

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.