***Candid Science IV: Conversations with Famous Physicists*.**
Edited by Istvan Hargittai,
London: Imperial College Press,
2004, 496-523.

Benoit B. Mandelbrot

Benoit B. Mandelbrot (b. 1924 in Warsaw, Poland) is Sterling Professor of Mathematical Sciences at Yale University, and IBM Fellow Emeritus at the IBM Thomas J. Watson Research Center, Yorktown Heights, New York. He is most famous for his pioneering work on fractals.

Mandelbrot's family emigrated to Paris in 1936 and survived the Second World War in Vichy France. He graduated as an engineer from the École Polytechnique in Paris in 1947, received his Master of Science degree from the California Institute of Technology (Caltech) in 1948 and his doctorate in mathematics from the Faculté des Sciences de Paris in 1952. Until 1958, he worked for the CNRS (National Center for Scientific Research) or Academia. Then he worked at IBM in the United States until he retired in 1993. At Yale University he started as Abraham Robinson Professor in 1987 and became Sterling Professor of Mathematical Sciences in 1999. He is a member of the National Academy of Sciences of the U.S.A., a fellow of the American Academy of Arts and Sciences, and a Foreign member of the Norwegian Academy of Science and Letters. He also belongs to other learned societies and has received numerous awards and distinctions. For the Barnard Medal for Meritorious Service to Science (1985) his citation read, "In the great tradition of natural philosophers past you looked at the world around you on a broader canvas." For the Franklin Medal for Signal and Eminent Service in Science (1986) his citation read, "For outstanding contributions to mathematics and the creation of the field of fractal geometry, and important and illuminating applications of this new concept to many fields of science." For the Wolf Prize for Physics (Israel, 1993) his citation ended by stating "He has changed our view of nature." We recorded several sessions of conversation in Stockholm, during the Symmetry 2000 Wenner-Gren symposium,[1] September 13-16, 2000 and what follows are edited excerpts from our conversations, together with last minute additions.

My parents were from Lithuania but I was born in Warsaw on November 20, 1924. In 1936, we moved to Paris and late in 1939, we moved on to Tulle in central France. Tulle is in a poor, mountainous area, where outsiders are not immediately accepted, but those who are accepted are accepted thoroughly. My uncle, Szolem Mandelbrojt, had moved to France many years before. When he was a professor in Clermont-Ferrand, he found an architect who was flattered to design a house for a university professor. Eventually my uncle moved to Paris to the Collège de France, the top of the French university system. But he kept his connections with the Tulle area, which became part of Vichy France.

Most of the French people who I came across during the war did not want to think about the events happening around them. In fact, a few were more interested in settling old accounts with the British. My family was lucky to be helped by the close friends of my uncle and also a Dominican and a Jesuit who had all kinds of connections. It also helped that I did particularly well in the high school, where my grades were better than anybody else's had been for a long time.

Foreigners were in grave danger and being a foreign Jew made matters worse. There were narrow escapes but until 1942 my parents found ways of handling the situation and we felt reasonably safe. Then the Germans occupied Vichy France and the situation changed from precarious to far worse. There was no question of attending college and I studied mathematics in old books that had many more pictures and explanations than the books of the 1930s.

Most of my Jewish friends tried to face events by staying together. We did not. I was supposed to be a member of a resistance group, which, however, never did anything. My younger brother, who was perhaps less adventurous, was with me most of the time and snap decisions often affected our survival. We were lucky. To survive we often had to gamble, and I have stayed a calculating gambler ever since, at least in my career choices. I also acquired a great deal of self-confidence.

It was best to move around. First, I became an apprentice toolmaker and proved to be very good at it. I had firm hands which were useful for the trade. Later, I raised horses in a small country castle.

In between, a stay of a few months in a school that prepared students for the elite French universities called "Grandes Écoles" revealed that I had an overwhelming geometric intuition. I also proved to be very good in drawing. When the war ended, I passed all of my examinations, in fact, became a kind of hero. The math professor could not resolve a triple integral in the big examination problem but I resolved it in record time by recognizing that in a better system of coordinates it became the volume of a sphere. When told, my professor could only say "of course, of course."

My uncle encouraged me to go to the extremely prestigious École Normale Supérieure in Paris. He argued that, even if I failed as a scientist, I could always become a respected high school teacher. My father was strongly against any civil service job. He thought that the Communists could come to power and I might flee again, perhaps ending up in Brazil where my diploma would be worth nothing. My mother had been a doctor in Warsaw but at age 50 (two years before Munich) abandoned her profession and became a housewife in Paris. My father was a scholarly man but much preferred professions independent of states and political change.

My father had no chance of winning against his brother, who was forceful, had a highly respected job, and moved in the best circles in France, so I took my uncle's advice. For a moment I was immensely proud of having outwitted Hitler and belong to École Normale. But my uncle had warned me that Normale was weak in physics and was becoming dominated by a very formalistic group of brilliant mathematicians calling themselves Bourbaki. I am anything but a formalist. Reality dawned and two days later I moved over to École Polytechnique.

IH: You must have had a strong mother.

BM: She was one of the first Jewish women to beat the restrictive "numerus clausus" at the Warsaw Medical School. My father went ahead to Paris and was very busy getting settled down and as a doctor my mother earned enough to raise us in Warsaw. But she wanted the family to reunite and Poland was becoming unlivable. She expected much of me and I was slow in developing. She lived long enough to be reassured, I think, but without this extraordinary piece of luck of bumping into fractals, I might have amounted to nothing compared with her expectations.

IH: Do I understand it correctly that your self-confidence originated more from early life experience than from later achievements in science?

BM: Definitely. By age twenty, my mother and early experiences had done a good job. When Nigel Gordon was preparing the film he made of my life (*Clouds are not Spheres.* Films in the Humanities and the Sciences, Princeton, NJ), he needed photographs of me as a child or young man. Putting them together, I was struck (so was also my secretary) at how decisive I looked even early on. Also, in every case, everybody recognizes me at first glance.

Your question reminds me of the day when an MIT dean I knew well stopped me in the corridor to report that everybody was calling me crazy because a rolling stone like me could gather no moss. But he felt that, to the contrary, my behavior was proving that I was the least crazy person he knew because I did not worry about who I was. I knew that independence would mean a harder life but I was prepared to pay for it.

My early work did not amount to much, but I always thought that it would become significant. So perhaps that Dean was wrong and I was crazy, after all. My first contributions to the study of difficult real problems came in the 1960s when I let myself become attracted to some very difficult problems of finance and of turbulence. I was well inspired in picking those problems and it was fortunate that I never distinguished among problems as belonging to either a high or a low-caste. I did not care what other people thought about the "intrinsic worth" of the problems I tackled.

There is something else I want to tell you. I had a peculiar education from beginning to end. For example, I started elementary school very late because, before I was born, my mother had lost a child in an epidemic and preferred to have me tutored by an uncle (not the one who became famous.) As a result, I never learned the multiplication tables properly or the alphabet. Because of this schooling, combined with the conditions of the war, I missed many standard topics. Many people are surprised that with such gaping holes in my education, I could be a university educator.

IH: For your Master's degree, you went to Caltech. There was an exceptional group of people there. One of them was Carleton Gajdusek who remembers your being an exceptional member of that exceptional group.

BM: Gajdusek was by very far the most exceptional young man there. By the time we were together at Caltech, he was already a full M.D. whereas I was a Master degree student. We became instant friends. Nobody is indifferent to Gajdusek. A minority admires him, but a majority has always hated him with a vengeance.

IH: Why?

BM: For performing so much better than most people in so many different ways and not hiding this fact at all. As you know, we could discuss Carleton for days. He went on to a brilliant and extremely odd career.

IH: He told me that you were close to his family.

BM: Yes. His mother was a formidable woman and no doubt Carleton inherited much of his "extravagance" from her. She lived in Yonkers between New York City and Scarsdale where I live. I used to go to see her often. After she suffered several strokes and moved to a retirement home, she was sadly diminished and I was one of the few people who visited her.

IH: Gajdusek told me about the sizzling intellectual life at Caltech .

BM: Indeed. After one year of a socially limited life in mathematics and aeronautics, I was told that a new postdoc had arrived with his wife from the Pasteur Institute in Paris. This was Elie Wollman. By and large, socially and intellectually, I began to live with Wollman's crowd around Max Delbruck. That is how I met Gajdusek who worked with fascinating people like Linus Pauling, Max Delbruck and George Beadle. Other members of the group were Gunther Stent, the geneticist and Jack Dunitz, the crystallographer. The post-war situation had brought together an extraordinary group of people, many of whom already had complicated lives behind them. François Jacob was not there, but somehow I associate his name with this group too. Not all the members of that group had been top students (although Gajdusek was). The Caltech students who became Nobel laureates were seldom Number 1 in their studies.

IH: What did you do at Caltech?

BM: My uncle kept telling me that it is important to have a "patron." I went to Caltech to study with a famous Hungarian. You will like this Hungarian connection! Unfortunately, Theodore von Kármán had just left for Paris although he came back for several visits during my stay. I greatly admired him and he was a genius with practical things but once he tore someone apart in front of me. The person was a fool, but Kármán acted in excess of what would have sufficed. He never married and his sister managed his household. She hated the German language and called herself de Kármán. When he was a professor in Aachen, Germany, they lived in Belgium and he commuted every morning across the border; in this way she lived in a French speaking country. Then they went to Caltech but as soon as the war ended, they returned to Europe and lived in an elegant hotel in Paris.

My two years at Caltech were wonderful and inspiring but professionally did not bring what I had hoped. What I wanted to do did not exist yet and I had to invent it for myself, which did not happen until many years later with fractal geometry.

IH: What did you do after Caltech?

BM: I returned to France for a year of service in the French Air Force. I could not study much but had amusing adventures, met interesting people, and discovered the opera. All this somewhat compensated for overall boredom. When I got out of the army the privilege of being an alumnus of Polytechnique was drawing me to join the French bureaucracy. Again, my father worked to prevent this from happening. He looked for job offers in a newspaper and found one from the Dutch company Philips. He liked very much the idea that if there was a revolution in France, that big international company would send me elsewhere. Philips was developing color television and wanted someone with my interests and training. So I went to work for Philips as a consultant. The other staff members did not know much mathematics, and I already knew all kinds of tricks.

At the same time, I started on my doctoral thesis. My uncle and other friends thought that my attitude I was irresponsible because I floated around and did not seek a patron. Finally someone agreed to write a report on my work. We did not like each other at all. I would have chosen Paul Lévy but he did not have the right to direct Ph.D. students. The regulations were incoherent therefore flexible. For me, this loose situation turned out to be far better than supervision by a maniac with an agenda, who would have forced me to accept a standard topic.

My Ph.D. thesis was very poorly written and no decent US University would have accepted it. But in my case it turned out that not having a patron was not all bad. In fact some calculations from that very old text remain useful.

IH: When did you get your doctorate?

BM: Late in 1952. By then, my father had died. My thesis was titled "Games of Communication," but really combined the two topics of statistical thermodynamics and power-law distributions. My ideas had what they call "long legs," since they developed in due time into something important. One can say that they really concerned fractals but of course without the word, which I did not coin until 1975, and without a clear concept of where I was going.

IH: And after the doctorate?

BM: My first teaching job was at the University of Geneva, Switzerland. Then France experienced a great sudden need for applied mathematicians and found that few people had the proper background so I could move to the University of Lille.
But I soon realized that my problems with the French educational system were more profound than not having a patron. The French complain that the Germans are better organized, but France remains the tightly-knit, extremely well-organized country fathered by Mazarini and Buonaparte. To survive as a maverick is almost impossible, which is perhaps why there is no French equivalent to the word "maverick" and why there are no "middle-level" mavericks in France. Two who rose above mere mortals were Louis Pasteur and Pierre Curie. Pasteur defied categorization by being a chemist and curing human illnesses and doing many other things at the same time. He did not let himself be pressed by the traditional expectations and he was carrying out his work amid constant attacks by the media. He was also badly treated by the French universities and was saved only when outsiders endowed an Institute for him. Curie was jobless until he received the Nobel Prize. René Descartes

was an earlier maverick who chose to work in Holland and after his death his works were put in the "Index of forbidden books." And French musicians are still baffled by Hector Berlioz.

IH: You still like to be in France.

BM: I can no longer stand the French university scene but I love the country and have many friends. We still have a very small apartment in Paris--my mother's former apartment with my mother-in-law's furniture. But I no longer try to work in France.

IH: Where do you feel yourself an insider?

BM: Nowhere. The situation was worse in France than it is in the U.S.A. I had said "No" to École Normale and later to an offer that looked glamorous but I knew would prevent me from bringing fractal geometry together. Each "no" was perceived as insulting.

In general, you don't change from being outsider to an insider, but you may become accepted if you are useful. This is what happened to Pasteur, Curie, Descartes, and Berlioz, who of course are now hailed in France as major figures.

IH: You joined IBM in 1958. Why did you leave academia?

BM: First by chance and then by necessity. By chance, because they invited me, accompanied by my wife and our baby, for a summer visit. In successive stages, this visit extended to one year, then three, then thirty-five. By necessity, because no better place was available and IBM worked out for me. The safe thing at that time would have been to keep my academic job in France. I was too little-known to create resentment. I might have lived peacefully and obscurely. I was extraordinarily lucky to escape that sad fate by going to IBM and then to Yale.

It took a while for my wife to feel comfortable in America. She was born in Paris, but her parents had also come from Lithuania, more or less.

IH: I wonder why IBM as a host institution was the right environment for your unorthodox activities.

BM: It is often said that scientific creation presupposes three elements: the right person, the right place, and the right time. This is largely so but, at least, in my case, those three elements depended on one another. In due time, IBM became a major scientific power and remained one until 1993. But when I arrived, it was starting out of nowhere and had a loose disorganized feel that for me was just right. For many years, the Bell Laboratories were better and better known and we followed their example. But Bell was overly organized for my taste; few people on its staff were oddballs and not part of a well-defined group.

At the time most scientists perceived taking a job at IBM a gamble. For me the main advantage was freedom. The availability of computers came second. You must be surprised, therefore I must elaborate. IBM did not market graphic devices, therefore it provided them to us with extreme reluctance. The computing center was meant to help design future products, not to carry out wild scientific experiments. Many other institutions had bigger computers and their graphics were far better. But they were used with little imagination and produced little of interest. To the contrary, the prevailing loose and free-wheeling mood adopted the IBM of my time into the right place and the right time for me and a number of other mavericks. They provided ample computer time during the nights and weekends. But they mostly provided an environment where I could befriend and cajole competent persons into custom building special instruments we needed. They connected parts that were lying around. Those instruments and their software were clunky but adequate for me and my very few associates. Otherwise my skills at visual thinking could not have been utilized.

That is, we did not do well because expensive tools were available, or because money was available to buy them and beat the competition. In fact, I needed a very limited budget and there was no competition of any kind. We did well because that straight-laced bureaucratic giant included some persons in positions of authority who realized that a few free-wheeling characters hardly showed on the big budget. Ralph Gomory, who was my boss at IBM for twenty years, eventually moved up to be number three in the corporation and always supported me. He reported to the Chairman of IBM who reported to God. I did mostly science, but also I did several things that saved money for IBM. First I was a member of the IBM staff. In 1974, they made me an IBM Fellow. That is what made IBM an ideal place for me and why I did not return to academia until much later when events forced me to retire.

The IBM Research Center was not planned to be what it became. What it became is due to fortunate circumstances that sound like a Greek tragedy, the fact that Thomas Watson, Jr. literally overthrew his father. The father's company was great at producing mechanical devices that worked forever. But in the fifties electronics arrived and the son faced a dilemma. Should he introduce the changes incrementally or start a brand new company within the shell of the old one? He decided to start a new company and it boomed. When I came to IBM, hardly anyone could imagine that a few years later, IBM would overtake RCA, a giant company manufacturing a broad range of products.

As Director of Research, Watson Junior hired Emmanuel Piore, who was born in Lithuania, got his Ph.D. around 1932 and had no real job until the war. Then he proved to be an extraordinary operator, but an operator with a vision and a soul. He organized the National Science Foundation and the Office of Naval Research as a fund-granting agency for pure research, and then built IBM Research out of nothing into a very large community. This started just when Sputnik went up and jobs had become so plentiful that the best group of scientists IBM could get included many "rejects" of the system. Some were justified rejects, many had career problems (not having a patron, or worse), and there were a few oddballs, like myself. IBM simply decided that something might come out of us and that the geographical isolation of Yorktown made it desirable to use us to build a small scientific community. The brilliance of Piore's vision was shown after the fact, when many of us became members of science academies and won prizes. Piore was prepared to take gambles and hired people prepared to take gambles as well. The same broad policy was followed by his most notable successor, who was Ralph Gomory.

IH: How did you get to the idea of working on fractals?

BM: There was a gradual process that began by exclusion. I had not liked the Caltech of Philips engineering and decided to return to science and obtain a Ph.D. One possibility was theoretical physics. However there was no role model in France and besides I did not want to work on anything related to the atomic bomb. I wanted to do geometry of the classical kind, but my uncle had convinced me that this topic was at a dead end. I hated algebra, so straightforward mathematics was out. I was an ambitious and rebellious idealist looking all around for some domain in a terrible mess to which I might try to bring mathematical order. All the while, my uncle was joking that my ambition was to become Kepler and I was late by several centuries.

But I did not think so because of something I had noticed. As the disciplines become organized, there is an increasing accumulation of observations that fit nowhere. I was free to look for potential treasure in dumps hidden in dark corners. Eventually, and against all odds I managed to clear several terrible messes and my romantic dream was completely fulfilled. For a person to be entitled to say so is truly overwhelming. An old man saying so is necessarily filled with awe. The process took a very long time and it is only recently that I began to see a system in my wild search. I realized that the fields I had excluded can be said to be ruled by smoothness, and my life work was to provide a theory of roughness.

IH: When you gave the world this new word "fractal," did you think long about it?

BM: A text I wrote in 1975 became the first book on fractals, but its original title was very clumsy. I gave the draft to Marcel-Paul Schützenburger, a dear friend and an extraordinarily brilliant man, who knew French thoroughly. He was a genius, but too much of a dilettante and so disorganized that he is already forgotten. The first thing he told me is that if I would try to peddle this **** (he used a French five-letter word), he would protest to my publisher because he knew I could write in proper French, therefore must do so. I re-read my work and realized that "Marco" (as we all called him) was right. That draft was full of anglicisms. Someone had to help restore my previous command of the language.

The second thing Marco said was that I had brought to life something that did not exist before, so I was entitled to give it any name. Roughness is a concept connected with fracture so I looked up a Latin dictionary and was reminded that the word "fracture" comes from the Latin adjective *fractus*, *fracta*, *fractum*, which means "like a broken stone". The Romans did not care much for abstraction; they used very concrete words. The proper neologism was the Latin word "fractum" but it rhymed with quantum. This made it pretentious, in bad taste. Next I hit on "fractal," which sounded good in French and English – and later in many other languages in which my book has been translated. It is now in every dictionary.

Of course, there were various linguistic difficulties. In Russian, should the last letter of "fractal" be hard or soft (fractal versus fractal') and what should be the gender? A committee decided that it should be the soft fractal' and it should be feminine. If you compare new scientific terms, "fractal" is less problematic than "chaos." Every old word continues to be burdened by its previous meanings but a new word has none. Of course, my word, fractal, has already attracted new meanings.

IH: So what is the mathematical definition of fractals?

BM: There is no single mathematical definition. However, even in mathematics everything that is important is difficult to define. If you don't believe me, try to define probability theory. Or note that "general topology" is defined as "the study of the notion of neighborhood." For general purposes, I follow this example and call fractal geometry "the study of roughness."

If pressed further, I add that it is a theory of self-similarity and related invariances. A self-similar shape is one that looks the same from nearby and afar. Every formal definition I know excludes something very important and I no longer look for a single all-encompassing definition. My first book gave no definition. Then I relented and gave a tentative definition based upon fractal dimension. But a prominent French mathematician, a formalist and a snob, criticized me sharply. He understood fractality and viewed it as a very fundamental notion. To the contrary, fractal dimension is a specialized notion that has many variants. Therefore he told me I should not define a fundamental notion on the basis of a specialized one. Physicists think differently. Fractal is a concept but fractal dimension is a number that quantifies roughness and can be measured empirically. Physicists detest concepts and worship numbers that can be measured.

An important question is whether or not fractal geometry will survive, whether or not it will continue to provide good service to science and economics. But this question has nothing to do with whether we can find an all-embracing definition.

IH: You created a new field, which defies traditional definitions and recognition. What are the highest awards you have received?

BM: The 1993 Wolf Prize for Physics and recently the 2003 Japan Prize.

The Japan Prize is in the field of complexity, a word that fairly describes the search that started in 1952 with my thesis for exciting facts thrown into diverse dumps hidden in dark corners.

The Wolf citation ends by saying that I "changed our view of nature." According to a rumor, the mathematics and physics sections were each looking for candidates that best fulfilled more traditional requirements, therefore kept sending my nomination back and forth. But then one section stopped worrying about definitions and selected me.

IH: Could it be that your receiving the physics prize surprise some colleagues?

BM: Perhaps so. But in my own view physics has always been broad enough to include much of my work. In fact, I had thought of presenting the same dissertation for a Ph.D. in physics. Let me comment on the situation today, first from a practical point of view and then from a fundamental one.

Consider my favorite early topics, finance, turbulence, hydrology, 1/f noises. At the time of my pioneering work (up to forty years ago!), none of these topics was attracting the attention of people calling themselves physicists. In this sense, society viewed them as being outside of physics. But in recent years each of these topics has been, how to say it, invaded by persons transferring from mainstream physics. One might say that the scope of physics has by now extended to include every one of my old favorite topics. This extension resulted from many social forces beyond my example. But the directions it took vindicate what I have been doing all along.

Let me now present a more fundamental second point of view. Historically, "physics" served as a common term to denote several activities, mechanics, optics, acoustics, thermodynamics, each of which arose in response to basic sensations like heaviness, light, sound, and hotness. For example, thermodynamics is the study of hotness. The sensation of taste (sweetness or acidity) gave rise to chemistry.

But what abut the sensation of roughness? It is as important as the other ones but had never been studied properly on its own. The first stage of every scientific approach consists in inventing a way to measure some vague sensation quantitatively and in the case of roughness, there was no quantitative measure until I developed fractal geometry. This would-be theory of roughness is very young and modest but is an unquestionable part of a suitably enlarged part of physics.

In my opinion, science has not reached its end, far from it, but it has reached a stage of sharply increased difficulty. Physics will have to recognize that it has a new frontier in the study of roughness. It seems to move slowly, not because of the shortcomings of the participants, but because the topics are intrinsically difficult. This is, of course, why they were left wide open.

The same is true of a less obvious concept related to roughness that I call "wild randomness." The bulk of known science is devoted to "mild randomness" and I introduced the distinction between mild and wild variability. It is somewhat parallel to the distinction that dynamical systems theory makes between non chaotic and chaotic solutions.

IH: At the age of 60, you went to Harvard University and soon resigned. What happened?

BM: I felt it was not the place for me. I was uncomfortable, did very little work, and lost sleep. Again, I took chances, followed my own instinct, and walked out. Harvard is the oldest university in America--a great one, no question about that--but to my taste too much of a collection of prima donnas. The faculty spend time fiercely competing for recognition, prizes, and academy memberships. For science, it is also a disadvantage that Boston is an extraordinarily interesting city. Dingy New Haven is much more prone to doing serious work. People can leave their doors open and the level of collegiality is very high especially among mathematicians. I am very happy at Yale.

IH: Do you lecture at Yale?

BM: Yes, of course, but only on a half-time basis. In particular, I am responsible for the introduction of an elementary course on fractals for non-specialists with over a hundred students attend it every year, because they like it, not because they are forced to. With few exceptions, the lectures are given by a colleague who is a superb teacher, which I am not. He spends endless hours with the students, which I could not possibly manage to do. What I am is an effective preacher and without me, the course would not have existed. That course is based on a simple idea. There are many ways of presenting the basic concepts of mathematics and it is the best way to begin with material that students spontaneously find attractive. This happens to be fractal geometry.

By now, many colleges have adopted this viewpoint. It is also rapidly spreading to high schools.

IH: Do you use grants?

BM: Of course. This is unavoidable. But I hate a nonstop search for contracts and prefer relative poverty. The impact of funding on the development of science should never be underestimated. Science is defined by its intrinsic value, but it also responds to external effects. Compare the support for atomic physics before and after the first atomic bomb. Statistical physics also became a huge field; then one day, IBM and Bell laid off most of their research physicists and funding became scarce. At Yale, I once had a group of seven people, but I found it too difficult to worry about their individual agendas. One of them was from Hong Kong where a job was waiting for him, whereas in the U.S. there was no job. I have cut down the size of my group, but I have visitors all the time.

Had I written about fractal geometry ten years earlier, I may have been inundated with money, and institutes of fractal geometry may have developed everywhere. So my timing was not very fortunate.

IH: Was it hard to gain recognition for your new field?

BM: Until 1982, recognition was slow, narrow, and very gradual. This made me very impatient but was natural. The risks I took did not only consist in joining IBM but also in interacting with fields in which I was way out of the Establishment. I would enter some existing community, provide my reasons why an approach they took could not possibly work, and propose a very different approach with no attempt to dilute its nature. For instance, economists were unfamiliar with the notions of invariance and symmetry. They found them ridiculous and an affectation. Each time I entered yet another existing field and met its practitioners it seemed that I was voluntarily walking in front of an eager firing squad. One good thing was that my job at IBM was solid.

Actual recognition began in 1962-1963 when I was a visiting professor of economics at Harvard University. Harvard did not mind that I did not have a degree in economics. That visiting position truly shook the world of finance and was an important sign of growing recognition for my work. Before that year had ended, the Harvard Division of Applied Sciences offered me a visiting professorship in 1963-1964. Those two years made me join the best company. But rough episodes at the University of Chicago, then at Harvard, made it clear that nobody wanted me as a regular professor.

I must add that at each stage I enjoyed enormous support from a few people. Take my main paper on turbulence and multifractals, which was invited in 1966 but only appeared in 1974. Why so? It took two years to referee the first version, because every possible referee said it was gibberish and contained nothing about turbulence. However, the editor, Professor Keith Moffatt of Cambridge, a brilliant man, advised me not to get discouraged. The second version took another two years and, the third version also took two years, and finally the paper appeared eight years from the time of its inception. If not for Moffatt, I would have diluted it out of existence.

This paper is now a classic but it was very difficult and few people could read about multifractals before they knew about fractals. To create an audience for it was truly a long-range enterprise. This is why a significant change in my career came when I decided in 1975 to write the book in French [*Objets fractals,* Flammarion, 1975] which I have already mentioned. For several years it sold poorly, but an English edition that followed did well [*Fractals: Form, Chance and Dimension*. W.H. Freeman, San Francisco, 1977]. Then came *The Fractal Geometry of Nature* [Freeman, San Francisco, 1982] which became an event. Everyone wanted to review that book. Taken together, those books and their many translations sold several hundred thousand copies, to which you may add hundreds of books of all kinds by other authors. My work had become very broadly recognized.

In 1985, I received the Barnard Medal, then other prizes came and many honorary doctorates from all over the world. Recognition grew and for a while something else came along. In addition to being a well-known and respected subject, fractal geometry became a fad. Every computer journal had articles about fractals. Fortunately, fads don't last and fractal geometry survived that period. I found it oppressive. However some people manifested an absolutely horrendous level of envy.

IH: Was it at IBM or generally?

BM: At IBM nobody felt threatened. Also, it is a collegial community and we made a point to nominate friends for prizes. Outside IBM, it was a different matter. When I received the Barnard Medal, someone warned me at Harvard that I would never be forgiven. Einstein and Fermi had received it in their time, so newspapers called me the second Einstein and second Fermi and whatnot. Of course, that was pure journalism, but it created enemies.

This is long gone. There was a special semester on fractals in 1999 at the Newton Institute in Cambridge. It was very pleasant, with no fights and most of the talks were about the mathematics of fractals. The mathematicians did not jump on the fractals bandwagon, far from it, but several of my conjectures turned out to be extremely difficult. As a result, many distinct groups are very much involved in different problems raised by fractals.

In physics, the story is different and change is faster. A large group of physicists jumped on the fractals bandwagon. Eventually, the feasible questions were solved. But the diffusion-limited aggregates (DLA) turned out to be very difficult to study. They no longer promise a fast academic career. However, physicists who had been involved with fractals find jobs nowadays in biology and finance and in other areas.

IH: You were the last student of John von Neumann?

BM: In 1953-1954 I was the last person at the Institute for Advanced Study in Princeton sponsored by von Neumann, another Hungarian. He was by then very much involved with defense but I met many other interesting people.

IH: Why did he invite you to the Institute for Advanced Study?

BM: I had sent him my thesis, which he found interesting. He sent word that I could come and see him any time. I visited him on a Saturday. We talked and he asked me if I wanted to come to the Institute for a year. I said that I would love to but it was late May and everything must have been settled for the next academic year. He told me that was not a problem but I should see Warren Weaver at the Rockefeller Foundation. I went there on the next Monday morning. Von Neumann had telephoned with a very strong recommendation and I did not even have to apply.

He was very encouraging. Once I gave a lecture at the Institute for Advanced Study. This lecture was terrible and after I finished someone attacked me bitterly. Oppenheimer jumped up, noted that my lecture did not present my work well, then in his own brilliant way, gave a summary of what I should have said. Then Von Neumann stood up and said that he too, was familiar with my work. He agreed that my presentation did not do justice to it then proceeded with warm compliments. On that great day I moved from giving the worst lecture to having both Oppenheimer and von Neumann as lieutenants.

Many years later at IBM, Gomory took off to do research and my situation suddenly became very bad. IBM policy was to never fire anybody, but my temporary boss was someone whom I despised and who handled me so terribly that I was afraid to be forced to accept some project that I would hate. I went back to Warren Weaver for advice and was told that when von Neumann was dying, he asked Weaver to watch out for me because I might get into trouble and need help. As a result, a fellowship was made available on the spot to enable me to visit any university of my choice. In the meantime, the man who had caused my trouble moved to another position, so my problems were solved.

Von Neumann was not exactly a warm person, so I was pleasantly surprised that he could be so completely understanding. Then I realized that despite his fame he was badly treated and unhappy at the Princeton Institute. The mathematicians found computers to be detestable. Some physicists disliked his strong involvement in military affairs. There was also the rumor that von Neumann had expected to be made director, but they elected Oppenheimer instead. This led to resentment. They had similar family and education backgrounds, and could have become either close friends or not friends at all. Von Neumann was very much an insider in Washington but no longer so in academia. In fact, he had accepted a position at U.C.L.A., but died before he could take it.

IH: Did you ever come across Erdös?

BM: I did. Many people admired Paul Erdös and so did I. My uncle thought that Erdös was not on a par with a Henri Poincaré or a David Hilbert because he did not open broad new areas. On the other hand, I am very well known for asking hard new questions. Some people joke that Erdös and I together would have made a truly wonderful combination.

IH: Did you come across Szilard, Wigner, and Teller?

BM: I met Edward Teller once at a meeting on thermodynamics and we had a good conversation. About Leo Szilard, something I wrote in thermodynamics revived and extended something he had done. I wrote him about a paper he wrote in 1925, but he was no longer interested in the topic. As to Eugene Wigner, I was very impressed when showed him the mountain-like fractal structures. His comment was "Very nice. Don't tell me how you did it. Let me guess." He thought for a while, mumbling aloud, and he guessed it right.

IH: Who are your heroes?

BM: Most have been mavericks. Among those I have met, von Neumann. Among historical figures, Henri Poincaré. More indirectly but perhaps even more strongly, Louis Pasteur before he became a national hero.

IH: Was Paul Lévy your mentor?

BM: Yes, in many ways. Not my teacher because in truth he never had students. Some of his very best work, not his most difficult but very imaginative, was completed in his late sixties.

IH: You also keep working at a late age.

BM: Yes, but now when I work too much by myself I get bored. I prefer to collaborate with some young fellow who works quickly, and can learn from me. I supply questions and most interpretations, but I am not up to the long and difficult calculations or experiments.

I see no age limitation to creative work of any kind. It was possible for Beethoven to stop composing for ten years and then start again and write greater music than ever. Giuseppe Verdi also stopped, then came back to write his best opera, *Falstaff,* at the age of 80 - and before that his second best opera, *Othello*, at the age of 75. The widely held thesis is that this could not happen to a scientist but I think that this thesis it is deeply flawed. In fact old age *per se* does not exclude creativity. Personal ambition may diminish, but not necessarily creativity, especially for people, like myself, who are prepared to constantly move and start something very different. I think that the importance of youth in science has been absurdly exaggerated.

IH: We have touched upon the question of categorization in science. Which category would be the most appropriate for you?

BM: My long scientific life defied and survived categorization. Several centuries ago, I would have been called a natural philosopher. I love the sound of this tem. Today, I am a living fossil, a throwback to ancient ways of being a mathematician which included physics in a broad sense and much else.

IH: Can we talk about "nationalism" among the sciences?

BM: During the 1960s and 1970s, physics and mathematics functioned in a totalitarian mode. Some official academia decided what was proper to do and tolerated nothing else. It was deeper than a question of funding. I am the least nationalistic of all scientists. In my mathematical mode, I speak with a foreign accent, but I am still a mathematician. When writing a physics paper, I am a physicist with a foreign accent. The main reason that I can do work in so many fields is that I am prepared to learn different tongues and do not worry about being unable to get rid of my accent. I do not doubt who I am and see no need to dress into a mathematician's cloth or a physicist's cloth or to put on an economist's hat to think as one.

IH: What is your main activity nowadays?

BM: I continue to write papers in mathematics, physics, and finance and I give many talks on art and music. But I mostly work on books. Together with a brilliant journalist, I am writing a book on finance. On the back burner I continue to compile somewhat idiosyncratic "Selecta" books. Three volumes are out and more are on the way. Half of each book is made of old papers of mine on a defined subject and the other half consists in new papers and discussions. I am also under great pressure to write memoirs. There are few interesting autobiographies by scientists because most appear to live unsurprising lives.

IH: Not you.

BM: Not me. My story has many elements of high drama. Many people think that I have a large number of co-workers but mostly I have worked by myself and created most of my ideas quite alone.

IH: Symmetry has brought us together. How did you develop your interest in symmetry?

BM: I heard Hermann Weyl lecture on the material of his famous book on symmetry. In physics, symmetry is connected with invariances and groups of transformation. I use this concept both in this sense but also in the ancient Greek sense, mentioned in Weyl's book, of harmony and balance. When I told you that I do not mind the fact that fractals remain ill-defined, I expressed the strong feeling that important concepts should never be defined narrowly. Therefore, I never worry about whether something is or is not symmetric according to a formal definition.

Much of science consists in a search for the proper invariance. Take something as messy as financial prices. There are many well-paid economists but economic knowledge remains very poor. In the early 1960s, I made great progress by injecting the notion that price charts are invariant with respect to certain semi-groups or groups of transformations called dilations or reductions. Those invariances and later generalizations I proposed led to three successive models that I now call mesofractal, unifractal, and multifractal. Clearly, symmetry is not merely a minor wrinkle in this work but a fundamental ingredient.

My position on these things has been controversial but now many physicists have moved into economics and they adopted my invariances quite readily.

IH: The Wenner-Gren Center, where the symmetry symposium is taking place, is very close to the hotel in Stockholm. But we have to cross a big square with a lot of zebra crossings and traffic lights, and a lot of traffic, and the crossing can be done in several different ways. I noticed that although you don't walk too fast, you get from the hotel to the meeting site faster than anybody else. Do you choose your route consciously?

BM: I found the most economical route almost instantly. My wife is continually surprised that when we go to a foreign city and walk a little bit around, we get back to our point of departure faster than would have been estimated from the distance covered. I create spontaneously a mental picture that can be very complicated. For example, Tokyo is not a simple city and many persons have a mental picture of the schematic map of the main subway lines, but not much more. Once my host was driving back to my hotel and got lost. I was able to volunteer as his guide.

In history, the career of several scientists was largely based upon visual acuity and memory. They are my heroes, of course. For example, Santiago Ramón y Cajal [1852-1934] was such a person. He is another of my heroes.

IH: I have a Spanish stamp with his picture. He was a Nobel laureate in medicine in 1906.

BM: An extraordinary story! He was born in Upper Aragon at two days distance (by foot or donkey) from the nearest city, Saragossa. First he became a country nurse, then a doctor. He had very old microscopes and his images were so poor that they could not be photographed, yet he could see structure where others failed, and remember it long enough to draw it. His many important discoveries in neuroanatomy withstand the test of time. Once he traveled on his own expense to a scientific meeting on the nervous system in Germany. He set up his microscope in the meeting corridor and asked all the dignitaries to look into his microscope and then at his drawings. An Herr Professor from Wuerzburg—Albert von Kolliker--stopped and looked into his microscope. On his return to Wuerzburg he confirmed what Cajal was showing and then a miracle happened: he did not appropriate Cajal's results but helped Cajal achieve recognition. This is how a man working in the most backward conditions in a then-backward country was soon awarded the Nobel Prize and became a national hero.

In 1980, when I was in the process of discovering the Mandelbrot set, I felt that I was retracing his steps. As I told you earlier, IBM was not into graphics at all. In any event, I made that discovery at Harvard. There, we had a very primitive machine and we could barely make out what was on the screen. We had to increase the contrast by repeated Xeroxing until we reached a level that could be published.

IH: How do you feel about globalization from the point of view of science?

BM: Today, it does not matter whether I sit at IBM or at Yale, I can communicate instantly with my associates at either place and everywhere else. Unfortunately, globalization also means that fashion in science tends to become the same everywhere.

IH: Do you think that diversity in a scientist's background is no longer appreciated?

BM: The situation is getting worse and worse. More than ever, I feel like a throwback. In the late nineteenth century, science was not yet professionalized. Chairs were less specialized. If a tenured professor's interests changed, he remained a professor and could be doing something entirely different of what his chair said. Funding was nonexistent so it did not impose restrictions.

At some time, many of my papers were being turned down and all my proposals for funding were being refused simply because they did not fulfill certain revoltingly arbitrary and narrow criteria. A community of tenured people form an invisible guild that wants to make sure that incoming people will become very predictable members.

Once I received an article that had many references to much earlier work of mine but claimed to be the first to include realistic mountain reliefs generated on the computer. I called up one of the authors to ask what this meant. He said that I need not worry (not that I worried in the first place) because their claim was narrow: to be the first among their community of computer graphics people. Let me conclude: globalization is one more reason why diversity must be defended.

IH: Do you have many followers?

BM: In a broad sense, they may be counted in millions. No week passes without dozens of letters from all over the world, the majority from young people. I view this as an enormous privilege and burden. For the young, I have become a legend, a part of history, and they are surprised to see me walking and talking.

In a narrow sense, there are no professorships in fractals and no funding is specifically set aside for work on fractals. If you want to work on fractals, you must have a professorship in something else. Even people who are most interested in fractals feel they must be professors of something else. Working at IBM allowed me to produce more by myself but kept me from training others. There was no search for somebody who would follow me. At Harvard and Yale, a group could not be implemented in a mathematics department. Nevertheless, I had several direct students and I am very pleased with them, but each follows up on some specific aspect of what I have done. (There are also charlatans, who are doing very well and have large budgets at their disposal.)

IH: Is there a Mandelbrot School?

BM: Once again, yes and no. A bit like in the case of Gajdusek. The fact that I created no organized institution was unavoidable, given when you heard. But it is extremely unfortunate and a real problem. Many users want to apply my techniques but then proceed very clumsily, then criticize the tool instead of seeking to deepen their own understanding. I regret very much having been deprived of a chance to start a chain reaction by training students, who would train more students and so on. This process could have "infiltrated," if that is the right word, many communities I could not reach.

IH: Considering your career, it is inevitable to ask this question, what do you consider will be your legacy?

BM: Nobody doubts that the concept of fractals will survive and diffuse everywhere. If it keeps or expands its foothold in education, my wildest dreams will have been fulfilled many times over.

Within science, fractality is inseperable from roughness and roughness is found everywhere so fractality (and also wild variability) cannot be avoided. Both raise hard problems therefore their study is bound to remain an increasingly important frontier of science. Unfortunately, this scientific legacy risks being split into seemingly disconnected pieces scattered all around with no trace of their common source. This "worst case" scenario is the normal course of events but I believe that it is not inevitable, is not desirable, and can be slowed or changed.

Today, most people publish the bulk of their work in the form of articles. But I wrote a book, *The Fractal Geometry of Nature* that has been described as a classic of the twentieth century. If it survives in one form or another, it will help preserve a unified fractal geometry.

What will remain of me as an individual who had a strong personality and a well-defined taste? I chose a difficult life quite deliberately but was not a Don Quixote. Schützenberger, whom I have already mentioned, observed that in the study of complexity there exists a Mandelbrot style that is not only adopted by my few direct former students or postdocs but also by a multitude of other writers who do not necessarily quote me – or perhaps even fail to realize that they follow me. This may explain why I invest so much in the memoirs that I mentioned. I know that writing memoirs is in part a manifestation of plain vanity. But please believe me that my main goal is different. I want to prove by describing actual events that to follow open curiosity can never assures comfort, intellectual or otherwise, but may be feasible.

IH: How would you formulate your present ambitions?

BM: It is too late for new ambitions. But the young need someone to personify science and I am keen on cementing the extraordinary position I attained among them and also among the so-called educated non-professional people. So perhaps my legacy will be to provide some kind of role model. Never before have role models for open curiosity and exploration been fewer and more needed than today. This may be the only grand issue to which I am keenly committed. My own role models, von Neumann, Poincaré and Pasteur left no memoirs. What a pity. The official stories have been "sanitized" and often contradict the conclusions I reached from contemporary documents. This sanitizing indicates a disdain of mavericks. But I deeply believe that society needs at least a few, and at this point the conditions for mavericks are far worse than ever. Ramón y Cajal did not equal Poincaré and Pasteur but his clumsy autobiography might do wonders, if it is well-edited. This is a task I keep pressing publishers to undertake but apparently Cajal is too unclassifiable for them.

IH: Suppose you could be 25 today. How would you chart your career today?

BM: No clue.

IH: Any message?

BM: Not really and only for those who do not want to follow any straight path. Times are bad. You must have clear long-range goals but don't try to pursue them in any logical sequence. Think ten years or more ahead, but on the short run, show flexibility and take advantage of circumstances. Expect to be forced to go to extraordinary lengths to explain and document your claims. Accept being called arrogant. Ask Lady Luck for help and help her.

I would not have amounted to much without a few lucky breaks, IBM, Yale, and, all along, my wife.

*** Many aspects of this interview are elaborated upon in old or recent items found on Benoit Mandelbrot's web page: http://www.math.yale.edu/mandelbrot.

---

[1] I. Hargittai, T. C. Laurent, *Symmetry 2000*, Parts 1 and 2, Portland Press, London, 2002.

Illustrations