PARADIGM CHANGE IN SCIENCE AND ECONOMICS

In this post, I discussed normal science, a term used by Thomas Kuhn in *The Structure of Scientific Revolutions* to describe the day to day work of scientists, focusing on the example of my brother's work on transmission of pain in the body. In normal science, Kuhn explains, people expect the puzzles they choose to work on will have solutions that can be worked out using the paradigm, and if the first try doesn't get the solution, scientists just keep plugging away, sharpening their instruments, their theories, their rules of engagement and trying to eliminate prejudices until they get a solution. And mostly, they do. That's a good description of my brother's work.

If not, generally they assume they failed, not that the answer doesn't have a solution inside the paradigm's limits. They put that problem to the side, and work on a related problem or maybe just move on to something different. Frequently the problem disappears as more and better techniques are created, measurements become better, theories evolve and prejudices are conquered. But if unsolved puzzles accumulate, there is growing pressure on the paradigm, and growing unease among the scientists working in the area. Kuhn gives examples:

> The state of Ptolemaic astronomy was a scandal before Copernicus' announcement. Galileo's contributions to the study of motion depended closely upon difficulties discovered in Aristotle's theory by scholastic critics. Newton's new theory of light and color originated in the discovery that none of the existing pre-paradigm theories would account for the length of the spectrum, and the wave theory that replaced Newton's was announced in the midst of

> growing concern about anomalies in the relation of diffraction and polarization effects to Newton's theory. P. 67, fn omitted.

This is the crisis state. It is a necessary, but not sufficient, condition for a change in the paradigm. Kuhn analogizes the situation to political revolutions:

> Political revolutions are inaugurated by a growing sense, often restricted to a segment of the political community, that existing institutions have ceased adequately to meet the problems posed by an environment that they have in part created. In much the same way, scientific revolutions are inaugurated by a growing sense, again often restricted to a narrow subdivision of the scientific community, that an existing paradigm has ceased to function adequately in the exploration of an aspect of nature to which that paradigm itself had previously led the way. P. 92

Another necessary condition for a paradigm shift is the existence of a new paradigm. Scientists cannot work without a paradigm, so until a new one obtains a concensus, they struggle on under the old one. New paradigms are suggested and tested, but Kuhn points out that there isn't any way to prove that one is better than the other, because proofs only exist inside paradigms. The new paradigm has to satisfy the relevant scientific community that it will solve the old problems, and open the way to new problems. But this is a matter of persuasion, not of scientific proof, because the standards of proof are connected to a paradigm; they do not exist in some Platonic state above it all.

One final point. Kuhn says that in scientific revolutions, the new paradigm completely replaces the old one, and he gives plenty of examples.

There’s more to be said about the process of paradigm change, but this will suffice for this post. In the wake of Kuhn’s work, several papers were published trying to identify paradigm shifts on the order of the Copernican Revolution in the history of economics. One such is *The “Structure of Revolutions” in Economic Thought*, a 1971 article by Martin Bronfenbrenner. He thinks the history of economics is more like the Hegelian dialectic, thesis, antithesis and synthesis, than the catastrophic destruction of the previous paradigm.

Bronfenbrenner identifies three revolutions in economics as

1. The classical school, based on Adam Smith’s Wealth of Nations and David Hume’s Political Discourses.
2. The marginal utility revolution, dating to about 1870, led by John Stuart Mill and David Ricardo.
3. The Keynesian revolution, about 1936.

He adds the response of the Chicago school as a possible fourth, and time has proved his suggestion correct.

It should be obvious that none of these revolutions destroyed the older view. Instead, they sit side-by-side, if uneasily and with some overlap. Bronfenbrenner doesn’t see a problem with the survival of the natural law as a partial explanation of 20st Century capitalism, and assumes that the future will include some of those ideas as well. This is clear from his approval of Paul Samuelson’s textbook. I point out the problems with that view in several posts here and at Naked Capitalism, including this one.

Like others, Bronfenbrenner points out that Kuhn’s definition of the term “paradigm” is loose at best. For purposes of this post, it’s sufficient to regard it as the entire set of theories, understandings, prejudices, instruments, and interpretations of the measurements of instruments that guide the

scientist in the course of normal science. It is, however, important to note that neither Bronfenbrenner nor any of the other writers I've seen so far try to explain the sense in which the Classical School, the Marginal Utility School, the Keynesians or the Chicago School, or, for that matter, any of the other schools, constitute a paradigm in a way similar to the way General Relativity acts as a paradigm for physicists and astronomers.

That offers two more or less neutral explanations of why economists aren't all freaked out by the failure of their theories demonstrated by the Great Crash. First, they may well assume that events like the Great Crash are just anomalies that future work will solve. That would explain the response of Gary Becker, "You need a theory to beat a theory." Link here. Becker couldn't imagine an alternative theory, so he just continued to work inside his old one, as if his Chicago School were a paradigm.

Second, Bronfenbrenner is right that old economic theories never die. They cannot die. Instead, in his view, they will be assumed into the heaven of some synthesis, hopefully with the favorite views of each economist on top.

As a road map for the rest of this series, what does all this say about the claims of authority of economists?