INTERVIEW OF B. B. MANDELBROT
By Anthony Barcellos
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A. Barcellos: Were any people or events particularly influential in your choice of mathematics as a career, and the highly individualistic manner in which you have pursued it?

B. B. Mandelbrot: The most influential person was an uncle. His being a prominent professional mathematician affected me in contradictory ways. The most influential events were the disasters of this century, insofar as they repeatedly affected my schooling. It was chaotic much of the time. In 1929, when I was five, my uncle Szolem Mandelbrojt became professor at the University of Clermont-Ferrand, and I was thirteen when he moved up to the top, as the successor of Hadamard and a colleague of Lebesgue at the Collège de France in Paris. Therefore, I always shared in my parents' (surprised) awareness that some people lived by and for creating new mathematics. Hadamard, Lebesgue, Montel, and Denjoy were like not-so-distant uncles, and I learned to spell the name of Gauss as a child, by correcting a misprint by hand in every copy of a booklet my uncle had written. At twenty, I did extremely well in mathematics in some very difficult French exams, despite an almost complete lack of formal preparation, and my uncle took it for granted that his gifted nephew would follow right in his steps.

However, we had entirely different tastes in mathematics. He was an analyst in the classical style (he had learned French by studying Poincaré's and Hadamard's works, and he had come to Paris because it was the cradle of classical analysis), and I call myself a geometer. For him, geometry was essentially dead except in children's mathematics, and one had to outgrow it to make a genuine scholarly contribution. It seems I did not like the idea of growing up in this fashion. Therefore, my uncle's plans for me backfired. While he took no interest in my work and never ceased to wonder what "had gone wrong," we remained friends. But he had a largely negative influence on my work, therefore on my life.

At this point, the influence of my father became dominant. He was very proud of having already helped raise my uncle, who was his youngest (sixteen years younger) brother. My father was a very scholarly person, and the descendant of long lines of scholars. In fact, it often seemed that everyone in the family was – or was expected to become – a scholar of some sort, at least part-time. Unfortunately, many were starving scholars, and my father – being a practical man – saw virtues in a good steady job. His own work was to manufacture and sell clothing, which he did not enjoy. He strongly believed that a scholar's independence and happiness had better hinge on a steady income from a very different source, preferably one that would not be overly sensitive to the world's catastrophes. Thus, in the wake of World War I, he had hoped that his gifted brother would go into the desirable field of chemical engineering (John von Neumann's father had also wanted him to go into chemical engineering). Again, in the wake of World War II, my father feared my uncle's success was a fluke, and preferred to see me make a living as an engineer. Because of my distress at the reported death of geometry, and because of my distaste for the obvious alternatives in science, I yielded to my father's arguments, and let myself drift farther and farther away from mathematics.

Eventually, I came back. In fact (most unexpectedly), I made good use of the classical analysis I had read under my uncle's prodding. However, I was never imprinted with the normal way to be a mathematician, a calling whose rules exist independently of what any individual can do, and which provides peers with successful living role models to whom one should conform. Those who accept such a calling perceive the normal unpredictability of life as unwelcome perturbations whose effects must be compensated for; it takes more than even a war or other such catastrophe to change their way of operating. I, on the other hand, allowed myself to drift, and I soon came to view the normal unpredictability of life as contributing layers or strata of experience that are valuable, demand no apology, and add up to a unique combination. Hence perhaps the impression that I encountered more than the customary amount of randomness! Looking back, I must agree that it is hard to see how I managed to survive professionally, and to accumulate the proper school ties, without ever settling down in an established career. I made several attempts to settle down, but then accepted the inevitable: none of the existing careers fitted my growing cocktail of interests.

Of course, the reason why you sought me out for this interview is that I eventually brought my interests together, in a way that is now attracting attention. I conceived, developed and applied in many areas a new geometry of nature, which finds order in chaotic shapes and processes. It grew without a name until 1975, when I coined a new word to denote it, *fractal* geometry, from the Latin word for irregular and broken up, *fractus*. Today you might say that, until fractal geometry became organized, my life followed a fractal orbit. Ultimately, the surprise is not that my manner of practicing mathematics should seem individualistic, but that I should be generally recognized as a mathematician. For I am also a physicist, and an economist, and an artist of sorts, and...

AB: What was the actual course of your studies?

BBM: Without ever trying, I did very well at avoiding being overly influenced by schools. It all began way back, by my *not* attending grades 1 and 2. My mother was a doctor and afraid of epidemics, so she did her best to keep me out of school. Warsaw, where I was born and lived, had been hard hit by the depression, and my uncle Loterman, who was unemployed, offered to be my tutor. He never forced me to learn the whole alphabet, or the whole multiplication table, but I mastered chess and maps, and learned to read very fast.

We moved to Paris in 1936, and in 1937, I entered the Lycée, at the age of 13, rather than the typical 11. Lycées are secondary schools; at that time, their main role was to prepare students for the Universities. Then World War II came, and we went to live in central France, at Tulle near Clermont-Ferrand. To an older boy from the big city, the Lycée de Tulle was ridiculously easy, but several marvelous teachers from famous schools were also stranded there, and they gave me hard work to do. In effect, they tutored me, mostly in French and history. By the end of high school, I had caught up with my age group, which moved on to rather intensive mathematics with a first-rate teacher. Then, poverty and the wish to keep away from big cities to maximize my chances of survival made me skip most of what you might call college, so I am essentially self-taught in many ways. For a while, I was moving around with a younger brother, toting around a few obsolete books and learning things my way, guessing a number of things

myself, doing nothing in any rational or even half reasonable fashion, and acquiring a great deal of independence and self-confidence. In French education of that time, attendance was not that important, but exams were vital. So when Paris was liberated in 1944, I took the entrance exams of the leading science schools: Ecole Normale Supérieure (Rue d'Ulm) and Ecole Polytechnique. Normale, which was exclusive and tiny (a class of 30, half of them in the sciences), prepared university and high school professors. Polytechnique had classes of about 250 (one out of 10 applicants), and led to the top technical positions in the Civil Service, and to other extremely diverse careers.

The two sets of written and oral exams take a solid month – a test of physical stamina as well as learning. I passed both very handily. Everybody else had spent two or more years in special preparatory classes, a kind of cramming college, but I had only a few months of that drill, so my passing was considered very unusual.

I did not do well because of my skills at algebra and complicated integrals – these skills demand training and I had had little formal training – but because of a peculiar inborn gift that revealed itself, quite suddenly in my mid-teens. Faced with some complicated integral, I instantly related it to a familiar shape; usually it was exactly the shape that had motivated this integral. I knew an army of shapes I'd encountered once in some book or in some problem, and remembered forever, with their properties and their peculiarities. More generally, I could instantly find geometrical counterparts to almost any analytic problem. Having made a drawing, I nearly always felt that something was missing, that it was aesthetically incomplete. For example, it would become nicer if one were to add its symmetric part with respect to some circle or some line, or if one were to perform some projection. After a few transformations of this sort, the shape became more beautiful, more harmonious in a certain sense; the old Greeks would have called it more symmetric. At this point, it usually turned out that the teachers were asking me to solve problems that had already been solved by just making the shape more harmonious. Classmates and teachers who watched me play my tricks told me later that it was a strange performance. You might say this was a way of cheating at the exams, but without breaking any written rule. Everybody else took an exam in algebra and complicated integrals, and I managed to take an exam in translation into geometry, and in thinking in terms of geometric shapes. Moreover, it did matter to my overall ranking that I was skilled at drawing and that I could write good French, so it did not matter that the answers in physics and chemistry could not be guessed. That's how I got away with my "legal cheating."

At this point, my uncle had returned from the USA, where he had spent the war years, and family and friends held agonizing discussions about which career I should choose, and which school I should go to. We were surrounded by the ruins and the hunger of 1945, which figured significantly in my decisions. I started Ecole Normale (ranked first among those who entered), but with the intention of avoiding my uncle's kind of mathematics.

Unfortunately, the only alternative was to follow "Nicolas Bourbaki." In the 1920's, my uncle had been among the bright young iconoclasts who founded Bourbaki as a pleasantly jocular club; they planned to write a good textbook of analysis together (to replace the aging treatises of Picard and Goursat). But my uncle did not rejoin them in 1945, when they started a dead earnest drive to impose a new style on mathematics, and to recreate it in a more autonomous ("purer") and more formal ("austere") form than the

world had ever known. Thanks to my uncle, I knew they were a militant bunch, with strong biases against geometry and against every science, and ready to scorn and even to humiliate those who did not follow their lead. Bourbaki was one of several conflicting social movements that flourished after the War, when the yearning for absolute values was especially strong and widespread. Anyhow, having no taste for Bourbaki, I gave up on Normale after a few days, and went over to Polytechnique. My father was relieved. Since there were to be no electives, I was receiving the gift of time: it seemed that the need to make a firm choice was postponed until graduation, but in fact, I was never to be forced to choose.

AB: Why was that?

BBM: Initially, because of a legal mix-up. Polytechnique offered well-defined numbers of several favored positions to its graduates, and the students chose a life career on the basis of their weighted grade point average over two years. Everything was graded, or so it seemed. The competition for the top slots was ferocious and left no free time. (If France wants to dominate world chess, the easiest way may be to teach chess at Polytechnique!) I would have competed for the top slot, but the school's legal people thought – wrongly, as it later turned out – that there was a Catch-22 that disqualified me from competing. (To explain it would require a lecture on law and history.) Since my rank did not matter, I allowed it to erode slowly by not studying enough for a few dreadful exams. Instead, I felt a free man. A friend introduced me to classical music, which he said I absorbed like a big dry sponge. And I did lots of interesting reading of every kind. Many course notes had lots of appendices that no one else could afford to study.

Today, Polytechnique is without a permanent staff, and it borrows professors from outside, mainly from various Universities. The same has long been true of Normale. But in 1945 Polytechnique had its own professors chosen by its own committees. Some were moonlighting scholars from the University, for example Gaston Julia, some were alumni who had never done any research, and some were research people picked outside of academia's mainstream, for example Louis Leprince-Ringuet and Paul Lévy. Julia was a brilliant teacher, but well past his research prime. I spoke to him once; no one could have predicted that my destiny was to belong to the small band who, thirty years later, were to revive his theory of iteration of functions and bring it to full glory. The professor I was most aware of was the Professor of Mathematical Analysis, Paul Lévy. He was lucky that Polytechnique had given him tenure when he was a promising scholar in the early 1920's, because his way of doing mathematics and his choice of topics went on to become less and less popular to the mainstream. He had an extremely personal style, even in his basic analysis course. The course notes given to the students were at the same time rather leisurely and surprisingly brief, and they were the despair of many of my classmates because many facts seemed to be declared "obvious." But I found his course to be profound and in a way very easy; I may have been the only one who liked it.

Paul Lévy was nearing sixty. Suddenly, he was becoming famous; it was being "discovered" that he was a very great man in probability theory, and that this new field was a branch of mathematics. It had the good fortune to be built on the great Norbert Wiener's work on Brownian motion, and to rest mostly on the shoulders of two very different persons: Lévy and the very mainstream Andrei Kolmogorov.

Having learned basic mathematical analysis from Lévy, I was used to his style, and could read his research papers much more easily than almost anybody else. One sometimes had to guess what he meant. Many major difficulties were not tackled at all, but were swept under the rug, more or less elegantly. Many respected Ph.D. dissertations or articles consist in the proper statement and proof of a single "obvious" fact from Paul Lévy. Several years later, a would-be faculty advisor recommended a Ph.D. of this sort, but I never tried. Eventually, as fractal geometry came close to being implemented, I found myself fully involved in observing further "obvious" facts about diverse shapes and configurations drawn by chance.

AB: Were you Lévy's student?

BBM: No. Several people later claimed they had been his students, but Lévy specifically disclaimed having had any students. Besides, it took years before I came to be called a probabilist.

Polytechnique requires two years of study, ending roughly at the level of a strong Master's degree in the U.S. During the last term at Polytechnique, I kept looking for ways to apply my mathematical gifts and my growing knowledge, to real concrete problems in nature. My hopes were thoroughly romantic: to be the first to find order where everyone else had seen only chaos. Someone who heard me say so commented that my dream was to have been Johannes Kepler, but that the Keplers' days were over. Luckily, someone at Polytechnique felt ashamed of the Catch-22 that I have mentioned, and helped me obtain French and American scholarships for studying in the United States. Also, a professor suggested study under Theodore von Karman, who was finding order in the chaos of transonic flight.

AB: Is that when you went to Caltech?

BBM: Yes, for two years. But I found that Kàrmàn had left Caltech, and that the students of transonic flight had split into a group of engineers building big rockets, and a group of mathematicians doing mathematics. Caltech was home to many people I admire greatly, and many of my best friends are people I met there. But there was nobody at Caltech that I particularly wanted to emulate at that time.

So I went back to France, first into the waiting arms of the Air Force, which kept me for a year, and then to search for a suitable thesis topic. A book review found in my uncle's wastebasket started me on a task which was extravagant in every way: to explain "Zipf's law," which is a surprising regularity in word statistics. To many people, this topic looked almost kooky, but I saw a golden opportunity to become the Kepler of mathematical linguistics. My explanation of Zipf's law received much praise, but a few years later I abandoned this line of work, and later watched it go into a dead end; mathematical linguistics developed in an entirely different direction.

My doctoral thesis at the University of Paris was defended in December 1952. It had been written without anyone's assistance – and was poorly written. The mathematical linguistics in the first half was formally a very exotic form of the statistical thermodynamics in the second half (thermodynamics in a space of trees). Some people described the combination as being half about a subject that didn't yet exist (they were

right), and the other half about a subject that was no longer part of active physics (they were ill-informed). As the thesis had to be pigeon-holed, we decided to say it was in mathematics, but it was obviously very far from the reigning pure mathematics. For reasons linked to my official advisor's personal ambitions, the committee chairman was Louis de Broglie.

The title was *Games of Communication*, largely because for several years before and after my Ph.D. I was very much influenced by the examples of John von Neumann and Norbert Wiener. Indeed, Wiener's book *Cybernetics* and von Neumann & Morgenstern's book *Theory of Games and Economic Behavior* had come out during this time, and they were precisely what I wished to emulate. Each seemed a bold attempt to put together and develop a mathematical approach to a set of very old and very concrete problems that overlapped several disciplines.

Unfortunately, cybernetics never really took off, and game theory became yet another very special topic. Colossal claims had been made when there was little to support them, and they were not immediately shrugged off only because of the authors' renown, based on earlier and very different work. It soon became good manners in academia to laugh when someone mentioned "interdisciplinary research." To my bitter disappointment, I had to agree that there was good reason for laughter. I wondered whether things would have been better if von Neumann and Wiener had had the desire and the ability to take an active interest in their progeny.

I was the last man whom von Neumann sponsored at the Institute for Advanced Study in Princeton, in 1953-1954. It was a marvelous year, and I again made many life-long friends. I became aware of the computer – but years were to pass before I became fully involved with it. Unfortunately, von Neumann was not there very often. He was becoming more concerned with defense than with science. But it seemed that he was living proof that one could do science without really belonging to a "guild." In fact, he was under extreme pressure at Princeton. From there, he left for Washington and was not planning to return. Luckily, von Neumann had realized that, by having failed to claim admission to any guild, I was leading a very dangerous life. A foundation executive told me much later that von Neumann had specifically asked him to watch after me, and to help in case of trouble. I hope it was a true story.

The papers I wrote during these years were praised individually, and many anticipated later developments in diverse areas. They had a recognizable common style, but they failed to add up, even in my own eyes. Every so often, I was seized by the sudden urge to drop a field, right in the middle of writing a paper, and to grab a new research interest in a field I knew nothing about. I followed my instincts, but could not account for them until much, much later. Anyhow, to work in many fields was not harder than to work in "only" two.

I returned to France, and then married Aliette, in 1955. I had moved to Geneva to attempt a collaboration with Jean Piaget, when the French Universities suddenly started expanding very fast, and started looking for applied mathematicians. They gave me this label, and a professorship at Lille; in addition, Lévy sought me out to help at Polytechnique. But Lévy was about to retire, and Bourbaki was to take over. It seemed I was about to be crushed between them and other big academic blocs that could only think of their own narrow interests.

AB: Is that when you left academia for IBM?

BBM: Yes. I was there as a faculty visitor in the summer of 1958, and decided to take the gamble of staying a bit longer. My wife was not enthusiastic at all, but she agreed. I wrote to the Ministry of Education in Paris to request a leave of absence for one year. They failed to respond to my letter, and later told me that my request had arrived a few days before my tenure was to be granted in writing, so they simply dropped me off their lists. They said they would take me back if I wished, nevertheless my "leave" had become open-ended.

It took my wife and me a long time to accept the fact that I had been lucky, and that as an environment to pursue my devouring yet ill-defined ambition, IBM Research was far better than any university department in either country. Much of the practical consulting I did was informal, and some was very exciting and had far-reaching consequences, for example my work with Jay Berger on transmission errors in telephone links between computers.

In academia, on the other hand, Bourbaki was only the extreme form of a generalized phenomenon. Each of the old departments was working hard at that time to "purify" itself – that is, to narrow its scope. And each of the newly established departments was working hard at finding criteria to define yet another narrow combination of skills, which could be rewarded in grown-ups, trained in the young, and endowed with a slice of the job market. The emergence of "pure applied mathematics" and "pure mathematical statistics" brought particular discomfort to my life, at times. Finally, each field acted as if it were destined to live forever. For all these reasons, the notion of an academic activity that voluntarily reduces to one man's fancies had become inconceivable.

Everybody's ideal seemed to be sports. Competition is important in life, hence in science, but why should science imitate track, where the mile race and the 1500 meters race (only 7.3% shorter!) are often won by different champions? And worse, while the decathlon survives as an Olympic discipline, the scientific decathlon that I seemed to practice was not acceptable in academia. The granting agencies were divided in the same fashion. An energetic young fellow could always find support. But mavericks develop gradually and slowly, and maverick enterprises are better off if they develop slowly and solidly. A gradually developing maverick enterprise could find no comfortable room in academia.

For many years, every group I knew viewed me as a stranger, who (for reasons unknown) was wandering in and out. Luckily, the striking (and often shocking) news I was bringing could not pass unnoticed, and I was acquiring a kind of fame. I became very popular in many diverse departments as a visiting professor, but no major university wanted a permanent professor with such unpredictable interests. IBM, on the other hand, took the gamble of sheltering my one-man project when Ralph Gomory joined the Research Division shortly after me, as the manager of a small group. He continued when I returned after two years at Harvard and he became departmental director. Eventually, he assigned a programmer to my project. Then – when Gomory had become Director of Research, and my 1975 French book was nearing completion – I was made an "IBM Fellow" and given a small staff. A few dozen IBM'ers are designated as IBM Fellows "in recognition of outstanding records of distinguished and sustained technical achievement

in fields of science, engineering, programming and systems. They are given freedom to choose and carry out their work in areas related to their specialization in order to promote creative achievements." Thus, it was stated officially that my work had become widely respected, and that I could proceed in my very own way.

AB: You said that your whole life followed a "fractal" orbit until fractal geometry became organized, and you mention some of the turning points in the epilogue of your 1982 book. Could you tell us about these milestones?

BBM: My wild gamble started paying off during 1961-1962. By then, there was no question in my mind that I had identified a new phenomenon present in many aspects of nature, but all the examples were peripheral in their fields, and the phenomenon itself eluded definition. To denote it, the usual term now is the Greek "chaos," but I was using the weaker-sounding Latin "erratic behavior" at the time. The better word "chaos" came later from others, but I was the first to focus on the underlying notion, and to specialize in studying the erratic-chaotic. Many years were to pass before I formulated fractal geometry, and became aware that I had long been concerned with the fractal aspects of nature, seeking them out and building theories around them.

But let us go back to the year 1961. Starting that year, I established that this newly unearthed phenomenon was central to economics. Next, I established that it was central to vital parts of physical science, and moreover that it involved the concrete interpretation of the great counterexamples of analysis. And finally, I found that it had a very important visual aspect. I was back to geometry after years of analytic wilderness! A later turning point came when I returned to questions of interest to those in the mainstream of mathematics. Economics is very far from what I had planned to tackle as a scholar.

However, after I had become bored with Zipf's law of linguistics, I proceeded to read the rest of Zipf's works, and became acquainted with Pareto's law of income distribution. I believe my work contributed to the understanding of this topic, but my interest was also giving signs of becoming exhausted, when I happened to visit Hendrik Houthakker at Harvard, and saw on his blackboard a diagram that I had already encountered in the study of incomes. On the grounds that such geometric similarity was bound to be the visible symptom of an underlying similarity of structure, I inquired about the problem that had led my colleague to the diagram in question, and was told that it referred to the variation of stock market and commodity prices. I became fascinated with this topic, because it involved marvelous examples of unquestionably important quantities whose variation is very erratic, very irregular, in a word, chaotic. I soon came to distinguish two syndromes in price variation; sudden
jumps and non-periodic "cycles," which I later called *Noah* and *Joseph Effects*.

Price variation was becoming a source of worry to a few economists, because it was resisting being squeezed into the accepted econometric mold, which had simply been copied from the physical model of a gas in equilibrium. I, on the other hand, pioneered a radically different alternative approach, based upon self-similarity. This is a widely familiar notion today, largely due to the physicists' work on critical point phenomena, but that work came much later. I showed that the stochastic processes obtained via self-similarity generate sample functions that are very rich in configurations, and can account for a great part of observed price variation. During this period, I was doing things

analytically, with very few diagrams. The most influential of these diagrams has already been mentioned.

The next major series of related developments concerned noise, then turbulence and galaxy clusters. My investigation of so-called "excess noises" started with a very practical problem at IBM, but continued long after that problem was settled. Again, my solution was grounded on postulated self-similarity; it necessarily involved random forms of the Cantor set, and its description demanded Hausdorff-Besicovitch dimension. A friend, Henry McKean, Jr., had written his thesis on the Hausdorff-Besicovitch dimension of certain random sets when we both lived in Princeton. Otherwise I might not have encountered this notion. It was very rarely used at that time, but I discovered that it had an essential application, first to noise, then to turbulence, next to galaxy clusters, and so on to fractals in general. Incidentally, I met Edward Lorenz in 1963. His work on chaotic behavior in deterministic systems had just been published, but had yet not drawn much attention. Erik Mollo-Christensen predicted that it, and my early papers would turn out to concern two faces of the same reality. This hunch is in the process of being confirmed.

In the mid-sixties, however, my message was not getting through well enough to satisfy my ambition. Each context in turn elicited a complaint I had often heard in economics. "Granted that such and such statistical expression is known to converge in all the other fields of science, how can it be that my field (my interlocutor would complain) is alone cursed by the necessity of facing divergent statistical expressions?" "When all the other fields of science can be tackled by proven mathematical methods from familiar textbooks, why should my field necessitate newfangled techniques, for which the only references are dusty tomes written in French, or even in Polish, or incomprehensible modern monographs?"

One had to agree that these situations were paradoxical, but I thought this could have a very different origin. I kept observing that, in many applications, these familiar and unquestioned statistical and mathematical techniques had been oversold: in fact, they had failed to come to grips with the truly important problems, as they were perceived by the brilliant but nonmathematical practitioners whom I trusted. This failure could be accounted for, if the "ills" I had already diagnosed in a few fields were in fact of wide occurrence.

In any event, noise, turbulence, and clustering are complicated phenomena, and the repeated experience of unvarying resistance to my increasingly unified approach, and in particular, to my use of Hausdorff dimension as a concrete notion, made me wish and search for a simpler illustration. I stumbled upon coastlines, and proposed that the irregularity of a coastline be measured by its Hausdorff dimension. A few (astonishingly few!) scholars had noted in passing that the notion of a coastline's length is meaningless, but no one had done anything about it. Perhaps the finding embarrassed them. (For example, Lewis F. Richardson's thoughts on this topic were found after his death among irrelevant unpublished drafts.)

AB: Could you elaborate?

BBM: The question I raised in 1967 is, "how long is the coast of Britain?" and the correct answer is "it all depends." It depends on the size of the instrument used to

measure length. For example, look at Figure 1. It does not represent a true coastline, but a fractal fake, made (years later) as a "model" sharing the significant properties of a coastline. It is clear that, as measurement becomes increasingly refined, the measured length will increase. Thus, all the coastlines are of infinite length in a certain sense. But of course some are more infinite than others. To measure their degree of infinity, I thought of Hausdorff dimension, and I was indeed able to show that the notion of dimension of a coastline is meaningful, and that its value can be measured quite accurately. This is how I came back to my true love, geometry, and went on practicing it in a very strong and intense fashion.

AB: Do you have any opinion on the way geometry is handled in schools and in research?

BBM: Oh, very much so. The old French fashion required years and years of high school geometry. I found that totally intuitive and childishly easy, which of course is why I considered going into mathematics as a career. So I am distressed by how little geometry there is in American high schools.
     For quite a while, geometry was in effect banned from university curricula. Now, the availability of computer graphics and, to some extent – I think even to a large extent – my work, have made many people realize that geometric intuition can enter into seemingly very abstract domains, like the theory of Kleinian groups or the theory of iterations of mappings in the complex domain. In both fields, I discovered a number of facts using geometric intuition and computers.
     We have entered a period of intense change in the mood of mathematics. Increasingly many research mathematicians use computer graphics to enhance their geometric intuition, others cease to hide (from outsiders, or even from themselves) the fact that they had been practicing geometry. This return of geometry to the frontiers of mathematics and of physics should have an effect on the teaching of geometry in colleges, high schools, and even in elementary schools, because so much geometry which had been quite impractical can now be easily done with the help of computers.

AB: It's rather curious that high school geometry in America was advanced, for the most part, as being good logical training for the mind, emphasizing the deductive process rather than anything having to do with images or visual intuition.

BBM: That's also the way it was described in the old French curriculum, but for me geometry was something entirely different. Perhaps I should be careful of my frequent use of the leaden expression "geometric intuition," but it would be better to take "geometry" back from those who really do not care about it.
     Furthermore, school geometry did instill discipline, but I doubt its value as logical training. To a student, the reduction to the axioms is largely a matter of satisfying the teacher. We were told that certain arguments were "okay" and that others had to be transformed to be made "okay." I never had the feeling that this "okay" was intrinsic, much less that it was the last word in logic, but achieving it gave me good grades, while I was engaged in the truly important task of training my gift to be able to think directly in terms of shapes.

In any event, the geometry of yesterday has become dull and dry, but a combination of Greek and fractal geometry would be alive, attractive, and very useful. The young are very much dominated by the eye, through media like TV and computer games, and a combined geometry would be a way of getting to their minds before one tries to expose them to dry logical constructs.

The establishment of such a curriculum would be very complicated, and I don't have any precise constructive thoughts on the matter. The main value of the Greek geometry I learned in France was that the problems were hard, but not abstract. When friends ask me whether their 14-year old wonder kids should first take calculus or number theory, I put in a word for old-fashioned books on geometry written about 1900.

Anyhow, I think it is a fact that some people think best in formulas, and other people think best in shapes. A hundred years ago, this was almost a platitude among mathematicians, but people who think in formulas now run the show in every branch of science, and for a while they could not tolerate even one person who proclaimed he thinks in shapes.

It may have become true that people who think best in shapes tend to go into the arts, and that people who go into science or mathematics are those who think in formulas. On these grounds, one might argue that I was misplaced in going into science, but I do not think so. Anyhow, I was lucky to be able – eventually – to devise a private way of combining mathematics, science, philosophy and the arts.

Let me try out a simile. Imagine that a hundred thirty years ago a pandemic virus had wiped out man's ability to sing, but left him with the ability to write music. Great opera scores remained widely available; they became an object of cult and intense study, and great new scores were written by Verdi (Wagner, if you prefer). Generations later, one person, then a few persons were born with immunity to the virus, and eventually everyone was reminded that if Verdi can indeed be read, he can also be sung. *Everyone* rejoiced! Well, one of the privileges of fractal geometry has been to make the classical hard mathematical analysis of Verdi's time sing out at long last.

AB: Now that fractal geometry is, as you have written, "taking ominous steps towards becoming organized," at what level of the school curriculum would you consider its introduction appropriate?

BBM: Fractals should be introduced first in the presentation of the derivative. This notion first bothered me when I started calculus, and many people would understand calculus far better if they know at the very outset that a continuous function need *not* have a derivative. Until recently, the only counterexamples were artificial and contrived. One could not bother young minds with Weierstrass functions. But I have shown that nondifferentiable functions are essential in very fundamental parts of natural science, and it has become easy to tell a student that a coastline has no tangent and that the components of the motion of a particle along a coastline have no derivative. It is desirable to introduce such notions very early. They will serve as antidote against the ridiculous idea we have already discussed, that the study of geometry is primarily a form of logical training and not a way of learning to reason on shapes. Secondly, fractals should be taught early in fields like physics and in geophysics, where they are important. For a first step, it is best to try out several short specialized courses entirely devoted to fractals; this

is being done in several places. Later, one will see how fractals should be added to the basic courses on mathematical methods. To my pleasure and surprise, undergraduates accept this stuff very well. Another place where fractals are becoming important is in teaching computer graphics. It seems that every computer graphics demonstration includes fractals.

As to the place of fractals in the training of mathematicians, it raises two distinct questions. Falconer's forthcoming Geometry of Fractal Sets is welcome, but it is a conventional mathematical monograph: its topics are suggested by needs created by my work, but the style is dry, as usual in mathematics. It will not be the last mathematical monograph on these and related topics, but one cannot make any prediction about the teaching of advanced mathematics, because there are few advanced students of fractals at present, and the instruction they receive depends on fashion and the instructor's taste.

A much greater change would occur if the training of mathematicians were to go back to leaving some room for geometric intuition. Fractals may be taught around the theory of Kleinian groups or the theory of iterations of rational functions. These theories became unmanageable quite a long time ago, at least in part because of their lack of intuitive aspects. But with the advent of fractals, large parts of these theories become quite easy and widely attractive. They show very graphically how one can, or should concentrate on thinking about shapes as live "wholes," and learn to modify an algorithm to affect the shape it generates.

AB: Geometric shapes are being introduced through computer graphics in quite a few elementary schools through such things as Seymour Papert's LOGO and turtle graphics. What do you think of such things?

BBM: I have only a very superficial familiarity with them. It seems that they too overemphasize the algorithmic aspects. For me a circle is not primarily something which is traced by a turtle running a certain course, but above all it is a circle! However, according to friends, it is easy to draw fractals using turtle geometry. This is most welcome, and to the advantage of the field.

AB: What role does computer graphics play in your research?

BBM: It plays two major roles: to help my work develop, and to help it become accepted. To take an example, I had long known of a very simple algorithm which generates distributions of points in space, and I proposed this algorithm to model the distribution of galaxies, and of clusters and superclusters of galaxies. The first tests, both numerical and visual, were amazingly satisfying.But a long look at these distributions revealed discrepancies which had been much less obvious to the other students of the field, who used ordinary analytic methods. To try to improve the fit, I did not scan any repertory of alternative models, but my repertory of alternative shapes.

The virtue of computer graphics is that it makes it easy to compare a model's imitations to the natural shapes from many viewpoints simultaneously, including some viewpoints that had not yet been formalized or recognized. I encountered the first example when I was interested in the long-term persistence that has been observed – and even mentioned in the Bible! – in the discharges of rivers like the Nile. A statistical

theory was of course available to represent this phenomenon, but no one had actually thought of looking at the sample functions which this theory generates. It turned out to be surprisingly easy to convince every hydrologist that these samples did not look at all like the records of river levels, whereas the corresponding curves drawn according to my alternative recipe could not be distinguished from the real ones, even by the experts, unless they knew the particular river intimately. Thus, computer graphics allowed the elimination of certain theories simply on the basis of the obvious unreasonableness of the shapes they generate. The same trick worked even better with coastlines and mountains. Graphics techniques gradually became better and better, and we could afford to do some fancy stuff.

One must say "we," because I do not program computers myself, but have found ways of working very interactively with several outstanding people: students and assistants, and also colleagues like Richard F. Voss. As a matter of fact, I developed a skill for helping "debug" programs that I cannot read, by analyzing the wrong pictures these programs produce.

AB: Did computer graphics play a role in your work on turbulence?

BBM: No, but geometry has been essential. The big question in the study of turbulence is: how does turbulence arise from the differential equations for the flow of fluid? The literature on partial differential equations is tremendous, but it never got even close to tackling the questions raised by turbulence. In 1963, the situation was that Kolmogorov (and other Russian scholars) had written down formulas concerning the intermittency of turbulence. With hindsight, one may say that they were only one step removed from introducing Hausdorff dimension, then fractals. But they failed to take these steps; in fact, they resisted them for a while after I took them. At the very same time, and quite independently, I had tackled intermittency in the context of noise, and had developed a very geometric mental picture of it. Upon seeing the facts about turbulence at a Harvard seminar Robert Stewart gave in the fall of 1963, I found it obvious that my methods could be translated wholesale in these new terms. Then I tried to explain the validity of the translation. This led me to conjecture that turbulence represented a singularity in the flow of fluids, and that this singularity is concentrated on a fractal. To form my intuition, I had looked at paintings and photographs of turbulence, had looked at records of velocity, and even listened to them (after transposition into the audible range), and used the summaries of measurements (such as power spectra) as further evidence.

This was an entirely new approach to the problem; for many years this work was viewed as exotic and even bizarre, and it took ten years to be published – not counting abstracts or allusions. But my hunches on the Hausdorff-dimensional properties of the singularities are in the process of being confirmed in several different ways. It was quite beyond my skills to prove them, and to guess what had to be proven was beyond the geometric imagination of those who provide the demonstrations. Very great minds had tried to tackle turbulence by analytical techniques; they did not succeed, while it seems I succeeded by looking at turbulence via the shapes that it generates.

AB: Kleinian groups and iterates of rational functions were reputed to be highly technical mathematical topics. When and why did you become involved?

BBM: In 1976, after I had read Hadamard's superb obituary of Poincaré (which everyone will soon be able to read – and should read – in an American Mathematical Society book on Poincaré). This obituary made it apparent that my work should be extended beyond the linearly invariant fractals, to which I had restricted myself up to that point. Indeed, the limit sets of Kleinian groups and of groups based upon inversions are fractals also; the latter could be called self-inverse. This forthcoming extension of self-similar fractals was mentioned in a last-minute addition to the 1977 Fractals, and then I set to work, namely, to play on the computer in order to acquire a "hands-on" intuition. The payoff comes very quickly, in the form of an explicit construction algorithm for the self-inverse limit sets. It took me longer to ascertain that, to my surprise, I had solved a problem that had stood for one hundred years.

A short step, then, brought me to some old work of my former teacher Gaston Julia, and some work of Pierre Fatou. My uncle had once lent me the original reprints of Julia's paper that everyone called "the celebrated Prize Essay" and of Fatou's equally long work on the iterates of rational functions. The "Julia set" of a rational mapping of the complex plane isthe repeller set of this mapping, that is, the attractor set of its multivalued inverse. I started playing with fairly complicated mappings, and was amazed to discover that sets that Julia and Fatou had characterized negatively, as being pathologically complicated, were in fact of extraordinary beauty. When they first emerge on the computer screen, they seem totally strange, but one soon comes to feel one has always known them. An example is shown in Figure 2.

AB: What about the "Mandelbrot set." Is it also a fractal?

BBM: Yes, it is. A few months of mindless fun with complicated mappings had prepared me for a detailed study of iteration. I decided it was best to start with the simplest mapping, which is the second order polynomial. There is only one significant parameter, and there is a Julia set corresponding to each parameter value. I drew the set of parameter values such that the corresponding Julia set is not a "Fatou dust," but a connected set everyone likes to call "dragon." It is illustrated in Figure 3. To paraphrase a famous phrase, this is the set of which I have the honor of bearing the name.

(Outside of France, this last phrase may be completely unknown. So let me retell the anecdote on which it is based. The Genevese-Parisian mathematician J. C. F. Sturm was famous for his modesty, but his teaching created many occasions where he could find no way of avoiding mention of the Sturm (-Liouville) equations. When pressed, he described them as "the equations of which he had the honor of bearing the name."

My study showed that this set is an astonishing combination of utter simplicity and mind-bogging complication. At first sight, it is a "molecule" made of bonded "atoms:" one shaped like a cardioid, and the other nearly circular. But a closer look discloses an infinity of smaller molecules shaped like the big one, and linked by what I proposed to call a "devil's polymer." Don't let me go on raving about this set's beauty.

AB: The Cantor set and nowhere differentiable functions have long been important examples in analysis and topology. Is your term "fractal" well accepted by now?

BBM: Yes, but with exceptions. Some mathematicians speak of my work as "generalized Cantor sets" or "concrete applications of the counterexamples of analysis" – these terms underrate the drastic novelty of my endeavor, and really imply that there is no unity to it.

AB: They still regard them as pathological?

BBM: That's one part of it, but mathematicians' lack of perspective can be breathtaking. Some would go so far as to call the whole of physics "a concrete application of harmonic analysis and differential equations"!

One must also be aware that mathematicians have a strange traditional way with words. Indeed, take the theory of "strange" attractors (which are a kind of fractal). To my great surprise, this use of "strange" does not bother anybody. Mathematicians like to take a familiar term, to turn it around, and to use it with a very different meaning. For example, the vocabulary of mathematics is full of terms like "ring," "field," "complex,", or "imaginary." The words "distribution," "irregular" and "singular" are used with hundreds of different meanings. Mathematicians rarely coin new words, and the new words they coin are hardly ever graceful.

This may also be why the word "fractal" is already being used in meanings different from mine, and which I consider confusing. For example, a topologist like James Cannon needs a clearly different notion, but the word "fractal" exists, so he uses it. I fear that when this word is accepted it will be as ambiguous as "irregular", but not much can be done about this. One must just let time take its course.

AB: Despite the resistance to fractals that still exists in certain quarters, it's clearly been accepted and applied in many different fields. You yourself have been involved in most of these branches from the outset. Have there been recent developments, nevertheless, which have surprised even you?

BBM: I would say – with regret! – that there has been no major surprise since my work on Kleinian groups, and iteration in 1978-79. What is surprising is that fractals have attracted few crackpots. Also surprising – and more significant – are the quality of the applications, the rapidity of their development after the inevitable initial resistance is broken, and the total absence of outright failures. Additional phenomena that were clearly worth looking into, but could have been complicated and messy prove to be comparatively civilized when tackled. Fractal geometry works, which is one reason for its growing popularity. But no new application seems to come out of the blue.

Another surprise is that the additional techniques and concepts which are needed keep coming in the same sequence in each study. For a long time, the role of fractal dimension had to be emphasized because it was the principal concept, and I was glossing over difficulties that require additional parameters. Then I introduced a second parameter, "lacunarity." It's very surprising that in many fields the demand for lacunarity materializes shortly after dimension has been fully understood. In this sense things have proceeded alike in different fields. As a matter of fact, scientists' reactions to fractals, both positive and negative, seem to be very much the same in all fields.

AB: In addition to providing useful tools in various fields, haven't fractals become a field in their own right?

BBM: My work was inspired by a strong belief that the division of science has been extremely harmful. On the other hand, new fields keep being created whenever a human activity wishes to insure or increase its chances of survival. We must wait and see.

AB: You mentioned that you weren't attracted by Bourbaki's antigeometric approach toward mathematics. Did you find that the Bourbaki influence posed a significant obstacle to acceptance of your fractal approach?

BBM: Bourbaki loomed large in my life in 1945, when I left Normale, and again in 1958, when I left France, but very little since then. They did not prevent me from doing my own thing, because for many years my audiences were sheltered from their influence, or did not know they existed. After the study of turbulence had inspired me to an isolated bit of "pure" harmonic analysis, I was able to team with Jean-Pierre Kahane, not in the least a member of Bourbaki, to complete and continue that work.

Furthermore, by a fluke, the timing of my books was perfect. They came out when the feeling was beginning to spread that the Bourbaki *Foundations* treatise, like a romantic prince's dream castle, was never to be completed, and that their older books would never fulfill their proclaimed goal of becoming the universal standard of mathematics. The Bourbaki constitution, phrased to insure that the group would remain eternally a cohesive young rebel, was – of course – not working. In a way, the whole enterprise had become boring. The pendulum was therefore beginning to swing back from this extreme to an uneasy but more reasonable balance in mathematics, and my manner was becoming less threatening. If I had formulated fractal geometry much earlier, Bourbaki could have been a major obstacle. But today they do little beyond sponsoring a seminar in Paris. As a matter of fact, I may have benefited from the backlash against their old arrogance.

Besides, one of the current leaders of Bourbaki, Adrien Douady, has spent the last several years developing ideas on iteration that I had pioneered; to welcome him has been a treat.

Finally, one of the founders of Bourbaki, Jean Dieudonné, has published various demonstrably wrong statements about the meaning of mathematics, which were of great help in making some of my main points. For example, he wrote that a Peano curve is so counterintuitive that only logic can comprehend it, and no intuition can be used to understand its properties. That was demonstrably wrong. Today the Peano curve is viewed as completely intuitive, because my work made it so. And I have the feeling that when Dieudonné heard the story, he was not hostile, but amused.

AB: Why did it take you so long to get what you call your "fractal manifesto" in print?

BBM: Today, when the status of fractal geometry is compared with that of other maverick enterprises, like cybernetics and game theory, it seems that fractal geometry has benefited from having developed relatively slowly – granted that I am alive and well to watch its coming of age.

Of course, I had intended otherwise, but science is organized into tight branches, and the only assured way to leave a mark on a branch is to visit it in person, so to speak. This demands adaptability on the part of the visitor, and takes enormous amounts of his time.

Linguistics allowed me to mention thermodynamics, but only because linguistics was not yet organized as a modern profession. When working in economics, I was similarly dying to be allowed to make it known in my research papers that my methods were part of a general philosophy, of a certain approach to irregularity and chaos, and that they also mattered in physics. Invariably, the referees asked me to take these statements out, and I ultimately decided to comply. Later on, I went on to study turbulence (which had to resemble the stock market, because the weather and the stock market are equally unpredictable), and again I wanted my papers to appear in the most prestigious specialized journals. Again, the editors forced me to cut out what they scorned as "dubious philosophy," and to give more formulas and more details on the manipulations. In each case, I was pretending to be a technician in the field, which was never completely successful because I always kept a strong "foreign accent," which was necessary and sufficient to get my papers accepted by the good journals.

These papers were excruciatingly difficult to write, nevertheless in many cases my foreign accent still gave them the reputation of being difficult to read. Also, I did not really learn to write English until tutored by my IBM office neighbor, Bradford Dunham in 1968. In any case, those who followed me invariably came up with their own renderings of my ideas. (One of them called himself a "master at repackaging," and all had great academic careers.

In the meantime, turned-down would-be prefaces to my papers were piling up. Moreover, several papers failed to gain acceptance by referees, and the hassle made me accumulate drafts that did not seem worth finishing. Many come in handy these days: when the scientific public becomes interested in a new topic concerning fractals, I often have an old draft that can be revived into a paper. But it is really too bad that I never published the "tentative fractal manifesto" read at the 1964 Congress of Logic and the Philosophy of Science in Jerusalem. It would have appeared in an unsuitable place, but it would have been in the record. Interdisciplinary *Science Reviews* will publish it after a twenty year delay.

Friends started telling me that I could not continue in this fashion: contrary to cliché, I would perish if I *continued* to publish as I was doing. Moreover, my work on galaxies was not to become acceptable until it was known, and would not become known until it was acceptable. Ten years ago, I was on sabbatical in Paris. The fear of nepotism had vanished because my uncle had retired, and the College de France could invite me to give a major talk. I saw a golden opportunity to present a general manifesto, and to explain how my different interests fit together. Preparing this talk revealed that my work was already more complete and more homogeneous than I myself had known it to be! My lecture of January 1973 was described by a friend as the most autobiographical scientific talk he had ever heard. It was received with much praise and no hostility whatsoever, which made me realize that my years in the wilderness were about to end. To denote my unified approach, I soon coined the term *fractal,* and the expanded text of this lecture in Paris became my French book, first published in 1975, and soon to be reissued in a slightly refreshed second edition.

To summarize, until 1973-1975, my "political" situation as an outsider in all the fields in which I was working was not strong enough to allow me to assert my philosophy and my interdisciplinary approach. Circumstances forced me to play games which I didn't believe in. The 1975 book marked the change from this piecemeal approach to the present unified approach. Soon afterwards, fractal geometry became organized. My way of life changed profoundly. You might say I became the slave of my creation.

AB: It seems to me that fractal techniques have been embraced fairly readily in the natural and physical sciences like fluid mechanics, astronomy, physics, and geomorphology, but what about other places where you have pointed out applications, like economics or linguistics? Are those techniques being used today by practitioners in those fields?

BBM: In linguistics, fractals will not revive. My early work was important to me but peripheral to the field. However, the mathematical procedures I devised for this purpose continue to survive in other guises.

But in economics, the rebirth and growth of fractal techniques can only be a matter of time, because the two main questions which I investigated are truly unavoidable, and cry out for more work. Poincaré wrote that some problems are man-made and other problems pose themselves. Well, the role of discontinuity in economics and the degree of reality of business cycles are problems of the latter variety, and they will not disappear until answered. When a fractal theory really starts moving by itself I tend to become technically underequipped to continue to participate, and it becomes wise to move on. But in economics it is clear that I did not stay long enough. If I come back and describe some new things that can be done, this application ought to start moving again.

AB: You are often referred to as "the father of fractals," and you have been called "tirelessly imaginative." You have, however, always taken great pains in your work to give complete citations to all earlier research which was connected in some way to fractals. Your style of reference could leave one to believe that you are cataloging old results rather than creating new ones. What is your reaction to those who think that in some cases you merely pour old wines into new bottles?

BBM: This impression is totally without merit, but I understand how it can come to be held by some *very* casual readers. I also understand why it is held by some mathematical extremists, who refuse to acknowledge that to build new physics upon existing mathematics is a very creative occupation, and not merely an exercise in relabeling. This is the price I pay for being called a mathematician, hence for being judged in part by mathematicians.

Allow me a homey comparison. Nearly every theory in my books can be regarded as an evolved model of a "machine" that I conceived. When designing and building the first model of each of these machines, I acted like any other tinkerer, happily using many, many existing "spare parts," but using hardly any for the originally intended purpose. Yes, many of my contraptions had precursors, in the sense that the legendary Icarus was the precursor of the airplane!

Given the fierce competition that prevails among scientists, the custom under such circumstances is to be content with a casual footnote acknowledging that "an idea – or a tool – somewhat analogous to the author's had also been used for a different purpose in Refs...". But my upbringing and my years in the wilderness – when I had no roots in the present, only unconfirmed roots in a distant past – led me to make an arrogant choice. I decided to buck the custom, and to give full catalog references. Of course, these may fail to indicate the ultimate sources, yet I think that giving them helps establish that science is – after all – more than a fast-buck business. Furthermore, I do not mind being a scavenger, and I seek original references for parts I had – quite literally – picked from shelves of remaindered books and from other trash bins of science.

I am pleased to report that the new models of my machines mainly use specially designed parts, and that, among the many reviews of my books, only one or two brief ones are by casual readers who misinterpreted my gratitude to suppliers of reusable parts.

AB: You seem to have done little work on any topic that is not at all related to fractals.

BBM: This is largely correct. The only major exception was my work in the statistical foundations of thermodynamics, which I should one day take up again and make better known.

Clearly, this unity of purpose could not have been planned in 1945, or 1952, or even 1962. And everyone has fallow years every so often, and is usually tempted, on occasion – just to keep going – to follow the lead of some other drummer. But I was never tempted; I wonder why.

You must know the line by the Greek poet Archilochus that "The fox knows many things, but the hedgehog knows one big thing." Since the actual meaning of this line has been lost, it is quoted in many contexts. Thus, before a recent lecture, the chairman introduced me as being a hedgehog *par excellence*. I found this very touching and very appropriate.

One should also note that, within this unified thrust, my work has bucked the custom, and has become increasingly *more* "technical" in many ways. My old works seem somehow "lighter" than the more recent ones.

AB: Do you have any favorites among fractals, any examples that you particularly like?

BBM: The unavoidable example is that of coastlines (Figure 1), and my line that "Clouds are not spheres, mountains are not cones, coastlines are not circles and bark is not smooth, nor does lightning travel in a straight line" has gained the supreme accolade of becoming an instant cliché.

The fractal structure of the vascular system of blood vessels is also a fact that people first find quite astonishing, and then very natural. And many people have quoted my assertion that "Lebesgue-Osgood fractal monsters are the very substance of our flesh."

The self-squared dragon curves may be the best illustration of the fundamental and amazing discovery that extreme complexity can result from very simple formulas.

But I don't have real favorites, because I tinkered with each of my machines very hard and for a very long time. It's like with one's children. One may be proudest of those who bring the widest renown to the family, but each of my intellectual children has brought equal renown to the fractals family in its part of the scientific world.
And in any case one can love different children for different reasons, but one cannot really have absolute favorites.