

A Q&A with Ian Hacking on Thomas Kuhn's Legacy as "The Paradigm Shift" Turns 50

Scientific American's review of Thomas Kuhn's *The Structure of Scientific Revolutions* in 1964 ended with the pat pronouncement that the book was "much ado about very little." The short piece, which appeared two years after the initial publication of *Structure* as a monograph in the *International Encyclopedia of Unified Science*, discarded as unoriginal Kuhn's critique of the positivist argument that science progresses relentlessly forward toward the truth.

The reviewer's glib dismissal missed the mark. Unquestionably, Kuhn's work has absorbed countless barbs over the decades. But it also counts as one of the most influential 20th-century works of philosophy and history of science. *Structure* has sold 1.4 million copies and the overused "paradigm shift" has attained the status of effete cliché in countless, numbing business conference PowerPoints. (See John Horgan's "[Reluctant Revolutionary](#)," an interview with Kuhn in the May 1991 *Scientific American*.)

Kuhn advanced the idea that scientists in a particular field share an existing set of practices—a paradigm—that allows them to labor away using common methods on like research problems—what he labeled "normal science." Eventually, experimental anomalies accrue that foment revolutionary change—the ballyhooed paradigm shift that overturns the existing order and ushers in a new era that may differ radically from the old.

That "incommensurability," as Kuhn termed it, held that after a true revolution, an existing body of theory bears no relationship to the new theoretical framework, the opposite of "standing on the shoulders of giants." In other words, the concept of mass in the new world of $E = mc^2$ differs fundamentally from the same designation in the old classical mechanics of $F = ma$. Kuhn's use of the word incommensurability has been distorted to mean that he advocated discarding the objective consideration of a new theory, exchanging rationality for "scientific relativism" in which the loudest voice for a pet idea prevails, what one critic characterized as "mob psychology."

Kuhn has procured a prize spot in the pantheon, but how have his ideas endured since the year that the Beatles released their first record. The prominent Canadian science philosopher Ian Hacking has written an introduction to the 50th-anniversary edition of *The Structure of Scientific Revolutions* that addresses the somewhat dated nature of Kuhn's work, even while casting an admiring eye on the original accomplishment.

[An edited excerpt of an interview with Hacking follows.]

Do you think that Kuhn's ideas about the importance of revolutions propelling scientific change still hold as much sway today as they did 50 years ago?

Kuhn was the intellectual of whom many scientists said: he's "telling it as it is" insofar as talking about a process of "tinkering" in terms of theory and experiment followed by radical changes. But often what Kuhn had in mind were some very spectacular incidents in the history of the sciences that changed our way of looking at the world. I'm not at all sure that that is going to continue as the way in which the sciences develop since now the life sciences in general have replaced physics as queen of sciences. I don't think that Kuhn's model for scientific change is necessarily going to prevail. But it still remains the case that Kuhn gave us a spectacularly new and vivid version of what he thought was going on. That will stand there forever, and that will be a permanent part of the repertoire of historians and philosophers and people in science studies in general.

What were some of those momentous events that had such an influence on his work?

At the beginning of the 20th century we had a radical overthrow in the way in which the world was conceived. Kant thought that we lived in a world of absolute space and time, that universal causality prevailed, that this was not just an interesting fact about the world but a precondition for thinking about it. People implicitly believed that.

Whereas what happened at the beginning of the 20th century was the overthrow of absolute space and time, and more important the overthrow of universal causality. These are really profound revolutions. They are not just changes in theory. They are changes in how the world is thought of as working—and I believe that those radical changes colored the way in which people thought about the sciences.

Both Kuhn and Popper [Karl Popper, the philosopher] were deeply influenced by these changes and then later moved on to the idea that they were extreme examples of something that would be happening quite commonly in the sciences. There was a tendency for them to generalize from these epoch-making changes in a way that is less appropriate today.

How are Kuhn's ideas perhaps less pertinent?

It's not so clear that there will be any more revolutions in physics. There will be a lot of surprises, but whether there will be revolutions is not at all clear. The stability of what's called the Standard Model of particle physics and its ability to make so many clever predictions with immense precision suggests that we may just be stuck with it, and there may never be an overthrow of that.

So the search for the Higgs boson is very much within the existing order of things?

It's obviously normal science in the Kuhnian sense. One of the things Kuhn said about

normal science is that people "expect" things to be discovered. Today scientists expect to find the Higgs. Once we see some exact numbers around it, there will be all sorts of new things to do. But those things will simply be a stabilization or confirmation of what people already expect. It's just possible, though, that all the structure around the Higgs is all wrong, and that would refute my claim that there would no more revolutions.

How has the rise of biology changed the dynamics of doing science since Kuhn's time?

So much work in the contemporary life sciences is much less theory driven and much more technique driven than was the case for physics, which was Kuhn's science 50 years ago. Kuhn himself really thought that all of the action was in the theory. It's not to say that we don't have a million theories about, say, molecular biology but what has really mattered there are new techniques for intervening in the course of life.

Do you think different analytical methods are needed to describe what's going on today in biology?

There are already lots of different analytical blueprints available. And there will be more as people attend more and more closely to the recent history of molecular biology or genetic engineering. I think it's unfortunate when people say that there is just one true story of science. For one thing, there are many different sciences, and historians will tell different stories corresponding to different things.

Can you give an example of an alternative blueprint?

I'll give you one not more recent than Kuhn. The Hungarian philosopher of science, Imre Lakatos, a follower of Popper, has this very powerful notion of a research program. He was strongly opposed to Kuhn because he thought Kuhn had made everything much too psychological. He had wonderful phrases like "according to Kuhn, science is just mob psychology." Okay, one doesn't have to take all of Lakatos's rants seriously. But I think that his picture of what he called a research program to which he gave a substantial structure is one of quite a few valuable ways of approaching development of a science.

Can you explain what a research program is and how it might pertain to the life sciences?

Lakatos's notion of a research program is that all theories have lots of and lots of little anomalies. They wallow in a "sea of anomalies"—he was very good at making up these little aphorisms. However, a research program has a hard core of beliefs which are never challenged, and then there's a whole bunch of auxiliary beliefs which can be modified. What distinguishes what he called a progressive research program from a

degenerative research program—two highly loaded expressions with a lot of Hegel and Marx behind them—is the way in which they deal with anomalies.

Lakatos thought the progressive ones explain the anomalies and enable one to broaden the scope of research whereas a degenerating research program develops a kind of protective belt in order to exclude anomalies in an ad hoc way. I think one can say that the whole history of molecular biology since 1962, when the first ream of Nobel Prizes came out, has been in Lakatos's terms an extraordinarily progressive research program.

So, for example, epigenetics, changes in gene activation without altering DNA, might be an example of an anomaly that modifies the overall picture while leaving the hard-core "research program" intact. It makes the overall picture more complex...

...more complex and, in the end, more interesting.

Molecular biology has routinely taken problematic things under its wing without altering core ideas. Indeed, the very expression often used of the "central dogma" of recombinant DNA is a good example of a hard core of a research program.

Fifty years on, there have been distortions, misappropriations and distractions related to Kuhn's work. Can you speak to that?

I don't know why the notion of a paradigm took off, why a totally obscure word became common usage within a few years—and not only in English: If you do a Google n-gram on "paradigm" in German or French or Italian, you find the same leap into common usage, which wasn't there before.

Some people have been upset about Kuhn's idea that science, like Darwinian natural selection, has no overarching goal.

Many people find it very disturbing. I don't. Of course many people have always found it very disturbing that Darwinian evolution seems to have no goal. Remember that Kuhn wasn't against progress; he just thought that progress wasn't "to" something. It was progress away from what didn't work very well, but that there isn't any kind of permanent goal.

What about the association of his ideas with scientific relativism? He spent part of his career trying to refute that.

Well, he wasn't a relativist. There's a long and complicated story of the rise of a desire for scientific relativism. Part of it may well be simply sort of rage against reason, the fear of the sciences and a kind of total dislike of the arrogance of a great many scientists who say we're finding out the truth about everything—and here [with Kuhn]

there was a way to undermine that arrogance.