge of dynamical systems whatsoever. For another, the students had no access to computers, and I had to resort to programmable calculators. Casting around for a topic sufficiently simple to fit into the 100 program steps and eight memories of these primitive machines, but still sufficiently rich to interest the students, I chose Newton's method for solving equations (among several others).

This was fairly easy to program. But when a student asked me how to choose an initial guess, I couldn't answer. It took me some time to discover that no one knew, and even longer to understand that the question really meant: what do the basins of the roots look like?

As I discovered later, I was far from the first person to wonder about this. Cayley had asked about it explicitly in the 1880's, and Fatou and Julia had explored some cases around 1920. But now we could effectively answer the question: computers could draw the basins. And they did: the math department at Orsay owned a rather unpleasant computer called a *mini-6*, which spent much of the spring of 1977 making such computations, and printing the results on a character printer, with X's, 0's and 1's to designate points of different basins. Michel Fiollet wrote the programs, and I am extremely grateful to him, as I could never have mastered that machine myself.

In any case, Adrien Douady and I poured over these pictures, and eventually got a glimpse of how to understand some of them, more particularly Newton's method for z^3-1 and z^3-z. At least, we understood their topology, and possibly the fact that we concentrated on the topology of the Julia sets has influenced the whole subject; other people looking at the same pictures might have focussed on other things, like Hausdorff dimension, or complex analytic features.

Also, I went for help to the IHES (*Institut des Hautes Etudes Scientifiques*), down the road but viewed by many at Orsay as alien, possibly hostile territory. There dynamical systems were a big topic: Dennis Sullivan and René Thom were in residence and Michael Shub, Sheldon Newhouse, and John Guckenheimer were visiting that spring.

I learned from Sullivan about Fatou and Julia, and especially about the fact that an attracting cycle must attract a critical point, and more generally that the behavior of the critical points dominates the whole dynamical picture. This suggested how to make parameter-space pictures, and the *mini-6* made many of these also. (These pictures are among the great-grand parents of the present volume, and whether the authors know it or not, they appear in the genealogical tree of most if not all the papers.) But having the pictures was no panacea: they looked chaotic to us, and we had no clear idea how to analyze them.

The year 1981-82: an ode to the cafés of Paris

At the end of 1977, I went to Cornell, where I thought and lectured about the results we had found. In particular, Mandelbrot saw the pictures that the *mini-6* had produced, and correctly calling them "rather poor quality," invited me to give a lecture at Yorktown Heights in 1978, saying that he had often thought about the "Fatou-Julia fractals," although he had never made pictures of them. But nothing much got proved until the next visit to France, for the academic year 1981–82.

By then, many things had subtly changed. Douady had understood that it was wiser to iterate polynomials before iterating Newton's method, as they are considerably simpler. His sister Véronique Gautheron had written programs to investigate the dynamics of polynomials. Computers had improved; Véronique used a machine, now long defunct, called a *Goupil* (later a small HP), but for me the arrival of the Apple II was decisive. Mandelbrot had access to the IBM computer facilities of Yorktown Heights; he had produced much better pictures of the Mandelbrot set than we had, and had published a paper about it. Feigenbaum had performed his numerical experiments, and the physicists were interested in the iteration of polynomials, more particularly renormalization theory.

Perhaps it was an illusion, but it seemed to me that holomorphic dynamics was in the air. Milnor and Thurston had long studied interval maps and were beginning to consider polynomials in the complex. In the Soviet Union (behind the iron curtain, very much in existence at the time) Lyubich and Eremenko, who were at the time just names we were vaguely aware of, were also starting to study holomorphic dynamics. In Brazil, Paolo Sad had produced a paper (hand-written) on the density of hyperbolic dynamics. The paper was wrong, and the result is still the main unproved question of the theory. (At the time I did not appreciate the importance of the result Sad had claimed, but I clearly recall coming back to our apartment on the Rue Pascal and hearing from my wife that Sullivan had telephoned all the way from Brazil with the message that Sad's paper "was coming apart at the seams.") But Sad's techniques led to one of the most important tools of the subject, the Mañe-Sad-Sullivan λ-lemma and holomorphic motions. In Japan, Ushiki had been an early advocate of computer graphics. His brilliant student Shishikura was beginning to take an interest in the field. In any case, the stage was set for a fertile year, and indeed 1981-82 was simply wonderful: I will describe three episodes, all of which occur in cafés, and all of which are somehow connected with computers.

• **The connectivity of the Mandelbrot set.** Mandelbrot had sent us a copy of his paper, in which he announced the appearance of islands off the mainland of the Mandelbrot set M. Incidentally, these islands were in fact not there in the published paper: apparently the printer had taken them for dirt on the originals and erased them. (At that time, a printer was a human being, not a machine.) Mandelbrot had penciled them in, more or less randomly, in the copy we had.

One afternoon, Douady and I had been looking at this picture, and wondering what happened to the image of the critical point by a high iterate of the polynomial $z^2 + c$ as c takes a walk around an island. This was difficult to imagine, and we had started to suspect that there should be filaments of M connecting the islands to the mainland. Overnight, Douady thought that such filaments could be detected as barriers: something had to happen along them, and found that is should be the arguments of the rays landing at 0.

In any case, Douady called the following morning, inviting me to join him at a café Le Dauphin on the Rue de Buci. He had realized that what we had discovered was that the Mandelbrot set was connected: over a croissant, he wrote the statement, $c \mapsto \phi_c(P_c(0))$ is an isomorphism $\mathbb{C} \setminus M \to \mathbb{C} \setminus \overline{\mathbb{D}}$.

Sullivan flew from the United States to hear Douady speak on our proof in the analytic dynamics seminar at Orsay that week. Sibony was also in the audience, as were Kahane and many others. The following week, Sibony announced he had an independent proof that the map proposed by Douady was a bijection.

Not long after, I made a list of all the quadratic polynomials whose critical point is periodic of period 5. Then I asked the computer (an Apple II, with a pen plotter attached) to draw all the Julia sets. Today this would be virtually instantaneous; at the time it took several hours. Then I looked really carefully at the drawings, trying to see what made them different from each other. After I marked the orbit of the critical point, in each case a tree was staring me in the face. A bit of reflection soon told me some necessary conditions a tree with marked points must satisfy in order to be a possible tree for a polynomial.

In a day or so I had drawn all the trees that could be drawn corresponding to the critical point being periodic of period 6. The fact that these did indeed correspond to the appropriate polynomials was strong evidence that the description was right. Not long afterwards Douady came up with the algorithm for external angles in terms of trees. The complex kneading sequence was born. These trees (now called Hubbard trees) together with external rays have now become a central tool of combinatorics and classifications.

• **Matings.** One night in the spring of 1982, I set the computer (the same Apple II, now equipped with a dot-matrix printer), to drawing Julia sets of rational functions of degree 2, running through a list of parameter values where the two critical points were periodic. Douady actively disapproved of this activity, thinking that we should focus on quadratic polynomials until they were better understood.

The next morning I had a pile of perhaps 40 such drawings (on those folded sheets with holes along the sides typical of dot-matrix printers), most of which looked like junk. But several evidently had some structure. I collected these, and met Douady at the local cafe (this time the café des Ursulines, near Ecole Normale, whose owner at the time was very welcoming to mathematicians). He looked at one of them, and after a while drew in two trees connecting the orbits of the critical point. One was the tree of the rabbit, the other the tree of the polynomial with the critical point periodic of period 4, with external angles 3/15 and 4/15. It was immediately clear that the picture really did represent this object.

This suggested many experiments to confirm the existence of matings, which we carried out; soon we came up with the mating conjecture: two quadratic polynomials can be mated unless they belong to conjugate limbs of the Mandelbrot set. But we had to wait for Thurston's theorem and the work of Silvio Levy, Mary Rees, Mitsu Shishikura, and Tan Lei, to see the mating conjecture proved for post-critically finite polynomials. As far as I know, there is still no reasonable mating conjecture in degree 3.

• **Polynomial-like mappings.** One of the great events of that year was Sullivan's proof of the non-existence of wandering domains for rational functions. Douady and I were both present at his first lecture on the subject, and immediately saw the power of his methods: invariant Beltrami forms and the Ahlfors-Bers theorem.

I had written my thesis (under Douady's direction) about Teichmüller spaces, and was quite familiar with these techniques, but had never thought of applying them to dynamics; Sullivan knew better. He had studied Ahlfors's work on Kleinian groups, and had realized that the same techniques could be used in holomorphic dynamics. In Sullivan's view, there is a dictionary relating Kleinian groups to holomorphic dynamics; the no-wandering domains theorem showed the power of this program. Trying to understand further parts of this dictionary has been an important motivating force for a lot of the research in the field, and more particularly Curt McMullen's.

In short order Douady and I used quasi-conformal mappings to show that the multiplier map gives a uniformization of the hyperbolic components of the interior of M, something we had conjectured but couldn't prove.

Soon thereafter, in some café in the north of Paris, Douady was ruminating about polynomial-like mappings. Of course, he didn't yet have a precise definition, but he had seen that if an analytic mapping mapped a disc $D \subset \mathbb{C}$ to $\mathbb{C}$ so that the boundary of D maps outside of D, winding around it several times, then many of the proofs about polynomials would still go through.

Thinking over what he had said, I saw that evening that if one could construct an appropriate invariant Beltrami form, then the polynomial-like mapping would be conjugate to a polynomial. This time I called Douady and asked him to meet me at the Café du Luxembourg, and presented my argument. My proof of the existence of the invariant Beltrami form was shaky, as I was requiring extra unnecessary conditions, but Adrien soon saw that if we got rid of these, the invariant Beltrami form was easy to construct. The straightening theorem was born.

The following year, back in Cornell, I was making parameter-space pictures of Newton's method for cubic polynomials. I saw Mandelbrot sets appearing on the screen, which wasn't really surprising, but was simply amazed to see a dyadic tree appearing around it. Looking carefully at this tree, I found that it reflected the digits of external angles of points in the Mandelbrot set written in base 2. Seeing that these angles were made by God, and not by man, was an extraordinary realization to me. I started my Harvard colloquium lecture two days later by saying I was changing its subject completely, because I had made an amazing discovery two days earlier. That colloquium was one of the best lectures I delivered in my life.

Quasi-conformal mappings have remained one of the central tools in holomorphic dynamics, eventually becoming a field in its own right called quasi-conformal surgery, and providing an amazing flexibility in chopping up and reassembling the very rigid objects of the field. Applications are too numerous to list, but Shishikura's sharp bound on the number of nonrepelling cycles of a rational function, and his construction of Herman rings from Siegel discs, stand out as early monuments to the power of this method.

That spring, I went to the United States for a couple of weeks and gave a number of lectures about these results, at Columbia and Cornell, and the SUNY graduate school. I had never met John Milnor, though I had practically been raised on his books. But he came to the Columbia lecture, and about a week later, back in France, I received a letter from him pointing out that the proof of the connectivity of the Mandelbrot set also proved that the external argument of real polynomials in the boundary of M is a monotone function of the parameter, and that this settled the entropy conjecture of Metropolis, Stein and Stein from the 1950's.

Another important event of that year for Douady and me was a Séminaire Bourbaki lecture in which Malgrange presented work of Ecalle on indifferent fixed points. The theory of Ecalle cylinders and the parabolic implosion grew out of that lecture. These results, and many others from that year, such as the existence of Julia sets that are not locally connected, and the connection of polynomial-like mappings with renormalization, are clearly part of the genealogy of all the papers of this volume; I would call them the grandparents of the present volume.

The Orsay notes

In the fall of 1982, I returned to Cornell. The situation was then as follows: a huge amount had been discovered, and largely proved. In particular, we had proved that if the Mandelbrot set is locally connected, then hyperbolic quadratic polynomials are open and dense, and we had formulated the MLC conjecture (the Mandelbrot set is locally connected). We had also constructed the combinatorial model $\overline{M}$ of the Mandelbrot set and the canonical mapping $M \to \overline{M}$, and proved that if MLC holds, then this map is a homeomorphism.

We had complete proofs of the combinatorial description of Julia sets of strictly preperiodic polynomials and polynomials with attracting cycles, although there were gaps for polynomials with parabolic cycles, which led to gaps in understanding the landing of rays at roots of components.

But our proofs were handwritten, mainly understandable only to Douady and me; next to nothing had been published. The only publications I can think of from that year were two *Notes aux Comptes-Rendus*, one by Douady and me on the connectivity on the Mandelbrot set and trees, and one by Sullivan on the no-wandering domains theorem.

Getting this material organized and written was essentially Douady's work: he gave a course at ENS and Orsay the following year, delivering each week the same lecture in both places and then writing it, using the notes of of Pierrette Sentenac, Marguerite Flexor, Régine Douady and Letizia Herault. Tan Lei and Lavaurs were in the audience, and become the first generation of students in this field. On my side, Ben Wittner and Janet Head were also first generation students in the field at Cornell.

I was not present for most of the writing of the Orsay notes, and am always amazed at the extraordinary new inventions present in the notes. The *tour de valse* was in our hand-written notes (where it was called the *lemme du coup de fouet*; this name was considered offensive by Pierrette Sentenac), but the *arrivée au bon port* and the local connectivity of the Julia set of $z^2 + z$ are among Douady's inventions of that year. Others contributed to the notes, including Lavaurs, who provided some of the central ideas of the parabolic implosion and Tan Lei with her resemblance between the Mandelbrot set near a preperiodic point and the Julia set for that point.

Other publications followed: the Mañe-Sad-Sullivan paper in 1983, and the polynomial-like mappings paper by Douady and me, in 1985. Together, I would describe these and the Orsay notes as the parents of the papers of the present volume.

Of course, the present volume has another parent: Bill Thurston's topological characterization of rational functions. Sullivan had linked holomorphic dynamics with quasi-conformal mappings, and Thurston invented another great new technique by linking holomorphic dynamics to Teichmüller theory. For a postcritically finite polynomial, there are 3 ways of encoding the dynamics: my trees, the external arguments of the critical values, and the Thurston class. Only the last extends to rational functions, and with this theorem it became clear that *post-critically finite branched mappings* provide the right way to encode the combinatorics of rational mappings.

The work after this is no longer history, but current events, and I leave an introduction to the present volume to Tan Lei.

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