

INTERVIEW

Vernon Smith

Economic theory is based on several assumptions of how markets work and people behave. Vernon Smith has spent his career testing those assumptions in the laboratory. The result is the burgeoning field of “experimental economics.”

Experimental economists observe people’s actions in controlled environments to determine, among other things, how and why markets react to changes in legal and regulatory rules. The insights drawn from such simulated market transactions can be applied to a number of issues, including financial market theory, natural resource economics, and the deregulation of industry.

In 2001, Smith joined the faculty of George Mason University in Fairfax, Va., where he directs the Interdisciplinary Center for Economic Science (ICES). And last year, he and Daniel Kahneman of Princeton University were jointly awarded the Nobel Memorial Prize in Economic Sciences. This made Smith George Mason’s second Nobel Prize economist: James Buchanan took home the award in 1986 for his pioneering work in the field of Public Choice economics.

Smith, 76, heads a team of seven researchers at ICES, all of whom came with him from the University of Arizona, where he taught for 25 years. Aaron Steelman interviewed Smith at the Federal Reserve Bank of Richmond on January 27, 2003.

RF: You majored in engineering in college. Why did you make the switch to economics?

Smith: When I was a senior at CalTech, I took an economics course and I got interested in the topic. I went to the library, and among other things, I ran across a copy of Paul Samuelson’s *Foundations of Economic Analysis*. It looked to me a lot like physics, which I was already doing. I also subscribed to the *Quarterly Journal of Economics*, and in one of the first issues there was an article by Hollis Chenery on engineering production functions — so it was engineering too! Little did I know that that was a very unrepresentative issue of the journal. But, in general, the discipline was mathematical and had an important applied empirical dimension, which appealed to me.

I then went on to the University of Kansas, where I received my master’s degree. One of my best teachers there was Richard Howey, who was an expert in the history of economic thought. I learned what deep scholarship was from him and developed an appreciation of where economics was coming from in the 18th and 19th centuries. From there, I went to Harvard, where I did my Ph.D. Among my teachers at Harvard was Gottfried Haberler, who warned us of the dangers of inflation. It was, as Joseph Schumpeter called it, “ze monster.”

RF: In addition to changing disciplines, you also changed worldviews. You entered college as a socialist, right?

Smith: Yes, I grew up in a socialist family. My mother was a socialist and was very proud that she had cast her first vote for president for Eugene Victor Debs. I held many of those views also. But gradually, as I developed a deeper understanding of economics, I began to rethink things. And the final blow came when I started doing experiments and my subjects taught me that markets work. That transition took place over a number of years and became stronger and stronger. Certainly by the time I had been at Purdue for five or six years — this would be in the early 1960s — I had changed my mind.

RF: You had been on many economists’ short list for the Nobel Prize for some time. Were you surprised to finally get the call last year?

Smith: I have been hearing rumors about the Nobel Prize for 20 years. In 1997, for instance, I was called by someone from Reuters, who told me that he had it on good authority that I would be awarded the prize that year, and it would be announced the following Sunday. I said, "Well, they don't announce the awards on Sunday." But clearly I had been getting some nominations since the early 1980s, and my friends all thought that I was poised to receive the prize. So, after a while, you get pretty thick skinned about it all and just try to do your work. The only thing that made this year any different was that in 2001 the committee had a symposium on experimental economics in Stockholm. They invited 12 of us to be the main speakers and 12 people to discuss those papers. There was speculation they were going to pick somebody out of that group, but we couldn't be sure who it was or if it would be multiple people.

RF: George Mason University has a reputation for being a haven for free-market academics. What, in your view, has contributed to the development of this relatively unique intellectual environment?

Smith: I don't know that much about the history of George Mason. But I think a pretty crucial factor was that they were able to attract Jim Buchanan, who, in turn, attracted other Public Choice economists to the faculty and graduate students who wanted to work with them. In addition, they have a number of excellent Austrian School economists who have strong free-market views. Also, I think the administration realized that they couldn't be like Harvard, Yale, Chicago, and other major research universities. So they needed to find a niche to become well known. But I really can't give you a complete answer to that question. The topic would make for an interesting historical study.

RF: When you came to George Mason, you brought several faculty members from the University of Arizona along with you. In a way, this isn't surprising: Many academics benefit from working with colleagues with similar interests. But is this particularly true in the case of experimental economics? Is there something about the nuts and bolts of actually doing the experiments that requires a core group of researchers?

Smith: Many contributions to economics are made

by lone wolves. That is, there are an awful lot of papers with single authors, much more so than in the hard sciences. For instance, you can find papers in *Science* and *Nature* that will have 100 authors. I think experimental economics lends itself far more to that type of approach. Certainly, it's always helpful if you have a certain mass of people to talk to, but here the actual problems of covering the operations and doing the work require multiple skills that often aren't embodied in one person. I feel that a really important part of my development with experimental economics occurred when I was no longer working alone. That started in the 1970s with Charlie Plott, Ross Miller, and other people at CalTech. And by the time I arrived at the University of Arizona in 1975, most of the efforts were joint.

RF: How was your work received by the profession when you first started conducting experiments?

Smith: Not particularly well. I first started giving seminars in the late 1950s and early 1960s, and at the time people didn't know what I was doing or why I was doing it. It didn't look like economics to them. But I found it very interesting and exciting, and I was careful enough to do other things in the meantime to get promoted.

RF: Tell us about the relationship between experimental economics and public policy.

Smith: I should say that we don't set out to do policy research or to answer policy questions. We're interested in the performance and function of markets, and that often gets you into policy questions.

The first problem that we addressed, after we got into computerized experiments, was the question of how you might design a market for airport



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access rights. In the late 1970s, the deregulation of the airline industry began. This involved the routes that airlines could fly and the prices they could charge. But nobody was thinking about the airports, where, of course, airplanes have to land and take off. If access to the airports were going to continue to be allocated by some bureaucratic organization, then the airline industry in a sense wasn't really being deregulated.

Take-off and landing slots are a commodity that have greater value in packages than in singles, because for every take-off, you have to have a landing, and the flight schedules need to be compatible. We saw this early on as a combinatorial problem. So we designed an auction mechanism to try to solve this problem and tested it in the laboratory. Much later, of course, combinatorial auctions became of interest to people who were thinking about how to allocate rights to the electromagnetic spectrum.

RF: Your Nobel lecture seemed to draw heavily upon the work of F. A. Hayek, especially his ideas about rationality. Briefly talk about the difference between “constructivist rationality” and what you have termed “ecological rationality.”

Smith: Economic theory is dominated by constructivist models, involving optimal knowledge for individuals, equilibrium in markets, and efficiency. But what we observe in experiments and the real world is often different: people's knowledge is incomplete, and they may not have any idea what it means to talk about equilibrium and supply and demand. Remarkably, though, the first time I got people together in the lab, the market they formed converged toward a competitive equilibrium. And I think that the history of experimental economics is, to some extent, the story of people achieving these outcomes that are unintended and unknown to them. This type of knowledge is what I call “ecological rationality.” It may or may not correspond to the rational model that constructivism posits. We have examples where people don't do as well as rational models predict. We have examples where they do better. And we are trying to understand why.

All of our work is driven by the notion that information is fundamentally asymmetric, dispersed, not knowable by any one person or group — and that it is the guy out there with the knowledge who needs to be motivated to reveal what is necessary to make the system efficient. And this, of course, is very close to what Hayek argued. Hayek was incredibly good at the abstract level, but people read him and have a hard time relating to him because there are few concrete exam-

ples. In fact, I became influenced by Hayek only recently, and I now see certain themes that could integrate a lot of the work in experimental economics. My Nobel lecture was sort of the beginning of that project.

RF: What do you see as the relationship between game theory and experimental economics?

Smith: There has been a lot of experimental economics research concerned with testing propositions from game theory. When it works, game theorists love it. When it doesn't work, they have a tendency to say that there is something wrong with the experiments. So we make some modifications to try to explain those anomalies — for instance, why Dutch auction prices are lower than first-price sealed-bid auction prices, though game theory predicts that they are identical. We had two models, and one did a much better job of explaining it than the other one. So I guess the answer is, we have to do it ourselves. I would like to see a little more division of labor. They are the experts in game theory, after all.

But, recently, we have collaborated with some sympathetic game theorists, and it turns out that they are the very best in their field. John Ledyard at CalTech and David Kreps and Bob Wilson at Stanford have been very sympathetic. Wilson has even tackled the very important and difficult issue of doing a game-theoretic analysis of the double auction. It takes someone like Bob to pass up the temptation of picking all the low apples and going for a high one instead. And in his paper he points out the very serious limitations of his own results and that the limitations are inherent in the game-theoretic framework. It takes great courage, honesty, and intelligence to make such a claim.

RF: There is an emerging school of thought called “behavioral finance,” according to which investors display certain predictable cognitive biases that can lead to significant deviations between the behavior of actual markets and the predictions of models with frictionless markets. Some even say that these biases can explain dramatic stock market runups like we had in the late 1990s. Does your experimental work shed light on this issue?

Smith: The explanations of the behavioral finance theorists are certainly possible. But I'm not prepared to say that these markets or their participants are irrational. And, by the way, neither is Daniel Kahneman. As far as Danny is concerned, that's the behavior and he's just reporting it. He doesn't really make a judgment.

One of the reasons I'm not prepared to call this behavior irrational is that, while it may not be good for the individual, it may be good for society as a whole. Maybe we are programmed to do some very risky things in an effort to get back in the game — and, in so doing, that behavior may produce benefits for society. We know that people tend to throw money at innovations and new entrants to the market, and so we have people bidding up the price of companies with no net profits. Many, maybe most, of those companies will fail. But a few will survive and throw off huge benefits for the economy, benefits that far exceed the money that was spent to get them off the ground. Consider the steam engine. It produced the steam ship and the locomotive, which were big drivers of reducing transaction costs and increasing the speed of things. We spanned a continent with rail in very little time. Well, many individual investors lost money, but there was long-term value created. The 19th century was an incredible growth century. And for all I know this risky, speculative behavior is the cost of that kind of growth. If I had a formula to protect individuals from their own tomfoolery, it would not necessarily be best for society. I can't be sure of that.

RF: Some observers have described your experimental work as showing that markets often malfunction, because in experiments prices often deviate from “fundamental” values. Others say your work is broadly supportive of the efficiency of market mechanisms, because prices exhibit a strong tendency to clear the market. Which is it: Do markets work well or do they often malfunction?

Smith: I distinguish between consumer markets and asset markets. With the asset markets, we often get bubbles and crashes in the laboratory, but the consumer markets tend to converge quickly. The asset markets eventually get there also, but it takes longer. We sometimes have to bring subjects back three times before they will quit trading away from fundamental value. So there is confirmation of ultimate fundamental value trading, but under conditions that are hard to imagine actually occurring, except for very stable companies. The distinction between consumer markets and asset markets is very important. If people don't make that distinction, they can get confused about what my work shows.

RF: What do you think is the most pressing issue in economics today?

Smith: I think economists' work is still colored

too much by their political preferences. You know, Hayek once wrote a paper called “Why I Am Not a Conservative.” I sometimes would like to write a paper about why I'm not a conservative or a liberal or a libertarian, so that I could state what I think the problems are with all of them.

It's important for economists to understand the phenomena they are studying before they start asking what can be done about it. And it seems to me that a lot of people are afraid of getting certain empirical results because of the implications it might have for the policies they favor. You see this in particular in the resistance to even the slightest suggestion that some of our behavior may have an inherited component. You have people like Steve Pinker who are talking about both the environment and heredity, and yet they are accused of being genetic determinists, when of course the alternative is just plain environmental determinism. It amazes me how much people are still arguing over nature versus nurture, when it seems clear to me that it's both.

RF: Some economists have done tremendous technical work but have not been particularly interested in reaching people outside the profession. Others have had the desire — and, perhaps more importantly, the skill — to do high-quality work and make it accessible for lay audiences. What, in your view, should be the role between the economist and the public?

Smith: I think every economist should ask himself the brother-in-law test: Can I explain to my brother-in-law, who is in a completely different field, what it is that I do for a living? The physicists try to do this. Some are better than others, but they try to explain what is implied by quantum physics or by relativity. And over the years, I think their ways of explaining these issues have improved.

There are an awful lot of people out there who don't have a lot of education but who are smart and want to know things. For example, my parents had an eighth-grade education, but they were always curious. We ought to try to reach these people.

Vernon Smith

➤ Present Position

Professor of Law and Economics,
George Mason University

➤ Previous Faculty Appointments

University of Arizona, California
Institute of Technology, University of
Massachusetts, Brown University,
Purdue University

➤ Education

B.S.E.E., California Institute of
Technology (1949); M.A., University
of Kansas (1952); Ph.D., Harvard
University (1955)

➤ Awards and Honorary Positions

Co-winner, 2002 Nobel Memorial
Prize in Economic Sciences;
Distinguished Fellow, American
Economic Association; Fellow,
American Academy of Arts and
Sciences

➤ Selected Publications

Author of more than 200 articles in
scholarly journals, some of which
are collected in *Bargaining and
Market Behavior: Essays in
Experimental Economics* (New York:
Cambridge University Press, 2000)



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