

# Hype! An Exchange of Views

## Ferdinand Verhulst, Steven Krantz & Neal Koblitz

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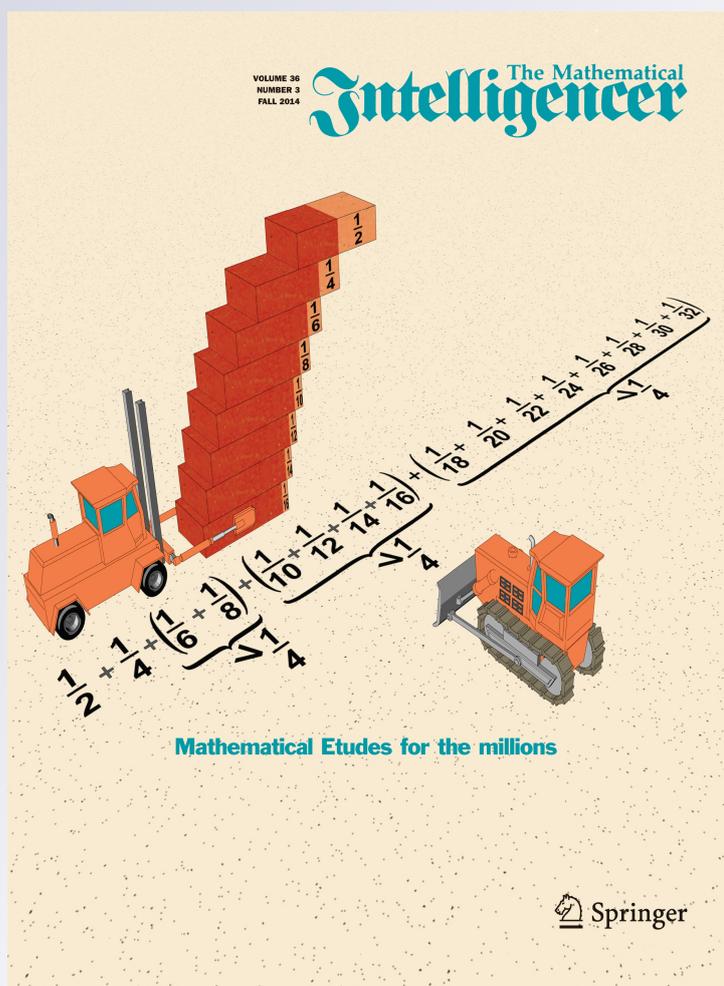
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# Hype! An Exchange of Views

**FERDINAND VERHULST, STEVEN KRANTZ,  
AND NEAL KOBLITZ**

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## Scientific Hype and Public Expectations

by Ferdinand Verhulst

**C**haos hype was a striking event. At that time—the end of the last century—a great deal of funding was obtained for research in dynamical systems and chaos; later the perspective changed. This raises questions regarding the expectations of the general public and their governments that have direct consequences for the funding of scientific research.

### Description of the Hype

The chaos hype of the 1980s included articles in newspapers, interviews with scientists, lectures, and popular and scientific books. The book *Chaos* by James Gleick [5] became very popular in the United States and in Europe; more than a million copies of the English edition were sold, and it was translated into many languages. Partly because of this book, the *butterfly effect* and *bringing order in chaos* became household expressions. The book itself is a readable account of the excitement of research and some of the ideas of chaos. As popular science, it is a clever product as it presents no technical details but is descriptive, using pictures and anecdotes without serious mathematical mistakes.

Did the chaos hype bring with it unpleasant connotations of “excessive publicity” and “exaggerated claims”? I will argue that the publicity was good for mathematics and that the claims were far from exaggerated. Of course, the general public did not see that the concept of chaos has a precise mathematical meaning, but clearly the ideas, pictures, and statements about predictability struck home. The science editor of one of the Dutch national newspapers approached me in June 2013 with the question “25 years ago chaos was all in the news, but it has completely vanished from the newspapers. Was it after all of little importance?” After an interview with me and several other colleagues, he wrote an enthusiastic article [6] about the present importance of chaos theory.

The scientific origin of chaos theory took place in mathematics, meteorology, and astronomy. Remarkably enough, the interest of physicists came a little bit later: classical deterministic thinking always had a strong position in physics. Also the idea that mathematicians and other scientists would be able to come up with something fundamentally new in dynamics was too exotic to be acceptable for many physicists. In the applied fields, the excitement started with computations by Edward Lorenz (1963) for heated air rising in the atmosphere of the Earth and computations by Hénon and Heiles (1964) for the dynamics of galaxies. At that time nobody saw a



**Figure 1.** Vladimir Arnold.



**Figure 2.** Jürgen Moser.

connection between the two applications and very few saw a relationship with recent mathematical results. The numerical calculations baffled the scientists until it was

discovered that Henri Poincaré had described such chaotic dynamics in great mathematical and picturesque detail [10] at the end of the 19th century. This realization was the beginning of a lot of fundamental work, a continuation of Poincaré's theory (finally!), and many applications. The work of the mathematicians Kolmogorov, Arnold, Moser, and Smale in the period between 1960 and 1970 turned out to be highly relevant for understanding so-called nonlinear phenomena; it received widespread attention among scientists. In 1976, Robert May obtained surprising results for interval maps. This was the start of another interesting topic in chaos research.

At the same time, fundamental research in chaos theory developed very quickly; it has now become a fixed part of books and courses on dynamical systems. The book by Peitgen, Jürgens and Saupe [9] is a beautiful introduction to this field, and there are journals mainly devoted to this subject. We can consider it a consolidated mathematical theory with many things still to explore.

The excitement about chaos is still alive among scientists. How is it possible that 30 years ago the worldwide hype clearly reached the general public, yet today the general public seems to have forgotten all about it? In the 1970s, Andy Warhol predicted that in the near future everybody could have 15 minutes of world fame. In our day it seems better to modify this to "at present nobody can be famous longer than 15 minutes and nothing can keep the attention of the general public longer than that."

### **A Revolution in Science?**

Thomas Kuhn's concept of "scientific revolution" ([7]), still useful in the history and foundations of science, is more attuned to the natural sciences than to mathematics. Such a revolution is characterized by a crisis in some field followed by a breakthrough in ideas; the knowledge-discontinuity ("epistemic rupture" [8]) produces a completely new view of the field with far-reaching consequences, including social ones. A well-known example is the realization that classical mechanics as developed by Newton, Euler, and others loses validity at high relative velocities. This led to relativity theory, with its fundamentally different view of time and motion. Social consequences in this case are nuclear energy, the atom bomb, and philosophical discussions; artists too have taken inspiration from relativity. For a discussion and additions, see [8]. Kuhn listed no mathematical discoveries as scientific revolutions: his book is restricted to examples from physics and chemistry, probably because of the lack of knowledge of mathematics on the part of most historians of science. But it seems straightforward to consider, for example, the axiomatization of geometry, the emergence of algebra, or functional analysis as scientific revolutions. Through the teaching of Euclidean geometry, the axiomatization of geometry has determined and influenced the development of thought in education for many centuries.

The phenomenon of chaos brings a conceptual revolution in science and in our thinking. We are faced here with unpredictability in deterministic dynamical systems for which we know all the laws that rule the system. The

motion of two coupled pendula or, to take a more complicated example, the solar system, are mathematically completely determined by known laws of mechanics but they contain unpredictable evolutions. Nobody before Poincaré had considered the possibility that such systems could produce chaos. Notably it was classical mechanics, which plays a part in time-keeping and other regular phenomena, that produced this big surprise.

The idea of chaos is a clear example of a scientific revolution by the completely new view it provides of certain mathematical and natural science processes, with applications from weather forecasting to economics.

### Public Interest and Public Funds

As noted previously, the chaos hype has been extinguished. Today interesting problems from other parts of science are catching the public's interest. For scientists who believe in the permanence and importance of their fields, this is a sobering observation. However, the emergence of such hype and the subsequent drop-off of public attention should be seen as an instructive phenomenon. In the past, say up to 25 years ago, we were allotted a reasonable amount of money for research. Now things are different. To obtain research grants, we have to deliver top quality as before, but we also have to surf the waves of public attention. Algebraic topology and bifurcation theory, for instance, have little or no publicity outside the sciences. A request for funds in algebraic topology may be successful if it is imbedded in research in high-energy physics and field theory; for bifurcation theory a request may be successful if we frame the proposal as climate research or engineering. Those who are not able to make such links will have no access to research money. Like it or not, fundamental research has to be packaged in a way that is attractive to the public.

Most mathematicians are very slow in adjusting to this picture. The reaction of many mathematicians in Germany in the 1980s to the modern theory of dynamical systems and chaos is typical: the generous publicity around dynamical systems produced cutting remarks and often unreasonable criticism. Envy and lack of imagination played a part, but so did the prevailing notion that a mathematician should produce theorems like a monk in his cell. In this outdated view, unreadable publications should be the mathematician's only contact with the world. This may be why there are relatively few research groups in dynamical systems in Germany.

The relation of science funding to the public interest can be illustrated by a brief description of the present situation in The Netherlands.

### Science Funding in The Netherlands

There are basically three sources of funding for scientific research in The Netherlands: universities, industry, and the National Science Foundation (NWO, Nederlandse Organisatie voor Wetenschappelijk Onderzoek). The university budgets are partly for teaching activities and partly for research. In recent decades, Dutch politicians and the government felt they had little grip on the research performed at the universities, so it was decided to take a few hundred

million Euros away from the universities and redistribute these monies through NWO. One consequence for the universities was that money for most Ph.D. positions would have to be obtained from national grants following approved proposals. NWO funds research in the humanities, the social sciences, mathematics, and the natural sciences, and, in addition, a number of independent research institutes. In 2012, the NWO budget was 650 million Euros.

Dutch politicians found these budgetary measures insufficient, as scientists were still relatively free to follow their own research interests. The Dutch government wants to encourage research in a number of specific top economic sectors. Following its directives, NWO has identified nine themes in which researchers and industry are brought together to ensure innovation. These themes are: 1. Agro, food, and horticulture; 2. Creative industry; 3. Sustainable energy; 4. High-tech systems and materials; 5. Healthy living; 6. Cultural and societal dynamics; 7. Connecting sustainable cities; 8. Water and climate; 9. Chemistry. Some of these themes make sense, such as 1, 3, 4, 8, or 9, but some of the others appear hyped.

It is difficult to recognize any fundamental physics or mathematics in these themes, but one should note that separate funding exists for talented researchers in a great variety of disciplines. It is not easy to establish the amount of money allocated to the nine themes and the amount for fundamental research, but a rough estimate is 40% to the themes and 60% to other topics. About 1% of the budget goes to fundamental mathematics through special talent grants and programs focusing on specific topics. Altogether, at the moment some 5000 researchers are funded in The Netherlands by NWO.

It is too early to judge how this reconstruction of research funding affects fundamental research, in particular mathematics. NWO had to perform a balancing act in allocating funds to top economic sectors and fundamental research. However, it is already clear that research by talented young mathematicians that is not organized in a theme, an independent institution, or a research program involving several institutes and researchers, has very few possibilities for funding. It will not be surprising if fundamental mathematical research in The Netherlands turns out to have seriously decreased in the two decades from 2000 to 2020.

There is another side effect of these political changes. Today the boards of the Dutch universities consist of so-called professional directors. The results are mixed, sometimes quite good, sometimes bad. For some of these people it does not matter what they do: running a potato-flour factory, a chain of restaurants, or a university, as long as the position is respectable and the salary high. Quite often these boards start imitating national policy. For instance, the University of Utrecht has identified as its major research themes: Sustainability, Institutions, Life Sciences, and Youth and Identity. After these themes have been established, they can be used as an instrument for hiring and firing people. In 2012, that university decided to dissolve the department of Astronomy and Astrophysics as it did not go well with the research themes. Excellent researchers in this field were fired.

Such developments are taking place in many Western countries. In general, the national science foundations seem to do a good balancing job (such as NWO) in funding both fundamental research and economic top sectors, but, more than before, we have to be aware of rapid political changes in society. The chaos hype was a gift to mathematics and adjoining fields; it was wise to exploit this. But now the hype is finished. At the moment individual proposals in dynamical systems and chaos will be handled on the same footing as, say, number theory or functional analysis. This means that funding is far from automatic; the proposals have to be imbedded in research programs with a certain public appeal or be connected to the economic top sectors. Mathematicians would be prudent to organize in themes and programs with cooperation between various institutions, both nationally and internationally. Fundamental research in all kinds of mathematical fields is essential for progress. Unfortunately, adjusting to some extent to the fashions and whims of modern society is a prerequisite for survival.

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## A Rejoinder by Steven Krantz

**T**hanks at least in part to James Gleick, chaos theory has enjoyed a vogue that is rarely seen among mathematical topics. Certainly the solution of Fermat's last problem, the proof of the Poincaré conjecture, and the proof of the positive mass conjecture have enjoyed nothing like the sheer *joie de vivre* that "Can the flap of a butterfly wing in Brazil cause a tsunami in Japan?" has generated.

The truth is that it is perfectly obvious that a butterfly flap in Brazil *cannot* cause a tsunami even 100 feet away. Even if it could, such a fact would not be very interesting. But it makes great cocktail-party conversation. Even the popular writer Tom Wolfe has expressed to me his enthusiasm for chaos theory.

Ninety years ago people were excited about relativity theory because it seemed to provide scientific substance to the idea that everything is relative. Eighty years ago people got pumped up about the Heisenberg uncertainty principle because it appeared to formalize the notion that things are uncertain. [Even someone as august as Carl Gustav Jung wrote a book about the role of the uncertainty principle in everyday life.] Today people are excited about chaos theory because it shows that our world is chaotic. What is next?

One must wonder what it is about chaos that has made it such a popular intellectual pastime. And the answer to that is obvious as well. There are many people out there—high-school math teachers, college math students, and parents of math students, to name just a few—who

love math at least in principle and would like to have some feeling for what is going on in the subject these days. But modern mathematics is quite recondite. It involves very technical language and subtle ideas and almost incomprehensible notation. It is difficult for the nonexpert to gain even a foothold in any part of the enterprise. One of Benoit Mandelbrot's many gifts was that he understood this point very clearly and thus sold fractal geometry to the public as the cutting edge of mathematical science. And who could argue with him? Who could speak ill of the lovely fractal pictures (which were invented by John Hubbard), and of the Mandelbrot set (which was invented by Brooks and Matelski), and of the vague rodomontade about fractal dimension (invented by Hausdorff), and of the length of the coastline of England (an idea cooked up by Lewis Fry Richardson)? Mandelbrot did not invent much of anything, but he surely knew how to market it. Poincaré, Lagrange, and Hadamard (among many others) were aware of chaos theory, but Edward Lorenz receives the credit for it.

We can be grateful for all the positive publicity that mathematics garnered riding piggyback on the frankly questionable enterprises of fractal theory and chaos. IBM played a significant role in this process, for it bankrolled Mandelbrot (including a Professorship at Yale and the Barnard Prize) and incorporated fractals into all its prime-time television advertising. We live in an age of marketing and flimflam. Why not get on board? The physicists have gotten a good deal of mileage from string theory and the prospect that they may now create a unified field theory. It may even be possible to explain gravity! How can we compete with that?

There will not be again soon a mathematician with the hubris and self-promotional skills of a Benoit Mandelbrot, one who could rise to the top of any heap and emerge the victor. Only history can judge what there is of lasting value in the fractal ideas. Similarly for chaos. It is an interesting idea that a small perturbation of the initial values in a system can have long-term, drastic, and unpredictable effects. This is an idea that anyone who knows how to integrate can figure out for himself or herself, but one that has gained great prominence and notoriety. In the study of partial-differential equations, this sort of instability can lead to profound new theorems. In the education of the public about mathematics, this can lead to happy faces and perhaps partially informed enthusiasts for our subject.

There is little doubt that the rise and proliferation of both fractal theory and chaos theory are intimately connected with the development of the personal computer. For ready access to computing power has made iterative experimentation a day at the beach; most anyone can generate pictures of fractals or pictures of chaotic behavior with just a few keystrokes—and perhaps even receive National Science Foundation funding for the effort!

I hold no brief for chaos and fractal theory. But I cannot see that they have done us any particular harm. Perhaps they have deflected some funding from more worthwhile to less worthwhile venues. Perhaps they have detracted from some truly profound mathematical breakthroughs. But we

mathematicians have a strong and profound sense of our values, and of what we hold dear. We know what we are about, and why we are about it. We should stick to our guns.

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## A Rejoinder

by Neal Koblitz

**H**ype (from “hyperbole”) means exaggeration. It means a sales pitch that overstates (or even fabricates) the merits of whatever it is one is advertising. That Verhulst likes to communicate his enthusiasm for his field to the general public is commendable. But he fails to acknowledge that his advice to mathematicians to hype their work, cater to popular fashion, and jump on bandwagons has a downside. Let me mention a few of the negative features.

1. When an area of applied math is overhyped, a lot of low-quality work receives support, and some unqualified (and on occasion even dishonest) people rush into the field. It's very hard to maintain quality control in a field that's become a big bandwagon. In contrast, high standards are maintained pretty consistently in most areas of theoretical mathematics.
2. One result of hype is a loss of credibility. When mathematical techniques fail to yield the promised benefits, people will start discounting everything mathematicians say.
3. Part of the process of hype is that scientists and mathematicians immediately go to the media so as not to be scooped in the press by a competitor. The result is that they are circumventing and weakening peer review, which is the main process for quality control in the scientific and scholarly world.
4. As William Thurston and others pointed out many years ago, the increasing reliance of U.S. mathematicians on funding from the military and the National Security Agency (NSA) has a corrupting effect on our values and compromises our independence. (For those reasons I have never applied for or accepted funding from the NSA.) A while ago I was asked to review an application for NSA funding in which the mathematicians argued that their proposed conference would increase the competitive advantage of American mathematics over “non-American” mathematics. Mathematics is perhaps the most international of all fields, but these funding applicants had started to think in nationalistic and jingoistic terms so that they could write their proposal in a way that they thought would appeal to the NSA.
5. There are some excellent theoretical mathematicians who have felt pressured into (mis)representing their work as somehow applicable to practical areas such as cryptography. I remember a Canadian NSERC grant application I was sent that was particularly sad. The P.I. was a top-notch

researcher, and should have been funded. But in my review of his proposal I had to point out that the claimed connection with cryptography was dubious.

6. Naive faith in mathematics to solve social problems leads to many abuses and misuses of mathematics. A couple of recent examples: (a) David Li's model of risk was used to justify multitrillion-dollar investment in exotic credit derivatives such as collateralized debt obligations, and this contributed to the financial meltdown of 2008 (see [3]); (b) the unfortunate consequences for American education of the fad-notion of “value-added modeling” (which claims to provide an objective quantitative measure of teaching quality) are explained in an excellent article by John Ewing in the *AMS Notices* [2]. Indeed, mathematicians can perform a valuable public service by clearly explaining the *limitations* of mathematics and statistics.
7. If a country fails to support basic science and mathematics, then before very long it will also fall behind in applied areas. Sensible political leaders and much of the general public understand that pure and applied mathematics are inextricably linked, at least in the long run. In Vietnam, for example, Fields Medalist Ngô Bảo Châu is a national hero, and it's not because his prizewinning work (proof of the Fundamental Lemma of Langlands theory) has any practical applications.

I suppose that in the 19th century there were some people who thought, “These mathematicians should be discouraged from working in abstract, useless areas such as group theory and complex analysis—they should all be working on spherical trigonometry.” And now there are people who might ask, “Why do Wiles and Châu waste their time on useless things such as the Taniyama Conjecture and the Fundamental Lemma, when the world needs more people working in financial math?” But mathematicians shouldn't cater to such ignorance.

In a recent *Opinion* column in the *AMS Notices* Jesús De Loera [1] describes troubling trends in public attitudes toward math and science in the United States and gives some thoughtful recommendations for mathematicians who wish to improve the situation (hyping and bandwagon-jumping are not among them). The only thing I would add to De Loera's comments is that we should not see mathematics as being in competition with other fields, but rather we should join forces with physicists and chemists who believe in the importance of basic science that has no immediate use in industry or warfare, with colleagues in the humanities and the arts who are working to combat anti-intellectual philistinism, and with organizations such as the American Association of Colleges and Universities that strongly support liberal education as a central mission of universities (and oppose bean-counting and excessive vocationalism, see [4]).

At the end of Verhulst's piece he has a perfunctory sentence acknowledging that “Fundamental research in all kinds of mathematical fields is essential for progress.” But the rest of the article—for example, his explanation of the skepticism of many German mathematicians toward the hype about chaos theory (“Envy and lack of imagination

played a part, but so did the prevailing notion that a mathematician should produce theorems like a monk in his cell. In this outdated view, unreadable publications should be the mathematician's only contact with the world"—suggests that he doesn't really believe this.

Like Verhulst, I also work in a much-hyped applied area (cryptography) that does not suffer from lack of funding. But this does not give me the right to mock or feel superior to my colleagues who write theoretical papers (yes, often unreadable by nonspecialists, as Verhulst complains) and work on basic questions that in the foreseeable future have no applications at all except in other parts of mathematics. Their work is “useless” in the same sense that great works of music and art are “useless.”

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