

Living a Paradigm Shift: Looking Back on Reactions to *A New Kind of Science*

May 11, 2012

*(This is the second of a series of posts related to next week's tenth anniversary of *A New Kind of Science*. The [previous post](#) covered developments since the book was published; the [next](#) covers its future.)*

“You’re destroying the heritage of mathematics back to ancient Greek times!” With great emotion, so said a distinguished mathematical physicist to me just after *A New Kind of Science* was published ten years ago. I explained that I didn’t write the book to destroy anything, and that actually I’d spent all those years working hard to add what I hoped was an important new chapter to human knowledge. And, by the way—as one might guess from the existence of *Mathematica*—I personally happen to be quite a fan of the tradition of mathematics.

He went on, though, explaining that surely the main points of the book must be wrong. And if they weren’t wrong, they must have been done before. The conversation went back and forth. I had known this person for years, and the depth of his emotion surprised me. After all, I was the one who had just spent a decade on the book. Why was he the one who was so worked up about it?

And then I realized: this is what a paradigm shift sounds like—up close and personal.

I had been a devoted student of the history of science for many years, so I thought I knew the patterns. But it was different having it all unfold right around me.

I had been building up the science in the book for the better part of 20 years. And I had been amazed—almost shocked—at many of the things I’d discovered. And I knew that communicating it all to the world wouldn’t be easy.

In the early years, I’d just done what scientists typically do, [publishing papers](#) in academic journals and giving talks at academic conferences. And that had gone very well. But after I built *Mathematica*, I started being able to discover things faster and faster. I had a great time. And pretty soon I had material for many tens—if not hundreds—of academic papers. And what’s more, the things I was discovering were

starting to fit together, and give me a whole new way of thinking.

What was I going to do with all this? I suppose I could have just kept it all to myself. After all, by that time I was the CEO of a successful company, and certainly didn't make my living by publishing research. But I thought what I was doing was important, and I really liked the idea of giving other people the opportunity to share the enjoyment of the things I was discovering. So I had to come up with some way to communicate it all. Publishing lots of piecemeal academic papers in the journals of dozens of fields wasn't going to work. And instead it seemed like the best path was just to figure out as much as I could, and then present it to the world all together in a coherent way. Which in practice meant I had to write a book.

Back in 1991 I thought it might take me a year, maybe two, to do this. But I kept on discovering more and more. And in the end it took nearly 11 years before I finally finished what I had planned for *A New Kind of Science*.

But whom was the book supposed to be for? Given all the effort I had put in, I figured that I should make it as widely accessible as possible. I'm sure I could have invented some elaborate technical formalism to describe things. But instead I set myself the goal of explaining what I'd discovered using just plain language and pictures. And as it turned out, countless times doing this helped me clarify my own thinking. But it also made it conceivable for immensely more people to be able to read and understand the book.

I knew full well that all this was very different from the usual pattern in science. Most of the time, front-line research gets described first in academic papers written for experts. And by the time it gets into books—and especially broadly accessible ones—it's not new, and it's usually been very watered down. But in my case, many years of front-line discoveries would first get described in a broadly accessible book.

Even in the [preface](#) I wrote for the book, I expressed concern about how specialist scientists would react to this. But my personal decision was that it was worth it. And when the book came out, it indeed for the most part worked out spectacularly. Most important as far as I was concerned was that a huge spectrum of people were able to read and understand the book. And in fact lots of people specifically thanked me for writing the book so it was accessible to them.

Many specialist scientists were also highly enthusiastic about the book. But much as I had expected, there was a certain component who just assumed that anything presented in a bestselling book couldn't really be important new science—and pretty much stopped there.

And then there were others for whom the book just seemed irrelevant; they were happy

with their ways of doing and thinking about science, and they weren't interested—at least at that time—in any particular injection of new ideas.

But what about people whose work was in some way or another directly connected to issues discussed in the book? I have to say that by and large I expected a very positive reaction from such people. After all, I had put all this meticulous work into studying things that they were interested in—and I believed I had come up with some exciting results. And what's more, I personally knew many of these people—and lots of them had benefited greatly from my efforts to develop the field of complex systems research some 15 or so years earlier.

So discussions like the one I described at the beginning of this post at first came as something of a shock. Of course, I had spent quite a few years as an academic, so I was well aware of the petty bickering and backstabbing endemic to that profession. But this was something different: this was people who were somehow deeply upset by what I was doing.

Some of the more sophisticated and forthright of them were pretty explicit with me, at least in person. Typically there was a surface reason for their reaction, and a deeper reason. Sometimes the surface reason related to content, sometimes to form. Those who discussed content fell into two main groups. The first—particularly populated by physicists—said that their immediate reason for being upset was basically that “If what you're doing is right, we've spent our whole careers barking up the wrong tree”. The second group—particularly populated by people who'd studied areas related to complexity—basically said: “If people buy into what you've done, it'll overshadow everything we've done”.

To me what was perhaps most striking was that these reactions were often coming from some of the best-established and most senior people in their fields. I suppose at some level it was flattering, but I had certainly not expected this kind of insecurity—not least because I thought it was completely unfounded. Yes, I believed new approaches were needed—that's why I'd spent so many years developing them. But I saw what I had done as complementing and adding to what had been done before, not replacing or overturning it.

And then there was the matter of form. “You've done something that's academic-like, but you haven't played by academic rules.” It was true: I wasn't an academic and I wasn't operating according to the constraints of academia; I was just trying to invent the best possible ways to do things, given my resources and the discoveries I was making.

A typical issue that came up was how the book was vetted or checked. In academia,

there's the idea that "peer review" is the ultimate method of checking anything. And perhaps in a world where everyone has infinite time, and nobody operates according to their own self-interest, this might be true. But in reality, peer review is fraught with error, often quite corrupt, and even in the best case strongly biased toward avoiding new ideas and maintaining the status quo. And for a piece of work as large, broad and complex as *A New Kind of Science*, even the basic mechanics of it seemed completely impractical.

So what did I do instead? It was a big exercise in perfectionism. First, we built a large system for automated testing, modeled on what we'd developed over the course of many years for *Mathematica*. And then we developed a process for getting experts in different areas to look at every page of the book, checking as far as possible every detail. So how did it work out? Impressively well. For even now, 10 years later, after every page of the book has been read and scrutinized by huge numbers of people, and every computational result has been reproduced many times, in all the 1280 pages of the book no errors much beyond simple typos have come to light.

One of the many challenges with the book was the practicality of actually printing and publishing it. Early on, I had hoped that one of the large publishers who wanted to publish the book would be able to handle it. But after awhile it became clear that their production methods and business models could not realistically handle the level of visual quality that the content of the book required. And so, somewhat reluctantly at first, I decided to have Wolfram Media do the publishing instead.

This certainly allowed the book to be printed at higher quality, and to be sold more cheaply. But the setup definitely seemed not to please some academics—particularly, I suspect, because it made clear that the book was simply beyond the reach of any academic network, however powerful that network might be in the academic world. Even if a couple of times I did hear things like "I'm so shocked about [some aspect of your book] that I'm going to campaign my university not to use *Mathematica*".

If one looks at a standard academic paper, one of its prominent elements is always a list of references—in principle a list of authorities for statements in the paper. But *A New Kind of Science* has [no such list of references](#). And to some academics this seemed absolutely shocking. What was I thinking? I always consider history important—both for giving credit and for letting one better understand the context of ideas. And when I wrote *A New Kind of Science*, I resolved that rather than just throwing in disembodied references, I would actually do the work of trying to unravel and explain the detailed histories of things.

And the result was that of the nearly 300,000 words of notes at the back of the book, a significant fraction are about history. I did countless hours of (often fascinating)

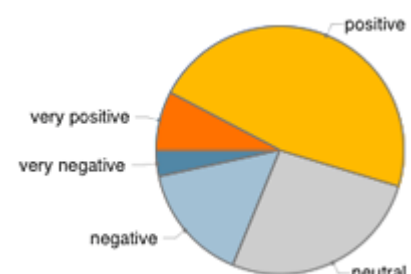
primary interviews and went through endless archives—and in the end was rather proud of the level of historical scholarship I managed to achieve. And when it came to traditional references I figured that rather than using yet more printed pages, I should just include in the notes appropriate names and keywords, from which anyone—even with the state of web search in 2002—could readily find whatever primary literature they wanted, at greater depth and more conveniently than from lists of journal page numbers.

When *A New Kind of Science* came out, I kept on hearing complaints that it didn't refer to this or that person or piece of work. And each time I would check. And to my frustration, in almost every case it was right there in the book—with a whole extended historical story. And it wasn't as if when people actually read it, they disagreed with the history I'd written. Indeed, to the contrary, many times people told me they were impressed at how accurate and balanced my account was—and often that they'd learned new things even about pieces of history in which they were personally involved.

So why were people complaining? I think it was somehow just disorienting for academics not to be able to glance down a definite “references section”—and see papers they'd authored or otherwise knew. But I'm pretty sure it was more an emotional than a functional issue. And as one indication, after the book came out we did the experiment of putting on the web—in standard academic reference format—the list of the 2600 or so [books](#) that I'd used in writing *A New Kind of Science*. And from our web statistics we know that vastly fewer people used this than for example the online version of even one chapter's worth of historical notes. (Even so, as a matter of completeness, I'm hoping one day to link all my archives of papers to the online book.)

I suppose another feature of the book that did not endear it to some academics was the very intensity of positive reaction that accompanied its release. Within days there were hundreds of articles in the media describing the ideas in the book—with journalists often doing an impressive job of understanding what the book had to say. And then, mostly slightly later, reviews started to appear. Some were detailed and well reasoned; others were quite rushed, and often seemed mainly to be emotional responses—probably more based on reading earlier reviews than on the reading of the actual 1280-page book itself. And after such a positive initial wave of media attention, later (often “catch up”) coverage inevitably tended to swing to the more negative.

When the book came out, I had all the reviews we could find diligently archived. And I always intended at some point to systematically read them. But somehow a decade has gone by, and I have not done so. And as I write this post, I have on my desk a daunting pile of printed copies



of reviews, as thick as the book itself. But back when they were archived, for reasons I don't now know, each review was at least put into a "star-rating" category, which we can now use to make a pie chart. And while I'm not sure just how much these statistics really mean, it is perhaps interesting that positive—or at least neutral—reviews overall significantly outweighed negative ones.

So what about the negative reviews? There are certainly some colorful quotes in them. "Why has this undoubtedly brilliant, worthily successful man written such a silly book?... I think it... likely that the book will be forgotten in a few months." "There's a tradition of scientists approaching senility to come up with grand, improbable theories. Wolfram is unusual in that he's doing this in his 40s." "Is this stuff really that important? Well... maybe. Frankly, I doubt it." "After looking at hundreds of Wolfram's pictures, I felt like the coal miner in one of the comic sketches in *Beyond the Fringe*, who finds the conversation down in the mines unsatisfying: 'It's always just "Hallo, 'ere's a lump of coal.''" "With extreme hubris, Wolfram has titled his new book on cellular automata 'A New Kind of Science'. But it's not new. And it's not science." "It was not the first time the names Wolfram and Newton have been mentioned in the same breath, and I suppose it might be taken as further evidence of an ego bursting all bounds." And, perhaps my favorite, a whole review simply titled "A Rare Blend of Monster Raving Egomania and Utter Batshit Insanity".

Realistically, much of the space in these reviews was not devoted to discussing the actual content of the book. High on the list of issues they discussed was that the book had not gone through an academic peer review process, and so could "not be considered an academic work" (not that it was meant to be). Then there were the complaints about the absence of explicit lists of references—often rather misleadingly with no mention of all the detailed historical notes, or perhaps a grudging comment that they were in too small a font.

Another common complaint was that the book was somehow just too grandiose. And for sure, any book with a title like "A New Kind of Science" runs the risk of being characterized that way. To be clear, I believed—and very much still believe—that what's in *A New Kind of Science* is very important. In presenting it, though, I suppose I could have somehow tried to hide this. But I was fairly sure that doing so would have a bad effect on people's ability to understand what was in the book.

The issue is quite familiar to those of us who have written lots of documentation for computer systems: if you have big ideas to communicate, you have to prime people for them—or they inevitably get confused. Because if people think something is a small idea, they'll try to understand it by straightforwardly extending what they already know. And when that doesn't work, they'll just be confused. On the other hand, if you

communicate up front that something is big and important, then people will make the effort to understand it on its own terms—and will much more readily be able to place and absorb it. And so—well aware of the potential for being accused of grandiosity—I made the [decision](#) that it was better for the science if I was explicit about what I thought was important, and how important I thought it was.

Looking through reviews, there are some other common themes. One is that *A New Kind of Science* is a book about cellular automata—or worse, about the idea (not in fact suggested in the book at all) that our whole universe is a giant cellular automaton. For sure, cellular automata are great, visually strong, examples for lots of phenomena I discuss. But after about page 50 (out of 1280), cellular automata no longer take center stage—and notably are not the type of system I discuss in the book as possible models for fundamental physics.

Another theme in some reviews is that the ideas in the book “do not lead to testable predictions”. Of course, just as with an area like pure mathematics, the abstract study of the computational universe that forms the core of the book is not something which in and of itself would be expected to have testable predictions. Rather, it is when the methods derived from this are applied to systems in nature and elsewhere that predictions can be made. And indeed there are quite a few of these in the book (for example about [repeatability of apparent randomness](#))—and many more have emerged and successfully been tested in [work](#) that’s been done since the book appeared.

Interestingly enough, the book actually also makes abstract predictions—particularly based on the [Principle of Computational Equivalence](#). And one very important such prediction—that a particular simple Turing machine would be computation universal—was [verified in 2007](#).

There are reviews by people in specific fields—notably mathematics and physics—that in effect complain that the book does not follow the methodology of their field, which is of course why the book is titled “A New Kind of Science”. There are reviews by various academics with varied “it’s been done before” claims. And there are a few reviews with specific technical complaints, about definitions or about phenomena like the emergence of quantum effects from essentially deterministic systems. Sometimes the issues brought up are interesting. But so far as I know not a single review brought up any specific relevant factual issue that wasn’t in some way already addressed in the book.

And reading through negative reviews, the single most striking thing to me is how shrill and emotional most of them are. Clearly there’s more going on than just what’s being said. And this is where the paradigm shift phenomenon comes in. Most people get used to doing science (or other things) in some particular way. And there’s a natural

tendency to want to just go on doing things the same way. And there's no issue with that for people whose subject matter is sufficiently far away—and who can successfully say, “I just don't care about your new kind of science”. But for people whose subject matter is closer, that doesn't work. And that's when the knives really come out.

I have to say that to me (as I discussed in the [previous post](#)) the progress of NKS seems quite inexorable, and unavoidable—and indeed a decade after the publication of the book, seems to be progressing well. But I think some of the reviewers of *A New Kind of Science* convinced themselves that if what they wrote was negative enough they could derail things, and maybe allow their old directions and paradigms to continue unperturbed. And perhaps the mathematical physicist mentioned at the beginning of this post expressed their attitude most clearly when he said in our conversation: “You're one of the most brilliant people I know... but you should keep out of science”.

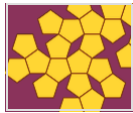
There's lots of analysis that could be done of the dynamics of opinions about *A New Kind of Science*. In 2002, there were fewer venues than today for public comments to be made. But I suspect that enough existed that it would be possible to piece together much of what happened. And I think it makes a fascinating study in the history of science.

I suppose I am myself by nature a positive person. And no doubt that is a necessary trait if one is going to do the kinds of large projects to which I have devoted most of my life. I am also at this point someone who is much more interested in just doing things than in other people's assessments of what I do. No doubt others would find attacks on as important a personal project as *A New Kind of Science* dispiriting. But I have to say that first and foremost my reaction was one of scientific interest. Having studied so much history about paradigm shifts, I found it fascinating to be right in the middle of one myself.

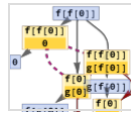
I certainly wondered what one could predict from the dynamics of what was going on. And here history has some interesting lessons. For it suggests that perhaps the single best predictor of good long-term outcomes from potential paradigm shifts is how emotional people get about them at the beginning. So for NKS, all that early turbulence in the end just helps fuel my optimism for its long-term importance and success. And a decade out, not least with everything my [previous post](#) discussed, things indeed seem to be well on their way.

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