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Overview of fractals and multifractals

THE PRECEDING PREFACE HANDLES ISSUES *specific* to this volume. This Overview tackles broad issues, and answers diverse *general* questions raised by fractal geometry, because of the multifaceted character of its subject-matter and the novelty of its techniques. Did the scope of this Overview run to excess? If it did, let it be. Many of my earlier answers to all those questions have become obsolete and this chapter is a good opportunity for an update taking the form of loosely related and significantly overlapping short essays. To help persons impatient with philosophy select what they care to read, the summary is rather detailed.

0. SUMMARY

Section 1 elaborates on a statement made in the Preface. In retrospect, the ubiquity of roughness throughout nature and culture – that is, works of Man, such as mathematics and finance – was always obvious. The great novelty in my work is the observation that scale-invariance is often present and that this gives us the first handle ever on a general study of roughness. Roughly stated, fractal geometry is *the study of scale-invariant roughness.* This study is motivated by both broadly based usefulness, and surprising and still-growing esthetic values in art and mathematics.

Section 2 expands on a lesson of past experience. It argues that the informal words, "study of scale-invariant roughness," may be the best characterization fractal geometry can hope to achieve. A proper formal definition neither exists nor should be expected to develop.

Section 3 is motivated by a mystery that gradually grew as fractal geometry was becoming the first stage in a rational study of roughness.

Why did this study come so late after those relating to the "sensations" of heaviness, hotness, loudness, and brightness? A serious possibility is that roughness involves a qualitatively higher level of complexity and applies across a broader range of fields and so is more abstract. This point of view will be motivated by introducing several flavors, coming from different horizons, of a contrast between "old and easier" and "new and harder" scientific endeavors. Those contrasts grew throughout my scientific work and several dominate this volume.

What matters is that the broad domain "of what is new and harder" also includes, next to fractal geometry, the statistical physics of critical phenomena. Those two subdomains arose quite separately (fractals came before renormalization of critical phenomena; but this is not the point) and had very close and fruitful mutual connections.

Section 4 sketches the far-reaching and highly relevant historic roots of fractal geometry. Before a hundred years of ostensibly pure mathematics, they also enriched – surprise! – millennia of decoration.

But why didn't the study of roughness spring into existence as soon as mathematics began to work with objects we now recognize as fractals? Is it because this mathematics was invented as a *negative* force, an art of counterexample, a force *against* every kind of science? Why did science not develop the main tools for itself as soon as the overall mathematical "environment" was ready? This long lag is baffling. More specifically, it is baffling that, all along, I encountered near-constant resistance and a near-total absence of direct competition. Computers were necessary for my work, but events proved they were not sufficient. The saying about "being the right person at the right time in the right place" may juxtapose three undefined "rights," but all this hard-to-fully-comprehend sociology added up to a great opportunity for me.

Section 5 dwells on an important, widely shared, and often misinterpreted observation. The "core" of fractal geometry is simple and has become widely familiar, even to many middle-school students. But away from the core, the complication of the pictures and the difficulty of the problems increase sharply and suddenly. Very often, they jump up together, from very low to very high. This helps explain why fractals achieved such a high level of popularity. It is, in my opinion, a very good thing; it had to be mentioned, and will be sketched and commented upon.

The lengthy and miscellaneous Section 6 provides a flavor of the content of fractal geometry, in 2001. All too many scientists are only aware of some single aspect that concerns their discipline. They may be tempted to assume that there are no others, therefore a correction was needed.

Section 7 relates to a hobbyhorse of mine: the continuing and essential importance, even in the "hard sciences," of a dialectic tension between the analytic and the geometric (synthetic) points of view. Of course, roughness, irregularity, and the like cannot be tamed without first being translated into words and then into an organized discourse made of analytic symbols. This translation led to fractal geometry.

Using everyday medical terms, and never forgetting that metaphors work up to a point and not beyond, it is useful to think of (synthetic) geometry and (mathematical) analysis as relating to each other as follows. Geometry is a counterpart of "old-fashioned" clinical skills enriched by skills in interpreting the typical medical tests, whose raw output is a still or moving picture. Analysis is (under) represented by those rare tests (like body temperature and blood pressure) that directly yield numbers. The ideal physician relies on every kind of test and also knows how sickness looks, sounds, or smells. Perhaps medicine will one day be completely reduced to numbers, but today it is not.

The balance between the roles of geometry and analysis is very different in hard sciences. Nevertheless, especially along the wild frontiers where I always dwelt, science should find a combination of analysis and geometry to be highly desirable.

The uncommon turns of my life also led to strong opinions on other topics of general interest. This Overview restates some points made elsewhere in this book, in M 1993f, and in earlier volumes of *Selecta*.

1. FRACTAL GEOMETRY FILLS AN UNWELCOME GAP: IT IS THE FIRST APPROACH TO MAN'S SENSATION OF ROUGHNESS

1.1. Paths from messes of "sensations" to quantitative measures

"The first and oldest problems in every branch of mathematics spring from experience and are suggested by the world of external phenomena." These words of David Hilbert cannot be disputed. Even more strongly, many sciences arose directly from the desire to describe and understand some basic messages the brain receives from one of our many senses.

Visual signals led to the notions of bulk and shape and of brightness and color; auditory signals, to the notions of loudness and pitch. Optics and acoustics arose when those notions were made quantitative. Similarly, the sense of heavy versus light led to mechanics and the sense of hot versus cold led to the theory of heat. Unquestioned proper measures of mass and size mark early writing and the dawn of history; temperature, which is a proper measure of uniform hotness, dates back to Galileo.

Brightness and loudness are less definitively settled. Physical measures like the decibel involve the logarithm of energy, in accordance with the Weber–Fechner law. However (last time I paid attention), followers of S.S. Stevens were replacing the logarithm by a power law.

Against this background, the sense of smooth versus rough was equally essential and equally ancient, but neglected in science. Before fractal geometry, no agreed-upon quantity could measure roughness, the way temperature measures uniform hotness.

The step that begins the transformation of a mess of sensations into a quantitative science was about the same for many sorts of science and is well illustrated by that of sound. Even today, concert hall acoustics are mired in controversy, recording of speech or song is comfortable with vowels but not consonants, and drums are filled with mysteries. Altogether, the science of sound remains incomplete. Nevertheless, it boasts great achievements.

Let us learn some lessons from its success. Typical of every science, it went far with healthy opportunism. Side-stepping the hard questions, it first identified an easy one. The idealized sound of string instruments proved to be an "icon" that is at the same time reasonably realistic and mathematically manageable. It clarifies even those facts it fails to characterize or explain. It builds on the "harmonic analysis" of pendular motions and the sine/cosine functions. Crowning centuries of efforts, Bernoulli and Euler made acoustics bloom in the eighteenth century when the theory of idealized vibrating strings provided a first understanding of the fundamental frequency and its harmonics. They and the full Fourier series are periodic, that is, translationally invariant.

More generally, it is broadly the case that the starting point of every science is to identify "harmonies" in a raw mess of evidence. In the long road from a mess of sensation to scientific acoustics, a key moment was marked by the successful identification of a specific portion of the overall mess, one that provides a "key compromise" between simplicity and breadth of applicability, that is, between harmony – in a broad meaning of this word – and realism.

1.2. "Harmonizing" the study of roughness; scale-invariance perceived as playing the role that harmonious sound played in acoustics

By contrast to acoustics, the study of roughness could not, until very recently, even begin tackling the elementary questions this paper listed earlier. This brings us to a point. In retrospect, much of my contribution to science is based on realizing that, like acoustics, the study of roughness could become a science only after someone had identified and studied its basic invariance or symmetry, thereby revealing the deep source of harmony common to many structures one can call rough.

Until the day before yesterday, an ordering of deep human concerns, from exalted to base, would have surely placed roughness and harmony at opposite ends. This is perhaps the cause of a glaring irony: the first-ever systematic approach to harmonious roughness arose from a thoroughly unexpected source in extreme mathematical esoterica. This may account for the delay, discussed in Section 1.1, in mastering roughness.

Fractal geometry argues that roughness cannot be measured by quantities borrowed from other investigations. The key to fractality being scale-invariance, roughness can be measured most intrinsically by the parameters that specify the invariance in question. Let me elaborate.

1.3. Prefractal measures of roughness

A root mean square (r.m.s.) deviation from an interpolating line or plane was the basic tool used to measure metal fractures' roughness as well as financial prices' "volatility." But metallurgists themselves rightly viewed this measurement as suspect because they were inconsistent: different regions of a presumably homogeneous fracture emerged as being of different "r.m.s. volatility." M. Passoja & Paullay 1984 found the same to be true for different samples that were prepared following precisely identical protocols. The same difficulty (though far less frankly acknowledged) occurred in finance.

Attempts to master roughness created a major challenge, because the old statistical techniques, instead of pointing to a proper theory, proved helpless or misleading.

Spectral analysis. This is a simple example of an often misleading technique. In many time series recorded in finance or physics, the spectral density is flat, "white," in clear contradiction with the fact that those phenomena differ drastically from thermal noise.

Another common reason for misrepresentation is silly but not funny. When – as often happens in this book – the spectral density is proportional to $1/f^B$, plots of the logarithm of spectral density against frequency itself create an artefact, namely, the appearance of spectral near-whiteness.

1.4. Fractal measures of roughness: dimensions, Hölder, and others

The study of roughness demands an altogether different toolbox, which is sketched in Section 6. Claims of novelty inevitably sound self-serving, so I call an independent witness, a clear-eyed economist quoted more extensively in M 1997E, especially in Chapter E17. Cootner 1964 observed that my "view of the economic world is more complicated and much more disturbing than economists have hitherto endorsed." (This was correct in 1964 and becomes increasingly so as time flies.) Elsewhere, the same author wrote that "Mandelbrot, like Prime Minister Churchill before him, promised us not Utopia, but blood, sweat, toil, and tears. If he is right, almost all our statistical tools are obsolete..., past econometric work is meaningless... . It would have seemed desirable not only to have more precise (and unambiguous) empirical evidence in favor of Mandelbrot's hypothesis as it stands, but also to have some tests with greater power against alternatives that are less destructive of what we know."

Reacting to the same quote in M 1997E, page 9, I commented that the call for improved statistical tests can be applauded without reservation. But the evidence is increasingly overwhelming that what Cootner called "what we know" is imperfect. And, unfortunate but true, the abyss between econometrics and the real world has not narrowed.

Allow me just one example. When fast Fourier algorithms became available around 1964, spectral analysis created great interest among economists. Tests showing some price series to be nearly "white" were interpreted as implying the absence of serial dependence. The result made no sense and was forgotten. But (as reported in M 1997E, Chapter E6) a promising non-Gaussian model of price variation is white, despite the presence of strong serial dependence!

It often seems that novel uses of known statistics may at the same time test a hypothesis and test a test! The latter often fails to pass.

Be that as it may, the study of financial fluctuations moved on since 1964, and became increasingly refined mathematically. But it held tightly on to what Cootner called "alternatives that are less destructive" and mostly continued to rely on Brownian motion and its close kin. Therefore, Cootner's list of endangered statistical techniques would now include the Markowitz mean-variance portfolios, the Black–Sholes theory, Itô calculus, and the like.

As to the fractal toolbox that I proposed, it began with borrowings from the already mentioned mathematical esoterica that were of no use in the study of previously tamed sensations. For this reason, these esoterica had been previously thought of as "pathological." Once the borrowings were exhausted, the toolbox was greatly developed on its own.

As shown in M, Passoja & Paullay 1984 and confirmed by follow-up studies (too numerous to list) the fractal dimension D, a characteristic of fractals, proves – unlike the r.m.s. – to be an invariant measure of the roughness of the fractures in metals. It is no accident that the words *fracture* and *fractal* use the same root, *fract.* Had I expected anything else, I would have coined a different word.

The quantity $3 - D$, called codimension or Hölder exponent by mathematicians, is now called "roughness exponent" by metallurgists. More generally, fractal exponents play a central role. For fractal functions, the derivative $P'(t) = \lim_{\varepsilon \to 0} (1/\varepsilon)[P(t+\varepsilon) - P(t)]$ does not exist and local roughness must be studied in a qualitatively different fashion. I proposed the parameters of a relation of the form $dP \sim F(t)(dt)^{\alpha}$. The most important parameter is $\alpha = \lim_{\varepsilon \to 0} \{\log[P(t+\varepsilon) - P(t)]/\log\varepsilon\}$, which is roughly a Hölder (Hurst) exponent.

A finer study introduces more than one exponent (via the study of multifractals and of self-affinity) and (in a less well-known development) introduces the notion of lacunarity which also requires the "prefactor" $F(t)$ that enters in the expression for dP.

1.5. Summary

Roughness is inarguably a ubiquitous phenomenon and nearly always concretely relevant. Lacking glamor, it was approached with unsophisticated tools, which failed. Apparently, no fully general approach was really tried. A sensible approach came forth very late and through a special case, after the vibration of strings was given a counterpart in "scale-invariant roughness."

The ubiquity of roughness helps explain why mathematical fractals are of broad practical relevance and why fractal geometry is not about to run short of new challenges.

2. FIELDS DEVELOP WITH A CLEAR ENOUGH SELF-IMAGE BUT WITHOUT DEFINITION

To a well-organized scientist, "study of the notion of roughness" might seem too casual, but it is not. The treatise by "Bourbaki" authored by several individually eminent mathematicians characterizes general topology as "the study of the notion of neighborhood." Section 2.1 shows that this vagueness is not an exception to be frowned upon, but exemplifies a situation that is tacitly accepted and general. Section 2.3 argues that, even in the context of the examples in Section 2.1, fractal geometry is an unconventional "virtual discipline."

2.1. Wide-ranging nonfractal examples

To begin with, consider the following statement found in a major research journal. "TOPOLOGY publishes papers in many parts of mathematics, but with special emphasis on subjects which are related to topology or geometry, such as ... ." What follows is a long list of very specific and reasonably well-defined activities the editors wish to encourage.

Moving on, how do experts characterize geometry? S.S. Chern (born in 1911) confided that "I am glad that we do not know what it is and, unlike many other mathematical disciplines, I hope it will not be axiomatized. {*Comment:* Chern rightly perceives geometry as being broader than the field that Hilbert axiomatized around 1900!} With its contact with other domains in and outside of mathematics and with its spirit of relating the local and the global {Hear!}, it will remain a fertile area for years to come." Later, Chern wrote that "A property is geometric, if it does not deal directly with numbers or ... the coordinates themselves have no meaning." Oswald Veblen (1880–1960) and J.H.C. Whitehead (1904–1960) wrote that "A branch of mathematics is called geometry, because the name seems good on emotional and traditional grounds to a sufficiently large number of competent people." André Weil (1906–1998), the founder of the Bourbaki school cited above, observed that "The psychological aspects of true geometric intuition will perhaps never be cleared up. At one time it implied primarily the power of visualization in three-dimensional space. ... Some degree of tactile imagination seems also to be involved."

On these experts' mellow authority, combining the two words, "fractals" and "geometry," should pose no problem. Neither should my stress on the fruitful tension between the analysis and the geometric (synthetic) points of view.

Next, consider "What is it that we call complex analysis?" In the overview of a truly splendid textbook, Boas 1987 notes that "Complex analysis was originally developed for its applications; however, the subject now has an independent and active life of its own, with many elegant and even surprising results." A review, Piranian 1989, approves: "Boas avoids the folly of an impossible definition by making a modest declaration. It does not characterize complex analysis; but complex analysts know that no reasonable description of their territory could ever have remained satisfactory for more than a quarter century ... . Today, complex analysis is primarily the study, by analysis and synthesis, and with geometric, topological, algebraic, number-theoretic, or other cultural orientations, of complex-valued functions in spaces of one or more complex variables."

Another example, chosen for the sake of contrast: "What is it that we call a curve?" Camille Jordan (1838–1922) and Giuseppe Peano (1858–1932) transformed a seemingly straightforward and ancient notion into one that is obscure and controversial. P.S. Urysohn (1898–1924) and Karl Menger (1902–1985) attempted in the 1920s to define and organize a proper "theory of curves," but had few followers.

Attempts to answer the question of "What is it that we call probability theory" fare even worse, with a quirky added complication. My mentor, Paul Lévy (1886-1971), failed to be accepted in his lifetime as a full-fledged mathematician. To gain acceptance, some of his heirs willingly narrowed the scope of probability theory to fit a clear definition. Yet, uncertainty remains about who is, or is not, a *real* probabilist.

Now move from mathematics to physics. Mercifully, fewer and fewer investigators attempt to define turbulence before studying what they can.

As to quantum theory, Griffiths 1995 observes that "there is no general consensus as to what its fundamental principles are, how it should be taught, or what it really 'means.' Every competent physicist can 'do' quantum mechanics, but the stories we tell ourselves about what we are doing are as various as the tales of Scheherazade, and almost as implausible. Richard Feynman (one of its greatest practitioners) remarked: 'I think I can safely say that nobody understands quantum mechanics'."

Last question, where does physics end and mathematics begin? André Weil had no contact with physics, witness his account of his visit to Göttingen in 1926. On page 51 of his autobiography, Weil (1992), we read this: "As I found out much later, physics was thriving in Göttingen at that time ... . [Max Born was coinventing a deeply mathematical approach in quantum mechanics.] I had not the slightest inkling of this development when I was there." In that environment, the current wave of mathematical physics began by being scorned and dubbed either "mathematics without theorems" or "physics without conceivable experiments." Keeping in mind that no meaningful and durable thin line could ever be drawn between mathematics and physics, who would dare to draw one, today?

2.2. The case of fractals

Section 2.1 makes it tediously obvious that the theme of this section is not foolish. Moreover, it is clear that *fractal geometry,* like complex analysis in Boas' description, "has an independent life of its own."

M 1975O, the first book to use the term *fractal,* was mature and sensible in characterizing its scope informally. Moreover, its Chapter IX on *multifractals* was also informal; pointedly – though perhaps not wisely – it even avoided coining a word for them.

To my later regret, in M 1977F and M 1982F{*FGN*} I gave in and recorded a "tactical definition." The very general but open notion of fractal with irregularities between two important and well-defined but narrow notions: the topological and Hausdorff–Besicovitch dimensions.

This link led to unintended and unfortunate consequences and I have since renounced it. For example, it excluded the devil staircases, for which both dimensions equal 1. In an attempt to "save" those staircases as fractal, I later attempted to link fractality to self-similarity and self-affinity, but Chapter ⋆H2 shows that the latter concept is too controversial to provide a solid foundation. Today, I prefer to hold on to the devil staircases and stop looking for tactical definitions.

To summarize, the following questions may never have precise mathematical answers. "What are fractals and multifractals?," "What is self-similarity?," "What is self-affinity?," or "What is globality?," and other questions raised by this book. Whether or not fractal geometry proves sufficiently useful to survive, I think that such questions of terminology will cease to be asked.

2.3. Fractal geometry perceived as a "virtual discipline"

To be a "fractalist," one need not contribute to the journal *Fractals*; and meetings covering all aspects of fractals, abundant in the 1980s, tend to be replaced by specialized ones. There are no departments of fractals.

In other words, one cannot, as of today, perceive fractal geometry as a "regular" discipline or even one interdisciplinary effort. As a matter of fact, the popular appeal described in Section 5 is largely due to the topic's very "singularity."

During the heroic past, a combination of analysis and computer graphics was a wonder to almost everyone (and anathema to a few). Back then, one could unify fractal geometry by describing its modus operandi as an original "method." Both the wonder and anathema having been dimmed by acceptance, this combination is less and less identifying. Connections between fractals and music and painting (Section 5) add to the complexity of the mix.

The resulting picture is far in spirit from any clear-cut activity, like "geometric optics," "wave optics," "rendering of light and depth on the computer." Fractal geometry is a "virtual" discipline. It is close in spirit to "science of light"; perceived as a collection of several disciplinary or interdisciplinary efforts that mostly move forward largely ignoring one another but, every so often join for a while. If light is replaced by roughness, the preceding words apply to fractal geometry.

3. CONTRASTS: OLD-AND-EASY VERSUS NEW-AND-HARD; NORMAL VERSUS ANOMALOUS/PATHOLOGICAL; MILD VERSUS WILD RANDOMNESS, AND LOCAL VERSUS GLOBAL

Once again, among aspects of everyday experience revealed by our senses, roughness was the last to be tamed by hard quantitative analysis. Rather than being an historical accident, its lateness appropriately reflected a higher level of complexity and difficulty. This belief was awakened by the contrasts listed in this section's title, as well as related ones to be injected shortly. Those contrasts arose independently of one another, become increasingly sharp, and dominate almost all my scientific work. None is defined formally at this point and their mutual relations are not all known precisely but (Section 2) this is neither unusual nor serious. What matters is that those contrasts largely agree and draw a line between two "kinds" of science. One is old and relatively easy. The other is new and qualitatively harder; it includes fractal geometry.

3.1. The compelling notion that randomness and variability fall into one of several distinct "states": "mild" and "wild," and (in between) "slow"

The "two stages of indeterminism." My talk at an International Congress of Philosophy of Science (Jerusalem, 1964) appeared very late, as M 1987r. It contributed a defiant response to criticism from P.H. Cootner reproduced in Section 1.4 (and more broadly in M 1997E, especially Chapter E17). My argument was that the bulk of successful existing statistical models constructed only a "first stage of indeterminism in science." This stage left out certain phenomena – including many aspects of turbulence and nearly every aspect of finance – that exhibit an altogether higher temporal and spatial variability. Everyone else was trying to reach toward those phenomena by using the statistical techniques that physics found sufficient in a fast-receding past. But I had concluded that neither those techniques, nor simple adaptations and extensions, would be adequate. An understanding of the Nature of finance and turbulence could not be achieved without recognizing that those phenomena bring physical and social science into a "second stage of indeterminism." Altogether new mathematical tools were needed.

The various "states of randomness and variability.". The 1964 "stages of indeterminism" drew a boundary between "something old" and "something new," based on arguments of convergence versus divergence to be discussed in Section 3.2. That theme was gradually amplified and deepened as the need for an intermediate state became inescapable. It received sharper terms as it grew into my present theme of "mild" versus "wild" or "slow" states of variability and randomness.

This theme is easiest to express by a metaphor. As is well known, mechanics relies on a unique set of general laws. However, matter comes in several quite distinct states. Change is quantitative as long as a system's temperature is modified but the system remains a gas, a liquid, or a solid. Change becomes qualitative when a liquid evaporates or freezes. It is equally well known that probability theory relies on a unique set of general axioms. However, in surprisingly clear-cut parallelism to the three basic states of matter, I found it not only useful, but necessary, to recognize the existence of several quite distinct "states of randomness" as well as states of random or nonrandom variability.

To anticipate a possible criticism, it is good to point out that, under certain conditions (passage through special points called "critical"), states of matter can change from one to another continuously. It is reassuring to know that the same is the case for the states of randomness.

Technical contributions to statistics that closely relate to the states of randomness or variability. First, the letters *R/S* in the book's subtitle refer to discussions of a "new" statistical technique. In the 1950s, the hydrologist H.E. Hurst had stumbled on an expression I call *R/S*; in the 1960s, the papers reprinted in Part VI made it into a statistical tool. To investigate global dependence, diverse shapes that are not Gaussian fractals are examined in this book because they enter naturally in the study of *R/S*.

Second, Chapter H29 applies *R/S* to point processes and also introduces yet another statistical technique that is worth examining.

Parallelism between wildness viewed as a second stage of indeterminism and chaotic dynamics viewed as a second stage of determinism. Having met

Edward Lorenz around 1964, I was present at the beginning of the modern stage of the theory of deterministic chaos. In due time, I contributed to it by discovering and describing the Mandelbrot set. That set's generating equation is a complex plane version of the logistic map, one of the paradigms of chaos in one dimension. More importantly, while that set is a "static" one in parameter space, it is a graphic representation of every behavior that can be observed in quadratic dynamics.

One should view the theory of chaos as a second stage of determinism. Earth in its own way, both second stages push back the "edge of messiness," beyond which description is not yet available.

3.2. The process of mathematical generalization and its pitfalls

In mathematics, generalization is nearly always highly respected, for good reason, but in the sciences some distinctions must be drawn. The lower level of generalization buttresses familiar conclusions by showing that they continue to hold in broader and perhaps unfamiliar contexts beyond the original environment. But many generalizations of familiar limit theorems alter the premises to such an extent as to reach *very different* conclusions. At some critical point, to use a philosopher's words, "quantity changes into quality." Critical points of this sort will prove to be fundamental to my distinction between states of randomness.

In the context of probability, the mild and slow states are characterized by the validity and direct relevance of several separate but interlinked asymptotic theorems: (1) the law of large numbers; (2) the limit theorem that asserts that properly renormalized sums are asymptotically Gaussian; (3) the property that diffusion is asymptotically Fickian, that is, the renormalized sum spreads proportionally to $\sqrt{T}$; and (4) the property that, in a sum of increasingly many random components, the largest is asymptotically of relatively negligible size.

Given their broad and spectacular success and forgetting that all theorems involve specific assumptions, those familiar tools came to be treated as "folk theorems" that can be relied upon under *all* circumstances. More precisely, change in scientific approaches is most easily accepted when it is gradual. It was practically taken for granted that, in due time, incremental extensions would make the usual methods applicable to phenomena previously left aside: price variation, anomalous noises, hydrology, motions of the atmosphere and the ocean, turbulence, etc., ...

Anomalies. This word is widely used to denote instances of exponents that contradict the Fickian $\sqrt{T}$ and other phenomena that do not fit the folk theorems. They tended to be tackled independently of one another and received less careful attention than "normal" phenomena. Unfortunately, "anomalies" that are not faced do not go away. As applied to the anomalies, the classical tools only brought confusion, not the clarification expected from the use of the appropriate language.

To the contrary, I adopted very early the view that incremental extensions will never suffice. The wild state of randomness or variability brings together new observations with behaviors that were neglected or mishandled; many so-called "anomalies" coalesce in one major phenomenon that deserves to be investigated on its own.

In any event, anomalies tell us that the classical limit theorems are flatly contradicted by at least some facts that can no longer be left aside. The study of mild phenomena is the easiest, because their properties can be handled locally. The study of wild phenomena is harder, because in one way or another, the interactions extend to infinity. The contrast between local and global is discussed in Section 3.4.

The watchful view of generalization in the context of the crucial contrast between stationarity and nonstationarity. M 1982F{*FGN*} (pages 383–386) observes that "Ordinary words used in scientific discourse combine: (a) diverse intuitive meanings, dependent on the user; and (b) diverse formal definitions, each of which singles out one special meaning and enshrines it mathematically. The term *stationary* has a single mathematical definition. However, experience indicates that many engineers, physicists, and practical statisticians pay lip service to the mathematical definition, but prove by their work that they hold narrower views."

By and large, a *mathematically stationary* process can be *intuitively perceived as nonstationary; if so, it is likely to exemplify wild randomness.* The apparent nonstationarity may be due to the "Noah Effect" (a scattering of extreme events) or the "Joseph Effect" (for example, persistence). This possibility provides genuine justification for distinguishing a narrower and a wider view of stationarity. As a matter of fact, M 1965c{N7} shows how investigations of wild variability led to an even broader and wilder generalization, conditional stationarity.

Spectral analysis concerns another highly relevant example of the process of generalization. The underlying algorithm became well understood. While delicate, it no longer requires extraordinary care, but only as long as there is little energy in high and low frequencies. Khinchin and Wiener generalized spectral analysis from periodic functions to random processes called "stationary of the second order," such as light, for which spectral analysis has a rigorous justification. The Wiener–Khinchin theory was further generalized in several directions. Along this series of generalizations, however, a great deal is lost, even when the theory is applied to Gaussian processes. To echo the end of the preceding paragraph, M 1967b{N10} led to an even broader generalization, "conditional spectral analysis."

3.3. Divergences and the notion that much in the works of Nature and Man is ruled by what past mathematicians boastfully called "pathology"

The distinction made in Section 3.2 is discussed in detail in Chapter E5 of M 1997E; those who read French will find many further details in Part II of M 1997FE. The contrast between wild and nonwild strongly depends on a criterion of divergence or reduction to 0. Let me elaborate.

One of the few "tricks" of wide utility in the sciences consists in recognizing that existing tools should never be wasted and their use should continually expand to new contexts. This is how fractal geometry spreads – with the wrinkle that "the old contexts" are often a few years old.

"Tool" often denotes a formula; for example (see Section 1.3) the definition of the mean square deviation. One would not write down any such formula without expecting – with no word or even a thought – that it will yield a positive and finite value. The contrary is perceived as "pathological," as is amply documented in Part II of M 1997FE.

In almost every one of my investigations, to the contrary, some kind or another of would-be pathology enters at an early stage. As a result, again and again, a key early step has very often consisted in allowing the possibility that the formula in question "degenerates" to infinity or zero. Consequently, one needs an approach that avoids degenerate quantities.

Rather than a mean-square, the divergent quantity could be a correlation, the integral of a correlation, or even a probability. One way or another, much of my scientific work encountered initial resistance because it postulated a "divergence syndrome." A consistent criticism described me as a trouble-maker seeking "exotic" and even "esoteric" or "pathologically behaved" solutions, and willfully introducing complexity where none existed. In fact, each divergence provided additional evidence of the need to invoke wild randomness or variability.

In the long-run, to show excessive zeal is better than to lack originality or boldness. A definite lack of boldness unfortunately characterized my failure to pursue a thought expressed in M 1956c. There, the last sentence of the first page observed that the theory presented in that paper "reminds one of the critical points of physics. The difference is that a model with infinite fluctuations is unacceptable in physics and must be replaced, while the present study is precisely concerned with cases of divergence." As a result, I did not move on to study critical phenomena.

Among the links between my work and modern statistical physics (MSP), this "ironical twist of fate" (to quote page 104 of M 1999N) was a minor detail. An important link was drawn in Jona-Lasinio 1975, who showed a deep uncommon relation between MSP and one of my principal tools, the Lévy stable distribution.

3.4. Contrast between locality and globality

The broadest theme common to M 1997E, M 1999N, and this book is that of *global structures.* In loose wording that requires careful restatement in each instance, a structure is called local when it includes "wholes" that are to a large extent mere juxtapositions of "parts," the latter being mutually independent and not "interacting" significantly with one another. To the contrary, a structure is called global when the wholes are more than mere juxtapositions and the parts interact significantly on all meaningful scales. The scales may range up to infinity or only up to a large but finite bound.

For the fractional Brownian function $B_H(t)$ with $H \neq 1/2$, global dependence is of very long (in fact, infinite) range. By contrast, Markovian dependence is of short range, therefore local in the sense that its range is finite. Originally, my attention was drawn to global dependence by the behavior of the Nile and other rivers.

When there exists a correlation function $C(s)$ and Γ is defined as the sum $\sum C(s)$ or integral $\int C(s)\, ds$ from $-\infty$ to $+\infty$, locality is expressed by $0 < \Gamma < \infty$, while globality is expressed by $\Gamma = 0$ or $\Gamma = \infty$. Alternative and synonymous statements associate globality with dependance that is infinite, or of infinite span, range, or run.

In the works in finance collected in M 1997E, the contrast between locality and globality is expressed in terms of the mesofractal or multifractal forms of the phenomenon of concentration. Details are found in M 2001d.

Quick allusion to slow randomness. Slow randomness is the difficult intermediate case characterized by locality in asymptotically large systems contrasting with the appearance of globality within small systems. A typical example, discussed in Chapter E5 of M 1997E, concerns sums of lognormally distributed variables. A power-law distribution is criticized for having infinite moments. Those who promote the lognormal distribution observe that it fits the "bulk of the data" just as well, but has finite moments, in fact, upsets no classical arguments. This may be true, but in the problems I pursue "the bulk of the data" matters little, as compared to the extreme data to which the lognormal attaches an unrealistically suitable probability. Chapter E9 of M 1997E presents my "case against the lognormal."

"States of nondecrease of a function". A secondary metaphor is less compelling yet useful. The definition of a nondecreasing function is of great generality, but Lebesgue recognized three very distinct "states of nondecrease": differentiable, discrete, and singular, each of which demands a very different treatment. M 1999N largely deals with *singular* variation.

4. HISTORICAL ROOTS: DECORATORS USED FRACTALS FOR MILLENNIA, MATHEMATICIANS, FOR A CENTURY; FRACTAL GEOMETRY ITSELF DATES TO THE 1970s

4.1. A loose analogy and some surprising lessons of history

The distinction underlying this section's title is illustrated by a well-known past event. Addition, multiplication, and rotation were known for millennia before the early 1830s, when Galois's group theory reinterpreted them as being very special groups. This story should help the reader distinguish between specific fractals, by themselves, and the organized study of fractality.

Early on, my historical and conceptual horizon was limited to one century of "mathematical pathology." Several "monster" constructs have been recorded around 1900 by the likes of G. Cantor, G. Peano, H. Von Koch, and W. Sierpinski (see Section 4.2). Those shapes were invariably described as having been "invented" by mathematicians searching for the "purity" imagined by Plato, and flying away from all shapes previously known to either Nature or Man.

For the sake of convenience and accuracy, the old monsters deserve, in my opinion, to be called *protofractals* and *protomultifractals* (in this instance the Greek root *proto* means *first in time*). In the framework of Section 7 of this Overview, it is revealing that, while those constructs were geometric, they were drawn very casually or even (as in the case of Peano 1890) not at all. The putative uselessness of nondifferentiable monsters had one exception, but one that plays a central role in my work. An (almost everywhere almost surely) nondifferentiable curve was proposed by Norbert Wiener in the 1920s as a model of physical Brownian motion.

In due time, the true historical background of the nonrandom monstrous shapes revealed itself as being totally different from what I had been taught: far longer, richer, and more interesting.

Just like addition and multiplication, those shapes had been known for millennia, not as monstrous but as decorative. Indeed, readers of M 1975O, M 1977F, and M 1982F{*FGN*} unleashed a flood of references and illustrations that proved that the principles behind those constructions were ageless and well represented in the arts and the decoration of diverse cultures. The "Sierpinski carpet" follows the pattern of perhaps half of the Buddhist mandalas that I saw in art books and museums; the "Sierpinski gasket" is found on the pavement of the Sistine Chapel in the Vatican.

Even the Cantor dust proved far older than thought. For example, its construction is prominent in the pattern of the capitals topping the columns of a temple in Pharaonic Egypt (Eglash 1999, page 207; reproduced in Frame & M 2001f and 2001p). Altogether, for decades, centuries, and even millennia, protofractals have been familiar in architecture, decoration, and design. In many cultures far removed from one another in time and space, they were growing in the fertile soil that combines magic and religion, decoration, and high art. This observation having been made, its development must be left to another occasion.

Be that as it may, mathematicians formalized old decorative shapes as monsters and fractal geometry sorted out the mathematicians' scattered and nameless monsters into fractals and nonfractals. The act of putting fractals to very wide use in the sciences showed that the putative "pathology" is a widely valued rule in Nature. This brought forth the fractals' true nature – namely, their link with roughness sketched in Section 1 – and made them multiply.

Every analogy is, at best, partial. Returning to group theory, that field moved from the top down. To the contrary, the goals and scope of fractal geometry continue to emerge very gradually as needs develop.

4.2. Eponymy

Lending a person's name to mathematical or scientific fact or procedure follows unwritten customs that vary greatly in time, in space, and between individuals, fields, nations, or "schools." Nevertheless, I view eponymy as a good idea in the sciences. It is often accurate and, if nothing else, bears witness that scientific progress is, ultimately, due to actual people.

Many keen observers' ambition goes further. They claim that credit for a discovery, invention, or idea should be granted to the "person who did it first." This claim raises many practical problems, for example, a continually improving awareness of the past would cause the same thought to be relabeled. Besides, there are many cases where eponymy was plainly *not* meant to imply credit. (The best examples, "Fuchsian" and "Kleinian" groups, would take too long to describe.)

Furthermore, there is deep truth in the statement by Arthur North Whitehead, that "everything of importance has already been said before, by someone who did not discover it."

For example, Section 1 of Chapter H8 will report that some subtle properties of random walk (RW) and Wiener Brownian motion (WBM) had been anticipated in 1888. This anticipation adds to our appreciation of the deep romance of scientific search and discovery. But apparently it had no effect on the history of ideas. Similarly, M 1982F{*FGN*}, pages 420–421, describes the origin of the notion of a continuous but nondifferentiable function. The contribution of K. Weierstrass remains a beacon in the history of ideas, but those of B. Bolzano and C. Cellérier are (charming) episodes of the romance of scientific search and discovery.

The preceding distinctions may seem overly subtle and I perceive them more clearly today than I used to. On past occasions, I was rightly criticized for over-citing old authors who truly do not matter.

How the fractal "carpet" and "gasket" curves came to be named after *but emphatically not* credited to *Waclaw Sierpinski.* To continue on the "mea-culpa" tone of the last sentence, Waclaw Sierpinski specifically wrote that the "gasket" and "carpet" were pointed out to him by others, who may have found them elsewhere, ... perhaps in decorative designs? However, at several points of his life, the man and the mathematician took steps that had enormous and very direct effects (unintended or contrary to intention) on my survival and my work.

Let me elaborate. Sierpinski was fanatically devoted to the foundations of mathematics and used the shapes in question as instruments to attack physical intuition. But he unwittingly prepared the tools needed to fight his own fanaticism. After I had made those shapes more important in physics than in mathematics, easily remembered labels became desirable. I chose to name them after Sierpinski.

The excuse is that he proved a specific mathematical property for each of those. But, under different personal circumstances, this may not have sufficed; anyhow, making me a follower of Sierpinski is more of a joke than my attaching his name to those shapes.

Arcadia. This brilliant and very successful recent play by Tom Stoppard made the Comédie Française break a centuries-old rule by producing a work by a living author not writing in French! Part of the action occurs in 1809, and Thomasina Coverly – a 13-year-old heroine – wants to tell everybody about her "New geometry of irregular forms"(!) She talks about fractals and chaos and quotes almost word for word from M 1982F{*FGN*}. In the play, her fantasies are not taken seriously in her time; her papers are unearthed only after fractals and chaos have been discovered. *Arcadia* serves as a powerful reminder of the romance that accompanies the historically documented cases of society's reaction to ideas that are put forth "before their time."

5. UNCOMMONLY INSTANT GRATIFICATION: SHORT PATH FROM SIMPLICITY TO GRAPHIC AND SCIENTIFIC COMPLEXITY

Few seekers of instant gratification are attracted by science. But fractal geometry is a bright exception: some of its facets reach to the child, the "common man," and the artist.

Testimony of the fractal pictures' impact on the understanding of the nature of music is provided by our great contemporaries, Charles Wuorinen in the USA and György Ligeti in Europe. In words I blush to repeat, the latter described fractals as "the most complex ornaments ever, in all the arts, like the *Book of Kells* or the Alhambra. They provide exactly what I want to discover in my own music, a kind of organic development."

My work in fractal geometry was once cited as having "changed our view of Nature," in large part because its sober investigations also have an unusual popular appeal. This important issue deserves a special section.

5.1. In fractal geometry, the path between what is simple and what is complex, even impossibly hard, is often surprisingly short

Anyone, adult or child, enthusiast or hater of mathematics and science, can call up a simple program that generates the Mandelbrot set and be near-instantly gratified by an eye-popping display. This enchants teachers who – like the contributors to Frame & M 2001f – want to expose their charges to mathematics and believe that it is best to choose a branch that is welcomed instead of resisted.

Among seasoned seekers for new mathematical facts, some are also blessed with the sense of shape that belongs to a large minority of humans. Others respond to a lesser degree, but for all, the above-mentioned eye-popping displays are, as we shall see in Section 6, a rich new source of extraordinarily difficult problems. There are sound reasons for their calling the Mandelbrot set "the most complicated object in mathematics!"

Seasoned seekers for new physical structures were enchanted when Witten & Sander 1981 described the diffusion-limited aggregates (DLA). Those coral-like shapes are essential to understand disorganized growth and the program that generates them takes a few lines. But for the past 20 years these shapes have resisted mathematical and physical argument.

"Cartoons" of mild and wild behavior are introduced in Chapter ⋆H1 and studied there and throughout this book. By design, their messages can be understood by the most numero-phobic observers. Somehow, they put off some formalists but, they are of the utmost benefit to the understanding of otherwise quite elusive points in both finance and physics.

5.2. The threats and great promise of the popular – even glamorous – aspects of fractal geometry

Will the instant gratification described in the last section last? Time will tell; the prospects are good, and one must not spoil present enjoyment.

Everyone was surprised when "fractals for schoolchildren" and "fractals for artists" suddenly became "glamorous." This happened in the *total* absence of any public relations campaign or either verbal or financial support whatsoever from the education foundations and/or art "establishments." As one of many bits of evidence of what happened, a search for fractals on the Internet overwhelms by sheer numbers. Those sites and the other aspects of fractals' wide acceptance in the general population may provide serious fractalists with a quick ego-trip, but what about the morning after?

To an elitist, the thought of a popular response is surprising or even embarrassing. To take an example, today's music and painting are widely viewed as confirming the opinion that it is no longer possible for art to be *both* popular and serious. And the number of charlatans claiming to "work between disciplines" suggests that the narrowest specialization is unavoidable.

Emile Durkheim (1858–1917), an authority among sociologists, went further. He promoted "the division of labor [as] a categorical rule of behavior, one that should be imposed as a duty. It is true that those who infringe it are not meted out in any precise punishment laid down by law, but they do suffer rebuke." I (repeatedly) "infringed it" and nearly always "suffered rebuke."

I think, to the contrary, that the popular responses to fractals are marvelous. They demand neither neglect nor apology. The institutions and media dealing with serious science are not perfect and the popular responses deserve gratitude for preventing some serious aspects of fractals from "falling in the cracks" instead of being widely accepted, as they were. The historical, sociological, philosophical, and even neurophysiological aspects of this acceptance are positive to an extent that cannot be exaggerated, a fact that should be pondered.

Political aspects on a stage of general interest statesmanship. Everyone knows that support for "hard" science demands a high level of public approval. Unfortunately, many scientists (with the possible exception of those engaged in star-gazing or healing) believe that *all* serious topics of science and technology are either (at best) ignored or (at worst) actively disliked and feared by "the people." This leads to the strongly felt need to compensate by public relations efforts on behalf of sciences and mathematics.

The only nice and lasting public relations efforts are also the least demeaning and most effective: they should be based on whichever topic is embraced most spontaneously. My proposal is that the welfare of science and mathematics may be helped by teaching fractals to non-mathematicians. The latter is the goal of a novel, ambitious, and gratifyingly successful course Michael L. Frame and I worked long and hard at establishing and rooting at Yale University; it is described in Frame & M 2001f.

Special interest political considerations. Every unifying "centripetal" notion ceaselessly struggles against powerful "centrifugal" forces. To minimize or deny fractal geometry's achievements is difficult, but they could easily end being scattered among other fields that wait with open arms. The glamor of "fractals for artists" and "fractals for schoolchildren" may defeat or delay the resulting great loss, the reason being that broad-purpose teaching of fractals cannot limit itself to one facet but relies very strongly on the coexistence and interactions between all facets. Thus, teaching at an early stage might provide fractal geometry with an unexpected durable "glue" that will help keep this virtual field unified under a widely and clearly perceived identity.

6. DIVERSE MANIFESTATIONS OF SCALE-INVARIANT ROUGHNESS IN MATHEMATICS AND THE SCIENCES

Once again, fractal geometry has one focus in mathematics and another in the broadly based discovery that scale-invariant roughness is ubiquitous (both in Nature and in man-made structures) but can be handled quantitatively. First viewed as shockingly surprising, fractality earned wide acceptance as an already almost banal form of structure. This section addresses the question, "What are fractal geometry's achievements as of today?" The soundness and usefulness of each facet should be evaluated on two grounds: as a contribution to fractal geometry, but mostly as a contribution to more specific issues shared with some other discipline.

This section's ambition is to be a useful sample centered on my own work. It is emphatically NOT an exhaustive or representative list of achievements. To fit so much in few pages, the references will be skimpy and individual entries will be very brief, hence largely directed to readers with a substantial background.

Another list of achievements, crude but surprisingly effective, resorts to an alphabetically ordered grab-bag *Panorama.* Frame & M 2001p is currently a website under continuing construction (and soliciting additional contributions). Presently, it supplements Frame & M 2001f and will, in due time, be made into a free-standing book. Any person who devoured encyclopedias at the appropriate young age will find in this *Panorama* a confirmation of the unreasonable effectiveness of the alphabetic order in bringing out, "spontaneously," a network of diverse relationships that no level of skill could list in logical linear order. (Of course, the advent of "hypertext" has obsoleted this feature of old-style dictionaries.)

The severe and unavoidable difficulties linked to a logical order are about to be illustrated in this section.

6.1. Fractal geometry's "practical side:" a quantitative and organized new "language of shape," a "toolbox" of statistics and data analysis, geared toward the study of wild randomness and variability

Section 7 will deal with the notion of "language of shape." "Toolbox" is a good term because it brings to mind electricians and plumbers. Their efforts are gritty, unglamorous, and never completed, but absolutely essential. Each of the problems with which this book is concerned is expected to end its life cycle with a glamorous proof or theory. But it invariably begins within a mess with no visible rhyme or reason. In some cases, that mess came up yesterday; in other cases, it was waiting for millennia or centuries until circumstances changed and allowed it to spring out as a beautiful butterfly everyone admires.

Section 3 of this Overview argued that professional statistics and the very informal statistics practiced by scientists and engineers were both designed with a built-in limitation, an unwitting but very strong one. Their near-exclusive goal was to deal with the restricted forms of randomness and variability that M 1997E called "mild."

A recognition of the ubiquity of scale-invariant roughness was necessary to carry out in parallel two mutually beneficial efforts: invention of new tools of statistics and data analysis and use of those tools wherever they fit, without being constrained by boundaries between disciplines.

6.1.1. The original and narrow fractal toolbox. The original fractal toolbox was started with adapted versions of existing down-to-earth findings and mathematical esoterica. Historically, the first was the "power-law" probability distribution $\Pr\{U > u\} \sim u^{\alpha}$, for which α is a critical exponent. This distribution had largely stayed on the margins of statistics. It remained "orphaned" largely because of its moments' "misbehavior": for the qth moment $EU^q = \infty$ if $q > \alpha$. I combined it with the non-Gaussian stable distributions, thereby changing the latter from being "exceptional" into a sturdy tool.

Later but more widely known borrowings were the Koch curve and a few other recursive constructions and the formula $D = \log N / \log(1/r)$ that yields fractal dimension in the self-similar case. Lurking behind, were nondifferentiable and infinitely discontinuous functions, singular monotone-increasing functions, the Hausdorff–Besicovitch dimension D_{HB}, and the Hölder exponent.

To give a flavor of the past, D_{HB} was an esoteric topic. In his real variables course (1960), Besicovitch insisted that it is a technical analytic device without applications and without direct geometric interpretation. But it can take on noninteger values, is based on metric properties, and gives the right values for the sets for which it can be computed. I put it "up front" for tactical reasons: without the authority of an author like Hausdorff, the idea of dimension as a measure of roughness might have received no hearing at all.

6.1.2. Notable additions to the fractal toolbox. Esoterica was soon exhausted. For quite a while now, almost every new use of fractals calls for the introduction of additional tools that may or may not have roots in old mathematics. Extensive material is planned for *Fractal Tools,* a forthcoming volume of these *Selecta,* referenced as M 2001T.

The increased variety of roles of the Hausdorff–Besicovitch dimension D_{HB}. Fractal geometry opened many challenges that increased the importance of D_{HB} *even from the purely mathematical viewpoint.* At this moment, the best-known example is that the boundary of Brownian motion satisfies $D_{HB} = 4/3$. This was conjectured in M 1982F{*FGN*}; in 2000 (see Chapter ⋆H3) it was proven to great acclaim.

D_{HB} *itself cannot possibly play any concrete role.* D_{HB} is defined for arbitrary subsets of metric scales by a process involving an "infimum" and hence it can't be measured for physical systems.

Old alternatives to D_{HB}. Also motivated by pure mathematics, they could be approximated by very practical algorithms called box, mass, gap dimensions, and the like.

New fractal dimensions that preserve the information present in the preasymptotics. Inevitable extension of quantitative measurement from the "degree of roughness" to the "degree of emptiness." Moving beyond pure mathematics, the use of fractal dimension soon faced its own fresh "anomalies." For example, in the study of intersections of sets, M 1984e, Part II, reported solid heuristic arguments that yield negative fractal dimensions. In the corresponding mathematically rigorous arguments that involve Hausdorff's definition and its old variants, all those negative values "collapse" to the dimension $D_{HB} = 0$ of the empty set.

This collapse occurs during a passage to the limit. The loss of information that accompanies asymptotics is familiar in mathematics and physics. Whenever the limit is nondegenerate, we view it as essential and collect the deviations from the limit into a less important "error term." However, when the limit is degenerate, that "error term" is automatically "promoted."

In this spirit, a careful and open-minded analysis, which culminated in M 1995k, revealed that many arguments concerning the notion of dimension can be rephrased to pay close attention to the valuable information present in the "preasymptotics" but destroyed by asymptotics. This is particularly the case for the heuristics that led M 1984e to define negative dimensions. Once made rigorous, the process revealed an altogether new and far more versatile "kind" of dimension. When it is positive, it is a familiar measure of degree of roughness. When it is negative, it introduces an attractive novelty, namely, a numerical measure of the "degree of emptiness." In the plane, the intersection of a line and a point, or of two points, are said to be, respectively, of dimensions −1 and −2. Surprising but true, such negative values can actually be obtained experimentally.

Multifractals I. These objects are singular measures that arose bottom-up. They began with special examples and further special examples continually lead to significant extensions. The first multifractal, conceived in M 1972j{N14}, grew in an attempt to set straight some work by Kolmogorov. The next, introduced in M 1974f{N15} and M 1974c{N15}, involved random multiplicative cascades and grew from a seed provided by A.S. Besicovitch, namely the binomial or Bernoulli measure. They – especially the random ones – proved to be very interesting mathematically, hence are cross-referenced in Section 6.4.1.

Multifractals II: the $\tau(q)$ and $f(\alpha)$ *formalism.* Much of the interest in multifractals centers on two functions, $\tau(q)$ and $f(\alpha)$, that are related by Legendre transforms and have become very widely known. They first occurred in M 1974f and M 1974c, respectively, but their relevance extends beyond exact multiplicative multifractals, as documented in M 1999N.

Multifractals III. A neglected second aspect featured in the title of M 1974f: the quantity $q_{\text{crit}} > 1$ *obtained as the solution of* $\tau(q) = 0$ *and the critical property that* $E\{[d\mu(dt)]^q\} < \infty$ *if, and only if,* $q < q_{\text{crit}}$. The possibility and role of a finite value for q_{crit} was also conceived in M 1972j and M 1974f and was immediately confirmed in full rigor; again, see Section 6.4.1. Unfortunately, it was beyond the scope of the influential but incomplete later heuristics, therefore it remained little known and used.

At long last, it is presently "breaking out" because a need was created by the new model for the variation of financial prices that was introduced as M 1997E. The new model, fractional Brownian motion (FBM) in multifractal time, is explored further in M 2001a, M 2001b, and M 2001c. It demands a finite q_{crit} because I had shown in the 1960s (see M 1997E) that financial data involve power-law distributions.

Barral & M 2001 have investigated broader multifractals that confirm my belief that a finite q_{crit} is not a curiosity but a "generic" possibility.

Extension of quantitative measurement beyond dimension and emptiness; the wide-open notion of fractal lacunarity. Even when two fractals are identical in their topology and fractal dimension, they may "look" very different. The holes or "lacunas" that are a conspicuous signature of fractality may be smaller in one case than in the other, yielding a "less lacunar" texture.

Galaxies. As reported in Chapters 34 and 35 of M 1982F{*FGN*}, the need to quantify those differences arose outside mathematics, in the description of the distribution of galaxies.

Satistical physics. The study of critical phenomena had found it fruitful to imagine hypercubic lattices with a noninteger dimension. They are not constructive notions but unrealized abstractions: assuming the existence of objects that satisfy certain desirable conditions, one can predict very successfully the properties of certain real physical systems. The mysteries of those lattices were lessened by a geometric implementation found in Gefen, Meir, M & Aharony 1983. They considered the limits of sequences of Sierpinski carpets of lacunarity that converges to zero, and showed that the relevant physical properties of those limits converge to those of said unrealized abstractions. Those limits are well enough defined physically, but mathematically they are ill-understood. We barely scratched the surface of their study and they cry out to be taken up again and expanded. New examples are bound to come out and become widely used theoretical tools.

However, the greatest potential role of lacunarity resides elsewhere. For better or worse, the earliest fractals created an unstated but widely held mental picture of how fractals "look like." Faced with a shape that "looks fractal," many scientists know which tools to use in order to seek confirmation of fractality and conduct further study. To the contrary, low-lacunarity fractals actually look nonfractal, do not necessarily invite the proper confirmatory tests, and may even remain mis-identified. My concern is not that the list of fractals in Nature is not long enough as it is. The main concern is that unidentified fractals will be studied with inappropriate tools, and instead of helping, those tools will potentially lead to confusion.

6.1.3. The positive past role of the smallness of the original core toolbox of fractal geometry, and the burning present, need to incorporate numerous extensions. Let us return to a question raised in Section 6.1.1. The practice in pedagogy is to handle simple structures and ideas before the complex ones. This creates the widespread tendency to take it for granted that history followed the same order as pedagogy. In particular, it happens all too often that outsiders and even part-time fractalists perceive the common core of fractal geometry as very slim.

For example, it seems natural to take for granted that the multifractals' complex structure arose *after* the simple core and after I coined the word *fractal* to satisfy the needs of M 1975f{H18}, M 1975w{H19}, and M 1975O. To the contrary, multifractals (minus the name) came *before* 1975. Specifically, consider M 1974f{N15}, which first described multifractals and eventually became well known. Annotations in M 1999N show that this paper was the laborious end-product of *six* years of refereeing marked by non-refereed publications, M 1969b{N13} and M 1973j{N14}. Referee reports on M 1974f made obvious that its acceptance risked being extraordinarily slow, ... unless a well-disposed community could be cajoled into existence. This is why M 1975O adopted an engaging style: an enormous amount of work was motivated by the dire need to create a home for multifractals.

That is, a core was carefully crafted to be small and nonintimidating in order to help an emerging discipline come out. However, acceptance having been achieved, that simple core's narrowness and simplicity is a brake on progress. Giving further evidence that the communications between disciplines are pathetically inadequate, several of the interdisciplinary aspects of fractal geometry now tend to develop in ignorance of one another. Work already accomplished in one science is all too often redone elsewhere, painfully and often inadequately.

I see no merit to the implicit belief that, beyond the core, additional developments are specific to one question. To the contrary, greater mutual awareness demands that the common core should expand.

6.2. Exploration and inventory (by structure and subject matter) of scale-invariant roughness in natural or man-made structures

6.2.1. As tools improve, a never-ending inventory task. As well as for the toolbox, the inventory began by giving a home to scattered "orphan findings" that seemed to belong nowhere. The first was already mentioned: it was the "power-law probability distribution" $\Pr\{U > u\} \sim u^{-\alpha}$, known since 1897 as Pareto's law but scorned by statisticians; its infinite moments lead to its being widely described as "improper." Scattered instances of this power-law distribution had been recognized in the past but all belonged to neglected (and often vocally despised) esoterica of the social sciences.

To the contrary, I recognized them as tokens of a phenomenon that proved to be widespread; it is scaling. Section 1 of this Overview has already stated that, in retrospect, the step that made roughness manageable was a process of seemingly drastic specialization – certainly not of mathematical generalization! Only gradually did I see that roughness with properties of scale-invariance was the proper first approximation to roughness in general.

As new tools join the fractal toolbox, scaling proves to be universal and this in turn requires new tools, and the cycle continues.

As already noted, to list all occurrences of fractals would be an impossible task. A modest practical *Panorama* is provided by Frame & M 2001p.

6.2.2. "Intermission" on the proper interplay between "practice" and "theory." The glamor of rigorous proof and full scientific explanation is deserved and will soon receive its due. In particular, good engineering is often greatly helped by the availability of full explanations. But the absence of explanation does not stop engineering.

Take turbulence. While it still waits for an explanation, its harmful effects continually decrease, in part, thanks to improvements in description.

Take traffic on the Internet. It is extraordinarily well represented by a multifractal. An explanation will allow engineers to improve the traffic, but this may take time. In the meantime, it would be worse than foolish to manage Internet traffic without acknowledging its multifractality.

More generally, in finance, hydrology and, surprisingly, many other practical aspects of the study of roughness, the challenges of engineering are all too easily overshadowed by the worship of glamorous explanation. But all the newly available fractal descriptions and tools should be put to immediate use without waiting for a theory explaining why they work.

In any event, elitist concern with explanation is not neutral. When necessity presses us on, the earlier scorn expressed for anything less than a full explanation tends to be forgotten and there is a rush to "quick and dirty curve-fitting," "mere phenomenology," or maybe "tradition." In the case of the Internet, tradition takes the form of the Poisson model, which is thoroughly at variance with the evidence.

I like the advice of Ludwig Wittgenstein that "Of things whereof one cannot speak, one must be silent." (This may be the only proposition of his that I understand.) A "middle way" that is often successful is implied when *Ecclesiastes* reminds us that "for every thing there is a season, ... a time to keep silence and a time to speak." It is also true that on the frontiers of science there is "a time to explain and a time to explore." The engineers (including those in finance) need not feel unduly apologetic when they disregard the scientists' goals, but satisfy their own.

6.3. Examples of crucially important fractals that demand both physical "explanation" and mathematical proof

6.3.1. The challenge to explain why so many rough facets of Nature are scale-invariant. This challenge concerns both physics and astronomy, and also man-made structures, such as finance and computers. While "mathematical proof" is a well-defined notion at a fixed moment in time, "physical explanation" is a tricky basic concept, more typical of the situation discussed in Section 2.

To "explain" used to mean to "identify the cause" (or "a possible cause") requiring no further reductions to more elementary considerations. But computer simulation and fractals each added an important wrinkle. For galaxies, clustering might not at all demand physics beyond basic gravitation. As argued in Section 6.3.3, computation suggests that Newtonian attraction suffices to create clustering. Second example: every one of the subtle properties of the clusters of DLA is fully caused by its generating algorithm – which is short and can be motivated reasonably well.

Fractals add a wrinkle provided by "absence of cause." A "principle" credited to Pierre Curie asserts that all the symmetries or invariances that are present in a problem must be preserved in its solutions. When the equations are scale-free, does it follow that the same must be true of the solutions, thus implying that the solutions must be fractal? Return to the example of DLA: its first-approximation self-similarity seemed to be explained by the fact that the construction involves only the scale of the atoms. It was expected that the transients due to this scale would soon disappear as the cluster grows; in fact, they turn out to persist far longer than expected.

The question is whether or not the preceding arguments *explain* exact or approximate fractality. The physicists' unanimous answer is *No.* This proves that, while giving lip service to causes, physicists really look for *detailed mechanisms* that yield numerical values. They ought to agree with experiment and be intuitive but need not be mathematically rigorous. (The opposite applies to mathematical proof.)

As already mentioned, 1/*f* noises are protofractals that entered physics long before the word. They are ubiquitous and seem unified by a formula of unbeatable simplicity. The first example was discovered in the 1920s and ever since the topic has seemed to be ripe for a simple and universal explanatory "magic bullet." But none has come forth.

In a different context, social scientists praised Zipf 1949 for putting forth a "principle of least effort" that no amount of effort made quantitatively useful.

Disappointing but true, and whether or not the fractal aspects of economics and finance are included, the situation remains unchanged. No *universal* magic bullet is known – even in the faintest outline – to apply to all fractals. Specifically, my work has shown that "1/*f* noise" is an illspecified notion that formally brings together several sharply distinct behaviors. The search for that magic bullet is at present quiescent and I hope it becomes discredited and replaced by modest case-by-case searches.

We must rejoice in the fact that many *specific* examples of wild variability in Nature (or finance) are more or less fully explained; examples will be given momentarily. Others can be *accounted* for (this term does not hide its openness and vagueness) in convincing but incomplete fashion. Many other examples remain more or less *mysterious.* That is, many brilliant scientists investigated them, did their best, but could not go far. The study of fractals has contributed to the solution of many problems but may perhaps have solved fewer hard problems than it has posed.

Section 3 of this Overview suggested that those investigations may well have opened a new frontier of science. A huge number of papers have been written on fractal themes and much has been accomplished, yet turbulence warns us that after a certain point, interesting questions will remain but progress risks being slow.

6.3.2. Attractors and repellers of dynamical systems; fractality in phase space is fully explained by chaotic dynamics. In many scientists' minds, explanation is best implemented in terms of a dynamic process that transforms an arbitrary initial condition into what is observed and must be explained.

From this viewpoint, it is widely known that chaotic dynamical systems' attractors and/or repellers are "generically" fractals. Those systems' structure is the "cause" of fractality. The mechanism is also known. A fractal's structure is, loosely speaking, hierarchical, and the kth step of the dynamical system implements the kth level of this hierarchy. Therefore, a dynamical system's successive step in time automatically creates a self-similar or self-affine "cascade" of increasingly fine detail. Details belong to a theory that combines chaos and fractals.

The preceding argument goes back to the 1880s. Poincaré's work on Fuchsian groups showed that the limit set can be "Cantor-like." Oddly, an algorithm showing its structure waited 100 years to be discovered; see M 1982F{*FGN*}, Chapter 18, M 1983m, and M 2001w. It is interesting at this point to paraphrase from a celebrated obituary of Poincaré that dates to 1912 and is reprinted in Hadamard 1968, page 1930. One reads that "Everyone will appreciate the deep difference that exists between, on the one hand, the Cantor dust, which is arbitrarily contrived with no goal and no interest other than ... an exhibit in a museum of monsters and, on the other hand, the same fact encountered by Poincaré within a theory rooted in the most common and general problems of analysis." (Each time one reads Poincaré or Hadamard, one finds more passing hints pointing to chaos theory and fractals. For many years, of course, those hints were not followed up. In the terms used in Section 4.2, they remained to a large extent a part of the romance of research.)

6.3.3. Fractal physical objects in real space and the open broad conjecture that fractality is typical of the singularities of solutions of partial differential equations (PDEs) of physics. Fractals that are important physical objects in real space include turbulence, galaxies, diffusion fronts, and DLA. Their fractality generated far greater surprise than the fractality of objects in phase space. They also remain far less well understood.

Several major real space fractals enter in problems that are ruled by partial differential equations, PDEs. The latter, a core subject in both mathematics and physics, have triumphed against many mysteries of Nature and preserve an endless intrinsic attractiveness. It is universally granted that physics is ruled by equations such as those of Laplace, Poisson, and Navier-Stokes. All differential equations imply a great degree of local smoothness, even though closer examination shows isolated singularities or "catastrophes." To the contrary, fractality implies everywhere dense roughness and/or fragmentation. This is one of the several reasons why fractal models in diverse fields were initially perceived as being "anomalies" that stand in direct contradiction with one of the firmest foundations of science. Are smoothness and fractality doomed to simply coexist without interacting?

My general proposal, made in Chapter 11 of M 1982F{*FGN*} is that there is no contradiction at all. One should expect fractals to arise unavoidably in the long time behavior of the solution of very familiar and "innocuous"-looking equations. In particular, many concrete situations where fractals are observed involve equations having free and moving boundaries and/or interfaces, and/or singularities are not prescribed in the statement of a problem, but determined by the problem's solution. As a suggestive "principle," it may be that, under broad conditions that largely remain to be specified, these free boundaries, interfaces and singularities converge to suitable fractals.

6.3.4. Turbulence, the Navier–Stokes and Euler equations of fluid motion and fractality of their singularities. Berger & M 1963 was near simultaneous with Kolmogorov's work on the intermittence of turbulence. I soon extended the fractal viewpoint to turbulence, and was led circa 1964 to the following considerations.

The fractal nature of turbulence. I conjectured that the property of being "turbulently dissipative" should not be viewed as attached to domains in a fluid with significant interior points, but as attached to fractal sets. In a first approximation, those sets' intersection with a straight line is a Cantor-like fractal dust having a dimension in the range from 0.5 to 0.6. The corresponding full sets in space should therefore be expected to be fractals with dimension in the range from 2.5 to 2.6.

Conjecture concerning the equation of fluid motion. The dissipation in a viscous fluid occurs in the neighborhood of a singularity of a nonviscous approximation following Euler's equations, and the motion of a nonviscous fluid acquires singularities that are sets of dimension about 2.5 to 2.6. To prove or disprove this conjecture, under suitable conditions, remains an open mathematical problem. Several numerical tests agree with this conjecture (e.g., Chorin 1981).

6.3.5. The distribution of galaxies in the wide range of distances where it is fractal; Newton's law as a possible sufficient generator of fractality. The ultimate problem underlying the large scale distribution of galaxies is widely believed to involve exotic forces and particles. But no one knows for sure. The near-universally held view use to be that the distribution of galaxies is homogenous, except for local deviations. To the contrary, Chapter 9 of M 1982F{*FGN*} argued that all the then-available data suggested that fractality holds over a broad range of distances. This view has been confirmed and developed by L. Pietronero and his associates. The issue of eventual crossover to homogeneity remains in dispute.

Contributing to the open question of the origins of fractality, I conjectured that – solely or in association – clustering may involve Newtonian attraction and Laplace potential, in a case where the singularities are not fixed. This conjecture results from a search for invariants that was central to every aspect of my construction of fractal geometry. Granted that the distribution of galaxies certainly deviates in some ways from homogeneity, two broad approaches were tried. One consists in correcting for local inhomogeneity by using local "patches." The next simplest global assumption is that the distribution is non-homogeneous but scale-invariant.

Another reason why fractality came to mind is because of what computer simulations suggest. Consider a large array of point masses in a cubic box in which opposite sides are identified to form a three-dimensional torus. The evolution of this array is a problem that obeys the Laplace equation, with the novelty that the singularities of the solution are the positions of the points, therefore movable. The numerous simulations

I know of (beginning with those performed by IBM colleagues around 1960) all suggest the following. Even when the pattern of the singularities begins by being uniform or Poisson, it gradually creates clusters and a semblance of hierarchy, and appears to tend toward fractality. It is against the preceding background that I conjectured that the limit distribution of galaxies is fractal, and that the origin of fractality lies in Newton's equations. Therefore, the problems of solutions of this form of the Laplace equation will continue to be a fascinating one, even if the study of galaxies takes a different course.

6.3.6. The shape of diffusion-limited aggregates (DLA) and diffusion fronts. A far less well-known but difficult third example is the shape of diffusion-limited aggregates (DLA). Computer simulation shows that DLA is fractal. The underlying equation is, again, Laplace, but here the boundary can move in response to the solution. This unprecedented feature introduces fascinating complexity.

Another little known example enters in diffusion fronts. The underlying equation is the finite difference counterpart of Fourier's heat equation. The fractality of diffusion fronts was first observed numerically, then explained by an extremely roundabout argument that reduces it to percolation clusters. In the other examples, fractality has been established numerically. This is, of course, not a full physical explanation, only an explanation-in-principle. The mathematicians' search for actual proof has not gone far, either. But the fact that diffusion fronts are fractal is now unquestionable.

Conclusion. Without waiting for the detailed developments everyone hopes to see in the future, equations like those of Laplace and Fourier must be viewed in a new light. Under certain well-known conditions, they rule smooth behaviors; under other conditions that are almost competitively open, they generate important forms of fractality.

6.3.7. Fractals and the clusters of statistical physics: the Brownian 4/3, percolation, and the continuing mysteries of DLA. A variety of "clusters" constitute a third category of very important fractals that raise problems straddling mathematics and physics and are currently undergoing rapid progress. Thus, in the case of the physical clusters discussed in the preceding section, fractality is the geometric counterpart of scaling and renormalization, that is, of the fact that the analytic properties of those objects follow a wealth of "power-law relations." Many mathematical issues, some of them already mentioned, remain open, but the overall renormalization framework is firmly rooted. Renormalization and resulting fractality also occur in arguments that involve the attractors and repellers of dynamical systems. Best understood is renormalization for quadratic maps. Feigenbaum and others considered the real case. For the complex case, renormalization establishes that the Mandelbrot set contains infinitely many small copies of itself.

Unfortunately, additional examples of fractality, such as DLA, led beyond the scope of the usual renormalization.

A foretaste of Chapter ⋆H3 is given by the dimension 4/3. Measurements – some of them preceded by "eyeball" examination – establish that in diverse "clusters" (Brownian, percolation, Ising and the like) the boundaries or "hulls" are of fractal dimension 4/3 or 7/4. Several simultaneous tasks arise. The observed facts are to be explained and/or proven. For the Brownian cluster, Lawler, Schramm, & Werner 2000 have at long last proved mathematically that the Hausdorff–Besicovitch dimension is indeed 4/3. Percolation clusters have the dimensions 7/4 (Voss) and 4/3 (Gossman, Aharony). This was at long last proven mathematically in Smirnov, 2001. Does 4/3 yield physical and/or mathematical lessons concerning the nature of the plane? The physicists' answers are unlikely to satisfy the mathematicians and conversely.

6.4. Several fields of "pure" mathematics that do not involve physics simply overflow with major fractal problems and conjectures

Many of those conjectures and the concepts to which they refer are uncannily visual.

Section 5 described how even raw beginners – for example, adolescents and nonspecialists – can often understand what is at stake. They all marvel at the shortness of the path that involves no long preliminaries, begins with simple formulas and telling pictures anyone can generate, and soon reaches to forbidding mathematical difficulties. Chapter ⋆H3 will tell how the 4/3 conjecture lasted 18 years. This section will sketch other such examples for the specialists. Contributing their share to the demise of Bourbaki, fractals helped bring back into mathematics a deep interest in physical intuition, seasoned with new problems, and a deep appreciation of the visual intuition the computer can help retrain.

6.4.1. Topics on fractals in pure mathematical analysis. *Sets of unicity of trigonometric series.* If two trigonometrical series converge to the same finite values over the interval $[0, 2\pi]$, then their coefficients must be identical. George Cantor's efforts to find other sets of unicity led to his development of set theory, and this search is the grandfather of all searches for "exceptional sets" in analysis, many of which are fractals.

Kahane & M 1965{N11} combined my "fractal before the word existed" physical intuition with Kahane's mathematical skills to greatly streamline an ancient key chapter of the theory of trigonometric series. Answers built over many decades suffered from the extraordinary complexity and complication typical of harmonic analysis. Kahane and I clarified and simplified this picture.

Multifractal measures. As mentioned in Section 6.1.2, my early papers on "multifractals before the word existed," raised many difficult conjectures. The first rigorous results, due to Kahane & Peyrière 1976{N17} opened up a broad and quickening stream of works.

The frontier in Stieltjes moment problem. Contrary to a heuristic that most scientists view as "intuitively" true, the sequence of moments of a random variable may leave its distribution undetermined. This would-be "anomaly" holds within a context whose boundary is poorly defined by conditions that are either necessary or sufficient, but not both.

A condition roughly equivalent to the classical ones arose when I was setting up the boundary between the mild and slow "states of randomness" (Section 3.1). My condition is $P_N(x)/NP(x) \to 1$ as $x \to \infty$, where $P(x)$ and $P_N(x)$ are the tail probabilities of a random variable X and of the sum of N independent replicas of X. This condition's possible relation to the moments problem deserves to be investigated.

6.4.2. Conjectures concerning the Mandelbrot set, for example, the "MLC" conjecture that asserts that "the boundary of the Mandelbrot set is locally connected." The major remaining problem concerning the Mandelbrot set of the map $f_c(z) = z^2 + c$ is best explained by the circumstances of the discovery of that set. Pure mathematics (namely the theory of Montel's normal families of functions) gave rise in 1917–1919 to the Fatou–Julia theory of iteration. But this theory ceased to be active, because pure mathematics provided no interesting new question. I "reactivated" it 60 years later on the basis of computer exploration.

I first became familiar with the actual shapes of the Julia sets defined as the repellers of $f_c(z)$. Then I continued by attempting to explore numerically the set M^0 of the values of c such that $f_c(z)$ has a finite stable limit cycle. To investigate this M^0 proved to be both tedious and imprecise, I moved on and attempted to explore M^0 indirectly, through a "surrogate." Since no theory could guide the choice of surrogate, I let myself be guided by convenience of computation and chose to study in depth the set M of values of *c* for which the Julia set is connected. It is clear that sufficiently crude numerical approximations contain M, and that M contains M^0. Comparing the two sets, I conjectured that the difference reduces to those limit points of M^0 that are not themselves part of M^0.

For 20 years, this conjecture has been the Holy Grail of sophisticated investigators. It was rephrased as asserting that "the Mandelbrot set is locally connected." That restatement means that a certain property holds for every point in M. By constant repetition it became known as MLC and partial proofs were provided for "most" points. They have drawn great admiration. However, more than 20 years after I put it forward, the conjecture still stands wide-open. As to several other (more detailed) conjectures which arose from my original exploration, some have been proven near-instantly, others took 5 or 10 years.

7. EXPLICIT AND VISUAL GEOMETRY LIVES ON WITH NO ALL-PURPOSE SUBSTITUTE; GEOMETRY AND LANGUAGE

To conclude, let us dwell on the renewed dialectical tension between the analytic and the geometric (synthetic) points of view in mathematics and the hard sciences.

Mathematics is the most formal form of discourse, and a neo-Platonist view follows the Evangelist who proclaimed that "In the beginning was the *Word.*" Goethe's *Faust*, reaching to earlier beginnings, tried to replace word by *Sense*, *Power,* and even *Deed*. Closer in spirit to this book, it seems likely that the *Word* was preceded by the *Image*.

Despite its glorious past, geometry has lately been scorned and shunned, and the allegation that "geometry is dead" is very old. Plato bears a very heavy share of responsibility and René Descartes is praised for having fully and permanently replaced geometry by analysis. This replacement is more the hope of many than an achievement; it may perhaps be implemented in the future, but at present it is not.

Among those who worked hard at eliminating geometry, a major early culprit was born in Turin: Giuseppe Ludovico Lagrangia grew to become the great Joseph-Louis Lagrange (1736–1813). The defense of geometry by Gaspard Monge (1746–1818) was publicly humiliated by Lagrange in 1794 during the first teaching year at École Normale Supérieure in Paris. This occasion, witnessed by the young and struggling Fourier, is described in Dhombres & Robert 1998.

In more recent times, dominated by the "Bourbaki" ideology, images and appeals to visual intuition vanished altogether from mathematics and other hard sciences. This was interpreted as one of many inevitable consequences of an irresistible flow of history. True, geometry was becoming less and less fruitful. But this merely reflected a passing stage of technology. Computer graphics have given geometry a fresh lease on life and it is helping master roughness and resumes is place next to analysis, which is a form of language.

Today, describing the eye as powerless and geometry as dead reflects self-inflicted ideological blindness and deserves scorn – or reeducation!

Just before the start of Section 1, this Overview compared geometry to a physician's old-fashioned clinical skills, and analyses to tests that yield numerical results. (The reader may want to skim that description again before proceeding.)

Pure mathematics prides itself in being able to strip complicated notions to essential structures that suffice as a foundation of rich and inspiring developments. This is an achievement I profoundly admire; it inspired my work as well, for example, when invoking scaling and other invariances as organizing and motivating principles.

However, reduction to "essentials" must *never* be either rushed, relentless, or irreversible. In many important contexts to be encountered in this book, features for which there was no immediate need were hastily labeled as inessential and disregarded. Eventually, they end up playing a vital role that excessive or premature reduction would have ruled out.

Particularly illuminating are the many "doublets" made of a formula and a picture that coexist as "abstracted" or "visualized" forms of each other. The formula is praised because it is true that organized language alone can be handled analytically. Moreover, it is claimed that it contains the same information in a form that is concise and not burdened by anything irrelevant.

Do the fundamentals suffice for all purposes? As a first counterargument, consider the "anticipatory" chapter of chaos theory built around the iteration of rational functions, Section 6.4.2. In 1917, P. Fatou and G. Julia did marvels with formulas and no picture. But the theory they created slept for 60 years – simply for lack of exciting new questions to analyze. It did not start again until, resisting the condemnation of pictures that was at that time *universal* in mathematics, M 1980n first reported on what came to be known as the Mandelbrot set. Its baroque and anything-but-concise complication became the source of new mathematical questions no one could have imagined without the help of pictures.

As a second counterargument and an introduction to the recurrent need for a new language, consider once again the diffusion-limited aggregates (DLA). The generating formula was simple and I joined the many scientists who first found it pointless and unpromising. But the computer could translate it into a graphic output. Persons already cognizant of fractal geometry recognized this output as being a very important fractal and promptly subjected it to analysis by fractal and multifractal techniques.

Without relying on fractal geometry, would anyone have seen a link between the algorithm and the Hausdorff–Besicovitch dimension? The question is not as hypothetical as it seems: DLA-like structures had been photographed in the 1920s and published in a periodical, but in a very real sense *not seen.* Indeed, the fractal part was interpreted as an insignificant noise and removed before any analysis. Attention was actively directed to features that turned out to be genuinely insignificant but had a virtue: they could be analyzed by existing tools ... although with no benefit whatsoever to those performing the analysis.

Closer to this book and equally illuminating is a third counterargument against the neglect of geometry found in the many problems whose investigations have been triggered by a sound or a picture, and only later expressed by a formula that can be proclaimed or implied to be a full representation of reality.

I think of the often-mentioned noises that, having become stripped of everything deemed inessential, were labeled by their common property of having a "$1/f$ spectrum." This description's concision seemed admirable, but turned out to be a resistant barrier to actual understanding. My contribution came from forsaking concision and generality. Instead, I examined with focused care the reputedly inessential geometry seen on an oscilloscope or heard on a loudspeaker. Such study reveals at a glance that $1/f$ noises from different sources exhibit unsuspected but deep geometric differences. Those differences are bound to reveal deep physical differences and make it obvious that a unique physical explanation for all $1/f$ noises is a hopeless dream. M 1999N, Chapter N4, argues that to identify a $1/f$ spectrum is *never enough.*

Let us all praise explicit and visual geometry. This is a theme that runs increasingly strongly in my work. Now that the computer has given new power to the eye and geometry, their usefulness in science and mathematics has been revived and should again be recognized.

Blind analytic manipulation *is never enough*.

Formalism, however effective in the short-run, *is never enough*.

Mathematics and science are, of course, filled with quantities that originated in geometry but eventually came to be used *only* in analytic relationships. In many cases, those analytic relationships *are not enough*.

For example, fractal codimension is often the exponent of a correlation. But correlation *is not enough*. To identify D with an analytic exponent impoverishes or even drains out its meaning; *it is not enough*.

Yesterday's mathematicians claimed to have completely reduced geometry to analysis. Of course, *they have not.* The same actual shape, when examined with increasing care, often goes on to reveal fresh geometric features that strike the eye and affect physics. Many of those geometric features have not been tamed by the existing analytic tools and therefore demand entirely new analytic tools. This is how I was led to introduce the notion of lacunarity.

Scaling geometry is not a mere reflection but an essential counterpart of scaling analysis. It is rich in features of its own and celebrates the power of simple rules to create geometric shapes of extraordinary and seemingly chaotic complexity, shapes which analysis is then called upon to explain.

P.S. It has been suggested that this Overview should be allowed to grow in an environment that leaves space for better balance and full references. It may become a companion to the *Panorama* whose address is:

http://math.yale.edu/fractals/panorama/welcome.html

No promises are made but those interested may check the address:

http://math.yale.edu/fractals/overview/welcome.html