Interview with Christopher Sims

The Princeton economist on inflation targets, vector autoregression and the importance of monetary policy.

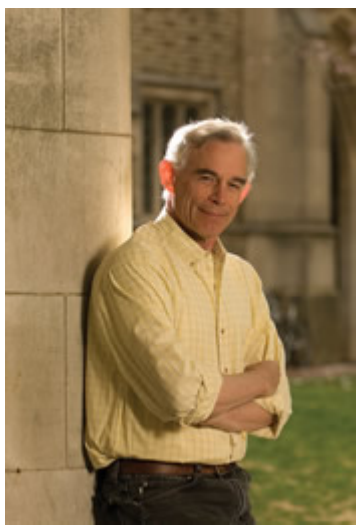
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AUTHOR



Arthur J. Rolnick

Senior Vice President and Director of Research, 1985-2010



Open Christopher Sims' Web site and you'll find a brilliantly colored painting, titled “Regimes, switching.” Hold your cursor over the painting, and a suggestion appears: “Graph colored and turned sideways to look like a Navajo rug?”

Intentionally or not, the “rug” illustrates the Princeton University economist's unique approach to economics. He takes conventional wisdom, spins it, examines it through a different prism and extracts a new picture—sometimes quite striking—of reality.

That approach has led to fundamental breakthroughs in econometrics. Sims pioneered vector autoregression (VAR) methods that generate economic forecasts of unequaled accuracy and then used "structural" VARs to predict the effect of policy interventions. He developed ingenious techniques to better establish causality—a tricky proposition in a world of spurious correlations—and has been responsible for numerous other theoretical and methodological advances.

As might be expected of a scholar who refracts conventional wisdom, Sims is often controversial. Unlike his colleagues (including many at the Minneapolis Fed) whose gold standard is a "dynamic stochastic general equilibrium" (DSGE) model based solidly on economic theory, Sims contends that economists shouldn't rely too much on theory since most theoretical assumptions strain credibility. "Use assumptions drawn from 'theory,' which actually means intuition in most cases,as lightly as possible," Sims has argued.

And while most of us couldn't distinguish a VAR from a DSGE if our economic future depended on it, the debates between their adherents are as heated as any in modern economics.

But if Sims is controversial, he's also widely respected and highly influential. Recently chosen as chair of the National Academy of Sciences' economics section, Sims is a former president of the Econometric Society and a visiting scholar at the International Monetary Fund and several Federal Reserve Banks.

In the following interview with Arthur Rolnick, director of Research at the Minneapolis Fed, Sims discusses a number of topics, ranging from his much disputed "fiscal theory of the price level" to the possibility of peace between VARs and DSGEs.

Importance of Monetary Policy

Rolnick: Let me start with a question about the importance of monetary policy. In a 1996 paper you and your co-authors wrote that "only a modest portion" of variation in output and prices in the United States since 1960 is attributable to shifts in monetary policy. And in a recent interview you said there's not much "evidence that monetary policy is as important as most people seem to think it is ... in generating business cycles."

Are you suggesting that monetary policy has relatively little impact on economic performance and stability, or is it that the Federal Reserve has gotten monetary policy largely right over the past 40 years, and as a consequence, business cycle fluctuations have been largely due to nonmonetary phenomena?

Sims: The key word in what I said before was that monetary policy is not as important as many people have thought in *generating* business cycle fluctuations. Back in the '70s, the monetarist position was that a large fraction of business cycle fluctuation was unnecessary because it was generated by erratic monetary policy. My view is that as we've gotten better at parsing out the sources of fluctuations, we've realized that even in the '70s that wasn't true, and it's even less true now because most of the monetary policy actions that we see taken are systematic responses to the state of the economy, so there's very little in the way of erratic, random fluctuation in policy that produces business cycles.

Rolnick: OK, but it does seem to beg the question, what if we *did* have an erratic fluctuation, a real shock to monetary policy? Now, maybe we just haven't had them, but...

Sims: Exactly. My view is that monetary policy, if it were erratic and unpredictable or perverse, could have very significant bad effects on the economy. And actually I think it *has* had that impact in some historical periods in some countries.

Rolnick: So it's not so clear, then, that you and, say, Milton Friedman, would disagree on that.

Sims: I think recently that Friedman was not pushing this notion that a large fraction of business cycle variation was due to erratic monetary policy. I think back in the '70s, he did take the position that if we shifted to a money growth rule it would reduce business cycle fluctuations dramatically.

Rolnick: So there may have been some evolution in that thinking.

Sims: Certainly I think what we've realized by mostly careful empirical work is that there really wasn't much of that. Money supply itself is close to a random walk. It's hard to predict its changes. And it is now, and it was before. But that's what you expect if the monetary authority is systematically smoothing interest rates and money demand is shifting around in response to various private sector disturbances. That's my view as to what explains most of the shifts in monetary aggregates.

Fiscal Theory of the Price Level

Rolnick: You've been a proponent of the "fiscal theory of the price level" (FTPL). It's controversial. Could you lay out for us what the theory really means, and how it fits in—or maybe it doesn't—with the monetarist view that the price level, or at least inflation, is determined primarily by money supply growth?

Sims: What was really new about the FTPL compared to standard theory is that it insists that the government budget constraint, in economies where large amounts of nominal interest-bearing debt are issued, has to be treated as one of the central equations determining equilibrium and determining the price level. Once you realize that, you realize that there is a kind of a simple connection between outstanding nominal interest-bearing liabilities, taxes and the price level that's comparable to the simple connection between outstanding money balances, demand for money and the price level.

These relationships are both true. That is, it is true that in equilibrium, the total nominal value of the debt, the total real value of interest-bearing debt, has to match expectations of future tax backing for that debt. And that's true in *any* model.

And then at the same time, it's true, of course, that in equilibrium the real balances held by the public have to match the demand for those real balances. It can be helpful, depending on the circumstances and as a crude first approximation, to think of the price level as being determined by the ratio of the nominal quantity of money to the real demand for money. Or it can be useful to think of the price level as being the ratio of the nominal value of interest-bearing debt outstanding to the tax backing for that debt. It's hard for people who get used to thinking about it in one simple way or another to realize that in equilibrium *both* these ways of thinking about it are correct.

Rolnick: Do they lead you to different policy prescriptions?

Sims: They can, because, for one thing, once you recognize that the fiscal balance has to be treated symmetrically and is equally as important as the money supply demand relationship, there are several examples of ways this can be important. For one thing, there's a class of equilibria for the economy that are invisible—that you don't realize exist—if you focus entirely on money demand. These are equilibria in which the monetary authority is completely passive, simply picks a nominal interest rate or in some other way agrees to accommodate any amount of debt issue by monetizing it.

In conventional models that kind of monetary policy leads to an indeterminate price level. But in a model in which the fiscal authority is committed to a fixed level of primary surpluses, there is a unique price level. That kind of situation, where the fiscal authority's primary surpluses can be treated as a given, is actually quite realistic in many economies, particularly some of the intermediate-income economies like Latin American economies. When an economy's fiscal effort level is at a politically feasible maximum, so you really cannot increase taxes, treating the primary surplus as a given makes sense. And when that's true, policy prescriptions that act as if by controlling the money stock you can end inflationary pressures are just mistaken.

In Brazil, for example, monetary authorities are very aware of the fact that interest rate changes have large direct impacts on the fiscal balance because so much of their fiscal expenditures are interest expenditures. Once that happens, the monetary authority is thinking about fiscal issues in setting monetary policy all the time. And it's easy to have a conceptual understanding of why that's important and where it comes from if you've thought about a model in which the fiscal balance is given as much importance as the money balance.

Rolnick: Did the Tom Sargent and Neil Wallace 1981 article "Some Unpleasant Monetarist Arithmetic" precede much of this?

Sims: That paper got one side of the fiscal theory of the price level. It recognized that—and this has been a theme of much of Sargent's work—if you have a fiscal imbalance that the monetary authority can't do anything about, then that can completely disrupt the ability of the monetary authority to control the price level.

What that paper didn't get was the other side, which is that a commitment to a positive fiscal primary surplus can produce a unique equilibrium price level even in the face of a passive monetary policy. That side of the fiscal theory wasn't in "Unpleasant Monetarist Arithmetic."

That's important for a different kind of policy issue. As means of payment get diversified with technological advancement, electronic payments and so on, paper money becomes less important, and if you're thinking entirely in terms of monetary quantities as the way the price level gets stabilized, it may seem that this will create unstable money demand, and it might get very hard to set the price level. But if you're thinking of it in a fiscal theory framework, in an economy with lots of outstanding nominal debt, there's no great problem in keeping price levels stable, even if paper money (noninterest-bearing liabilities) becomes an unimportant part of government debt—which of course hasn't really happened yet.

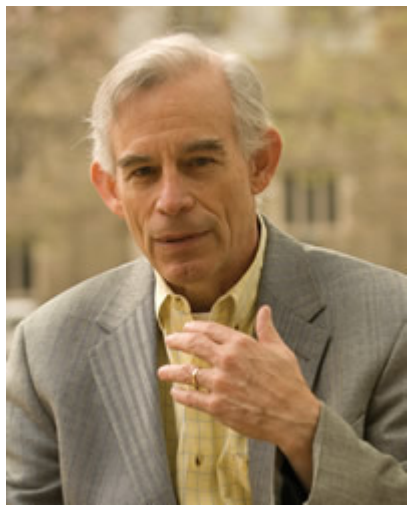
Inflation Targeting

Rolnick: Let's move on to a different, related topic.

As you know, we have a move toward inflation targeting now. Many central banks have adopted inflation targets. The Fed is talking about it. The new chairman is very interested in at least looking at that framework. There's a lot of discussion currently, not just outside the Fed System, but within it.

You've argued that there are serious limits to the usefulness of inflation targeting. Could you elaborate on both the limits and the virtues of inflation targeting regimes?

Sims: The limits I've argued about don't really apply very much to the United States. Those limits came out of a fiscal theory point of view. And I'm not the only one who has made these points. Rick Mishkin [now a Fed governor] has a descriptive article about inflation targeting in economies with less advanced fiscal and monetary systems that makes some of the same points.



An inflation targeting regime only makes sense when the monetary authority really has control over the price level. And if there's a bad fiscal environment, the monetary authority may not have such control. In that case announcing inflation targets may make things worse because if you're announcing inflation targets you can't hit, that simply undermines credibility. That's the kind of limitation I've emphasized.

The big advantage for an economy like the United States is what inflation targeting does to transparency.

In the United States, where there's some resistance to the target itself, duplicating the transparency parts of an inflation targeting regime without being so explicit about the target might be a reasonable thing to do.

When you set an inflation target at a central bank, you're forced to get into a cycle of projections of what's going to be happening to inflation, and usually also output, on a regular, periodic basis. Explaining when your forecasts haven't been met and why, and making clear the extent to which your forecasts are uncertain—that process of regular reports on where the economy is going and what actions the central bank is taking to keep inflation in line, I think, is very valuable.

You could, of course, do that without actually having a fixed inflation target, and it looks to me like maybe that's the direction in which the Fed might be moving.

Rolnick: It might. I mean, that's what's being debated, is how far does the Fed go in terms of publicizing its forecasts, and how far does the Fed go in terms of publicizing an explicit target, if it has one? I think that's just open for debate now, trying to measure the costs and benefits, if you will.

But there are some who argue that the advantage of an explicit target is commitment and that it's all about time consistency. And leaving it implicit opens the door for what I guess I would say is too much flexibility.

Sims: I agree with that to some extent. I think that you get quite a bit of the benefit of commitment by regularly having to say where you expect inflation to be in two years. So that it becomes news if you change your mind and stop predicting 1 1/2 percent and start predicting 4 percent, then people are going to ask why, so I think you get some of the benefit of commitment. I think once you are regularly announcing where you expect inflation to be in two years, there would be a lot of pressure on the Fed not to make arbitrary changes in where that prediction goes.

Rolnick: If the Fed does publish its inflation forecast, wouldn't this tie the Fed's hands?

Sims: And as you say, to some extent, tying hands is what it's all about. I agree that an inflation target is a useful commitment device. If you're going to try to commit to a single nominal anchor, it's probably the best, because it's actually something that everybody cares about. Committing to an exchange rate or a money growth target [is problematic]. If you miss the target and start imposing costs on people to get back to it, but things like the inflation rate haven't indicated there's any need for a policy intervention, I think you get into serious problems of credibility. Whereas if you miss the target on inflation and you say, "We missed the target because of this or that shock, and we've got to get back and so we're taking action," people understand that.

Rolnick: I think that's part of the appeal, too.

Dollarization and Pegging

Rolnick: China has pegged its currency to the dollar, and it's been criticized for doing that. Some of us would argue that pegging its currency is a good policy, not just for China but for those who trade with China. Having a pegged exchange rate means the Chinese are committed to the United States' low inflation regime. So we shouldn't criticize China for pegging or other countries for dollarizing—it makes world trade just that much more efficient.

In the past you've questioned that policy, I think because it implies a country is giving up a policy tool.

Sims: It's not just a tool. You know, Canada dollarizing might be a lot more plausible than China. You're pushing adjustment onto the domestic price level. And China's range of products, its labor costs, what's going on in that economy is very different from what's going on in our economy, so you'd expect that if the inflation rates stay the same in the two economies, the exchange rate might have to adjust quite a bit. And if you insist on pegging the exchange rate, you're forcing all that adjustment onto the domestic price level, and that could be costly.

The other thing is there's a big difference between pegged exchange rates and dollarization. The arguments I've made are mainly about economies that start to make all their government debt dollar debt, and that gives up the tool of being able to adjust to fiscal shocks with changes in the price level. If you have nominal debt, then unanticipated inflation and deflation can bear part of the burden of adjusting to shocks to your primary surplus. Historically, most countries have actually made substantial use of that possibility when there are big shocks like wars or natural disasters, big surprise changes in the price of your single export—things like that. I think that that's a cost that countries that consider dollarizing in the sense of going to 100 percent dollar-denominated debt need to consider carefully as a possible loss. China's pegging the exchange rate certainly doesn't do that.

Rolnick: So your concern is different from the often-heard contention that China is manipulating its currency to encourage more exports.

Sims: Yes. Of course, once you factor out nominal price levels, there's the question about what the right equilibrium is. It seems likely that there would be problems of trade imbalances and the effects that has, whether the Chinese peg their exchange rate or not, I think. But, of course, it might change to some extent, at least in the short run, according to whether you let the exchange rate or the domestic price level absorb the shocks.

The EMU and ECB

Rolnick: In 1999, you suggested that the fiscal foundations of Europe's Economic and Monetary Union were "precarious" and that a fiscal crisis in one country "would likely breed contagion effects in other countries." More recently, in 2003, you wrote that if the European Central Bank doesn't resist a constant temptation to let its balance sheet and net worth grow through seigniorage, "this could create serious long run problems for both the bank and for the European political system." Are you satisfied with the EMU's progress to date in shoring up its fiscal foundations?

Sims: I don't really think there's been any real progress. The founding documents of the ECB did not recognize that the central bank can need fiscal backing, and it hasn't needed it so far. A central bank, especially one like the ECB, whose assets are primarily foreign exchange assets, always has some risk of a sudden shift in exchange rates that leaves it in a negative net worth position. A negative net worth position for a central bank is important only because it creates limits on how aggressive it can be in controlling the domestic price level. This scenario of negative net worth for the ECB is far from being on the immediate horizon. Right now I think it's in a high positive net worth position.

Rolnick: I'm a little confused here. I think of the ECB as issuing fiat money, so liability is in name only. It doesn't offer you anything in return for this fiat money, so why does a negative net worth matter?

Sims: It matters only if, because of inflationary pressures, you want to do open market operations to retire some of that fiat money. You're not committed to taking it if somebody comes to the door. There can't be a run on the fiat money, but if you want to guarantee that that fiat money trades for other stuff at some rate, then you have to start doing something that's like offering a promise to redeem it. And if you don't have any assets to back it, then you can be in trouble.

Rolnick: In other words, you're worried that a central bank may not have enough assets to meet its inflation commitment.

Sims: Right, and if the public sees they don't have enough. If they have a policy that says, "We're going to aggressively push up interest rates when inflation increases by open market operations," the public may see that, well, if this policy is pursued, they're going to run out, and then that can make the inflationary pressures worse. So that's the disaster model, which isn't really on the horizon now.

The other question is, do they have in place mechanisms to guarantee that they don't accumulate arbitrarily large amounts of net worth? They do hand out a formal measure of seigniorage. But they don't have, I think, any formal mechanism to recognize that if they make permanent capital gains on foreign exchange shifts, that those capital gains also need eventually to get handed back to governments. You don't want this institution which has no democratic checks on it to get so big that politicians start to ask, "Why are there no controls on it?"

Limited Ability to Process Information

Rolnick: My next question has to do with what you call "rational inattention." You've suggested that such theories provide a useful alternative to other explanations, like sticky prices, of deviation from models of continuously optimizing agents in continuously clearing markets. Could you tell us what you mean by "rational inattention" and what advantages it holds as an explanation of how markets

work? How does it differ from Bob Lucas' idea of signal-extraction problems?

Sims: The formal theory I've worked on is theory in which we use an idea imported from engineering communications theory. Engineers have a notion of information transmission capacity. We're actually all familiar with it now as the bits per second or kilobytes per second that our connections to the Internet give us as a measure of speed. What's valuable about it is that it's a measure of the capacity to transmit information that doesn't really depend on the physics. You can have a fiber optic connection or a telephone wire connection. The physical details of the connection get abstracted from in this measure.

So my idea was, well, let's suppose that when we model an individual looking at market prices and taking actions, that he's got a physical limit on his ability to process information so that the flow from the market price stochastic variations to the individual's actions has to be modeled as limited by an information flow capacity limit, in that engineering sense. And it turns out that that idea produces sluggishness, randomness and delay in behavior that looks a lot like what we actually see in the way people behave. I think it captures a lot of people's intuitions about the sources of inertial behavior in the economy.

In terms of explaining what we observe, it has the advantage that it explains why in fact it's not just that prices are sluggish. Everything moves sluggishly in response to things like monetary policy. The simple Keynesian theory that says that prices are sluggish is usually taken to imply that real stuff should move really fast in response to disturbances. But we don't see that.

Models that can explain both real sluggishness and price sluggishness in the conventional way start to bring in costs of adjustments of many kinds in many places in the economy on the basis of no real microeconomic evidence. A model that's full of physical costs of adjustment or people's strong distaste for rapid changes—that's another element that goes into these models—has very strong welfare implications that say that fluctuations are very undesirable because they're so costly. Whereas if people are sluggish just because most of the time they're not paying that much attention to market signals, it may not be that they're so averse to rapid changes. It's just that they don't usually change very rapidly because they're not paying attention, so the welfare implications are very different.

Rolnick: [Stanford economist] Pete Klenow's work suggests that lots of prices adjust much more quickly than we thought. What implications would that have for rational inattention theory? It's not clear to me what data you're matching up with the theory.

Sims: The data that I think of as matching this are the aggregate data where monetary policy disturbances show slow smooth responses of both prices and real stuff. And if you take the rational inattention perspective and look at the micro evidence, in recent research, I and some students here have been finding that rational inattention implies discreteness in people's behavior.

One of the ways people are sluggish is that they don't respond to information by smooth changes. They jump. When you recognize that, you recognize that the frequency of changes wouldn't correspond for rationally inattentive agents of this kind to the frequency with which they calculate what the new optimal price is. And most of the models in use assume that when the price actually changes, a complete set of calculations of the new optimum is done. I think that in fact a lot of the price changes we observe are done mechanically and the real calculations of whether a margin should change or whether a position vis-à-vis a competitor should be changed, those get done much less frequently.

Rolnick: Is there a crisp test of rational inattention theory that you've come upon?

Sims: I think it's very hard to get a crisp test of it because, for one thing, we only have it worked out formally in a few special cases at this point. At this point it's giving you some insight rather than giving you a sharply testable prediction. In one case that's worked out, it gives you a prediction that is exactly the same as the Lucas signal extraction; it's observationally equivalent. The only difference is that rational inattention theory places some restrictions on the nature of that signal extraction problem and says that the nature of the signal extraction noise will shift according to what monetary policy is, for example. So it makes some predictions, but if you just looked at one economy with stable policy and looked at its time series, you couldn't distinguish whether it was just signal extraction or rational inattention.

Vector Autoregression (VAR)

Rolnick: What are your thoughts about how VARs have contributed to the development of economic theory over the past 25 years, and I'd add to that economic policy as well? What has been their largest impact? And here we could reference your 1986 *Quarterly Review* article, "Are Forecasting Models Usable for Policy Analysis?"

Sims: That paper was an early example of using structural VARs. In other words, taking VARs that had been used successfully then and still are being used successfully as forecasting models and asking, "Instead of going the route of dynamic stochastic general equilibrium models, where a whole raft of strong identifying restrictions that nobody believes are exactly true are imposed, to arrive at estimates of policy effects, can we instead try to introduce identifying assumptions one at a time, in a sense, and in minimal amounts to allow interpretation of results?" That's what the structural VAR literature does.

Actually, the first paper in which I did something that was really a structural VAR was an *American Economic Review* paper called "Comparison of Interwar and Postwar Business Cycles" [May 1980]. In that one, I observed that monetary aggregate shocks did not look like monetary policy shocks in their effects. Whereas interest rate shocks did look kind of like monetary policy shocks in their effects. And it showed that those shocks had smooth, slow effects on prices and that they produced predictable movements in money stock.

I think that the biggest contribution of structural VARs is that they're the foundation of the way people think about monetary policy now as being based on interest rate adjustments and with the mapping from interest rate adjustments to effects on prices being slow and smooth, delayed for a year or so before they have their peak effects, and with a somewhat quicker but less long-lasting effect on real output.

Those things have emerged in studies of structural VARs with many different approaches to making weak identifying assumptions. And they also turn out to match up with fitted dynamic stochastic general equilibrium (DSGE) models. But I think the fact that you can verify these conclusions so many different ways using structural VARs has actually conditioned the way people think about monetary policy these days.

Rolnick: Should future research focus on structural VAR or DSGE models?

Sims: My current research project is to build on the work of Marco Del Negro [at the Federal Reserve Bank of Atlanta] and Frank Schorfheide [of the University of Pennsylvania], who have the idea of closely coupling a DSGE with a structural VAR, treating the DSGE not as a model of the data but as a generator of prior distributions for the structural VAR. I think this is a very promising approach. Structural VARs as they have been used have used almost entirely restrictions on the nature of monetary policy and its connection to the economy. But economists have rough ideas about at least long-run effects of lots of kinds of disturbances other than monetary policy disturbances.

If you go all the way over to trying to make DSGEs fit the data in detail, you're forced to make assumptions about fine time unit dynamics that we really don't have a good idea about from theory. And the Del Negro-Schorfheide approach allows the possibility that you could rely on the DSGE for what you believe about longer-run behavior but free the structural VAR to pick up the dynamics in a much less restricted way.

What they've done doesn't quite do that but it's pretty close, and I'm trying to actually do that with my current research project.

Rolnick: Is that having any impact on other central banks, besides the Federal Reserve System?

Sims: Yes, there are people trying to look at formally fitted DSGEs and to some extent connecting them to structural VARs at the Swedish Riksbank, and there's a research project under way to do this at the New York Fed. There are people capable of doing it who are talking about it at the Atlanta Fed. The Norwegian central bank is interested in it; I don't know to what extent they're actually doing this formally. And in fact, the first big estimated stochastic general equilibrium model that seemed to fit the data pretty well was done by people at the ECB.

Models and Policy

Rolnick: My last question refers to a 2002 paper you did for Brookings Institution reviewing policymaking by central banks. You seemed pretty pessimistic then about models and policy. You wrote: "The academic branch of the economics profession has not been contributing much to the progress of policy modeling. ... Academic interest in these issues has been low for years, and I am not sure how this can be changed."

But your comments just now indicate that maybe it is changing. Is that the case? Are you more optimistic now?

Sims: It's changing, but the same dynamic is there, unfortunately. It's changing in that there are some academics who are very interested in this. There is more communication between academics working on econometrics and theory that could be applied to policy models than there has been. But within the macroeconomics community, there are people who have no interest in this and who regard it with a certain amount of contempt. It's very complicated and technically demanding, and it requires understanding technical stuff that a lot of macroeconomists have not been trained to grasp. So there are macroeconomists who dismiss it, and I think in part because they can't really understand it and are happy to believe that it's not important that they understand it. This dynamic is part of what went on before, and it's still part of the sociology and politics of the profession. I don't really know how to predict how it's going to work out.

Rolnick: Well, it sounds like there are people at some central banks who are starting to look at this seriously.

Sims: On the central bank side, there's lots of interest. On the academic side, there are quite a few pockets of interest, so I think it certainly is more optimistic than it was 15 years ago.

Rolnick: Thanks very much, Chris.

April 4, 2007

More About Christopher Sims

Current Positions

Harold H. Helm '20 Professor of Economics and Banking, Princeton University, since 2004; Director of Graduate Studies since 2003; Professor of Economics, 1999-2004

Visiting Scholar, International Monetary Fund, since 2003

Visiting Scholar, Federal Reserve Bank of New York, since 2004; and 1994-97

Previous Positions

Henry Ford II Professor of Economics, Yale University, 1990-99

Professor of Economics, University of Minnesota, 1974-90; Associate Professor, 1970-74

Assistant Professor of Economics, Harvard University, 1968-70; Instructor, 1967-68

Visiting Scholar, Federal Reserve Bank of Philadelphia, 2000-03; Federal Reserve Bank of Atlanta, most years since 1995

Consultant, Federal National Mortgage Association, 1999-2002; Federal Reserve Bank of Minneapolis, summer 1983 and 1986-87; Control Data Business Advisors, 1981-83

Director of Graduate Studies, Department of Economics, Yale University, 1992-94

Visiting Professor, Massachusetts Institute of Technology, 1979-80; Yale University, 1974

Professional Activities and Honors

Member, American Academy of Arts and Sciences, since 1989

Chair, Economics Sciences section, National Academy of Sciences, since 2006; Member, Report Review Committee, 2000-03; Member, Editorial Board of Proceedings, 1996-2000; Member of Academy since 1989

President of the Econometric Society, 1995; First Vice President, 1994; Second Vice President, 1993, Executive Committee member, 1992; Council member, 1990-92; Council elected member, 1979-80, 1990-91; Fellows Nominating Committee member, 1980, 1985; Summer Meetings Program Committee member, 1971, 1984, 1998; Fellow since 1975

Member, Commission for Behavioral and Social Sciences and Education, National Research Council, 1992-98

Co-chairman, Program Committee, 1990 World Congress of the Econometric Society; Program Committee member, 1980

Fellow, Minnesota Supercomputer Institute, 1987-91

Member, S.I.A.M. FCCSET Workshop on Research in Large Scale Computational Science and Engineering, 1987

Member, National Science Foundation Program Advisory Committee for Advanced Scientific Computing, 1984-86

Member, Committee on National Statistics of the National Research Council, National Academy of Sciences, 1982-85

Member, Brookings Panel of Economic Activity, 1975-76, 2001-02; Senior adviser, 1977-

Editorial Board member, *International Journal of Supercomputer Applications*, 1987-89; *Journal of Economics and Philosophy*, 1985-94; Associate Editor, *Journal of Applied Econometrics*, 1986-89; *Journal of Business and Economic Statistics*, 1985-94; Co-editor, *Econometrica*, 1977-81

Publications

Author of more than 70 articles published in economics journals, proceedings and compilations, with particular interest in econometric theory for dynamic models, macroeconomic theory and monetary policy

Education

Harvard University, Ph.D., economics, 1968

Harvard College, B.A., mathematics, 1963, magna cum laude