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Foreword

Fractals and the Rebirth of Experimental Mathematics

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To contribute to a book from the Dynamical Systems Laboratory of the University of Bremen is always a great pleasure I am unable to resist. But this pleasure is necessarily associated with a challenge: my admiration for their efforts and achievements is so well-known that simply to state it again publicly could seem an example of back-scratching among good friends.

I have been asked to respond to this book as I have responded to two earlier books from the same Laboratory in Bremen. That is, I have been asked to use this foreword to indulge in some more history, philosophy and also (when appropriate) autobiography and critique of current events. The broad issue I shall address here concerns the present standing and nature of concrete geometry. More generally, it concerns the new 'experimental mathematics' that is arising from some mathematicians' response to the computer, and has already brought mathematics (to quote David Mumford) 'to a turning point of its history.' There is now a *Journal of Experimental Mathematics*, showing that the field has recently reawakened with a vengeance, or perhaps has been reborn. Some of the events that have accompanied this rebirth have been attracting wide attention, which they certainly deserve.

In a rapidly changing world, a dubious privilege of age is that it gives historical perspective. The previous major change in mathematics started before I was born, but I was present when it created its own institutions and yesterday's order became firmly established. This is why I feel that the inclusion of some autobiography will be of help.

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The Gray and the Green

We must begin by noting that experimental mathematics *does not* imply an attempted invasion of pure mathematics by the applied. *Applied mathematics* has always been permeated with science, hence with experiment. This feature greatly contributed to its being thoroughly unpopular with those believing that applied mathematics is bad mathematics. But *experimental mathematics* means something different: it means injecting experiment back into core parts of mathematics that need not — at least at present — have any contact with science.

Its most striking impact may be that it underlines the reality of an essential distinction we shall encounter repeatedly, between mathematical *fact* and mathematical *proof*. I realize that many fine mathematicians insist on defining their field narrowly, as beginning with proof, and give short shrift to facts. This may be because they have grown accustomed to seeing new mathematical facts almost exclusively suggested by the proofs of old mathematical facts. But the historian knows that in the past, the development of mathematics has relied upon many other sources, both of observation and of experimentation.

Today's experimental mathematics does not even spurn the kind of observation that has been characteristic of the least 'sophisticated' among empirical sciences: natural history. But it primarily relies upon active experimentation. Mathematical proof can, if mathematicians so choose, preserve much of itself in the form to which they have become accustomed in recent decades (and which we shall discuss here and there). That is, I neither wish nor expect — and never have either wished or expected — to see proof *replaced* by mere pictures. All that is happening now is that new methods of searching for new facts provide mathematics with a powerful 'front end' of unexpected character, one that involves more than just the proverbial pencil and paper. Thus, pictures have already demonstrated their astonishing power to *help* in early stages of both mathematical proof and physical theory; as this help expands, it may well lead to a new equilibrium and to changes in the prevailing styles of completed mathematical proof and of completed physical theory.

In other words, we may well be witnessing the re-emergence of a new active 'doublet' of an experimental and/or theoretical study. Experimental and theoretical physicists seldom live in perfect harmony, but they know they *must* not only coexist, but actually listen to each other and otherwise interact. Few in either party want to annihilate the other. In mathematics, the situation is very different: there has been a long history of conflict, as beautifully expressed in the following lines by a poet:

Gray, dear friend, is every theory.
And green the golden tree of life.

These words appear in the play *Faust*, by Goethe (1749–1832), in a famous scene in which Mephistopheles puts on the robes of old Professor Faustus, and describes the various academic programs to an awed passing student. The devil dwells on medicine (which has the virtue of bringing many fair maidens into a Doctor's life), then concludes (lines 2038–9) by his great description of two cultures. In the original,

Grau, teurer Freund, ist alle Theorie,
Und grün des Lebens goldner Baum.

For two centuries, practitioners of the hard theoretical sciences had every reason to resignedly acknowledge this devil's wisdom. Even though most scholarly institutions have ceased to compel their Dons to uniforms and to celibacy, many Dons continue to take pride in the fact that outsiders view their subjects of study as irredeemably gray.

Recently, however, a new tool has come into being: the computer. When taken by itself, it is as 'gray' as can be. But it has brought two gifts to science. Its first gift is vastly enhanced calculations. They will not concern us here, though it is worth mentioning that much of the early justification for the computer in the 1940's did not come from business users, but from those seeking new understanding about differential equations. One was John von Neumann; in his youth, he had been a near-'normal' mathematician, but by the 1940's he was no longer viewed as one and was deeply involved in weather predictions. Another pioneer of the use of computers was Enrico Fermi, a physicist's physicist, who was inspired by a desire to put the computer to use in understanding some other kinds of nonlinear mathematics.[2] Computer calculations have already caused many changes in mathematics, but these changes can be called quantitative, a matter of degree rather than of kind. Take number theory; it was an experimental discipline up to the time of Gauss and had been proclaimed experimental by Edouard Lucas, so no one could argue against experiment in this discipline.

The second gift of the computer is graphics, which tells an altogether different story and has brought a profound qualitative change, hence a fair amount of upheaval. Being myself far from the mathematical mainstream, as will be seen later in this Foreword, I welcomed computer graphics almost before it existed, and have had the good fortune of being able to demonstrate that the folk wisdom that the above verse by Goethe had expressed as an

[2]E. Fermi, J. Pasta, S. Ulam, *Los Alamos document LA-1940*, 1955. Reprinted in the Collected Papers of Enrico Fermi, Vol 2, pp. 978–88. Also in S. Ulam, *Sets, Numbers and Universes*, M.I.T. Press, 1974, pp. 490–501.

incontrovertible devil's truth is of more modest value. Its apparent universality simply resulted from a period when technology lagged behind abstract thought, followed by a period when mathematicians lagged in their acceptance of new technology. Computer graphics has repeatedly allowed me the privilege and the delight of taking up theories in mathematics and in physics whose grayness had seemed unimpeachable (and had in some cases been certified by a century of commentary), and of proving that if they are suitably transformed, these very same theories are enriched in their own mathematical or physical terms. And they also generate patterns that readily pass as forgeries of Life, of Nature, and even of Art, in their unfathomable complication. That is, not only does one part of old theory cease to be gray, but it becomes colorful enough to rejoice even the artist.

Seen from close by, the role of computer graphics covers a wide range. All too often, it boils down to *mere visualization.* This idea applies when a scientist hands his data to a specialist who knows how to put it into pretty pictures — the goal being in many cases simply to impress a visiting committee. In a way, this is how I started myself in the 1960's, before the emergence of tools anyone would call computer graphics. My very practical goal was to impress upon reluctant colleagues that some two-line formulas of mine might fail to be deep mathematics, but were indeed capable of generating 'forgeries' of the stock market, the maps of galaxies, and the weather. In doing so, a more interesting fact emerged at the opposite extreme of mere visualization. The use of computer graphics is now in the process of altogether changing the role of the eye. The hard theoretical sciences had banished the eye for a long time, and many observers used to believe, and even hope, that it would remain banished forever. But computer graphics is bringing it back as an integral part of the very process of thinking, search and discovery. Let us treat these two roles in greater detail.

I must confess a deep dislike for the term *visualization.* Of course, I am pleased that the loneliness I had experienced in the 1960's and 1970's has been replaced by a maddening crowd. I am pleased when visualization impresses a visiting review committee, and I look forward to the riches we shall all reap from the industrial trends that created this term. But to me, it is redolent of the bad old times from which we have recently emerged. To me, *visualization* seems a term invented by algebraists. Some algebraists think, for example, that the term 'circle' denotes the equation $x^2 + y^2 = r^2$. For them, that lovely curve shaped like the edge of the full moon does not exist by itself, but only to visualize this isotropic quadratic equation. Poincaré is reported to have written of his teacher that 'Monsieur Hermite never evokes a concrete image; yet you soon perceive that the most abstract entities are for him like living creatures.' This surprises me as it seemed to surprise Poincaré, but I do not deny that it may be true. When people like Hermite acquire too much political power over mathematical life, nothing can be left to survive from the times before Descartes injected analysis into geometry. Over a century ago, it was taken for granted (and expressed eloquently by Felix Klein and Henri Poincaré) that geometers and algebraists are two distinct kinds of scientists. Unfortunately, the academic tests that recruit new scientists have come during our century to give less and less credit for skills in geometry and increasingly full weight to skills in algebra. In that respect, the United States stood as an extreme case, because it never went for the serious study of geometry characteristic of all the countries of Europe. This may explain in part why the refugees from Russian and Germany found 'native' U. S. mathematics to be strong, but mostly pure and algebraic to a fault, well before this came to be also the rule in Europe.

Naturally, those who provoked geometry's fall from grace described it as inevitable, as yet another proof that there is progress, that history moves relentlessly forward, never to turn back. But in this area, as in many others, the notion that events are ruled by an inevitable destiny has been sharply contradicted by recent events. That is, it now seems that relentless algebraisation was not inevitable. To a large extent, it manifested on the part of all theoretical scientists a spontaneous, practical and appropriate adaptation to the lag in technology that has been mentioned. It was hard not to acknowledge the exhaustion of the old tools of geometry, and the lack of new ones, and to act accordingly. But now the computer is pushing this adaptation and the resulting expedients into a historical limbo.

To interrupt for an autobiographical aside, I happen to have made myself a student of this anti-geometric trend since the 1940's. Being a dyed-in-the-wool geometer and very much beholden to my eye, I consider it a boon that I took the notorious French examinations when geometry was still in the saddle. Watching from the perspective of fractal geometry, to which the reader must know that I have devoted most of my working life, I have seen new trends develop, and have kept searching through the past for events that led to the banishment of the eye from hard theoretical science. Therefore, allow me to recount a few old but lively stories I have read, and to reminisce about recent stories, one in physics and the other in mathematics, in which I have been a prime participant.

Pluralists and Utopians in the Greek Golden Age

The common feature of these stories is that they concern the conflict that arose during the Greek Golden Age, at a time when mathematics and science were being formulated in nearly their present form, and when the notion of proof was being developed. The two sides of this conflict can be called pluralistic and utopian.

The pluralistic view is wonderfully expressed in these words: 'Certain things first became clear to me by a mechanical method, although they had to be demonstrated by geometry afterwards because their investigation by the said mechanical method did not furnish an actual demonstration. But it is of course easier, when we have previously acquired, by the method, some knowledge of the questions, to supply the proof than it is to find it without any previous knowledge. This is a reason why, in the case of the theorems that the volumes of a cone and a pyramid are one-third of the volumes of the cylinder and prism respectively having the same base and equal height, the proofs of which Eudoxus was the first to discover, no small share of the credit should be given to Democritus, who was the first to state the fact, though without proof.' The author of these words might easily have been a man near our time, but in fact he was Archimedes.[3] Please don't let your eye glaze over all these names of ancient heroes. Please, read on!

The reason why the views of Archimedes deserve to be called pluralist is because he acknowledges a proper balance between the role of proof and the role of experiment, including the role of the senses. He sees no harm in acknowledging experiment and the senses as tools in what must be described as a search for new *mathematical facts*. The existence of mathematical facts has long seemed to me undeniable, but experience proves that other authors deny any meaning to the very notion, and view it as internally self-contradictory. Thus, a loud buzz was heard in mathematical circles after a mathematical congress that met in Kyoto in 1990 gave the Fields medal to the physicist E. Witters. Letters to the Editor went flying, describing *mathematics without theorems* as something that should not be accepted as a part of 'real mathematics.'

Thundering against experience and the senses is a satisfying idea in our culture, but it is surely not a new one. Would it not be nice, therefore, to be able to identify the first person to have expressed this idea? Judging from the tone of the above quotation, it seems that Archimedes was responding to someone else's already well stated opinion. It must have been the Utopian view held by Plato (427–347 BC), a man of very great power, both in intellect and in influence. Yes, it happens that the curses I hear too often being cast today against the return of the eye into the hard sciences are not new, and do little but echo Plato. And the pluralists who welcome and praise the return of the eye, thinking of themselves as down-to-earth and modern, may not know much of Plato, yet are actively fighting his shadow.

[3] Archimedes (287–212 BC), Democritus (460–370 BC), Eudoxus (408–355 BC).

The most widely quoted evidence on Plato's views occurs in Plutarch's *Life* of the Roman general and politician, Marcellus, who led the siege of Syracuse where a soldier killed Archimedes. Quoting from the Dryden translation, 'Eudoxus and Archytas had been the first originators of this far-famed and highly prized art of mechanics, which they employed as an elegant illustration of geometrical truths, and as means of sustaining experimentally, to the satisfaction of the senses, conclusions too intricate for proof by words and diagrams ... But ... Plato [expressed] indignation at it, and [addressed] invectives against it as the mere corruption and annihilation of the one good in geometry, which was thus shamefully turning its back upon the unembodied objects of pure intelligence to recur to sensation and to ask help (not to be obtained without base supervision and deprivation) from matter'.

Since Plutarch's anecdote about Plato's bossy manners was written 400 years after the fact, it should be approached with caution. But it is true to Plato's own words, that geometers 'talk in most ridiculous and beggarly fashion ..., as though all their demonstrations have a practical aim ... But surely the whole study is carried on for the sake of knowledge.'

When Plato's double curse, against physics and against the eye, first came to my attention, I had been toiling for decades at the task of rebuilding the destroyed icons. I found myself wildly rooting for Eudoxus. It was a delight to know that Eudoxus was a pioneer, not only in mechanics and astronomy (as implied by Plutarch), but also in geometry (as stated by Archimedes). In fact, he is often viewed as the most creative of the ancient Greek mathematicians, while Euclid — who flourished about 300 BC, was an encyclopedist. As to Plato, he was not at all a creative geometer. To quote Augustus de Morgan, 'Plato's writings do not convince any mathematician that this author was strongly addicted to geometry.' In the name of purity, he wanted to restrict geometry to operations with the rules and the compass. Plato was an ideologue and author of more than one harmful Utopia. In fact, Plato's curses against physics and the senses were very much in line with his political ideal of an authoritarian state published in his *Republic*.

Under Plato's influence, Greek mathematics underwent a remarkable transformation — anti-empirical and anti-visual — which elicits the most extreme reactions. It is praised by some as the greatest and the most durable achievement of Greece, and harshly faulted by others. Thus, de Santillana[4] blames it for the failure of Greeks to develop physics in parallel with mathematics, with such disastrous effects on Greco-Roman technology that it may bear a share of responsibility in the fall of Rome.

[4]de Santillana, G., *The Origins of Scientific Thought*, University of Chicago Press, 1961.

Plato (like Hermite many centuries later) believed in the full reality of Ideas, which implies that mathematical objects and truths are discovered, not invented. (This belief has few concrete consequences, but I agree wholeheartedly.) But Plato also believed that the physical world possesses only 'relative reality.' This is what led him to the formulation of a Utopia, in which mathematical truths must be discovered and studied without reference to anything concrete and without the use of the 'senses,' which certainly includes the eye and may include 'intuition.' The Utopians who drove the pictures out of mathematics were themselves driven by a fervor so close to religion that it is appropriate to call them *iconoclasts*. *Icon* meant *image* (as is known to many computer-literate people today), and had the connotation of *an idol*. *Clasts* are those who break or destroy.

For most of the time between Plato and the present, most mathematicians paid little heed to Plato's words. Matter and the senses were providing far too many exciting facts to play with and prove.

The Two Inseparable Faces of the Coin of Mathematics

Returning from Plato's days to ours, we find the situation different, new and fluid, and opinions sharply divided. We hear ringing acclaim from many, including the young and their teachers, but certainly not from everyone. Herein lies an interesting story. When I was younger, and experimental mathematics had not yet reawakened, the stage was occupied by just one kind of mathematics. This was already the case when I studied in Paris in the mid-1940's, first briefly at the Ecole Normale Supérieure, and then for the usual two years at Ecole Polytechnique. When in high school, I had become utterly fascinated by a very difficult subject called *geometry*, which occupied a large place in the curriculum. To me at least, this was the study of objects that had two properties that could have been contradictory, yet went together: like glove in hand, like the two faces of a single coin, or (a better image) like body and soul, each necessary to the other. One could reason on them in abstract style — perhaps dry, but noble beyond anything else: the style that had been pioneered by Euclid. But this was not all. For me, mathematics was concerned with completely real objects; they could actually be seen and manipulated, as drawings and also as plaster casts from the shelves in the office of the Mathematics Chairman. As a teenager, I loved it when I heard that certain numbers, originally introduced as formal square roots of negative reals (their origin led them to be called 'imaginary'), had soon enough turned out to be identical to points in the plane. This seemed to prove that the original way of introducing them was incomplete, and no one was promoting it — to my knowledge. To carry out algebra without this interpretation would truly deserve to be called a 'complex' procedure. I also loved the Euclidean representations for the non-Euclidean geometries, which revealed that another coin that in the 1830's had seemed to have a single face was, in fact, properly endowed with both. It became my wild but deep hope that occasions when a shape remained 'abstract' were simply proof of a temporary lack of visual imagination on the part of the geometers.

Jumping well ahead of the story, one can imagine my glee when fractal geometry found that many of the so-called 'monsters of mathematics' were as 'real' as can possibly be.

Needless to say, this view influenced my way of handling the heavy mathematics homework we were given. After I had become acquainted with the basic outline of a new problem, I did not rush to worry about the questions being asked, but instead hastened to draw some kind of picture. When the problem was stated geometrically, this task was straightforward. When the problem was stated algebraically or analytically, this was the hardest step. Once a picture was available, it received my undivided attention; I played with it and introduced all kinds of changes. In particular, I modified it, trying to make it (somehow) richer, more attractive and more symmetric. At some point, 'geometric intuition,' something we shall discuss momentarily invariably rewarded me with a sudden shower of observations. Only then did I look up the questions we had been asked, and as a rule found that all the answers were 'intuitively' obvious. Invariably, again, the formal proofs of these guesses were the quickest and easiest steps in the process. I do not recall having been stumped once, but of course this fact helps describe the mathematics taught in France in those years. The same situation prevailed throughout Europe, but apparently never took hold in the USA.

Geometric intuition is a much maligned ability that deserves a moment of attention. I have heard all too many people assert that it does not exist, never imagining that they may be describing their own disabilities. Other people delight in warning against intuition's pitfalls and its inadequacies. They do not realize that intuition is not something fixed, but rather the fruit of past experience; it is easily destroyed, yet can be trained.

Returning to my student years, a combination of intrinsic interest and of formalism was to me necessary for a full enjoyment of mathematics. But, even in high school, I heard the rumor that there were problems in my paradise. Then the end of World War II brought back to Paris an uncle who was a professor of mathematics at the famed Collège de France. He made me one of the best advised among 20-year-old mathematics students in Paris. He started by informing me (gently but firmly) that, as a topic of active research, the geometry I loved was dead. Worse even than reduction to some small but lively stream, it had been dead for nearly a century, except in mathematics for children. He too had been a whiz at geometry when in high school. But he felt that in order to make a genuine contribution to mathematics, one had to outgrow geometry. I heard all this in the 1940's, but Brooks (1989) echoes my uncle's opinion when he describes my 'mathematical sensibility' (then as today) as being 'rather infantile and somewhat dull.'

More precisely, I was told that *geometry*, as a word, was very much alive, but had lost the last trace of its old hands-on applications. For example, algebraic geometry was being saved from a bunch (mainly Italians) who could not define or prove anything properly, and was one of the bright new kids on the block, reborn as a purely algebraic enterprise destined to a future better than its present.

As a first alternative, my uncle suggested that I move into his own field of complex analysis, which he described as being farthest from the growing mood towards abstraction. For example, he told me of the Fatou-Julia theory of iteration, and suggested that a bright new mathematical idea might enable me to do something truly worthwhile — and worth rewarding. He was one of the few to be aware of the Fatou-Julia theory. He viewed it as admirable, and was deeply annoyed that it had not advanced much in the thirty-odd years between 1917 and 1945 or so. He gave me the original reprints they had given him. Unfortunately, reading these authors' great works definitely showed me that this was not the geometry I loved. Besides, Gaston Julia himself was very much around (he was in his fifties and after moving to Polytechnique, I had him as a teacher — of differential geometry). Yet hardly anyone but my uncle seemed to know about J-sets (this was before the term, 'Julia sets,' for which I bear part of the responsibility). In fact, hardly anyone but my uncle had a half good word for Julia.

The second and more obvious alternative to studying geometry was to fall in line behind a group of mathematicians who called themselves 'Bourbaki.' That 'fall in line' is the correct expression is confirmed by an attractive autobiographical essay by E. Hewitt.[5] 'From Stone and his fellow mathematicians at Harvard, I learned vital lessons about our wonderful subject:

> Rule #l. Respect the profession.
> Rule #2. In case of doubt, see Rule #1.'

Who was Stone? Aside from being a great creative mathematician, Marshall Stone had been raised to exert the natural authority of the son of a future Chief Justice of the USA. And he described 'the

[5] Hewitt, E., *Math. Intelligencer* 12, 4 (1990) pp. 32–39.

profession' in the following ringing tones.[6]

'While several important changes have taken place since 1900 in our conception of mathematics and in our points of view concerning it, the one which truly involves a revolution in ideas is the discovery that mathematics is entirely independent of the physical world...

'When we stop to compare the mathematics of today with mathematics as it was at the close of the nineteenth century we may well be amazed to see how rapidly our mathematical knowledge has grown in quantity and in complexity, but we should also not fail to observe how closely this development has been involved with an emphasis upon abstraction and an increasing concern with the perception and analysis of broad mathematical patterns. Indeed, upon close examination we see that this new orientation, made possible only by the divorce of mathematics from its applications, has been the true source of its tremendous vitality and growth during the present century.'

The Bourbaki used the words 'structure' and 'foundation' on every imaginable occasion. For them, the terms were very 'positive,' associated as they were with the noble tasks of building or rebuilding. But the Bourbaki were inconsistent in stopping the search for foundations before it came to concern logic. More important to my mind (and I never saw a reason to change), they were working very far from the hardier ones who really lay foundations after having dug holes through messy and uncertain ground. They were moving furniture around, like decorators and not like builders. Even worse, they often seemed simple-mindedly committed to a task of compulsive house-cleaning, housekeeping and hectoring. My feelings towards them were strong and simple (which marks me as familiar with many mathematicians' propensity to strong feelings). Their 'formalisme à la française' was not a useless task, to be sure, but it was ridiculous to allow it to rule mathematics, to rule the choice of who was to become a mathematician, and to extend its influence wherever it could. Therefore, I loathed and feared Bourbaki.

The death of geometry and the emergence of Bourbaki were the reasons why I gave up the envied position of number 1 in the entering class of the exclusive Ecole Normale. (In mathematics and physics combined, my class was thereby reduced to 14 students, for the whole of France.) Later, I left France. As described,[7] both decisions have turned out to be very wise, because Bourbaki was growing and was about to take over, not only Ecole Normale, but much of French academia.

[6] Stone, M., *American Math. Monthly* 68 (1961) pp. 715-734.

[7] Mandelbrot, B. B., *Math. People,* D. J. Albers and G. L. Alexanderson eds., Birkhauser, 1985, pp. 205–225.

Eventually, Bourbaki died off, but only after it had trained many younger mathematicians who had known nothing else in their lifetimes. They find it difficult today to comprehend the intensity of the emotions Bourbaki evoked among friend and foe. For this reason, I wrote down a few facts and thoughts concerning Bourbaki.[8]

In any event, I would not have been a happy Normalien; and in later years I would not have been happy in France as a professor of mathematics, when the colleagues who belonged to the ruling club viewed me dimly, and certainly not as a gentleman. But a move out of academic departments of mathematics into IBM has allowed me to retain permanently the 'infantile sensibility' I enjoyed as an adolescent. As the computer became easier to use, and as primitive graphics became available to those willing to pay the very steep admission price in effort and aggravation, I made them, not a tool to be called only if needed, but a constant and integral part of my process of thinking.

This brings us to the question that is very old but has been asked especially sharply in this context: what are, in a discovery, the respective contributions of the tool and of its user? The puzzle is that different kinds of tools continue to be treated differently. Galileo wrote a book to complain bitterly about those who belittled his discovery of sunspots, claiming this was only due to his having lived during the telescope revolution. Fatou (a cripple) and Julia (a wounded war hero) are — quite rightly — praised for their theory of iteration, and no one would dream of belittling their work as being due to their having lived during World War I and in the age of Montel.[9] Today, there are some who belittle work based on the computer as solely due to the worker's having lived in the computer age.

If it were so, we would be faced with a mystery. Why should experimental mathematics have attracted so few practitioners for so long a time after von Neumann and Fermi (mentioned earlier in this foreword) had shown in which way mathematics can benefit from the computer. Their example was ignored. When I was new at IBM, which I joined in 1958, opportunities to use computers were knowingly and systematically spurned by every noted mathematician. Even the example of S. Ulam is interesting. He contributed to an (already-mentioned) famous early paper on experimental mathematics,[2] and might have been expected to become a herald of the new trend. Yet the preface he wrote in 1963 to a reprint of that paper asserts the following: 'Mathematics is not really an observational science and not even an experimental one.

[8]Mandelbrot, B. B., *Math. Intelligencer* 11, 3 (1989) pp. 10–12.

[9]In 1912, Paul Montel introduced Fatou's and Julia's key tool, the normal families of functions, and soon afterwards — like nearly all young people in French academia — he was called into the Army.

Nevertheless, the computation which [Paul Stein and I] performed were useful in establishing some rather curious facts about simple mathematical objects.'

The opinion that the tool was all that mattered is certainly not applicable in my case, because graphics became essential to my work well before the computer era started; I recall quite vividly the time I spent looking at a record of coin tossing to be found in a famous probability textbook. It is reproduced as Plate 241 in my *Fractal Geometry of Nature*. It led me to all kinds of useful models. William Feller, the textbook's author, once told me whether the random numbers were taken from a table or obtained by a physical coin, but I have forgotten his answer. But surely they were neither computer generated nor computer plotted, and no other textbook of probability felt such a figure was needed.

Computer assisted graphics first became critical to my work in the late 1960's when a series of papers I wrote with J. R. Wallis used a pen plotter to draw, next to one another, a series of actual weather records and of records generated by surreal models of weather variability. This turned out to be very important, but was very far from mathematics. The first seriously mathematical application occurred elsewhere, in a context of interest to the harmonic analysis specialists I. P. Kahane and J. Peyrière. Heuristic calculation helped in an essential way, but pictures led me to a series of mathematical conjectures about certain random singular measures (later called *multifractals*). I could only prove special cases, but Kahane and Peyrière proved the full conjectures, and went on to very interesting justifications.

A second serious application, published very belatedly in 1983, provided the first fast algorithm for the construction of the limit sets of certain Kleinian groups. This brings up a significant episode. At that time, I knew of no expert in that field, but I had long known Wilhelm Magnus of NYU. He had written a book about Kleinian groups, so I paid him a visit in 1978 or 1979 to inquire whether my algorithm was known to him and others. He said that it was, which greatly encouraged me. Then Magnus gave me a file of computer generated limit sets sent to him by various people. Not one among the authors of those illustrations had used them in the search for new mathematical facts! This came as a profound surprise to me, and also a strong source of encouragement.

Nonetheless, the above investigations were nothing but appetizers. From my own viewpoint (and also from a wider viewpoint, held by many people), mathematics took a sharp turn in 1979–1980, when the Fatou-Julia theory that I had spurned in the 1940's again became a wide awake component of mainstream mathematics. This event came about because the topic was thoroughly changed by a new tool. As already mentioned, the last time it had been thoroughly changed was in the 1910's, when Paul Montel's normal families set Fatou and Julia to work. But — to my uncle's repeated and bitter disappointment — the new tool was not 'purely mathematical.' It did not come from within, but from outside mathematics. The methods of fractal geometry had already allowed me to use the computer to bear upon many problems of physics, and it occurred to me that it could also be used in mainstream mathematics. More precisely, it has already been said that the Fatou-Julia theory of iteration had fallen out of the mainstream. But it came back into it with a bang after some investigations that I carried out in 1979–1980 on a set that is pedantically called *the locus of bifurcation of the map* $z \longrightarrow z^2 + c$. In my early papers, this set was called 'μ-map,' because physicists used to denote the constant c by $-\mu$, hence used to write the map in question as $z \longrightarrow z^2 - \mu$. In the same papers, the locus for the map $z \longrightarrow \lambda z(1-z)$ was called 'λ-map.'

My observations concerning this locus were presented in May 1980 at a special seminar at Harvard, then in November 1980 at a seminar run by David Ruelle in Bures near Paris, at the Institut des Hautes Etudes Scientifiques. The Bures seminar was widely attended, and it appears to have had a profound effect on Adrien Douady, who was there. He, and then his former student John H. Hubbard, dropped their previous work (he was still a leader of Bourbaki!), and they have since 1980 devoted themselves fully to the locus I had described to him at this seminar, and subsequently at many private meetings. Soon after that, Douady and Hubbard proposed denoting this locus by the term *MandelbrotSet* and the letter M.

The M-set

The M-set continues to draw attention. Many people (justly, in my opinion) view the work it has inspired as very special in the coming of the new experimental mathematics. This is also perhaps why its precise origins have come to attract an exceptional amount of attention. Less praiseworthy are the undocumented anecdotes that are being thrown about, either to prove that experimental mathematics is a terrible idea, or to prove that it is a great idea, but one that either happened just by historical necessity (e.g., solely because of the computer) or happened by other hands. Being told that assertions that are not denied are viewed as correct, I have reluctantly resolved to sketch these attempts at controversy. I conclude from their having backfired that there is no competitor for credit to the discovery of the first and most striking list of properties of M.

It should first be mentioned that several scholars have confided to me that the thought of studying M had entered their mind; but they never followed up, and make no claim. By 1988 (interestingly enough, not when the study of M was new), one scholar (who — charity reigns — will not be named) acted differently: believing perhaps that he *ought to* have performed this work, he took the unfortunate step of expressing his claim in print, but without providing any evidence one could see or judge. A second published claim is, on the contrary, documented: a paper by Brooks and Metelski[10] includes a rough drawing of M. An extensively circulated letter from Brooks to B. Branner brought this drawing to wide attention in 1988, and also pointed out that it dates from 1979.

Thus, the M-set was sighted nearly simultaneously in two places, in both instances through a deep fog. However, as I have tried to explain elsewhere,[11] the date of first sighting is devoid of interest. Friends and foes of experimental mathematics agree on one thing: by itself, a picture can have no interest whatsoever. (This is particularly so here, given that the drawing in Brooks' and Metelski's paper is grossly mislabeled.) For the experimental mathematician, what matters is not the first sighting, but the mathematical ideas — if any — that the pictures suggest.

Brooks and Metelski reported no mathematical idea. Precisely to the contrary, my first glimpse of M as a vague form raised an irresistible challenge to apply to iteration the same tricks that had worked so well for me over the preceding fifteen years. (This is why — as has already been said — Brooks later described my 'mathematical sensibility' as 'rather infantile and somewhat dull.') What followed in 1980 need not be repeated, having been described in sufficient detail in a feature I contributed to the famed book by Peitgen and Richter[12] (This feature was written in early 1985, for the Catalog of their *Frontiers of Chaos* exhibit.)

If Brooks wished to be heard, his wish may well have been over-fulfilled in 1989. His case was taken up by S. Krantz, who went on to achieve renown as a purveyor of mathematical anecdotes concerning events in which he was not a participant, or even a witness.[13] He has also won less favorable renown for the accuracy (or lack of it) of his tales.[14] In 1989, our anecdotist took up the Brooks-Metelski paper, to make assertions contrary to those of Brooks.[15] He claimed, first, that the drawing in question dated to 1978, and second, that it was well-known to the community of mathematicians. If the latter had been true, it would have saved

[10] Brooks, R., Metelski, J. P., *The dynamics of 2-generator subgroups of PSL(2,C)*, in: Riemann Surfaces and Related Topics, I. Kra and B. Maskit eds., Princeton U. Press, 1981.

[11] Mandelbrot, B. B., *Math. Intelligencer* 11, 4 (1989) pp. 17–19.

[12] Peitgen, H. O., Richter, P. H., *The Beauty of Fractals*, Springer-Verlag, 1986.

[13] Krantz S., *Math. Intelligencer* 12, 3 (1990) pp. 58–63.

[14] Krantz S., *Math. Intelligencer* 13, 4 (1990) p. 5.

[15] Krantz S., *Math. Intelligencer* 11, 4 (1989) pp. 12–16.

Brooks the need to do anything in 1988, and would have given Douady and Hubbard no reason to name the M-set after me.

In 1991, the very same anecdotist is being quoted again: he argues that the connectedness of the M-set proves that, 'even in [its] heartland,' experimental mathematics can fail. Numerous other accounts are flying around, based on the belief that experimental mathematics is both straightforward and ineffective. But the practitioners know better: in skilled and cautious hands, one experiment leads to another experiment, then combines with proven mathematical facts, and ultimately leads to new mathematical conjectures. In the particular instance of the M-set, the experimental method had led me substantially further than is credited in the diverse accounts one hears. The story ought to be more widely known since it warranted pages 155–157 in the already mentioned feature I wrote for *The Beauty of Fractals*. It may have been seen there by our chief anecdotist, since he quoted from my piece in his review[15] of Peitgen and Richter's book (if 'review' is the right word). Recent advances in experimental mathematics and its growing social acceptance make my old feature worth reading again as a realistic description of a successful use in mathematics of the experimental method I have been practicing for so long, and to which so many are rallying today.

The connectedness of M was only one among many empirical observations that I made concerning M, and that have led to splendid, fully proven theorems. In addition, what I called the hieroglyphic character of M was refined and proven by Tan Lei, and the fact that the boundary of M is of Hausdorff dimension 2 was proven very recently by M. Shishikura. I would have been quite incapable of providing even one of those proofs. But let me say it again: contrary to persistent anecdotes, I cherish equally the heuristics (graphical or other) and the proof. I do not systematically denigrate work I do not understand or cannot myself accomplish.

We hear that traditionalists (true to their role) fear that in embracing experiment again, mathematics may lose something special. It is indeed probable that it will lose something just as it will gain something else. One good thing is that it has already lost the monolithic structure that characterized it in the 1950's and the 1960's.

It is time to bring this story back to its key point without paying undue attention to these recent disputes. Their overly personal character used to annoy me greatly, but from a distance I see them as fresh episodes, however trivial, in a long battle for souls that has raged since Plato and Archimedes.