The Art and Practice of Economics Research: Lessons from Leading Minds

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Simon W. Bowmaker, New York University

Interview with Susan Athey, Harvard University (July 13, 2010)

Susan Athey was born in Boston, Massachusetts in 1970 and graduated with a bachelor's degree in economics, mathematics, and computer science from Duke University in 1991, before obtaining a PhD in economics from Stanford University in 1991. She was an Assistant Professor of Economics at the Massachusetts Institute of Technology between 1995 and 1997, an Associate Professor of Economics at Stanford University between 2001 and 2004, and since 2006 has been a Professor of Economics at Harvard University.

Professor Athey's research interests include mathematical methods and tools for theoretical modeling, auctions, industrial organization, econometric identification, and organizational design. Her most-cited articles include, 'Single crossing properties and the existence of pure strategy equilibria in games of incomplete information', *Econometrica* (2001), 'Optimal collusion with private information', *RAND Journal of Economics* (2002), co-authored with Kyle Bagwell, 'Identification of standard auction models', *Econometrica* (2002), co-authored with Philip Haile, 'Monotone comparative statics under uncertainty', *Quarterly Journal of Economics* (2002), and 'Collusion and price rigidity', *Review of Economic Studies* (2004), co-authored with Kyle Bagwell and Christopher Sanchirico.

Professor Athey's academic awards include the John Bates Clark Medal (2007) and the Elaine Bennett research award (2000), given every other year to an outstanding young woman in any field of economics. She was elected as a fellow of the Econometric Society in 2004. President Obama named Athey as an Appointee for Member to the President's Committee on the National Medal of Science in 2011.

Professor Athey's current professional activities include being co-director of the NBER Working Group on Market Design, as well as acting as an associate editor of *Theoretical Economics* and the *B.E. Journals in Theoretical Economics*. She is a Research Associate at the National Bureau of Economic Research (NBER) and, since 2007, has served as Chief Economist to Microsoft Corporation.

I interviewed Susan Athey in her office at the Department of Economics at Harvard University. It was mid-morning of July, 13, 2010.

BACKGROUND INFORMATION

Bowmaker: What was your attraction to economics?

Athey: I got started in computer science, but I didn't get excited about research there. When I discovered economics, it seemed like a wonderful opportunity to apply abstract ideas and mathematical techniques to something really important. And I was exposed early on to a policy problem through a summer job. My mentor at Duke, Bob Marshall, showed me how to take a real-world problem about procurement auctions (that I discovered in a summer job), and translate it into a possibility to change policy in a way that would make procurement more efficient. He ended up testifying before a Senate sub-committee on the research that I assisted in, and that was transformative for me. I got to see how you start with a real-world problem and use mathematics to break it down and explain what the policy problem is, and come up with the proposed solutions. And then we had a chance to go in and influence public policy. In the end, I think that's a lot of what the power of economics is about, and it was incredibly appealing to me.

Bowmaker: Was Bob Marshall your most influential professor as an undergraduate? How about as a graduate student?

Athey: Bob Marshall made a big effort to pick up promising undergraduates. He got me a full-time research assistant job with summer funding and a little office. He was really influential in getting me into economics, which I had never considered. And I also hadn't considered the possibility of being a professor before. He was a big believer in mapping the real world into mathematical models and using rigorous techniques to analyze the problems, while emphasizing the institutional details and making sure that your work was going to be relevant. That's something that I evangelize today; I believe it's an important part of my own research to dig into applications deeply, while solving real-world problems with full rigor.

One thing Bob did for me as an undergraduate was to show me Roger Myerson's papers on mechanism design, and Paul Milgrom's seminal work and his PhD thesis. I was just amazed; all those problems in one beautiful mathematical framework that delivered enormously powerful insight. Interestingly, today I explain the Revenue Equivalence Theorem to top executives at major firms, and I think back to when I first discovered it and understood it as an eighteen-year-old undergraduate. I think Bob did a great job in showing me work that would be very appealing to me.

Then in graduate school, my advisors, Paul Milgrom, John Roberts, and Eddie Lazear, were all very influential, and gave me enormous amounts of time and very close mentoring. They were great role models, as well as very hands-on advisors. And the influence of Bob Wilson, the grandfather of Stanford GSB and Milgrom's advisor, was everywhere.

Bowmaker: Why did you decide to pursue an academic career?

Athey: It was the chance to change the world through research. I didn't think going in that the life of a researcher was likely to appeal to me. I saw myself as being in maybe more of a more social type of work environment. But I found, to my surprise, that even the solitary aspects of research were incredibly enjoyable. That said, I collaborate in all of my projects. I don't even think about writing a paper by myself. And I've done a lot of work advising students. So, I feel like the academic career still gives me the chance for that sort of personal interaction; it's a good mix.

Of course, if you're at a top school, being a professor is a dream job. I'm now getting to reap the benefits. You get called for policy questions, and when you're a recognized world expert, there's a world of possibilities open to you from that platform, and amazing opportunities to take your ideas and put them into practice.

Bowmaker: As a researcher, which colleagues have been most influential or inspirational?

Athey: My advisors, Paul Milgrom and John Roberts, have maintained an influence on me throughout my career. As junior faculty, I leaned on them very, very heavily for support and guidance, like getting through the publication process, which I didn't find easy initially. And in the last couple of years, I've been very active in market design. There are a number of senior leaders in the field of market design who I think have done a great job in showing how you can spend time solving important applied problems without sacrificing any of the rigor of research or the excitement or quality of research. And when I think about people who have been very successful at maintaining that balance, Paul Milgrom is a great example, as well as Al Roth, Jon Levin, and Rob Porter. They are using both theory and empirical work and applying them in practice.

GENERAL THOUGHTS ON RESEARCH

Bowmaker: There is an increasing emphasis at many economics departments on applied research. Is this true at Harvard?

Athey: I think Harvard has a healthy mix, although we might do better to have one or two more pure theorists. I like working at all parts of the spectrum.

Bowmaker: What do you see as the value of pure versus applied research in economics?

Athey: Some kinds of pure research are part of a food chain that eventually feeds more applied research. Roger Myerson's classic mechanism design paper is very abstract and difficult to read, but it's influencing practice decades later.[1] And some of the ideas in the other papers from that area are still being read by engineers at Google or Microsoft or Yahoo! as critical reading before they go into business meetings. Of course, very few theory papers have that kind of an impact, but in some sense, all the pay-offs are in the tail, and we're trying to come up with conceptual frameworks which have a lasting impact and help organize your thinking.

Bengt Holmstrom likes to describe theory as like a laboratory; you want to have a beautiful theoretical model that can be used like a laboratory. And then you change one assumption, and then you change another assumption, and each time you change one assumption, you've isolated an effect and created some insight. I would think of that as pure theory, not applied theory, in the sense that an applied theory model is tailored for a particular application, whereas some of these more basic models are used over and over again, and applied in many different circumstances. But there's also room for theory that isn't part of the food chain and is more of a philosophical exercise.

Bowmaker: How would you describe the dialogue between theory and empirics in economics?

Athey: When I go to find an economic problem, I like to organize my thinking about an applied problem using a conceptual framework, whether that's formalized as an explicit theory model or whether the problem is simple enough that it can be conceptualized verbally. I certainly think that having a clear and crisp conceptual framework of the data-generating process will greatly improve the quality of the work; the empirical methods that will be appropriate, the limitations of those methods, and the generalizability of the empirical results.

One of the things that I love about economics is that all the different approaches have their place and value. And when there's an economic problem, I come to it and ask about the best approach. Is it a randomized field experiment? Is it a structural model that's going to give me counterfactuals? Is it a combination of those two things? Is it a theoretical model because we don't really understand what's happening here? And one of the things that I love about market design is that most of the scholars in that area are not religious about methods, and most of them have developed expertise in applying multiple methods to problems; it's all about the best way to answer the question. Most

[1] Myerson, R.B. (1979), 'Incentive Compatibility and the Bargaining Problem,' *Econometrica*, Vol. 47, No. 1 (January), pp. 61-73.

sub-fields that are motivated by policy, and trying to get the right answer, tend to not think about methodology as a religion, but as a means to an end. And they will learn and develop expertise in the methodologies that are best suited to their applications.

Bowmaker: How would you characterize your own research agenda and how has it changed through time?

Athey: I've gone through phases. I certainly had a period when I was more exclusively focused on theoretical research than I am right now. But, in some sense, the things that first appealed to me about economics when I was an undergraduate are exactly the things that I'm doing today. And so I'm finding real-world problems and using whatever tools in the spectrum of methodology are useful to tackle them. And I'm most excited when I'm doing something that's going to change the world. Right now, I'm working closely with Microsoft to implement changes to their online advertising options based on my research. That's incredibly exciting and motivating. And when I write a paper, I know I'm writing about something important, and I know I understand the real-world issues as well as anybody. I have a lot of conviction that what I'm doing is, in fact, useful in changing the world, as well as intellectually interesting research.

Bowmaker: Do you think it is important to have broad research interests?

Athey: I think it's important to develop expertise. And so the question is what you develop expertise in. Early on, it's important to develop expertise in a field so that you can understand what it means to have a depth of knowledge, and to master a set of techniques and methods so that you aren't just applying what somebody else did, but understanding the limitations of what you're doing. Specialization is crucial to have any depth in your work and to make a lasting impact.

My research is probably too broad. And I've compensated for that by working harder *[laughs]*. I've worked very, very hard over time to develop multiple methodological expertises so that when I come to a problem, I can use whatever the appropriate tool is without sacrificing quality and depth. That took many years; to develop theoretical skills that were deep; to develop empirical skills that were deep; to develop structural modeling skills that were deep; and recently doing some field experiments. Each of those endeavors requires writing research papers, advising students, refereeing papers, and teaching; the whole spectrum of activities that are required to become a thoughtful and critical scholar of a certain area.

But if I'm going into an area where I still feel like I'm not deep enough, I will certainly bring on collaborators. So, I've collaborated with econometricians on papers that required deeper econometrics than I had. I could contribute fully on the conceptual parts, but my co-authors brought the knowledge of asymptotic theory, for example. I want the papers to be high quality. If I have an econometric theorem in my paper, I want it to be meeting the standards of the highest quality econometrics. And therefore I wouldn't really attempt to do that without a co-author who had that depth.

Bowmaker: Do you think there is any difference in the type of work done by researchers at different stages of their career based on tenure concerns, publication requirements or other pressures? Should there be a difference?

Athey: I did empirical work while I was junior faculty and it was certainly a risk; it could have slowed me down towards tenure because it took so much time to develop the expertise that I didn't have coming out of graduate school. There was a lot of time spent learning every aspect of the process, and the clock was ticking. When you come up for tenure, you are only judged by the very best papers. And so if I had spent all that time on the empirical papers, but I hadn't gotten them into the very top journals, I don't think they would have helped my tenure case.

One thing about specialization that's very important to keep in mind for younger scholars is that unless you become very good at something, it doesn't really contribute to your tenure case. A side project can be quite fun and interesting, but if you're working in, say, industrial organization and you write a paper in macro, then the IO people may not even comment on that macro paper in their evaluation letters, and it may not count at all for your tenure case. For something to matter for tenure, it has to be good enough so that it's noticed by people who will write your letters. And it has to be a big enough part of your portfolio so that, in fact, letters are requested by experts in that area. You can easily end up in a situation where the IO people say, "She's a theorist who's interesting to us," and the theorists say, "She's an IO person who's interesting to us," but nobody says, "You're the best at what I do"; and you need that for tenure.

One thing that I advise people to do when they have broader interests, or they don't fit neatly into a box, is to look for a set of similar senior people at peer institutions. For instance, Rob Porter is an example of someone like me; he's done theoretical work on collusion and empirical work on auctions. I've done those things, and he will like my portfolio of work and be able to evaluate it. If there are enough similar senior people at peer institutions, then your portfolio is probably fine, even if it's eclectic.

I would say that in the market design area, there's a large group of people with that set of skills; for example, me, Jon Levin, Phil Haile and Ken Hendricks. And as more people get tenure who have a similar portfolio, it becomes easier for junior people to follow this path that has a breadth to it and that doesn't fit into traditional fields. But without that, you can be lost *[laughs]*.

IDEA GENERATION

Bowmaker: Where do you get your research ideas?

Athey: They've come in two ways. The first one has been real-world problems. My very first research was looking at timber auctions as an undergraduate. Then in graduate school, my first theory paper was about mentoring and diversity, and that came out of my experience noticing that some of the male students were getting better research assistant opportunities because they were participating in men-only athletic events with faculty. That observation turned into an *AER* paper.[2]

My next set of papers were about organization theory. I was observing organizations in action and I was interested in incentives, how organization worked and so tried to model those.[3] And then my

[2] Athey, S., Avery, C. and P. Zemsky (2000), 'Mentoring and Diversity,' *American Economic Review*, Vol. 90, No. 4 (September), pp. 765-786.

[3] See, for example, Athey, S. and J. Roberts (2001), 'Organizational Design: Decision Rights and Incentive Contracts,' *American Economic Review*, Vol. 91, No. 2, Papers and Proceedings of the Hundred Thirteenth Annual Meeting of the American Economic Association, pp. 200-205; Athey, S. and S. Stern (2002), 'The impact of technology on emergency health care outcomes,' *RAND Journal of Economics*, Vol. 33, No. 3 (Autumn), pp. 399-432.

theory work came from working on applied theory and being somewhat dissatisfied with using special models and functional form assumptions. There was part of me that wanted to see the bigger picture, the abstract ideas that were behind the special results I was proving in applied theory papers. And so a lot of my pure theory came from noticing patterns and common problems that were coming up in applied theory that could benefit from a clearer conceptual insight.

And my econometric theory was very similar. I'd be working on an empirical paper, and I'd say, "How can I think about the conditions under which this empirical approach would work?" And I would read papers with informal descriptions of the reasoning, and I would be dissatisfied. And so I would say, "Let me write this down, and if I do it formally, maybe then I'll understand." As I started writing things down, I would realize that there was a deeper, more general idea. And I felt that other people would benefit from having that clarity of conceptual insights in their own empirical work, and so I wrote it into econometric theory papers.[4] Almost all of my very theoretical papers have been motivated by trying to solve an applied theory problem, and realizing that I would have more clarity about the specific problem if I understood the generality.

Bowmaker: Do your research ideas ever come from your consultancy work?

Athey: Oh, sure. Right now, I'm working on market design, where the goal is to design markets to make them work more efficiently. Some of the big successes of this field have been in public auctions. Why is that? Because they're often big, they're often important, they're actively designed, and the government asks for expert help. And so when you're asked for expert help, you tend to figure out what we know and what we don't know. And when you understand what we don't know, then you develop new theory and test it in order to see how it's going to work.

The most recent new wave of auction design has been coming from large firms in the private sector like Google, Microsoft, and Yahoo! Fortunately for us, they have been open and interested in getting the advice of academic economists to design their markets. It's a little bit more awkward because it's a private firm doing these things rather than a government, but the public policy implications are fairly similar. The size of the online search advertising market is somewhere around 30 billion dollars, and the advertiser surplus and the consumer surplus that's mediated through that are on the same order of magnitude. You're talking about an overall economic value going through the platforms in the range of 100 billion dollars.

It's important because it's big, but also because it affects public policy, as people find their information through search engines. And the way that the auction is designed affects what information people get, and it affects whether certain businesses even have access to consumers, because the businesses have to succeed in the auctions in order to be visible to them. Of course, the firms have their own private interests, but their decisions have large efficiency implications.

[4] See, for example, Athey, S. and P.A. Haile (2001), 'Identification of Standard Auction Models,' *Econometrica*, Vol. 70, No. 1 (November), pp. 2107-2140; Athey, S. and G.W. Imbens (2006), 'Identification and Inference in Nonlinear Difference-in-Differences Models,' *Econometrica*, Vol. 74, No. 2 (March), pp. 431-497; Athey, S. and G.W. Imbens (2007), 'Discrete Choice Models with Multiple Unobserved Choice Characteristics,' *International Economic Review*, Vol. 48, No. 4 (November), pp. 1159-1192.

Right now, most of my ideas are coming out of that general area. I've been writing some empirical papers and applied theory papers, and it's been fascinating.[5] It's impossible to run out of ideas, because every day there's a new problem that comes up that needs to be solved.

Bowmaker: At what point does an idea become a project that you devote resources to?

Athey: I tend to hold a pretty high bar on a project. Either I need to see the path to it being a strong submission to a top-five journal, or if it's a smaller idea, then I need to see a path to a specific outlet that I could publish in, like the *Papers and Proceedings*, which has been a nice outlet for shorter applied theory papers, or a conference might be a good outlet for shorter, more succinct points that I want to make.

If I don't see either of those outlets, then I'll typically write something up and leave it on my website, but I won't devote resources to taking it to publication. And that's partly because I have plenty of papers on my vita, but also because it's an enormous amount of work to put a paper through the publication process. It's not any easier to put a paper into a second– or third-tier journal than into a top journal; you still have to do the revisions, write the responses to the referees, and read the galleys.

I'm happy to explore lots of projects, but before I really jump into them, I keep refining and generalizing the idea until I see the path that's going to have a fairly major impact.

IDEA EXECUTION

Bowmaker: What makes a good theoretical paper?

Athey: I think there are a number of categories of impact that a theoretical paper can have. One is that it's a method or a tool that will be applied by other people. If you're part of a literature, you'll often be aware of the need for a method or a tool; there's a result that's open, like an existence theorem or a comparative statics theorem. And so you can see clearly that if you solve this problem, people will use it.

A second category of paper is about providing insight—a new idea—and that's very hard. A good, experienced economist often is pretty familiar with a lot of ideas. The one question I ask about a theoretical paper is: Would you need to see the model after reading the abstract? For many applied theory papers, a good, experienced applied theory economist doesn't need to read the model. They could write the model in about a day once they read the abstract.

Papers like that can be still excellent and have a big impact, but the idea should be pretty important in terms of changing the world. If it's not, and you didn't really need the model, then you kind of wonder what you're doing *[laughs]*. And so, in some areas like, say, organization theory, incentive theory, or information theory, the basic trade-offs are well understood, and it's very hard to have a

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[5] See, for example, Agarwal, N., Athey, S. and D. Yang (2009), 'Skewed Bidding in Pay-Per-Action Auctions for Online Advertising,' *American Economic Review* (Papers and Proceedings), Vol. 99, No. 2 (May), pp. 441-447; Athey, S. and J.S. Gans (2010), 'The Impact of Targeting Technology on Advertising markets and Media Competition,' *American Economic Review* (Papers and Proceedings), Vol. 100, No. 2 (May), pp. 608-613; Athey, S. and G. Ellison (forthcoming), 'Position Auctions with Consumer Search,' *Quarterly Journal of Economics*.

brand new idea. That's one reason why some of those areas of applied theory have slowed down and haven't been making the general interest journals as often.

I think there are also theoretical papers that are just very elegant. Those papers may not have much of a lasting impact, but the theory community will appreciate a paper on a fairly narrow point if it has a very subtle and elegant proof of a major theorem.

A final thing would be something that pushes the literature forward. For example, we understood a certain class of problems, and now we understand a more general class of problems. Or, there's a class of models that's very important from an applied perspective, and so here's two or three more important facts and theorems about that class of models. Those papers may not be transformative in the sense that they didn't invent a model, but they can be very important.

And I guess I didn't say a class of papers that everybody wants to write: inventing a new model and coming up with a new laboratory. Everybody wants to come up with an elegant, flexible model that can be applied in lots of different situations. That's a beautiful laboratory and incredibly valuable. But only a few people have succeeded in building these workhorse models. Bengt Holmstrom's moral hazard models are one example.

Bowmaker: What makes a good empirical paper?

Athey: Again, I think there are various categories of good empirical papers. The first question is whether the methodology is right for the question, and the second question is whether the data can identify the question that you're asking. That same second question can be posed of a fairly simple, empirical model like a regression, and it can also be posed of a structural model, where you're trying to estimate the parameters of a theoretical model. In either case, the answer might be "yes" or "no" to whether the data can estimate accurately the parameters that you're interested in, or whether it is the assumptions that you've put into the model that are actually driving your parameter estimates.

And so a good empirical paper chooses exactly the right method for the question and an empirical setting where the data is rich enough and has enough variation to answer the question. And, of course, you would like it to answer a specific question that is important, or one that's going to push forward our general knowledge in economics.

Bowmaker: When you hit a "brickwall" on a project, do you continue to work on the problem or do you take a break and work on something else?

Athey: A lot of times when you get to something hard, you need to find a couple of days to sit down and focus on it and don't do anything else. It's not productive to just take an hour here and there and keep circling around something that you've gotten stuck on before.

Bowmaker: Do you scrap many projects?

Yes, I have tons of abandoned projects.

Bowmaker: What would you say has been the biggest change, in the course of your career, in how researchers in your fields conduct research?

Athey: In the early '90s when I was in graduate school, I think pure theory and applied theory were more unified. They were coming up off the success of the '80s when Paul Milgrom's work on auctions was really central to theory, and when game theory and industrial organization were developing hand in hand. For a while, applied theory was making methodological contributions in the process of doing the work, but then after a lot of those basic ideas were worked out, some people wanted to go on and do more foundational work, and other people were really interested in the problems. And so I would say the field of theory has gotten a little more polarized or divided than it was in the early '90s.

I feel like there have been enormous advances in the quality of empirical work in all fields, but especially in industrial organization, over the last twenty years or so. A long time ago, people were very divided. There were those who did reduced-form empirical work and there were others who did structural empirical work. There was a lot of antipathy between the groups, and debates about what was the right way to go. But students are now better educated in all of the different kinds of empirical work. They have an appreciation for things like identification of models, being very clear about which assumptions are important for your conclusions, and which assumptions are simplifying assumptions. There's been a huge improvement in the clarity of thought, in tolerance, and in education across different approaches.

THE WRITING PROCESS

Bowmaker: Which aspect of the writing process do you find most difficult?

Athey: Polishing—once you know what you want to say and then making sure every single detail is right. It's just so tedious, especially if you've developed something over a long period of time and you've changed notation. I like my papers to be very, very clean and elegantly written, but it just takes so much time to achieve that.

Bowmaker: What steps have you taken in the course of your career to improve the quality of your writing?

Athey: Economic theory in the area of mechanism design and auctions is pretty sloppy with notation. And early on, my own writing also inherited that sloppiness. But about two or three years into my career as junior faculty, I just had an epiphany that this was confusing and created needless ambiguity. And so I decided almost overnight that I wanted every paper to use better and correct notation. And once I noticed it and observed it, I got very frustrated when other people didn't do it as well. I think I really just came to appreciate the value of careful notation. For example, when you write an expectation, what's the random variable? That's a simple thing, but it can cause a reader to get completely confused. And one thing that I also learned to do was to get two or three technical graduate students to proofread all of my papers.

Bowmaker: How do you divide up the writing tasks among co-authors?

Athey: That depends on the co-authors. I tend to want to have a substantial final edit, though. I'm probably more particular about writing clarity and grammar; I want every term to be formally defined, and I want to use the terms consistently.

COLLABORATION

Bowmaker: When you do collaborative work, how do you decide with whom to work?

Athey: That's varied through my career, but, as I mentioned earlier, I want to have my work at the highest possible quality. And so if I'm going outside of my area of core knowledge, I want to collaborate with a really outstanding person who has the right set of skills. Even if I thought I could spend the time to become the expert on a new area, it's more fun to be taught by someone else. And that's both on technical, as well as substantive, areas.

As I've gotten more senior, I do like to work with younger people who are more motivated to get papers out quickly, who are out at every conference publicizing the work, who are up to speed on the "very latest," and who are buddies with the other young people who are doing the most work. I'd like to continue that trend of working with younger co-authors. I have young children, and so it's harder for me to travel.

Bowmaker: How do you prefer to interact with your co-authors (face-to-face, phone, e-mail)?

Athey: It's a lot of fun to work with people face-to-face, and especially for a new co-author early in a project. But once a project gets going, being face-to-face isn't important.

Bowmaker: What are the main challenges associated with collaborative work and how do you overcome them?

Athey: Just keeping the projects moving forward. With multiple busy people, it is easy to let things go for months, especially once the fun part of conceiving the paper is done.

RESEARCH ASSISTANCE AND FUNDING

Bowmaker: How do you use graduate and undergraduate research assistants?

Athey: I use graduate research assistants for a couple of different things, apart from the proofreading that I mentioned earlier. If I'm doing a theory project where I haven't got all the results that I want, I find it very useful to have people do simulations or examples. I will have a conjecture, and to see if it's true, I'll ask them to find some parameter values, or to put in a functional form to see if they can find a counter-example. I think that's fun for students as well. It gives them a chance to engage in a theoretical project and really think about the problem.

On empirical projects, I use graduate students very differently. They're invaluable; they can do everything from cleaning data to running regressions. When I'm doing work myself, I'm busy and going quickly, and not always documenting and keeping careful track. And so I'll often prototype some kind of empirical analysis, and then have a student assistant write a full program that starts from the beginning and loads in the data and every step is documented.

I do like to play with data myself and make sure I understand what I'm doing, and I get my ideas that way. But the graduate students really keep it organized, make sure there are no mistakes, and that it's all replicable. Because they have a lot more time, they'll often discover things that I wouldn't have discovered. And so I might give them very open-ended exploratory tasks, and they then discover anomalies in the data or other things. Undergraduates can be used that way too, but they need a lot more structure.

One thing I have discovered recently is that some faculty have hired full-time research assistants, and Microsoft Research has done this for me. And I have learned that, while there's a lot of work to hire them and a fair bit of work to administer them, a full-time research assistant is much more than twice as productive as two half-time research assistants. They're just much more motivated and focused. And so, if it's possible, that's an incredibly useful way to go. But it's a huge risk because, of course, if the person doesn't work out, you spend a ton of money on nothing.

Bowmaker: How important is funding for getting your work done?

Athey: For a long time, I would say it was only moderately important, but for empirical projects now it's much more so. Because I have full-time research assistants, I'm doing things I would never have been able to do otherwise; I'm taking on many more empirical projects, and projects that are much more data-intensive.

Bowmaker: Do you have any advice for a young scholar on the funding process?

Athey: I've been on the NSF Panel and know that it's very competitive among young people. People needed to submit projects at exactly the right stage, so they had to have some part of the work really well developed and then some parts that hadn't been done yet. It was difficult.

Looking for good support from your institution is a very important negotiating tactic. Sometimes, young scholars are negotiating more on things like salary, which is less important. Getting an extra $1,000 or $2,000 isn't a big deal because you still can't hire a research assistant for that. But having enough money to actually hire a research assistant is a big difference in your productivity.

SEMINAR PARTICIPATION AND NETWORKING

Bowmaker: What are the benefits to attending a seminar that is closely related to your work versus one that is not closely related?

Athey: One of the biggest challenges for me is too many seminars, because I work between fields. For a long time, I was doing double duty; I was going to IO and theory seminars, and also to both the student lunches and the faculty seminars. At some point, I realized I couldn't do double duty and get all of my other work done. And so, then I started trying to split my time, but that leads to its own problems because, of course, the people in each field feel like you're shirking *[laughs]*.

I do see a lot of young people spending so much time going to seminars. You have to put a limit on it, and figure out which ones are really important. You have your whole career for consumption. You have to spend some time locked in your office finishing your papers.

Bowmaker: How important is professional networking to success in research?

Athey: I think it's very important. Again, because you don't have all the time in the world, you have to be very strategic and selective about it. One piece of advice I give to young people is to, about once a year, write down the list of twenty people who could write tenure letters for you. Figure out what conferences are they going to, and what institutions are they at. That helps set your priorities.

It's not that your whole life is about getting tenure letters, but it's also correlated with a lot of other things. The leaders in your field will be from peer institutions or better institutions than yours. Their choice of conferences to attend probably tells you something about what's important. You want to learn from those people, and from people like them. And so, rather than giving fifteen or twenty random seminars in a year, like I did during my junior faculty year on leave, you want those seminars to help advance your career among your pool of letter writers and referees.

Generally, you will find those letter writers and referees in the same place. If for some reason there's a divergence, you also have to think about who your referees are going to be. And young people sometimes think that the refereeing is random and anonymous, but, in fact, it is quite predictable. As an editor, you look at the references in the paper, and you try to find the experts in the area. It's actually pretty easy to figure out who the fifteen or twenty people would be that might referee a particular paper.

Again, you might think that there's a bunch of referees and tenure letter-writers out there who, when given your paper or packet, will dutifully sit down and spend an entire day reading your paper or two days reading everything in your packet. That's just an illusion. A lot of people will do things very quickly, and they'll base a lot of their efforts on things they already know. And so if you've presented in front of them, if you've talked to them, if you've explained things to them, if you've gotten their questions, and incorporated their feedback in advance, you're going to be much, much more successful in the process. But you have a limited amount of time, and you've got to be in your office writing your papers, too. You have to figure out the highest-impact ways to achieve those objectives.

Bowmaker: To what extent is the absence of departmental colleagues working in one's research area a major disadvantage?

Athey: It's certainly a disadvantage. You can get a much different kind of feedback from colleagues, and much earlier on. It's great fun to be somewhere where there are five or six people in your area. If you can get that, that's golden. But most people can't get that. And so there are two other things that are really important to me. One is a buddy, a younger person. They don't have to be exactly in my field, but someone who I can go to lunch with, talk to, bounce ideas off, or give me quality feedback. You don't need ten buddies, but if you don't have one, you'll be miserable. And the second thing you need is a senior person who is invested in you and wants you to succeed. Now, you may not get a perfect buddy and a great senior person in the same place, and then you have to trade those off. But those are the two things that are probably most important.

COMMUNICATION OF RESEARCH

Bowmaker: How do you achieve the right balance between communicating your research at an early stage versus the "close-to-finished" stage?

Athey: I see young people making two kinds of mistakes: not presenting until something is done, and presenting something that's half-baked. Generally, you have to realize that when you go out and give a seminar, you need to be prepared that people in the audience will have their main impression of you formed by that seminar. If there's a group that you regularly present to, then it's a little bit safer, but the world is full of Bayesians—people will update a lot based on one signal. Something that you present doesn't have to be done, but you better be intelligent about it; you better be very clear on what you have done and what you haven't done. And the part that you have done had better be good. Senior people and good people will be very happy to give you feedback on things that aren't done. They'll respect the research process. But what you don't want to do is have something where the whole idea doesn't make sense, where you haven't thought it through, and where people will feel like you're wasting their time, or they'll just decide that you're stupid.

It's important for young people to practice. They practice for the job market, but probably underestimate the importance of doing it later. You see a lot of people mismanage their time. I find it very difficult to practice *[laughs]*. It's a difficult thing to discipline yourself to do, but, until you become really comfortable and fluent, it's crucial. And you also need to think from the perspective of the audience, particularly in terms of what questions people might have.

PUBLICATION

Bowmaker: How do you decide upon the appropriate journal to send your research to?

Athey: First of all, you want to look at the response time of the journals. There are some journals that are very poorly run, and you just shouldn't submit to them as a young person. But that can vary by editor; a journal can be generally good, but one editor is better than another. And so, if you can find out about the specific editor, that's the most important piece of information. Then I look to see if I'll get an expert editor. Is the editor going to be positively disposed to the paper? Will the editor know how to find good referees? Sometimes, that's a problem. *Econometrica* has often not had an expert in empirical IO, for example. You might have a complicated paper, and the editor is outside the field. If the referee says something that doesn't make sense, you can't count on the editor to be able to fully appreciate that, and discern the good comments from the bad ones. That's a pretty risky strategy, but sometimes you have to do it to go for a top journal.

Beyond that, you look at the associate editors. If you're coming from a very good school, the editor will probably send it to one of the associate editors. And so you can pretty well figure out who one of the referees will be, as associate editors have a lot of influence.

You also want to think about the audience of the journal. Again, you have your list of twenty potential letter-writers or referees. Look at where they're publishing. That's going to tell you something about what journals your target audience respects, and what journals they read.

When you're junior and trying to get tenure, all that matters are the people who evaluate you *[laughs]*. And so, your audience is the set of experts in your area. As you advance in your career, you have a lot more flexibility about who you want to reach. And then there are trade-offs. I've been reaching more of the computer science community lately, so I've been giving a lot of talks at computer science conferences. As a result, I've been going to fewer economic theory conferences. My influence grows in one area and shrinks in the other. Once you're tenured, it's just a matter of personal preference of the group in which you think you'll have the most influence.

Bowmaker: How would you best describe your approach to dealing with a "revise and resubmit" request from a journal? How about an outright rejection?

Athey: You make sure that you thoroughly and carefully address every single referee comment, and prepare a very succinct and easy-to-read response to the editors. On occasion, you can ask for clarification from the editor, especially if you think there's a mistake in the referee report or if you have a real disagreement. Editors have many, many initial submissions, but they don't have many "revise and resubmits" on their desks. Generally, they want to publish a good paper, so I think once you have a "revise and resubmit," they will be more willing to work with you. You shouldn't be a pest, but if there's a real issue, you shouldn't be afraid to communicate with them for clarification. Again, you have to remember that the editor probably handled twenty papers on the day that he wrote your letter and may not have thought about it that hard. And so, rather than just sitting there and being frustrated, you can communicate.

I would not protest an outright rejection, unless you have a real, factual problem. You don't want to develop a reputation for protesting, although some people have protested successfully many times. But I don't advise it as a general principle. Generally, when you do get a rejection, you want to think hard about the referee reports. Sometimes, I have decided that I got the wrong referees; they weren't my audience. And so, I'm going to send it somewhere else where the referees are the right audience. And maybe I need to revise my introduction and my cover letter to make sure that the editor gets the clue of who my audience is.

On the other hand, you don't want to be too thick-headed. Sometimes, referee reports are thoughtless and crazy, but a lot of times, there was something you could have done that would have reduced the chance of getting that report. Again, whether that is signaling better who the referees should be, or that the way you wrote and presented things allowed someone to persist in an incorrect assessment.

Bowmaker: Do you think that the current structure of the publication process facilitates or impedes scientific understanding and knowledge production?

Athey: I find my own research being influenced just by the large fixed cost of publication. That's sad. I don't do projects that I think would be fun because the fixed costs are so large and also the delays are long. It's so hard to get motivated to come back to the paper after you're done with it—you've presented it, everybody's seen the results, everybody knows the results, people are citing the results, and still you're being tortured through this awful revision process that may or may not be that productive.

I'm not a huge fan of our current process. I wish it were faster, and I wish we had more outlets for shorter papers that have faster turnaround. In computer science, they have another model where people publish almost primarily in conference volumes that are very carefully refereed, but often they're getting their papers published within six months. It's kind of frustrating to see the pace at which they're going relative to our pace. On the other hand, the computer science articles are often shorter and much more incremental. I think that our format of longer, more substantive papers is fine. I like the fact that we spend longer on our papers, that they're more complete and often deeper. But I wish once you have spent two years writing it, you would be done with it *[laughs]*.

Bowmaker: What has been your best and worst experience during the publication process?

Athey: One thing I've noted is that different sub-fields have very different standards of refereeing. I've had papers that have sailed through the refereeing process that weren't my best papers, but the referees and editors loved them. And I've had papers that I thought were fabulous that have struggled in the refereeing process, even though they were clearly having an influence outside of that. The often negative correlation between the ease of publication and the quality of the paper is frustrating, even though the publication process eventually has been kind to me and I'm a beneficiary of being at a leading institution, which gives you much better treatment in the process. I think a few editors have worked very hard to create good experiences, and when that happens, I'm forever grateful.

But as junior faculty, I submitted a paper to the *Journal of Political Economy*. I had a year to get one referee report, which was a "revise and resubmit". And then I sent it back, and I waited another year to get a single referee report from a different referee that was a rejection on completely different grounds. That was a really big setback for me. I'll never forget it. It was just sloppiness and laziness on the part of the editor.

REFEREEING AND EDITING

Bowmaker: How do you decide upon whether or not to accept a refereeing job? What would you say are the benefits to refereeing?

Athey: Early on, refereeing helped me write my own papers because it made me understand the mindset of a referee. And so, when I write a paper, I immediately think "Okay, what are the first two paragraphs of the referee report that are going to get this paper published in a top journal?" And if I haven't written that for the referee in the introduction and the conclusion, then I've screwed up *[laughs]*. There's nothing like being a referee to help you appreciate that.

I think refereeing is also very good for forcing you to think more deeply about papers and not just read them superficially. But there can be limits. For a while, I was probably refereeing about 30 papers a year, but when I had my first child, I slowed down on refereeing. I realized it was a lot more fun to go to the zoo on Saturday morning than it was to write a referee report *[laughs]*. It's good to have in mind a budget of how much time you want to spend on refereeing, and how many referee reports you want to do every year, and try not to go over that budget. Also, if it's a journal that I don't usually interact with, I feel that I don't really owe them a lot of service.

Bowmaker: How do you decide upon whether or not to accept an editing position? What would you say are the benefits to editing?

Athey: I thought very hard about whether I wanted to be an editor. And I was an editor most recently for a year, and I found it not to be that compatible with my work style, because it was difficult to meet my very high standards all the time. It was a constant guilt-inducing thing and I felt it was always hanging over my head. I think the people who succeed at it are able to compartmentalize it, are very organized, and they have a certain time every week they do it. That time is blocked, and they get in and they focus on it. And I wasn't able to be that structured. And so, sometimes it would build up, and I would feel terrible about it. And then other times I would be really on top of it, but then it might interfere a lot with my research. I may revisit it later in my career when I would be willing to make more sacrifices.

When I think about editing, there are a couple of ways to influence the profession outside of your own research. One of them is to advise students, to teach, and to try to do a lot of mentoring. Another is to edit. And, of course, you have some people who've done a lot of both, like Rob Porter and Glenn Ellison. But I found that I had somewhat limited time, and so I thought if I was going to focus on one thing, I would rather do more direct mentorship of the students. And at a school like Harvard, I'm getting fabulous students.

TIME MANAGEMENT

Bowmaker: How do you divide up your working day, both in terms of quantity and timing of different kinds of work?

Athey: I think as you get more and more senior, the time management gets to be harder and harder because everybody wants a piece of your time, and you have so many different things that are non-research pulling at you. And so now I rely on co-authors and research assistants to help me manage my research time. I have scheduled meetings with co-authors and with research assistants. I have deadlines. When a research assistant finishes something, I give them another task. That forces the research time to be spent, and it keeps the projects moving. And that's one reason to have young, motivated co-authors who are just making sure that the ball moves forward.

When I was younger, after dinner I did no administrative work and no refereeing. And so then my day ended up shifting; I would do research from 7:00 p.m. to 2:00 a.m. every day. That time was blocked. I would know that I had five or six hours in front of me completely uninterrupted. And that was very, very helpful for me because I would tackle the harder, more difficult tasks that you would otherwise procrastinate on.

One thing that I've done that was extremely helpful, and I highly recommend, is that all of my student meetings are electronically scheduled through a sign-up sheet. I'll send out an e-mail to my students to say I'm having office hours and, during the year, it's at the same time every week. The students sign up (they don't e-mail asking for an appointment) and they come back-to-back. I might meet with ten or eleven students in a row—boom, boom, boom.

If they have written work, I try to read that out of the time, but I'll also spend the blocks with them just sitting there reading the paper and commenting verbally. That saves me time, and it actually gives them better quality feedback on some things like exposition. You can write a comment that says "This is unclear," but if they're sitting with me, I can explain to them why the paragraph isn't working, and I'll give them more color as to what the problems are.

At the end of the day, I go home exhausted. But if I didn't focus and concentrate my students in a specific time interval, I could ruin my entire week.

Bowmaker: How do you balance multiple research projects?

Athey: I always have had too many *[laughs]*. It's nice to have a suite of projects where each of them requires a different kind of work, so that no matter what mood you're in that day, you'll be able to be productive on at least one research project.

Bowmaker: How about the balance between your personal and professional lives?

Athey: That's also very difficult. I sometimes worry that I don't set a good example here, because people do see me working very hard, and I feel like I may have set unrealistic expectations. When I was junior faculty, I was working all the time and at a level that wasn't sustainable long term.

I think that you have to figure out for yourself what the right kind of rest is, whether it's exercising every day or taking all day Saturday off; whatever the production process that works right for you. And all I can say is that you should spend a lot of thought on it *[laughs]*. There's no one right answer, but you should try different ways until you find a routine that works.

If you're sitting in your office, surfing the Web, doing your e-mail and not doing research, then there's something wrong with the way you're organizing your life. You then need to decide how you are going to change it. If I'm surfing the Web two hours every morning, then maybe I should have student meetings first thing in the morning for two hours, or that's when I should schedule my co-author meetings or my teaching. If you have any block of time that's systematically unproductive, find another way to use it, unless you figure out that actually that two hours is what your mind needs to settle down and get to work.

Once you have kids, it's much harder; there are a lot of sacrifices, especially when it comes to travel and evening events and so on. And I've tried to have certain times that are very well-protected for my children. I don't schedule things too early in the morning, so I have a nice, relaxed morning with them and do drop-offs at school. I try to make the mornings not rushed and a pleasant time for them, and for me. In the evenings, sometimes I have conflicts, but again I try to make sure that I am there and completely focused on them, not trying to do multiple things, like doing laundry and picking up the house. It's kid time and that's all there is. My kids are pretty happy with that.

I'm very selective about travel. I skip things that are connecting flights, and when I go to conferences I take "red eyes" to get home, which is hard. If it's a "West coast," I try to do it on Friday, so I can fly out at 6 p.m. and then come back on a red eye on Friday night and be there on Saturday. They don't mind so much if I'm gone for dinner on a Friday or a Sunday night because they see me all weekend.

The other really important decision you can make when you're young is how you manage your money. And that's because you can spend money in ways that could make you much, much more productive. And that means you have to have not already committed that money to a large house or a new car or something else. Take nanny versus daycare. A high-quality nanny can save you enormous amounts of time versus daycare. They can cut the time you're spending on mundane non-child-related tasks, like doing the laundry, emptying the dishwasher and cooking dinner, so that your household time is almost 100 per cent devoted to the kids. They can travel with you so you can bring your kids on longer trips. And that's also how you can achieve a balance where your career and kids don't suffer. But it does cost money *[laughs]*.

REFLECTIONS AND THE FUTURE OF ECONOMICS

Bowmaker: What are the biggest challenges facing your research fields?

Athey: In the field that I'm working in actively right now, market design, the biggest challenge is just legitimizing itself as a field in which people can get jobs in the junior market and people can get tenure. I think we've had some good examples of that, like Parag Pathak and Michael Ostrovsky, who have just gotten tenure at top-three schools.

Micro theory has become fragmented. It has slowed down on major innovations; it's now more ordinary science and more specialized. It would really benefit from new paradigms and new methodologies that might unify a larger group of people and get them all excited about the same kinds of things.

As I said before, empirical industrial organization has made amazing progress, and I think the quality of work is extremely high right now. But the challenge it often faces is that the empirical projects are on specific industries and on more narrow, specific problems. There's accumulative contribution to knowledge from all these papers, but they're individually often not that well-cited. And so the challenge for the field is to figure out how to market our work more broadly to the rest of economics so that the general interest matches the quality of the work.

Industrial organization theory has struggled a lot because it's been out of fashion. Many schools don't really want to hire an industrial organization theorist. The micro theorists think that it's applied theory, not pure theory. Applied theory has been in no-man's land, and it's been very difficult for young people to make careers doing it. And so the challenge is how to latch onto an applied field so that the work has a bigger audience and a longer-term impact. And I think in those cases, since most of the applied fields are pretty empirical, it's partly having the applied theory maybe link a little more closely to the issues that are of interest empirically, either directly or indirectly.

Bowmaker: What are the strengths and weaknesses of your own research?

Athey: I'm proud of the fact that when I've tackled new methodologies, I've really gone deep. Despite the breadth, which is generally a liability and a huge challenge to overcome, I think the papers that I've written have been viewed as high quality. I've avoided being viewed as a dilettante who came in, didn't read the literature, didn't reference the papers, and just wrote something everybody in the literature already knew. That's really important, and it's something I've worked hard at, too.

In some areas, I think it's always hard to write a paper that is hugely influential. A lot of the areas that I work in naturally have somewhat smaller audiences, and so I've recognized that there is a trade-off. I have a large number of high-quality, carefully written papers that all are very influential within their literatures, but there's not one paper that's got a 1,000 cites or something like that. Maybe I'll write a paper like that someday, but that's not my primary goal. There's a certain kind of research that I'm good at, that I like, that I'm attracted to, and that I'm motivated by. I'm going to try to be good at what I think I'm good at, rather than try to make myself into something that I'm not.

I've been slow getting my papers out sometimes, and that's partly because I'm working on a lot of different things, and also because, just like everybody, the miserable process of revisions is not appealing.

Bowmaker: In the end, has the profession helped to bring out and shape your research for the best?

Athey: I've gone against the grain a bit. In some ways, the profession made it hard for me to do exactly what I wanted to do early on because I felt a lot of pressure to fit into certain boxes. I feel like, within certain research areas, there is a big tendency of people to discount work in other areas. Parts of the theory community, in particular, view it as almost selling out or abandoning the principles of theory if you do applied work. That tension has been difficult for me because I do like to do theory; that was my original home base. And similarly, when I've been doing empirical work, I didn't initially feel part of that home community, because I wasn't traditionally a pure empiricist. When you're doing something that's in-between, the profession can make it challenging to blaze your own path.

But, again, a few role models who had also made non-conventional decisions showed me that it was possible to follow my heart and do the research that really excited me. I can work 75 or 80 hours a week productively if I'm inspired. And if I'm supposed to do something one week that I don't want to do, like revise a paper where I'm basically done with the paper and I just have to do what other people want me to do, which, in my mind, makes the paper worse, I can get nothing done *[laughs]*. For me, productivity is a function of following my passion. I can't force myself down one path or another. And so to maintain my productivity, it's been very important to continue to be inspired by new things.

I think the positive side is that the profession has helped to create institutions around new sub-fields. For example, we have a working group on market design at the NBER, and we've had conferences at Stanford on market design that have helped validate that sub-field. Almost all of the researchers share my values. They don't think that it makes you a worse theorist to go out and change the world *[laughs]*. By creating those institutions, I hope that the next generation of scholars doesn't feel the same tensions that I felt, and that we don't have to tell them that they're sacrificing their tenure probabilities if they follow this particular path of mixing theory and empirical work, and being more organized around real-world issues like matching problems or auction problems than being a theorist or empiricist.

Bowmaker: Do you have any professional regrets?

Athey: No big ones. I've been very lucky because I've taken some gambles that have maybe caused short-term grief or could have worked out badly, like moving from theory to empirical work, and investing in econometrics. Those are non-standard choices that I wouldn't advise other people to follow. But luckily they worked out for me, so in the end, I'm happy that I've followed my heart.

You always wish you could be better at time management. I still regret somewhat that I spent so much time in my most productive research times doing non-research things. If I look back at my late-20s and early-30s, I wish I'd written a few less referee reports. I also did a lot of department administration for a while. Some of that was really useful, but most of it wasn't.

A big regret that I have is that for a long time I judged myself component-wise against the best people at everything. I used to say, "This person's a great referee," or "This person's a great advisor," or "This person is doing all this department service." What I wouldn't do is realize that there was not one person excelling at all of those things at the same time *[laughs]*. There are a few people who can, but I don't think that should be one's aspiration. What I try to remind myself is that you have a long career, and you don't have to be the best at everything at every time.

Bowmaker: What are your professional ambitions?

Athey: I think my most important ambition is to maintain my passion. That's one of the harder things to do as you get older; you get distracted, you get pulled in a lot of different directions, and you feel like maybe you've already done your best work. And so maintaining that passion may mean making changes at various points. I needed a change a couple of years ago, and now I'm really passionate about online advertising and Internet economics. That makes me want to work as soon as I get up in the morning, and it has me thinking about it when I go to bed at night. And so my ambition would be that five years from now if I'm bored with that, I can find something else that has me feeling the same kind of passion and energy.

The other thing is that I've spent a lot of time on students for a long while, and I'm probably spending a little bit less time on them now. But I really would like to look back and see that I've helped teach people how to think in various ways, and even though they might have taken it in their own directions, that I still had some influence on them, especially on their ability to be true to what attracted them to economics to start with. I think a lot of people come to economics for the same reasons that I did; they have some motivation or passion for changing the world. Sometimes, you have that beaten out of you. But you can try to keep that motivation or passion for doing rigorous work that, in the end, makes a difference, is real, and matters…and to believe that that's possible.

Bowmaker: How would you describe the state of economics today? Are you optimistic about its future?

Athey: I am very optimistic. Economics is healthy, it's popular, and it's got a lot of real-world challenges. I think that we've been very influential in changing policy in all sorts of different areas. The tools that we use are often the best for a range of policy problems and business problems and real-world problems. And I'd say that when I've gone out into the world in the last couple of years, and I've been interacting with engineers and computer scientists and business people and firms all trying to solve problems, I've just been amazed at what my economics training has brought to the table: the ability to conceptualize empirical work, to design experiments, to put structure on decisions, to help forecast the future of an industry, and to guide antitrust policy and privacy policy. There are so many problems that come to you, but I feel that I'm the best prepared of all the people in the room to address the questions. And my input and insight are valued and appreciated, and I have a lot of demand from people wanting to learn from that economic approach.