

The Opinion column offers mathematicians the opportunity to write about any issue of interest to the international mathematical community. Disagreements and controversy are welcome. An Opinion should be submitted to the editor-in-chief, Sheldon Axler.

Fractal Geometry Steven G. Krantz

Editor's note: *The following articles by Steven G. Krantz and Benoit B. Mandelbrot have an unusual history. In the fall of 1988, Krantz asked the Bulletin of the American Mathematical Society Book Reviews editor, Edgar Lee Stout, whether he could review the books *The Science of Fractal Images* (edited by Heinz-Otto Peitgen and Dietmar Saupe) and *The Beauty of Fractals* (by Heinz-Otto Peitgen and Peter Richter) for the Bulletin. Subject to editorial approval, Stout agreed. Krantz submitted the review in mid-November. The editor requested a few changes, they were made, and the piece was accepted. Krantz received the galley proofs in mid-January of 1989.*

Meanwhile, Krantz circulated copies of the review to a number of people, including Mandelbrot, who took strong exception to the review and wrote a rebuttal. Stout encouraged Krantz to withdraw his review from the Bulletin and to publish it in a forum that accepted rebuttals. Krantz refused to withdraw his review, but he suggested that the Bulletin publish Mandelbrot's rebuttal along with the review. However, the policy of the American Mathematical Society (AMS) prohibits responses in the Bulletin to reviews.

Stout then asked Krantz to make a number of revisions to soften the review. Krantz made the requested changes. After further thought, Stout decided that even the revised review (printed here) was not appropriate for the Bulletin, and he retracted his acceptance of the review.

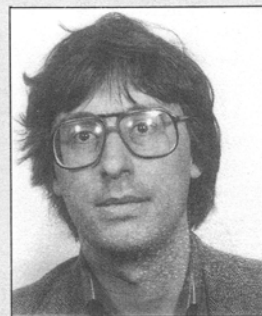
Krantz appealed the matter to the Council for the American Mathematical Society, which decided to support Stout's editorial prerogative. The AMS Council suggested that Krantz's review and Mandelbrot's response be published in the Notices of the AMS.

Krantz felt that the Bulletin should not reject a previously accepted review. Because Krantz was dissatisfied with his treatment by the AMS, he did not agree to have his review printed in the Notices of the AMS. The Mathematical Intelligencer, which welcomes controversy, is happy to publish Krantz's review and Mandelbrot's response.

A recent cocktail party conversation at my university was concerned with the question of whether academics are more eccentric or more depressive than the average functioning adult. At one point a clinical psychologist joined in and asserted that the matter had been studied in detail and the answer is "no." In fact, no profession seems to have more eccentric and depressive people than any other. The only exceptions, he went on to say, are mathematicians and oboe players.

Apparently the property that mathematicians and oboe players have in common is that both do something that is quite difficult and which few others ap-

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preciate. Be that as it may, we have all struggled with (or chosen to ignore) the problem of explaining to nonmathematicians what it is that we do all day. Other scientific professionals can throw up a smoke screen with phrases like "genetic engineering," "black holes," "plasma physics," and "string theory." Although listeners are no better informed after hearing these phrases than before hearing them, they are at least comforted by having heard them before. We mathematicians could not hope for a similar effect with phrases like "exotic cohomologies," "Einstein-Kähler metrics," "pseudoconvex domains," or "weakly strongly singular integrals." Nonmathematicians have no forum for encountering the terminology of mathematics.

That is the nature of the beast: most of the deep ideas in mathematics are couched in technical language. But a consequence of the rarification of our subject is that the public tends to be intensely unaware of us. The history of mathematics in the popular press has until recently been virtually void. John von Neumann graced the cover of *Time* because of his work on stored program computers. Hans Rademacher was written up in *Time* for "proving" the Riemann Hypothesis. But few if any nonspecialists got even a whiff of the Kodaira Vanishing Theorem or Weil's proof of the Riemann Hypothesis for function fields over finite fields. In his *Mathematician's Apology*, G. H. Hardy crowed that he never had done nor would he ever do anything "useful." By implication he also would never do anything that anyone but a mathematician would care about.

Times have changed and for several reasons. The American Mathematical Society (AMS) has an advocate in Washington. A public relations firm has been hired by the AMS to promote the cause of mathematics nationwide. One result: The U.S. Congress has decreed one week per year to be "Mathematics Awareness Week." In addition, there have been administrative and pecuniary pressures for pure mathematicians to interact with the applied world. Conveniently, the ready availability of high-speed digital computing equipment has served as a catalyst and a common language in this collaborative process. And the collaboration prospers.

It is also significant that several mathematicians, notably Ron Graham, have made a point of cultivating contacts with the press. Thus they can serve both as a sieve and a buffer between us and the world at large. On the whole, the effect of this effort has been positive. In particular, we owe to this the copious attention given to Freedman's solution of the four-dimensional Poincaré conjecture, Thurston's work on three-manifolds, the ill-starred solutions of Fermat's last theorem and the three-dimensional Poincaré conjecture, and Karmarkar's algorithm. Charles Fefferman was even written up in *People* magazine!

Heady stuff, that. But now there is a mathematical development that threatens to dwarf all others for its potential publicity value: the theory of fractals. While the sets called fractals have been studied for many years (in harmonic analysis, in geometric measure theory, and in the theory of singularities, for instance), the term "fractal" was coined and popularized by Benoit Mandelbrot (1975). By his own telling "the first steps of the development of a systematic fractal geometry, including its graphic aspects, were taken at the IBM T. J. Watson Research Center, or wherever I happened to be visiting from the IBM base." In *The Beauty of Fractal Images* Mandelbrot elaborates on this theme:

No more than six years ago! Only ten and twenty-odd years ago! On many days, I find it hard to believe that only six years have passed since I first saw and described the structure of the beautiful set which is celebrated in the present book, and to which I am honored and delighted that my name should be attached. No more than twenty-odd years have passed since I became convinced that my varied forays into unfashionable and lonely corners of the Unknown were not separate enterprises.

Hailed as a *lingua franca* for all of science, the theory of fractals is said by some to be the greatest idea since calculus.

The subject of calculus has played a special role in the history of modern science: Most of physics and engineering, and important parts of astronomy, chemistry and biology, would be impossible without it. Thus it is a compliment of the highest order to compare any new development with the calculus. Let us discuss that subject for a moment.

In the early days of calculus, it was practiced by a handful of fanatics. And so it had to be, for the theories of fluxions and fluents were virtually devoid of rigor and were full of internal contradictions. Bishop Berkeley's broadside *The Analyst: A Discourse Addressed to an Infidel Mathematician*, which ridiculed infinitesimals as "the ghosts of departed quantities," was a much needed breath of fresh air. It forced mathematicians to re-examine the foundations of analysis. There followed two hundred years of intense effort by the best minds in Europe. The result was the rigorous calculus we know today. What makes calculus important and what fueled in part Berkeley's frustration and fury is that calculus solves so many wonderful problems: The brachistochrone, Kepler's Laws, and many other deep properties of nature follow with calculus from a few elegant physical principles.

Like the fathers of calculus, the founders of fractal geometry constitute a cadre of dedicated fanatics. They should not be hampered by lack of rigor, for they share in the hard-won wisdom of the last 300 years. Yet there is not even a universally accepted definition of the term "fractal." It seems that if one does not prove theorems (as, evidently, fractal geometers do

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not), then one does not need definitions. One notable difference between fractal geometry and calculus is that fractal geometry has not solved any problems. It is not even clear that it has created any new ones. This is a rather strong contention and requires elaboration.

One definition of "fractal" is that it is a set whose Hausdorff dimension exceeds its topological dimension. Many examples are self-similar sets: Pick a neighborhood of a point in the fractal, dilate the neighborhood, restrict the dilated set to the original neighborhood, and *voilà!* the picture is unchanged. Fractals abound both in mathematics and in nature. The von Koch snowflake curve is a fractal, as is (the construction of) the Peano space-filling curve. Perhaps the most famous example of a fractal in nature is the coastline of England, which has the property that the closer you look, the more it wiggles. Thus the coastline is nonrectifiable and has infinite length.

Attached to many fractals is a numerical quantity called its fractal (similarity) dimension. If a fractal S can be divided into N congruent (in the sense of Euclidean geometry) subsets, each of which is an r -fold dilation of the original set, then the fractal dimension D of S is defined to be

$$D = \frac{\log N}{\log (1/r)}.$$

This formula is emblazoned in 24-point type on page 29 of *The Science of Fractal Images*. Even though it is but a pale shadow of the truly deep concept of Hausdorff dimension, fractal dimension is one of the big ideas in the subject of fractal geometry.

Thus many (but certainly not all) fractals have a fractal dimension, and naturally we want to compute this quantity. We learn that the perimeters of projections of certain clouds are fractals and that their fractal dimension is $\frac{4}{3}$. That turns out to be the same fractal dimension as that of a certain Cantor set. What have we learned? Better still, it has the same fractal dimension as the staircase in a certain engraving of M. C. Escher. Does this demonstrate some intrinsic structure in the universe? Are we, like Thomas Hobbes, on the verge of a calculus of ethics? Or are we, like Erik von Daniken in *Chariots of the Gods*, celebrating form over substance?

My mention of Escher is not a frivolous one. The books under review invoke the names of Escher and Ansel Adams as a means of lending both charm and credence to their subject. Other august names—John Milnor, Dennis Sullivan, and William Thurston (to enumerate but a few)—are mentioned as examples of

mathematicians whose work has apparently been inspired by fractal geometry. And this is a point worth noting explicitly: Some of the pictures of fractals have provoked the thoughts of Mandelbrot (who is good at dreaming up pretty questions) and of the aforementioned mathematicians. The latter have, as a result, proved some deep and interesting theorems in iteration theory. I don't think that Mandelbrot has proved any theorems as a result of his investigations, but that is not what he claims to do. By his own telling, he is a philosopher of science.

There is an important issue implicit in this discussion that I would now like to examine. A famous counterexample (due to Celso Costa) in the theory of minimal surfaces was inspired by the viewing of a Brazilian documentary about samba schools—it seems that one of the dancers wore a traditional hat of a bizarre character that was later reflected in the shape of the example. I once thought of an interesting counterexample by lying on my back and watching the flight of seagulls. Whatever the merits of samba dancers and seagulls may be, they are not scientists and they are not mathematicians. Why should fractal geometers be judged any differently?

Writings on fractal geometry find fractals in the work of many fine mathematicians, but that is as much insight as the theory of fractals lends to pre-existing theory. What we have is a language which is sufficiently diluted that it says something (of a descriptive nature) about almost anything that you can think of. I would be foolish to accuse fractal geometers of poaching from other fields. What fractal geometry has to say about other fields is not sufficient to make that a viable possibility.

An important ambiguity needs to be clarified at this stage. Some fine mathematics, such as the theory of sets of fractional dimension, Hausdorff measure, nonrectifiable sets, currents, etc., has been swept under the umbrella of fractal geometry (see [2], which has the misleading word "Fractal" in its title but which actually describes some beautiful, pre-fractal mathematics). When I criticize fractal geometry I am criticizing specifically the activities described in the two books under review—not the substantive areas of mathematics that have been caught up in the whirlwind of publicity surrounding Mandelbrot.

When one opens the books under review, it appears that fractal geometry is a science—evidently a mathematical one. However, nowhere in either book do I see a theorem, and there are few definitions. As noted above, there is no precise definition of the term "fractal." As a mathematician I find that this bodes no good. Look what happened to set theory when Russell's paradox was discovered.

The trouble with any subject that relies more on computer output than on theory is that one has to think of something to say about it. The result is that

much of the writing turns out to be anecdotal. Although the following passage from *The Science of Fractal Images* is not representative of the best that fractal theory has to offer, it serves to illustrate my point:

The overall outline is now reminiscent of a dog's head while just the upper portion could be the Loch Ness monster. Shapes with a fractal dimension D about 0.2 to 0.3 greater than the Euclidean dimension E seem particularly favored in nature. Coastlines typically have a fractal dimension around 1.2, landscapes around 2.2, and clouds around 3.3.

I once heard a talk by an eminent mathematician about automata theory. He confessed at the outset that he had a lot of questions and no answers. The rest of the talk consisted of looking at a variety of computer printouts and saying "this looks like a gopher's hole" and "this looks like a thundercloud." All quite boring and disappointing. It seems to me that if a subject is to be called a science, then one should be able to say more about it than this.

Of course, the books under review are not research journals, nor are they monographs. One hardly expects to see *Theorem-Proof-Theorem-Proof*. What one does expect to see is a development of ideas leading to some crescendo, the artful synthesis of concepts to give new insight, the formulation of precise mathematical discoveries accompanied by convincing arguments or proofs. I cannot find any evidence of these in the books under review.

No discussion of fractals would be complete without due homage to the pictures. They are wonderful and are apparently the *raison d'être* for all the uproar over fractals. Pictures of Julia sets and Mandelbrot sets are astonishing in their complexity and diversity. I do not accept the assertion (page 177 of *The Science of Fractal Images*) that the Mandelbrot set "is considered to be the most complex object mathematics has ever seen." This type of hyperbole may appeal to readers of pop-

ular magazines but rings untrue to the trained mathematician. However, my main point is somewhat different: I wish to establish a distinction between *fractal* computer graphics and some other computer graphics of recent note.

Dave Hoffman, Jim Hoffman, and Bill Meeks at the University of Massachusetts have received considerable attention for the graphics they have generated in connection with the study of minimal surfaces. But the work of Hoffman, Hoffman, and Meeks was moti-

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vated by a deep and important scientific question: Do there exist non self-intersecting minimal surfaces of high genus? The startling answer is "yes," and it was determined by generating models numerically, staring at the graphic realizations of the models, figuring out what is going on mathematically, and *proving a theorem that answered the original question*. In my view this type of work is a prime example of the most important new use of computers—not just for number crunching but for doing "what if" calculations that we could never do by hand.

In the preface to *The Science of Fractal Images*, Mandelbrot suggests that fractal geometers also use computer graphics to develop hypotheses and conjectures. But the difference is that the hypotheses and conjectures that the fractal people generate are (like the objects which they study) *self-referential*. One generates the pictures to learn more about the pictures, not to attain deeper understanding. That the pictures have occasionally inspired fine mathematicians to prove good theorems seems serendipitous at best.

It is this admittedly rather fine distinction that troubles my mathematical sensibilities. Good mathematicians do not always answer the questions they originally set out to study. Fritz John once said that when the answer to your question is "yes" then you've asked the wrong question. One *expects* good questions to open doors, and there is nothing more stimulating than following one's nose into new terrain.

The assertion that the relationship between fractal theory and mathematics is symbiotic is Mandelbrot's—not mine. But the true nature of the symbiosis is muddled by the terminology of fractal geometry: "Fractal" appears to be a new name for sets of fractional Hausdorff dimension; the "Weierstrass-Mandelbrot function" is a small variant of the Weierstrass nowhere differentiable function; the "Mandelbrot set"

It is true that Fourier had the opinion that the principal object of mathematics was public use and the explanation of natural phenomena; but a philosopher like him ought to know that the sole object of the science is the honor of the human spirit and that under this view a problem of [the theory of] numbers is worth as much as a problem on the system of the world.

Carl Gustav Jacobi

was *not* invented by Mandelbrot but occurs explicitly in the literature a couple of years before the term "Mandelbrot set" was coined (see [1, p. 68]). In fact, Fatou and Julia initiated the study of the iterates of the function $z \rightarrow z^2 + c$ but Mandelbrot, at least by association, receives much of the credit for it these days.

The enormous publicity of fractal theory has led to harmful government policies toward mathematics.

As previously noted, fractal geometry has a special aura in part because it has roots in the work of Pierre Fatou and Gaston Julia and because it apparently serves as an inspiration to mathematicians like Adrien Douady and John Hubbard. Let me state quite plainly that the work of these researchers and others on dynamical systems, iterative processes, and related topics is truly excellent—it is some of the best mathematics being done today. But these mathematicians don't study fractals—they prove beautiful theorems. To the extent that fractal geometry has received praise from the mathematical community, it has been indirectly through praise of the work of Douady, Hubbard, Thurston, and others. In fractal geometry one uses some mathematics to generate a picture, then asks questions about the picture—which generates more pictures. Then one asks more questions about the new pictures. And so on. One rarely, if ever, sees a return to the original mathematics.

What I find most bothersome, and this is no fault of the books under review, is that the public's perception of what mathematicians do these days is derived in large part from reading books about fractals, reading James Gleick's book *Chaos*, and reading about various incorrect proofs of long-standing conjectures. The latter item is just too bad, but the first two are terribly misleading. Both the theory of fractals and that of chaos are in their infancy. It is too soon to tell whether either will blossom into mature subjects.

A point of pride for fractal theorists is that their language is being picked up by laboratory scientists such as physicists. One third of the submissions to *Physics Review Letters* concern themselves with, or at least mention, fractals. It is easy to come away with the impression that fractal theory must have something important to say about the world around us. Leo Kadanoff (Nobel Laureate, and Professor of Physics, University of Chicago) addresses this point in [3]:

Unfortunately, although this single rather primitive measurement [fractal dimension] enables us to distinguish among objects, it never enables us to give a convincing case for their essential identity. Some progress has been made in identifying other qualities, beyond the fractal dimension, that might be universal. However, further progress in this field depends upon establishing a more sub-

stantial theoretical base in which geometrical form is deduced from the mechanisms that produce it. Lacking such a base, one cannot define very sharply *what types of questions* might have interesting answers. One might hope, and even suspect, that eventually a theoretical underpinning . . . will be developed to anchor this subject.

Without that underpinning much of the work on fractals seems somewhat superficial and even slightly pointless. It is easy, too easy, to perform computer simulations upon all kinds of models and to compare the results with each other and with real-world outcomes. But without organizing principles, the field tends to decay into a zoology of interesting questions and facile classifications. Despite the beauty and elegance of the phenomenological observations upon which the field is based, the physics of fractals is, in many ways, a subject waiting to be born.

Indirectly, the enormous publicity of fractal theory has led to harmful government policies toward mathematics. In some circles, it is easier to obtain funding to buy hardware to generate pictures of fractals than to obtain funding to study algebraic geometry. Since algebraic geometry has withstood the test of time and fractal geometry has not, one must wonder what considerations led to such funding decisions. My own theory is that bureaucracies can cope with hardware more easily than they can cope with ideas. In any event, it is depressing to predict the long-term effects of such policy.

The subject of fractal geometry is young. We should watch its development closely. Who knows? In 300 years it may prove to be as important as calculus. Meanwhile, the books under review provide a delightful invitation to the subject. The prose is clean and clear, the illustrations profuse and attractive, and the concepts are enjoyable. One of the principal emphases in these books is the description of algorithmic techniques for generating fractal graphics images on a computer system. In this respect the books are a great success. However, as to the assertion that they provide a glimpse of a new science or the language for developing a new analysis of nature, I would say that any contribution that fractal theory has made in this direction has been accidental. In short, the emperor has no clothes.

References

1. Robert Brooks and J. Peter Matelski, The dynamics of 2-generator subgroups of $PSL(2, \mathbb{C})$, *Riemann Surfaces and Related Topics: Proceedings of the 1978 Stony Brook Conference* (Irwin Kra and Bernard Maskit, eds.), *Annals of Math. Studies* 97, Princeton: Princeton University Press (1981).
2. K. J. Falconer, *The Geometry of Fractal Sets*, Cambridge: Cambridge University Press (1985).
3. Leo P. Kadanoff, Fractals: Where's the Physics?, *Physics Today* (February 1986), 6–7.

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