

PARADIGMS IN ECONOMICS

I am fascinated by the fact that economists do not seem fazed by the failure of their almost unanimous policy recommendations of deregulation and tax cuts, as I discuss here and here. Almost in unison, they chanted for decades that reducing taxes and regulation would spur growth for the benefit of all of us. The Great Crash didn't faze them, as these posts show. So why not?

One plausible explanation is that these people are acting in bad faith in the sense Sartre uses this term. They are free to change their minds about their theories, but they are not willing act on, or even to face, that freedom because it might cost them something. This explanation seems to be behind several of Paul Krugman's recent columns and blog posts, asking how people can have a claim to expertise when they give the same advice no matter the circumstances, and when the evidence and even the structure of their explanations contradict their advice. I think there are plenty of intellectually dishonest economists, but surely there are plenty of intellectually honest economists too.

After my previous posts a correspondent suggested I take a look at Thomas Kuhn's *The Structure of Scientific Revolutions*. In the wake of Kuhn's book, a number of scholars attempted to apply the theory to economics. I think it's helpful to look at the failures of economics through this lens.

Kuhn starts by describing what he calls normal science: the day to day practice of scientists. Their work is based on an infrastructure consisting of theories of various strengths, instruments, and techniques that together make up a paradigm. This paradigm organizes their thinking so that they have an idea of what they are doing when they do physical and thought experiments. Kuhn says that normal science uses

the paradigm to solve puzzles. The puzzles themselves are set up by the paradigm, and the scientist expects to be able to solve them using the rules and equipment of the paradigm.

Here's an example. One of my brothers was a scientist with a deep interest in the transmission of pain through the nervous system to the brain, and in analgesics, pain-killers. In the 80s, he began to wonder about the pain-killing effect of marijuana. Here's a reasonably comprehensible paper he co-wrote in 2001, discussing the state of work on cannabinoids.

In the paper he talks about single-cell studies. We talked about this a couple of times while he was doing this work. He told me that his lab had worked out a technique for inserting a tiny filament into a brain cell of an anesthetized rat and counting how many times and how often it fired, and some other things about it. He explained how he thought that happened, and what it meant physically. He described the instruments he used in general terms, and some of the interesting ways he was using computer chips to monitor the results. I asked why. I thought it might be useful, he said.

For him, neurotransmission of pain was a huge puzzle. He wormed away at it most of his adult life. Each little step he took seemed likely to advance a detailed understanding of the puzzle, or create an instrument that might help him and his colleagues take another step. A giant puzzle. A game. The same things were going on in other labs, as the footnotes show. One of the researchers he cites wondered if the body generates substances like cannabinoids. That guy found an endocannabinoid, a naturally occurring cannabinoid, which he named anandamide, from the Sanskrit word for internal bliss. Not only a puzzle, but an opportunity for cool puns.

Kuhn's examples are older, and from physics and chemistry, but they exhibit the same pattern. In both cases, normal science depends on a collegial understanding of the instruments, the things being measured and a shared general

understanding of the way the thing being studied works.

Kuhn offers three foci of normal science: learning about the facts that the paradigm suggests are most revealing about the nature of things; facts that can be used to check the paradigm; and empirical work to articulate the paradigm in the greatest possible detail, clearing up ambiguities and reaching for further problems suggested by the paradigm.

How does economics fit into this picture? What is the paradigm? What are the problems economists are trying to solve? What is “normal economics”?

Here’s one explanation from David Andolfatto of the St. Louis Fed:

But seriously, the delivery of precise time-dated forecasts of events is a mug’s game. If this is your goal, then you probably can’t beat theory-free statistical forecasting techniques. But this is not what economics is about. The goal, instead, is to develop theories that can be used to organize our thinking about various aspects of the way an economy functions. Most of these theories are “partial” in nature, designed to address a specific set of phenomena (there is no “grand unifying theory” so many theories coexist). These theories can also be used to make conditional forecasts: IF a set of circumstances hold, THEN a number of events are likely to follow. The models based on these theories can be used as laboratories to test and measure the effect, and desirability, of alternative hypothetical policy interventions (something not possible with purely statistical forecasting models).

In previous posts I note that recommendations arising from models that do not and cannot

predict crashes is worse than useless, it's downright dangerous. Another kind of problem is that there are big disagreements about the models: whether the assumptions are correct, what they actually model, how they do it, why and whether they work and under what circumstances. Further, there are a number of schools of economics each with its own models and its own set of assumptions, overt and covert. In fact, it isn't quite clear what the economics paradigm is, or are. These and other issues are for another day.