

## **A Nobel for Freshwater Economics**

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This week's [announcement of the Nobel Prize in Economics](#) got me thinking about the state of the subject, and my thoughts weren't very positive. Three years after the great financial crisis of 2008 discredited the ruling orthodoxy in macroeconomics and finance, the Royal Swedish Academy of Sciences has chosen to honor one of the leading creators of that orthodoxy: Tom Sargent, of New York University. And judging from the reactions to the Nobel announcement, most academic economists heartily approved of it.

A couple of years ago, it seemed like there was at least some willingness inside the economic profession to take outside critiques seriously. The successful launch of George Soros's [Institute of New Economic Thinking](#), which attracted participants from a wide range of backgrounds, appeared to reflect a new spirit of openness. More recently, there has been a closing of the ranks, and I fear that the Nobel award will only strengthen that movement. What next? A Nobel for Gene Fama, the exponent of the efficient-market hypothesis? If Sargent deserves a Nobel, surely Fama deserves one, too.

If the economics Nobel is about rewarding influential economists, Sargent's award is a no-brainer. But aren't Nobels supposed to have a higher purpose? Aren't they supposed to be about rewarding people who put forward ideas and theories that turned out to be useful and practical? Looked at from this perspective, the Nobel committee should have picked out one of the economists who stood up against the ruling orthodoxy in macro, not one of its highest high priests.

Sargent shared the prize with Christopher Sims, of Princeton. The award to Sims I have no quibble with. Starting in 1980, he developed a new statistical approach to analyzing the economy, which today is widely used in central banks, universities, and economic-research firms. (Over at Marginal Revolution, Tyler Cowen has [an informative post](#) with some good links.) Known as Vector Auto Regression, or VAR, Sims's methodology enables researchers to make predictions and analyze policy changes without committing to a particular economic theory, but, rather, by seeking to let the data speak for itself. Whether VAR truly represents a big improvement over other statistical methods, such as the construction of large-scale Keynesian models or the calibration of smaller real-business-cycle models, is still up for debate. Some smart

people, such as the authors of [this paper](#), think it is economics without the economics. So far, though, VAR appears to have passed the market test.

Sargent's award is another matter. In one sense, it is long overdue. The economics Nobel is an inside job: the Economic Sciences Prize Committee, which advises the Royal Swedish Academy of Sciences, is largely made up of academic economists. Year after year, they give the award to one (or more) of their own (mostly Americans) whose work twenty or thirty years ago generated a lot of subsequent research, as Sargent's has. Back in the mid-seventies, he helped create the "rational expectations" approach to macro, which, speaking roughly, says that people form expectations about the future on the basis of an accurate model of the economy that they have in their head. This idea isn't very plausible, but for reasons partly having to do with the problems that Keynesian policymaking was encountering at the time, and partly having to do with the difficulty of incorporating expectations into economic models in any other manner, it proved immensely influential.

Today, no self-respecting economist would dispute the fact that forward-looking expectations matter a lot and that the actions of policymakers help to shape them. Even economists who refer to themselves as Keynesians are very careful in how they treat expectations.

That is largely Sargent's legacy. It has always been a mystery to me why he and his collaborator Neil Wallace, who now teaches at the University of Pennsylvania, didn't share the 1995 Nobel, which went to another pioneer of rational expectations, Robert Lucas, of the University of Chicago. The papers that Sargent and Wallace wrote in the mid-seventies, and the [graduate textbooks](#) Sargent published, proved just as influential as Lucas's work. It was Sargent and Wallace who purported to demonstrate that under the assumption of rational expectations changes in monetary policy on the part of the Fed that are anticipated by the public will have no impact at all. If the Fed printed more money to boost output and employment, workers and firms would revise wages upward in the expectation that inflation would rise; real (inflation-adjusted) wages would remain unchanged, and so would output and employment. This argument, which is known as the "policy-ineffectiveness proposition," helped to discredit Keynesian fine-tuning in some quarters.

The Lucas, Sargent, and Wallace approach to economics wasn't just about criticizing government interventionism. It came with its own

methodology, which involved trying to build everything up from micro foundations with some snazzy new mathematics. (New to economists, that is.) This abstract approach, which is often referred to as “freshwater economics,” eventually came to dominate the teaching of macroeconomics, first in the United States and then in other countries, too. The students and acolytes of Lucas and Sargent took over many of the top professorships and journals. Eventually, it got to be difficult for young scholars to publish articles that challenged or ignored the Lucas-Sargent methodology.

Unfortunately, in case it needs restating, freshwater economics turned out to be based on two ideas that aren’t true. The first (Fama) is that financial markets are efficient. The second (Lucas/Sargent/Wallace) is that the economy as a whole is a stable and self-correcting mechanism. The rational-expectations theorists didn’t refute Keynesianism: they assumed away the reason for its existence. Their models were based not just on rational expectations but on the additional assertion that markets clear more or less instantaneously. But were that true, there wouldn’t be any such thing as involuntary unemployment, or any need for counter-cyclical monetary policy.

Perhaps because they wanted to head off this sort of criticism, the Nobel committee said that it was honoring Sargent not for his original work on rational expectations and economic policy but for his contributions to “econometrics”—the application of statistics to economics. But if this is a distinction it is a very fine one. Sargent’s econometric research explored the implications of the rational-expectations hypothesis for testing theories. In the mind of every economist I know, he is still closely associated with rational expectations and its later refinements.

To be sure, he also did some other interesting work. In the eighties and nineties, he delved into the more realistic environment of [bounded rationality](#), in which people don’t know the true model of the economy but seek to learn about it in a systematic way. In such a world, as you might expect, it is much more difficult to reach sharp conclusions, or offer black and white policy advice, than under the assumption of rational expectations.

But Nobels aren’t awarded for interesting research: they are awarded for research that changes how we think about the world. Freshwater economics did that. Eventually, though, it came a cropper. And it is not as if Sargent has acknowledged its failings. In a [2010 interview](#) with the Federal Reserve Bank of Minneapolis, he staunchly defended it, saying it was designed to explain how the economy works in normal times, not in

times of crisis. But this won't do, surely. The lesson of the 2008 financial crisis and subsequent recession is that in a modern financially driven economy, it is what happens in "normal times" that gives rise to instability.

This was the central point that the late Hyman Minsky [put forward repeatedly](#), and which economists of the freshwater persuasion resolutely ignored. Unfortunately, Minsky isn't around to collect the Nobel Prize, and neither is Charles Kindleberger, who helped to popularize Minsky's theories. Even if Minsky and Kindleberger were still alive, I doubt the Nobel committee would have invited them to Stockholm. If they had wanted to honor somebody who has done interesting work in the tradition of Keynes-Minsky-Kindleberger, which actually had something useful to say about the financial crisis and subsequent recession, they could have picked out Axel Leijonhufvud, of U.C.L.A., Paul Davidson, of the University of Tennessee, or Jean Pascal Benassy, of the Paris School of Economics. But to make such a bold move would have been to publicly acknowledge what Willem Buiter, a former London School of Economics professor who is now the chief economist at Citigroup, [wrote on his blog](#) in March 2009: "The typical graduate macroeconomics and monetary economics training received at Anglo-American universities during the past 30 years or so, may have set back by decades serious investigations of aggregate economic behavior and economic policy-relevant understanding. It was a privately and socially costly waste of time and resources."

It will be interesting to see what Sargent has to say in Stockholm. Hopefully, now that he has finally got his Nobel, he will be more willing to take seriously the criticisms of freshwater economics. But I wouldn't bet on it.