INTERVIEW

John A. List

In the 1950s, Vernon Smith — then teaching at Purdue University and influenced by the work of Edward Chamberlin, one of his instructors at Harvard University — began conducting experiments to see how people responded to various market incentives and structures in a laboratory-type environment. At first, many economists questioned the importance of those experiments' results. But by the 1970s, others, including Charles Plott of the California Institute of Technology, began using experiments to better understand decisionmaking in various market settings, and in 2002 Smith was awarded the Nobel Prize in economics along with the psychologist Daniel Kahneman of Princeton University.

In the mid-1990s, John List, who believed that experimental work had provided unique insights into human behavior, began conducting experiments of his own, but in the field rather than in the lab. By setting up carefully designed experiments with people performing tasks they are used to doing as part of their daily lives, List has been able to test how people behave in natural settings — and whether that behavior is consistent with economic theory. List's field experiments, like Smith's lab experiments, were initially greeted with skepticism by many economists, but that has changed over time. List has published more than 150 articles in refereed academic journals during the last 15 years, many on field experiments and related work.

List began his career at the University of Central Florida, with stops at the University of Arizona and the University of Maryland before arriving at the University of Chicago in 2005. While at Maryland, List served as a senior economist with the President's Council of Economic Advisers, working largely on environmental and natural resources issues. He is co-editor of the *Journal of Economic Perspectives* and serves on the editorial boards of several other journals. Aaron Steelman interviewed List at his office in Chicago in May 2012.

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RF: Could you briefly describe what you mean when you speak about field experiments in economics — and what methodological issues should economists be concerned with in order to do field experiments well?

List: A good place to start is to think about how economists have used measurement tools in the past. The semi-automatic approach has been to go to your office and write down a model and then go out and look for data. You don't generate your own data, but look for secondary data. After you find mounds and mounds of data, you then overlay assumptions on those data to make causal inference. If you use a propensity score matching model, for example, you invoke a conditional independence assumption. If you use instrumental variables, you have exclusion restrictions. If you use a difference in difference model, you make assumptions about the correlation between the error term and the regressors. So that's the typical approach. The overarching idea is that the world is messy, so we need to write down a model, go gather mounds and mounds of data, empirically model those data, and then try to say something beyond a correlation — try to make a causal statement that's within our theory.

About 50 or 60 years ago, Vernon Smith enters the picture and says that we can learn about economic relationships using laboratory experiments. He starts to run lab experiments in the 1950s, mainly using undergraduate students, and he finds some very interesting results. And this was before many of the measurement tools like instrumental variables had been fully developed. Economics had a very Victorian sensibility at that time. Now the beauty behind experimentation is that you need to make one major assumption to identify the treatment effect of interest, and that's proper randomization. So while the other empirical approaches typically have assumptions that economists view as quite contentious, experimentation has one that can be externally verified.

You can then ask, why don't we all just run lab experiments with students? For me, a first inclination is not to gather data in the lab, but go to the field, though I have always been sympathetic to the laboratory approach. I was hit over the head when I was working in the White House in 2002 and I was arguing that as we revised the cost-benefit guidelines we should take into account Danny Kahneman's, Dick Thaler's, and Jack Knetsch's work that shows students have reference-dependent preferences in the lab. Unfortunately, no one at the White House took me too seriously. So when Glenn Harrison and I wrote the paper for the *Journal of Economic Literature* in 2004 on field experiments, our first thought was: What is the first step outside a typical lab experiment with student subjects that would still have the environment of the lab but would capture better the idea of a representative population? And that's what we call an artefactual field experiment. The first step is not to really go outside the lab, but it's to go and collect data from a group of experts — farmers, CEOs, members of the Chicago Board of Trade, whomever is of interest — and run those people through a typical laboratory exercise. The field element is the person in this case. You could say, well, you have now dealt with the issue of representativeness, but it's still a very sterile and artificial environment when we gather lab data.

So the next step that Harrison and I talk about is what we denote as a framed field experiment. And what that means is that we slowly add naturalness to the environment by asking subjects to perform a task that they are used to performing, using the same stakes that they typically use in their everyday lives. It's having them do things that they normally do, but they still know that they're taking part in an experiment.

The last step in this process is to have randomization and realism. And that's what we call a natural field experiment. In this type of data-generating exercise, I now have what the naturally occurring data has, which is realism — that is, I observe people behaving in the markets in which we want to study. And then I use randomization to identify my treatment effect. We essentially have our cake and can eat it too with natural field experiments.

Beyond having the power to measure important treatment effects, the point of all these levels of field experiments was to see if ideas like reference-dependent preferences dictated behavior as strongly in everyday life as the lab evidence seemed to suggest.

RF: Could you give an example of how this is done?

List: A real problem with artefactual and frame field experiments is that it's possible that the act of experimentation is influencing the participants' behavior. So let's go through an example whereby I think I can convince you that I am in a natural environment and that I'm learning something of importance for economics. I first got interested in charitable fundraising in 1998 when a dean at the University of Central Florida asked me to raise money for a center at UCF. I went out and talked to dozens of fundraising practitioners and experts, and they had several long-held beliefs about such things as the benefits of seed money and of using matching funds. Many charities have programs where they will match a donor's gift. So your $100 gift means that the charity will get $200 after the match.

Interestingly, however, when you go and ask those charities if matching works they say, "Of course it does, and a 2-to-1 match is much better than a 1-to-1 match, and a 3-to-1 match is better than either of them." So I asked, "What is your empirical evidence for that?" They had none. Turns out that it was a gut feeling they had.

I said, well, why don't you do field experiments to learn about what works for charity? Let's say the typical way in which a charity asks for money is a mail solicitation. So what we are going to do is partner with them in one of their mail solicitations. Say they send out 50,000 letters a month. We will then randomize those 50,000 letters that go directly to households into different treatments. One household might receive a letter that says, "Please give to our charity. Every dollar you give will be matched with $3 from us." Another household might receive the exact same letter, but the only thing that changes is that we tell them that every dollar you give will be matched by $2. Another household receives a $1 match offer. And, finally, another household will receive a letter that doesn't mention matching. So you fill these treatment cells with thousands of households that don't know they're part of an experiment. We're using randomization to learn about whether the match works. That's an example of a natural field experiment — completed in a natural environment and the task is commonplace.

I didn't learn that 3-to-1 works better than 2-to-1 or 1-to-1. Empirically, what happens is, the match in and of itself works really well. We raise about 20 percent more money when there is a match available. But, the 3-to-1, 2-to-1, and 1-to-1 matches work about the same.

RF: How does charitable giving in the United States compare to other countries? And what do you think are some of the reasons that may explain these differences?

List: I have co-written with Michael Price a recent paper titled "Charitable Giving Around the World" and something that we bumped up against right away is that it's hard to find good, comparable data around the world. So, with that caveat in place, two stylized facts jump out. One is that people in the United States give at extraordinary rates. We give roughly 3 percent of GDP every year. And that represents individual gifts — that doesn't include corporations. When you compare the United States to other developed countries, the U.S. is well above other developed countries. However, when you look at volunteerism, the U.S. is well below other countries. So we give a lot more money, but we volunteer our time much less often than citizens in other countries.

So as an economist I ask, what are the economic reasons for these patterns? What you observe in other countries is that governments tend to provide a lot more public goods. In Europe, for example, their marginal tax rates tend to be well above ours because they provide more public services or public goods. When you talk to folks from Europe, what they tell you is, "I don't need to give to that particular cause, because the government already provides it." If you look at U.S. history, functions such as helping the poor have varied over time — during some periods helping the poor was spearheaded by the government; in others, private organizations did the bulk of the work. So a lot of charitable organizations were formed and still are active in that space. That said, in many European countries more individuals are increasingly willing to give money as, say, public funds for universities are being cut. I receive phone calls all the time from European universities that are considering approaching their alumni for donations.

I think economic differences — levels of taxation, the provision of public goods — can explain a lot of the differences across countries. I think culture also has a lot to do with it as well, even though "culture" is sort of the catch-all term that can explain just about everything. Still, it's true that we have a culture of giving money in the U.S., while in other countries they have a culture of giving time. And if you see that your parents generally give money rather than time, or vice versa, you tend to do the same thing.

RF: Are there certain types of issues that have features which make them particularly well suited to field experiment work? And are there some areas that you think field experiments would yield little to our understanding of those issues?

List: Let's start with the types of issues we might exclude. I think that a lot of macro policies, like the effect of interest rates on the macroeconomy, fit into that category. It's hard for me to envision that when you have a policy that affects the entire nation at once, like a change in interest rates, you could effectively think about a field experiment that could give you great insights. And the reason why is because you don't have the proper counterfactual. If you could randomize different states into different interest rate environments and people couldn't lend across state lines, then you could maybe get somewhere with a question like that. But when you don't have the proper counterfactual, it's really hard to envision a field experiment lending many insights. So I think there are a lot of questions in which the field experimental method is not the best approach. It is just impractical for many important economic questions.

But there are other questions where field experiments could be very useful. How much discrimination is present in a market and what is the nature of that discrimination? Why do people give to charitable causes and what keeps them committed to the cause? Are prospect theory preferences important in markets and can those with market experience overcome those biases, or do people learn to have behavioral biases? What education reforms can work most cost effectively? What are the best ways to reduce the racial achievement gap? What public policies can work to lower teen criminal activity? All of these questions and many others are fair game using the field experimental method. Further, I believe that field experiments are the best approach, to, first of all, find out whether there's a causal relationship between variables of interest, and then also determine the underlying channels for that relationship. I think field experiments, better than any other approach, can measure whether it's occurring and tell you why it's occurring.

For instance, it's really hard to look at mounds and mounds of data and determine why one person is discriminating against another in a market. Economists have two major theories. One is Gary Becker's taste-based discrimination — people discriminate because they have a taste for discrimination; for example, because they don't like that certain person or group, they are willing to forgo profits to cater their prejudice. Years before that, Arthur Pigou discussed third-degree price discrimination — entrepreneurs, in their pursuit of profit, will discriminate. With mounds and mounds of data, it would be very hard for you to parse those two models. But if you have the correct field experimental treatments, you cannot only measure if discrimination exists, but you can decipher which of those models is at work. I did this in my 2004 *QJE* paper on discrimination and in more recent work across several markets with Uri Gneezy and Michael Price.

RF: Why do you think economists have largely been opposed to methodological approaches such as field experiments and do you believe that is beginning to change?

List: First of all, when economists started using experimentation it was in the lab. And I think many people in the profession were already skeptical of what we can learn from laboratory exercises because they were already tainted by their distrust of psychology experiments. So I come along, and I say we really need to use the tool of randomization, but we need to use it in the field. Here's where the skepticism arose using that approach: People would say, "You can't do that, because the world is really, really messy, and there are a lot of things that you don't observe or control. When you go to the marketplace, there are a lot of reasons why people are behaving in the manner in which they behave. So there's no way — you don't have the control — to run an experiment in that environment and learn something useful. The best you can do is to just observe and take from that observation something of potential interest."

That reasoning stems from the natural sciences. Consider the example with the chemist: If she has dirty test tubes her data are flawed. The rub is that chemists do not use randomization to measure treatment effects. When you do, you can balance the unobservables — the "dirt" — and make clean inference. As such, I think that economists' reasoning on field experiments has been flawed for decades, and I believe it is an important reason why people have not used field experiments until the last 10 or 15 years. They have believed that because the world is really messy, you can't have control in the same way that a chemist has control or a biologist might have control.

That's what people often think about — the scientific method. In physics, we have vacuum tubes; in chemistry we have very clean test tubes. If you don't have a very clean test tube, then you can't experiment as the theory goes. And I think people have generalized incorrectly, and here's why: When I look at the real world, I want it to be messy. I want there to be many, many variables that we don't observe and I want those variables to frustrate inference. The reason why the field experiments are so valuable is because you randomize people into treatment and control, and those unobservable variables are then balanced. I'm not getting rid of the unobservables — you can never get rid of unobservables — but I can balance them across treatment and control cells. Experimentation should be used in environments that are messy; and I think the profession has had it exactly backwards for decades. They have always thought if the test tube is not clean, then you can't experiment. That's exactly wrong. When the test tube is dirty, it means that it's harder to make proper causal inference by using our typical empirical approaches that model mounds and mounds of data.

So I think there are two main reasons. People have traditionally thought of experimentation through the lens of the lab, and they have not liked that because of perceived problems of representativeness of the population or representativeness of the situation. And secondly, they have flawed thinking about how you identify your treatment effect with your field experiment.

RF: Under which conditions does prospect theory seem to explain behavior that cannot seemingly be explained by conventional neoclassical models?

List: I think a general statement about behavioral economics would be as follows: If I want to take a trip from Chicago to Fenway Park — say I want to go watch the Red Sox play the Yankees — neoclassical theory will get me to Cambridge. But I need behavioral economics to get me from Cambridge to my seat in the 25th row of Fenway Park. And what that means is that I think behavioral economics is important to explain behavior at the individual level, but if we want to get into the vicinity of the correct answer, neoclassical economics can get us there. And then around the margin, behavioral economics does really well at pinpointing and helping us refine that answer.

I think prospect theory is a perfect example of a behavioral manifestation that is important. One of the most important elements within prospect theory is something called loss aversion — people value a one unit loss much more than a one unit gain. How do you leverage that insight? We have done so in several places. One example is that we — Tanjim Hossain and I — have gone to manufacturing plants in China and they have asked us what are the best ways to incentivize their workers to work hard. What we typically do is we give them a few dollars more if they produce at higher levels, and we tell them this is a conditional bonus. We first give them the money and then say, if you do not achieve that goal, we will take that money away from you. We find that just by framing, we can increase productivity by 1 percent. And that occurred for more than just a few hours; that occurred for six months.

You can say, well, does that work in other walks of life? What's been really hard in the area of education is to use incentives to get teachers to try harder. So teachers will say, "Look, I try as hard as I can already." And we have incentive schemes that have been tried in the United States that don't seem to work very well. These incentive schemes tend to be structured something like this: In September we tell you, if your students do a lot better than everyone else's students, then you are going to receive $4,000 in the spring. What we have found is that doesn't really work very well. But if we give them the $4,000 in the fall and tell them we will take that money away in the spring if your students do not achieve, they will perform remarkably better. And one explanation consistent with such behavior is loss aversion.

It also works for students. For example, we have compared two groups. First, we have gone into the testing room the morning of a test and said, here's $20 and if you improve your test scores from last fall, you can keep the $20, but if you don't improve, we will take it away. Second, we have told a different group of students that they will receive $20 after

John A. List

Present Position

Homer J. Livingstone Professor of Economics, University of Chicago

Previous Faculty Appointments

University of Maryland (2001-2005), University of Arizona (2000-2001), and University of Central Florida (1996-2000)

Education

B.S., University of Wisconsin-Stevens Point (1992); Ph.D., University of Wyoming (1996)

Selected Publications

Author of numerous articles in such journals as the *American Economic Review*, *Journal of Political Economy*, *Quarterly Journal of Economics*, *Econometrica*, *Journal of Public Economics*, and *Science* the fact if they improve their scores. The first group performs much better than the second. And I think this is because people do have an aversion to losing something.

You can say, OK, how does that affect markets? And that's what I have thought hard about. As I found in my 2003 *QJE* piece on prospect theory, if you go to a market that has active traders, what you find is that the inexperienced people trade as though they have loss aversion but the really experienced ones don't. And then you ask yourself, well, is that because of selection or treatment? Maybe some of us are born with prospect theory preferences, while some of us are not. Or is it that the market has taught the experienced traders? Is it that the people who survive don't have prospect theory preferences, and if they have them, then they don't survive in the market? Now you can test that because you can randomly give people experience. How I have done that in a recently published piece in the *American Economic Review* is by giving some people free goods and telling them to go off and trade them and you incentivize them to trade; in the control group, you don't give free goods and you don't incentivize them to trade. And you look via experimentation whether the first group exhibits prospect theory preferences after six months versus the second group. What happens is that the market does weed out those people who have real biases, but people do learn. So the act of trading induces people to learn to overcome their prospect theory preferences.

In the end, is the market price determined by people who have prospect theory preferences? No. I think behavioral economics in this form is important to get people to do things you want them to do, but in determining prices and allocations in more mature markets, there is not strong evidence that such preferences importantly influence prices.

RF: To what extent do additional entrants in the certification market tend to improve information provided to consumers — and which consumers tend to benefit most from additional firms entering that market?

List: Product certification is used in many markets. And you can ask yourself, well, is product certification important, does it improve the welfare of people, does it improve information in the market? When you think about how you answer these types of questions, it seems like a field experiment is a really good approach to lend initial insights. That's what co-authors at the University of Maryland and I did when we researched in this area when I was a professor at Maryland. We looked at the market for sports cards and what you see is that before 1987, there was no third-party certifier in that market. In 1987 a company called Professional Sports Authenticator (PSA) enters. They start informing sports card buyers, sellers, and dealers the quality of their sports cards. Is it authentic, for example? Does it have sharp corners? Does it have good centering? And what they essentially did was develop a scheme that was very coarse. They gave a card an integer grade of 1 to 10, and what you find is that the information they provided is useless to those really experienced in the sports card markets. Sports card people who already had experience — the dealers — already knew the information that PSA provided.

But those really inexperienced consumers received a wealth of information from that ranking scheme. So when you think about a market that begins to evolve and when you have a monopolist certifier, it will provide information to the market, but only a certain type of individual will benefit from that information.

So then we observe behavior from 1987 to 1999, and now two more sports card graders enter the market — Sportscard Guaranty (SGC) and Beckett Grading Service (BGS). What these two firms do to secure market share is to offer a more differentiated product. Now your card can receive a 7.5 instead of just a 7 or 8, which is what PSA offered, and now that information, in its more detailed form, is adding insights to even the most experienced people. As a whole, that increases welfare. And since then the market has become even more developed, with many other firms entering. So you see this great evolution of a private certification market, and because we can overlay a field experiment on it we can then measure the welfare implications of that evolution.

RF: One of the things that you mentioned in your 2011 *Journal of Economic Perspectives* paper, "Why Economists Should Conduct Field Experiments and 14 Tips for Pulling One Off," is that it's important to do field experiments about things that you know well. This seems like a good example.

List: Absolutely. I started as a sports card dealer back in high school in the mid-1980s. I didn't really see it then, but I was actually running field experiments, because I would start off the bargaining process differently depending on the characteristics of the potential buyer — whether the buyer was male or female, young or old, for instance. In a way I had experimented already with bargaining propensities without knowing it. And then I arrive at the University of Wyoming as a graduate student in the early 1990s and I learn that there's this emerging literature on laboratory experiments. So I thought, well, why don't we study this market using field experiments? And when I tried to sell that to my professors at Wyoming no one was interested at first. I said, I know economic theory, and I know the sports card market very well. How about if I use that as my laboratory? I never really imagined that we would care about sports cards in and of themselves — it's too small of a market. But it also seemed like a market that was well suited to these types of experiments because I knew it well, and the broader behaviors that I was trying to learn about should be generalizable to more important markets.

So I ended up starting to run my first scientific field experiments in Denver in the early 1990s for my dissertation and for future work. I always thought that the main advantage I had was that I knew my laboratory well — by knowing how the market functioned, I could implement various treatments with confidence that my interpretation of the data was correct. For example, I could run a certain kind of auction and everyone would find that to be natural. I knew I could approach dealers and bargain in a way that they would think there's nothing unusual happening. I knew that there were aspects of this market that could tell me things about loss aversion, about discrimination, about product certification, about bargaining, and about many other issues economists found interesting. I don't think I could have done that had I not understood the market — the motives and the values and the preferences of the participants — as well as I did.

I think that's one of the two main features that you must have before you actually go out and run field experiments: You really need to understand the market so you know what you are testing and you know how to test it in a natural way. I think the other main feature is that you always need economic theory as a guide. You are setting up your experiment based on economic theory and also to test economic theory. Theory provides a framework to help design the experiments, and the experimental results give you a view of the theory that you could never have without randomization. In this way, the theory is a lens into not only the data but also the world at large.

RF: Your paper with Roland Fryer and Steven Levitt came to a somewhat ambiguous conclusion about whether stereotype threat exists. But do you have a hunch regarding the answer to that question based on the results of your experiment?

List: I believe in priming. Psychologists have shown us the power of priming, and stereotype threat is an interesting type of priming. Claude Steele, a psychologist at Stanford, popularized the term stereotype threat. He had people taking a math exam, for example, jot down whether they were male or female on top of their exams, and he found that when you wrote down that you were female, you performed less well than if you did not write down that you were female. They call this the stereotype threat. My first instinct was that effect probably does happen, but you could use incentives to make it go away. And what I mean by that is, if the test is important enough or if you overlaid monetary incentives on that test, then the stereotype threat would largely disappear, or become economically irrelevant.

So we designed the experiment to test that, and we found that we could not even induce stereotype threat. We did everything we could to try to get it. We announced to them, "Women do not perform as well as men on this test and we want you now to put your gender on the top of the test." And other social scientists would say, that's crazy — if you do that, you will get stereotype threat every time. But we still didn't get it.

What that led me to believe is that, while I think that priming works, I think that stereotype threat has a lot of important boundaries that severely limit its generalizability. I think what has happened is, a few people found this result early on and now there's publication bias. But when you talk behind the scenes to people in the profession, they have a hard time finding it. So what do they do in that case? A lot of people just shelve that experiment; they say it must be wrong because there are 10 papers in the literature that find it. Well, if there have been 200 studies that try to find it, 10 should find it, right? This is a Type II error but people still believe in the theory of stereotype threat. I think that there are a lot of reasons why it does not occur. So while I believe in priming, I am not convinced that stereotype threat is important.

RF: That raises a related question: How strong do you think publication bias is in the economics profession?

List: It's really hard to publish a paper that goes against the mainstream way of thinking. And I just think about some of my own experiences, such as the prospect theory paper I mentioned before, which was published in the *QJE* in 2003. The paper, when it started, was a very short exercise showing the power of market experience and because people did not believe it, I had to continue to do new experiments — new field tests — and eventually this paper consumed my life for years and ended up being a 30-page paper. Was it a much stronger contribution? Absolutely, the editorial and review process really helped a lot. But the main message was always contained in a paper that could have been 10 pages. To overturn the mainstream way of thinking, however, you have to go above and beyond. And that's often hard to do because the burden of proof is on you.

That said, could I tell you right now what are the five things that I think the profession has wrong? I couldn't, because I think the profession has most things right. It might not have all the details right, but I believe most of the first-order thinking is right.

I think in many ways, it's harder to overturn entrenched thinking in parts of the nonprofit, corporate, and public sectors, where many things are not subject to empirical testing. For instance, why don't we know what works in education? It's because we have not used field experiments across school districts. Each school district should be engaged in several experiments a year, and then in the end the federal government can say, "Here's what works. Here's a new law." It's unfair to future generations to pass along zero information on what policies can curb criminal activities, what policies can curb teen pregnancy, what are the best ways to overcome the racial achievement gap, why there aren't more women in the top echelon of corporations. We don't know because we don't understand, we haven't engaged in feedback-maximization. There needs to be a transformation, and I don't know what it's going to take. I mean, are we going to be sitting here in 50 years and thinking, "If we only knew what worked to help close the achievement gap, if we only knew how to do that"?

I hope my work in education induces a sea change in the way we think about how to construct curricula. Right now, we are doing a lot of work on a prekindergarten program in Chicago Heights and in a year or two I think that we will be able to tell policymakers what will help kids — and how much it will help them. But unless people adopt the field experimental approach more broadly, it will be a career that's not fulfilled in my eyes.

RF: Do you think the market for placement of new economists works relatively well? I am interested in both your empirical work on this topic as well as what you believe you have learned from your own experience.

List: My personal experience is sort of a checkered one. When I graduated from the University of Wyoming in 1996 I applied for 150 academic jobs. The ASSA meetings that year were in San Francisco. So I flew to San Francisco from Laramie, and I'm beaten down. I applied to 150 schools and only two schools agreed to interview me at the meetings. One was the University of Central Florida and one was Montana State University-Billings. So at that point I didn't think the market worked very well, because I thought I was a reasonable economist and I should receive more attention. But the majority of economists obviously did not agree with me. I was really lucky that I ended up securing a job at the University of Central Florida, because I'm not sure really what would have happened otherwise. My dad is a truck driver and maybe I would have gone back to Wisconsin and ended up driving trucks. Luckily enough, I did get an academic job that year.

I continued to do field experiments at Central Florida. Vernon Smith noticed some of my work and I ended up moving to the University of Arizona in 2000. Unfortunately, when I arrived at Arizona, Vernon told me that he was having problems with the administration and that the entire experimental group was moving. He wasn't sure where. At the time he was talking to Purdue and Caltech. He ended up going to George Mason. That winter, some people at the University of Maryland had read a few of my papers on field experiments and I had a little bit of luck in placing them at top journals, so they called me. I ended up moving there, which is close to George Mason and allowed me to continue doing some work with Vernon's group.

I then had a really good publication year in 2004, and the profession started to recognize that I'm writing these papers that could be paving a new way to think about empirical economics using field experiments. And that's when I moved to Chicago and I've been here since 2005.

So in my case you would say the market worked pretty well. I was coming from a school that was not highly ranked, so not many schools were interested in me. In fact, if I had sent my application to Chicago in 1995, I'm sure that they would not have even opened the envelope because it said the University of Wyoming on the cover and that would have been viewed as a bad signal. I think I got more or less what I deserved; I got what the market said I should get. What would have been a sign that the market did not work would be if I were still at the University of Central Florida with the exact same number of publications and the exact same number of projects going on and Chicago still said no because I graduated from the University of Wyoming.

Now, my own experience got me interested in how this market actually operates. So I started to do survey work and field experiments on what determines a person's success in this market. What do people look at when they hire Ph.D.s for the first time? And that's when I started writing these articles about what it takes to get an academic interview or government interview or business interview, because I was so fascinated and disappointed by my own experience. What I found in that work were kind of the typical things: It hurt me not coming from a top 5, top 10, or top 20 school; it hurt me that I did not have a well-known, Nobel-type economist writing letters for me; and perhaps what hurt me the most is that I didn't have much published research at the time. But the silver lining is that in the end if you work hard, you can increase your stock and you can move up. I have aged a lot in this process. It's been many years of sleepless nights working on research. I have loved every minute of it, though.

RF: Do you think your experience is typical in the respect that you have to make several moves, some of which might be considered lateral, before arriving at what might seem like the appropriately matched institution or department?

List: I do often wonder, did I really have to move three times to get to Chicago, or could I have just waited and moved directly here in 2005 or maybe a little earlier from Central Florida? There is not a lot of evidence on that; there are some stylized facts. Something like 90 or 95 percent of people secure their first jobs at departments that are lower ranked than the departments that they graduated from. This is because the top schools graduate many more people than they can hire. And then where you get tenure is typically at a department ranked lower than where you got your first job.

RF: Which economists have been the most influential in shaping your thinking about economic policy issues and how those issues should be addressed?

List: Vernon Smith and Gary Becker, but for different reasons. Vernon because he got me interested in generating your own data and framing questions in the appropriate ways. Gary because he showed me the importance of having a disciplined way to think about the problem and understanding that standard neoclassical economics can go a long way in explaining, or helping us to explain, major problems. I think above all else, those two traits have shaped the way I think about policy problems and economics more generally. **RF**