

The Art and Practice of Economics Research: Lessons from Leading Minds
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Simon W. Bowmaker, New York University

Interview with Ellen R. McGrattan

Federal Reserve Bank of Minneapolis (January 28, 2011)

Ellen McGrattan was born in New London, Connecticut in 1962 and graduated with a BS in mathematics and economics from Boston College in 1984 before obtaining a PhD in economics from Stanford University in 1989. Between 1989 and 1992, she was an Assistant Professor of Economics at Duke University and then joined the Federal Reserve Bank of Minneapolis, where she has remained ever since, currently serving as Monetary Advisor in the Research Department.

Dr. McGrattan's research focuses on the aggregate effects of monetary and fiscal policy, in particular the impact on GDP, investment, the allocation of hours, and the stock market. Her most-cited articles in chronological order include 'Money as a Medium of Exchange in an Economy with Artificially Intelligent Agents,' *Journal of Economic Dynamics and Control* (1990), co-authored with Ramon Marimon and Thomas Sargent, 'The Macroeconomic Effects of Distortionary Taxation,' *Journal of Monetary Economics* (1994), 'An Equilibrium Model of the Business Cycle with Household Production and Fiscal Policy,' *International Economic Review* (1997), co-authored with Richard Rogerson and Randall Wright, 'Sticky Price Models of the Business Cycle: Can the Contract Multiplier Solve the Persistence Problem?' *Econometrica* (2000), co-authored with V.V. Chari and Patrick Kehoe, and 'Can Sticky Price Models Generate Volatile and Persistent Real Exchange Rates?' *Review of Economic Studies* (2002), co-authored with V.V. Chari and Patrick Kehoe.

Dr. McGrattan is an Adjunct Professor at the University of Minnesota, a Research Economist at the National Bureau of Economic Research, and an Editor of the *Review of Economic Dynamics*.

I interviewed Ellen McGrattan in her office at the Federal Reserve Bank of Minneapolis. It was early afternoon of Friday, January 28, 2011.

BACKGROUND INFORMATION

Bowmaker: What was your attraction to economics?

McGrattan: I have to say that when I went to Boston College as an undergraduate, I had no idea what the field of economics was or what an economist did. I was a math major but didn't think that was going to be my career, and so I started looking around and took a class, *Principles of Economics*. I remember it very clearly. It was taught by a professor named Richard Tresch, whose job was to get the kids excited about economics. He came in one day and said, "Today, we're going to talk about money." Then he grabbed some change from his pocket and threw it at the audience! [Laughs]. Of course, it was a serious class, but he sucked you in. I didn't see the math applications at that point, but you could tell there was something behind those curves being shifted around.

I pursued economics on the math-oriented track, and then when I reached my junior year, I thought, "This is cool, but I still have no idea what it means to be an economist." And so I asked one of my professors, Frank Gollop, if I could get a summer job as an RA. It turned out that somebody along the hall was a buddy of Bill Nordhaus from Yale. Bill kindly hired me to be part of his team of undergraduate RAs and it was the best summer ever. The most exciting part of the job was seeing the graduate students, like John Campbell, Andy Caplin, Steve Durlauf, and Michael Haliassos working on their theses, stressing about their topics, and going out on the job market. Another thing that was interesting was a survey of forecasters that I did for Nordhaus. I had to figure out how to get professional forecasters like Otto Eckstein and Larry Klein on the phone long enough to answer roughly 20 questions. The question in the survey that I thought was the most interesting was, "What is your modeling philosophy? Are you a monetarist, a Keynesian, or a rational expectationist?" At first, I pronounced Keynes as it looks rather than *Canes*. Nordhaus said, "My God, where have you been educated, or should I say, not educated? The man's name is John Maynard Keynes [stresses correct pronunciation]." I did know that monetarism had something to do with Milton Friedman, but I didn't know anything about rational expectations. It sounded intriguing.

When I went back to BC, I told Frank Gollop that I wanted to learn about rational expectations. He said, “Oh, good news. We just hired a guy from Minnesota. His name is Scott Freeman, a student of Neil Wallace.” Scott allowed me to do the undergraduate honors thesis with him. He went to his shelf and picked up the galley for Tom Sargent’s first major book and said, “See what you can do with this.”¹ I devoured it. And that’s when I realized that I wanted to become an economist.

I applied to a bunch of places for graduate school, including Minnesota, because Scott said I had to go there. But some of the faculty at BC who were MIT grads said, “Minnesota? The people there are *crazy*.” I didn’t know what crazy meant; I thought it might have something to do with Marxism. They should have told me that they were doing all the work on rational expectations. And so I ended up going to a place that nobody had said anything bad about—Stanford. I could have gone to MIT—a subway ride away—but I sat in on classes there and didn’t hear anything about rational expectations. I threw a dart at the places where I couldn’t take the subway. How dumb is that?

Anyway, I ended up at Stanford and had a miserable first year because they weren’t teaching what I thought was the cutting-edge material. I told this to my now-husband, who was a student in aerospace engineering at the time, and he said, “You’re a first-year graduate student. You know nothing. You need to take more courses and figure it out. Give it a year before you abandon ship.” And so I waited, and sure enough, Tom Sargent shows up. I didn’t know it was him; I had only read his book. There was a guy walking down the hall wearing those funky glasses that turn dark when you’re outside. I thought, “God, that guy has the weirdest glasses.” I knew he had to be a professor because of his age and he said “hi” to me. I thought maybe he was lost and was having trouble seeing with those funky glasses [*laughs*]. I didn’t realize it was him until he started teaching, and I was so excited. I showed up to his first class too early—for a few minutes, I was the only one there—and Tom was at the front. I was sitting at the back feeling awkward. Even though I had read every page of his book, I didn’t know him. But then he says to me, “Scott says ‘hi.’” He had gotten a letter from Scott that said, “I told this woman to go to Minnesota and work with you. Stupidly, she didn’t go, but now thankfully you’re at Stanford and she’s there. Please look after her.” That was so nice.

I remember the econometrics class that Tom taught. He said, “In this course, we’re going to use a lot of linear and quadratic methods. I know many people think that’s not high-tech, but it put people on the moon, and that’s good enough for me.” He was great. And then I worked as his RA. He’ll deny this, but the only thing he ever asked me to do was to fix the latest, greatest machine in his office when it locked up. He would come in to tell me, “I’ve broken the machine.” And I’d say, “You mean you took a hammer and smashed it?” He’d reply, “No, but it’s hung again.” And so I would just press the ‘escape’ button and, if that didn’t work, I’d reboot it [*laughs*].

It was wonderful being around Tom. He would bring in people like Randy Wright, Richard Rogerson, Rody Mannelli, and Hugo Hopenhayn, which made it lively intellectually, and they were fun people, too.

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

McGrattan: My career outside of the Fed was pretty short. I was at Duke for only three years, and then I got a call from the Minneapolis Fed. I realized that the number of like-minded people I would be talking to on a day-to-day basis would be much greater than at Duke. The only problem was that I was married to a guy who worked on super high-speed fluid dynamics at North Carolina State, which has a big engineering school. So I asked him, “Graham, could you ever imagine life in Minnesota?” He said, “I went there on the job market and it’s filled with Sputniks. That’s code for professors hired in the 1960s.” I told him to give a talk there to see if it had changed. He came back

¹ Thomas J. Sargent (1979), *Macroeconomic Theory*, New York: Academic Press.

and told me, “Well, a bunch of the Sputniks have died. I could imagine giving it a try.” And so we came to Minnesota, which is where most of my mentoring has taken place. Ed Prescott has been the biggest influence but, at first, I could not understand him. Not long after I had arrived at the Fed, he came into my office, without any introduction, and started talking about planetary movements and how it related to economics. He was looking at the wall as if it had a window and a view of the planets. I told him, “You know what? I have absolutely no idea what you are talking about.” It was like I had turned a switch on. He said, “Oh, you don’t understand that? Let me explain ...” It was if he spoke two languages: English and something I could not understand. I’ll give you another example. One day he asked me, “Why should I think you’re anything other than a second-rate economist?” Now that I know Ed very well, I’ll translate it: “What do you think your major contribution to the science is going to be?” I remember telling Bob Lucas that story and he said, “Wow, I would have been totally intimidated!” But then I told him how I responded to Ed, which was, “Well, I don’t have a good answer right now, but someday, I’m going to dazzle you.” That pleased Ed.

What I learned from him was that I needed to be ready for when he came into my office, because he always wanted to know what I was working on. He’d say, “What are you doing?” followed by the hardest question, “And why?” And so I started to get used to thinking in terms of something I call the ‘want operator’. What are we trying to do here? What is success and what isn’t? What is the big picture? In the first slide of my presentations, I always begin with a question and some motivation for why answering it is important. Sometimes my research goes all over the place, but Ed taught me how to stay focused. I have never met anyone as good as he is at forcing me to focus. I always tell the graduate students, “Look, Ed changed my life by asking me: *Why* are you doing this? Don’t wait until you get into the Fed’s bag lunch seminar for me to ask that question in front of a whole group of people.” And it also got me thinking about policy analysis. Do we have the right tools? Maybe not. But there is always a question in the back of my mind that I am trying to answer. That’s how I plan out all my new projects, and there is no downside; perhaps, I don’t get the answer that I was expecting, but I learn from it. There are many in our profession who want a detailed model of ‘reality’ but have no specific question in mind. Thanks to Ed, I realized early on that that’s the wrong route to take.

GENERAL THOUGHTS ON RESEARCH

Bowmaker: How would you describe the research setting at the Minneapolis Fed? How is it different to working in a traditional economics department at a university?

McGrattan: One nice thing about the Minneapolis Fed, relative to any other research or academic institution that I know, is that we have a lot of time for research. Most of us teach one class at the U [University of Minnesota] and we advise graduate students, but we have the luxury of coming into the office and thinking all day long.

The environment is very exciting. We have fabulous seminars, great visitors passing through, and a large concentration of researchers who are interested in related topics. And they’re brutally honest. If they think you’re doing something stupid, they will tell you. The feedback is incredible because it’s such an interactive environment. At universities, you will often see all the doors closed; they don’t want students wandering in. But at the Minneapolis Fed, everyone’s door is open. In fact, people don’t even say, “Hey, can I bother you for a minute?” They just come into your office and start writing on your board. That’s how we communicate.

I remember being in Dave Backus’ office at NYU. He and Espen Henriksen wanted to tell me about a paper they were working on. I asked them, “Where is the board?” because it’s so natural for

me to see someone write down an equation or sketch a graph on a blackboard. Mind you, if it's Ed, it's a T account. He cannot write equations! As far as I know, all of his intuition comes from T accounts. I might be struggling with the math, and he'll say, "Well, suppose Company A has \$100 ... " I used to find these discussions completely unhelpful, but now I can follow his logic. And there are a lot of other very talented people here who have such a variety of skills and different ways of thinking.

Bowmaker: What is the value of pure versus applied research in economics?

McGrattan: Everything for me is applied. Take Arrow–Debreu as an example. That established a language to solve applied questions. I interviewed [Kenneth] Arrow for our *Region* magazine, and I asked him, "Would you have ever guessed that future macroeconomists would include a reference to your work in their papers?" He told me "no" because macroeconomics when he was younger meant disequilibrium. But we use Arrow–Debreu all the time. And back in the '70s and '80s, Neil Wallace, John Kareken, and Tom Sargent were doing what was viewed at the time as pure research on money and banking. But here we are in 2011 talking about 'too big to fail,' which all stems from their basic theoretical frameworks.

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

McGrattan: It depends on what you mean by empirics. I count myself as a quantitative theorist—I want to use theory to answer questions. And so I need the theorists who write down beautiful and elegant theories. But there are economists who do theory-free work, and claim that they have a theory in mind. Unfortunately, there are assumptions that have to be made to run their regressions. Why not write down the theory and be explicit about the assumptions?

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

McGrattan: Coming here and being asked, "What are you doing and why?" definitely gets you focused and out of the mindset that we have to explain the world, and into thinking that we have to ask and answer questions. Some of those questions are tough, like 'Why did GDP fall so much between '29 and '33?' I've learned over time that we have to keep trying to answer these tough questions. Another thing that I've learned is to be more aware of measurement and how important it is. For example, I'm now more careful about constructing GDP in my models. GDP is an accounting measure and not necessarily the analog of output in our theories. In some cases, the distinction matters.

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

McGrattan: I think it's important to be open-minded but narrow in the sense that you're an expert in a certain area.

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

McGrattan: Yes, I think tenure concerns push young researchers into too many low-risk projects. If I were king of the world, I'd get rid of job protection for everybody. People say to me, "Oh, you're at the Fed, so you can't be fired and effectively have tenure." That's not true. If I'm not doing research, I *can* be fired or moved. I remember when Rao Aiyagari was working at the Minneapolis Fed and was considering a job at Rochester. I said to him, "But Rao, why do you want to leave? This is the perfect job." He told me, "It's so high stress. Every day, everybody asks me, 'What are you doing and why?' I'm going to retire into academia." Unfortunately, Rao died at a relatively young age, but my point is that we should scrub tenure. Patrick Kehoe had tenure at Penn and he was going to give it up to come back to the Minneapolis Fed. Somebody asked him, "Are you crazy?" He said, "No, what's crazy is being in a department with people who think they need a tenure system. I can go and get another tenured job. I want to be at a place where people are kicking butt, no matter what." I agree with Patrick. And so my suggestion would be to offer term-contracts to professors because they incentivize people better.

IDEA GENERATION

Bowmaker: Where do you get your research ideas?

McGrattan: I have a set of standard theories in my head and if I see things that are completely contradictory, I will ask, "What's going on?" And that usually happens when I'm trying to answer policy questions. Gary Stern [ex-President of the Minneapolis Fed] used to ask us questions like, "Why is the current account balance falling so dramatically [in the early 2000s], and should the Fed do anything? Why is unemployment rising so dramatically, and should the Fed do anything?" And there are other sources for questions. For example, between 1970 and 2000, the stock market tripled, and John Campbell and Bob Shiller were saying that it would have to crash eventually. Ed came into my office and said, "Campbell and Shiller say that the stock market is overvalued. I'm worried because I have all my money in stocks." I said, "That's funny because you're the guy who says we get this huge premium from equities. But I'm in the same position too, and obviously the rise in the stock market relative to GDP can't go on forever." We started thinking about the predictions of basic theory, which tells us that the value of the stock market is the value of corporate capital. This capital includes tangibles like factories, office buildings, and machines, but it also includes intangibles like patents, brand names, and firm-specific human capital. The BEA [Bureau of Economic Analysis] doesn't put a value on these intangibles, so we had to do this by using data on corporate profits and tangible capital stocks and an estimate of the return on capital used in the corporate sector. We found that the value of capital, including its use outside the country by US subsidiaries, was about 1.8 times GNP, which was the same as the value of the stock market in the first half of 2000.²

Then we presented the paper in the brown bag lunch here ... and our colleagues practically threw tomatoes at us. "So you think that you can do economics by showing us one point in time?" they told us. We were pretty proud of our little mechanism for coming up with the intangibles, but we realized they were right. And so that got us thinking about why corporate valuations change so much over time, which introduced taxation and regulation, and that led to another paper.³

² McGrattan, E.R. and E.C. Prescott (2000), 'Is the Stock Market Overvalued?' *Federal Reserve Bank of Minneapolis Quarterly Review*, Vol. 24, No. 4 (Fall), pp. 20–40.

³ McGrattan, E.R. and E.C. Prescott (2005), 'Taxes, Regulations, and the Value of US and UK Corporations,' *Review of Economic Studies*, Vol. 72, No. 3 (July), pp. 767–796.

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

McGrattan: When you know that you have a possible answer to a good question.

IDEA EXECUTION

Bowmaker: What makes a good theoretical paper? Can you give an example?

McGrattan: A good theoretical paper provides a module that everybody has at the core of their theories. An example is ‘Asset Prices in an Exchange Economy’ by Bob Lucas.⁴

Bowmaker: What makes a good empirical paper? Can you give an example?

I like the interaction of theory and data. I want people to stick their neck out, and put the predictions of the models and the data in the same table or graph.

A good example is Kydland and Prescott’s ‘Time to Build’ paper.⁵ That paper was the motivation for my PhD thesis. What I loved about it was that they were willing to match up theory and data, and I hadn’t seen that before. I thought, “This is it. We’re going to show all the warts and pimples; figure out on what dimensions we are off.” They had a model of the business cycle in which the labor market predictions were way off because the monetary and technology shocks that they included didn’t affect how people intra-temporally substitute between leisure and consumption. This is needed in order to get action on the hours worked dimension. They completely missed that, but their reported statistics demonstrate that clearly.

When I was a graduate student, I would read papers for a class or listen to presentations, and sometimes the claims made no sense. I wrote a code called ‘the planner,’ in which you could type in utility functions, production functions, and so on. It allowed me to check on their claims.

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

McGrattan: For me, a brick wall is when I’m getting an answer that makes absolutely no sense. For example, Patrick Kehoe, V.V. Chari and I wanted to demonstrate the Dornbusch view that a little bit of stickiness in prices can generate a lot of variation in real exchange rates. So, we wrote some code, put it in the computer ... but got nothing. Then we went to the pad and pencil. When we wrote it out, we realized that conventional wisdom was wrong unless the model had empirically implausible production technologies or demand functions. They had never written it down; they had just assumed it. John Taylor is a brilliant guy, who can write down the correct first-order conditions, but he doesn’t necessarily tie back the terms in there to the things that are fundamental. And so that brick wall turned into a paper that is probably the most cited of all of ours because we took some conventional wisdom and found that it was not right.⁶ The old paper and pencil won the day.

⁴ Lucas, Jr, R.E. (1978), ‘Asset Prices in an Exchange Economy,’ *Econometrica*, Vol. 46, No. 6 (November), pp. 1429–1445.

⁵ Kydland, F.E. and E.C. Prescott (1982), ‘Time to Build and Aggregate Fluctuations,’ *Econometrica*, Vol. 50, No. 6 (November), pp. 1345–1370.

⁶ Chari, V.V., Kehoe, P.J. and E.R. McGrattan (2002), ‘Can Sticky Price Models Generate Volatile and Persistent Real Exchange Rates?’ *Review of Economic Studies*, Vol. 69, No. 3 (July), pp. 533–563.

Bowmaker: When a project isn't going to turn out as hoped, do you scrap it or aim to send the work to a second-tier or field journal?

McGrattan: Sometimes I will use the failures for teaching. I say to my students, "All I'm going to do is just put a bunch of questions in your head without answers. And guess what? Think about those for your thesis. Because if you can answer any of those questions, that's what we would call a home run." They like that because it gives them brick walls that people have been hitting, or ones that they won't admit to hitting. And I also tell them, "Here's what you're going to say at the beginning: We're at a brick wall, but now I've figured it out; I've figured out why our theory and the data are so at odds, and I'm going to tell you why it's important that I get this right."

Failure is good because (a) it gives our graduate students things to think about, and (b) it keeps us very humble.

Bowmaker: What has been the biggest change during your career in how researchers in your fields conduct research?

McGrattan: I think we have become more timid about doing quantitative theory. I wish economics were more like engineering where, if someone finds an important factor, people say, "Wow, that's a big deal. Yes, the model and data don't line up perfectly, but we're one order of magnitude closer." That constitutes progress in my opinion.

THE WRITING PROCESS

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

McGrattan: I find it all difficult. I never learned to write as a kid. I don't know what I would have done without Kathy Rolfe, who was the editor of *The Quarterly Review* for many years. What did she teach me? Every paragraph is a theorem; it has to have a theorem statement, followed by the details, and then the summary that leads you to the next theorem. And you've got to have the paper's one main point up front.

I used to look at other people's writings. I love the way Bob Lucas writes, for example. But I can't get away with writing as he does, in such an authoritative tone.

I also read the work of the Public Affairs Department here, whose job it is to communicate our research to the world. Doug Clement is the editor of their magazine, *Region*. He wrote an article on the research Ed and I were doing on intangible capital.⁷ He put the two of us into a room together so that we could have a conversation, and then he wrote the article based on it. It's so cool. He described why the work was important, how the world looks confusing when we think about stock markets and business cycles without intangible capital, and how the world isn't so confusing once we introduce it. You're just sucked in. I'm always amazed when people write that well. I just think, "Damn, I wish I could do that." [Laughs.]

Bowmaker: How do you divide up the writing tasks when you work with co-authors?

McGrattan: When I'm working with Patrick and Chari, they're like one person who writes. Patrick sits at the computer and says to Chari, "Okay, what do I write next?" Chari will say something, but

⁷ Clement, D. (2005), "The Untouchables," *The Region*, December, pp. 30–33, 52–57.

Patrick will tell him, “They’re not going to get that.” And so Chari dictates it and Patrick paraphrases it so that everyone will get the point. It’s a good combination. And I’m fine with that since I’m not the best writer in the world.

It’s tricky when I’m working with Ed, because one of us has to be the primary writer. And that’s usually me. He doesn’t like to read and that means that I have to make sure that the main finding of the paper is in the first paragraph. But that’s extremely useful because there are all sorts of impatient people out there.

And I’ve had other co-authors. One example would be Ravi Jagannathan. We wrote a paper on the CAPM debate because I wanted to understand it.⁸ I told him, “Pick the top three papers on that issue and I’ll read them and then write the paper. You can be the second writer because you know all the fine, boring details.” And what’s what we did.

So, it’s great if I can find somebody to write beautifully for me—a Patrick-Chari team—but it’s not always possible. You have to go with complementary skills, right? And with Ed, that means those damned T-accounts [*laughs*].

COLLABORATION

Bowmaker: When you work with co-authors, how do you decide whom to work with? Is it all about those complementary skills?

McGrattan: I need co-authors who can deal with my bossiness. And I love it if they can do the things that I hate, like clean-up work; for example, when a referee asks you to relate your work to the other 800 papers in the literature.

I like doing the nerdy work; thinking about the theory, exploring the data, and doing the computation. And I like giving presentations. I spend a lot of time on them. For example, I had to give a plenary talk last summer at the SED [Society of Economic Dynamics] and prepared for two months. It took me the first month just to come up with the title: ‘Intangible Gains to Openness.’ And I did seven mini-bag lunches with my colleagues. For each one, I told them that they had to pound me into shape. And so I revamped it little by little. By the end, it went well. The preparation is a pain, but I like the feeling of getting the ideas out there. And I don’t mind doing a show. In fact, I probably would be a good MBA teacher. It’s just the nagging and whining that I wouldn’t be able to handle.

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

McGrattan: Face-to-face. Ed is at ASU [Arizona State University] half the year. I just flew down there for the week. My husband was upset. He said, “But it’s Christmas time, we’ve got so much to do.” And I told him, “I know, it’s the perfect time to go.” [*Laughs*].

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

McGrattan: For me, collaborative work is fun. But here’s one problem with Ed: sometimes I need time to just scrounge out my math and think about it, and all he wants to do is his T accounts. And so I say to him, “Go to your office and think about it your way and I’ll think about it my way. Then

⁸ Jagannathan, R. and E.R. McGrattan (1995), ‘The CAPM Debate,’ *Federal Reserve Bank of Minneapolis Quarterly Review*, Vol. 19, No. 4 (Fall), pp. 2–17.

we'll get together and figure out if we're on the same page." He'll get halfway down the hall, flip around, and tell me, "Okay, I thought about it." But I'll say, "No, go *all* the way down the hall and stay there for a while." Ed's a learning-by-talking person, and so is Chari. But sometimes I just need time with my pad and pencil.

RESEARCH ASSISTANCE AND FUNDING

Bowmaker: How do you use research assistants?

McGrattan: Mostly for note-checking and tracking down things in the library.

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

McGrattan: It's not. I work in the institution that prints the money [*laughs*].

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?

McGrattan: We invite people from all fields to give talks at the Fed, but most of the seminars are in macro. I get a lot out of the back-and-forth discussions with the audience.

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

McGrattan: You might say that my profession is central banking. Our ex-Head of Research here, Art Rolnick, used to say, "I give you guys a certain amount of money to go to conferences; don't waste it." What he meant was, "Don't bother going to those 'system' conferences with only researchers from the Federal Reserve system; go to the academic conferences with researchers from everywhere." That gave us a bad reputation in the system for being jerks, but he was right. Why not just go to the main conferences? That's where the networking goes on. And because I have to edit a journal, I like to figure out who knows what. Plus, I always like to self-promote: "Don't you want to teach my paper in your class?" [*Laughs*].

COMMUNICATION OF RESEARCH

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

McGrattan: I'm always worried about 'too early.' And so, I definitely don't give any talk without a paper. I'll even write up the slides as a paper just because I want some structure. The best thing is good slides. People should get the main ideas even if the presenter says nothing.

PUBLICATION

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

McGrattan: It's tricky. If you want to increase the chances of publication, it helps to look at the kind of papers that are coming out in a particular journal, and who's on the editorial board. Do the editors know who is working on this topic when they send it out to referees, or are you just going to get some random person? Doing a little bit of research at the beginning can save time.

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

McGrattan: A lot of people complain that referees ruin their papers. They don't. I think they're honest brokers. They're giving you their objective opinion, and that's vital. They make you go over a certain bar, which I need. And so I value the refereeing process. Do you want the blogging system where anybody gets to write nonsense?

What if you get an outright rejection? You kick some dirt and move on. That's life.

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

McGrattan: I would love it if people didn't have to write so many papers. Stop counting the number of *AERs* someone has. Have they influenced the profession? Have we gone in a different direction? That's what we should be aiming for.

Bowmaker: What have been your best and worst experiences during the publication process?

McGrattan: I would say the only bad experience I've ever had is with an editor that felt that they had to have unanimity of the referees. Editors must be able to use their own judgment. I recently had a good experience at the *AER* with our 'Technology Capital and the US Current Account' paper.⁹ The referees basically said, "The central mechanisms at work here are A and B, but you provide no micro evidence. We want to see it." And so Ed and I had to figure it out. We got the right referees, they gave us the right bar, and we were able to jump over it, and ultimately get the paper published. Some people might have grumbled and said they made us do a boatload of work. But if it makes the paper great, I'll do it. Bring it on!

REFEREING AND EDITING

Bowmaker: Do you have any advice for a young scholar on being a referee?

McGrattan: My advice is don't do too much. And only take on things that are super-close to what you're doing. That's useful for building human capital.

⁹ McGrattan, E.R. and E.C. Prescott (2010), 'Technology Capital and the US Current Account,' *American Economic Review*, Vol. 100, No. 4 (September), pp. 1493–1522.

Bowmaker: Do you have any advice for a young scholar on being an editor?

McGrattan: My advice is don't take on any big editor jobs. I'm an editor at *RED* [*Review of Economic Dynamics*], which requires less work. Usually, what I'm doing is getting a paper from a young guy whose main result shows up in Figure 8, Panel D and I'm telling him to get rid of Figures 1 through 7 and the first parts of Figure 8 so that the main result is not buried. They tend to follow my advice because they're just looking for the kind of feedback that I get from my colleagues here at the Fed.

TIME MANAGEMENT

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

McGrattan: Many research departments in the Fed system use a rule: X per cent of your time is for bank work and $1 - X$ is your own research time. But when our new Research Director [Kei-Mu Yi] came here, I told him, "We don't think that way. One hundred per cent of our time is policy-oriented. But a necessary input for good policy analysis is good research."

Bowmaker: How do you balance multiple research projects?

McGrattan: I'm a parallel processor, so usually I have two projects going on at the same time. That helps, because if you get stuck on one of them, you can just work on the other.

Bowmaker: How do you balance your personal and professional lives?

McGrattan: The trick is marrying another academic [*laughs*]. We know how the business works. But my husband is an engineer and has to do more dog-and-pony shows; he gets \$4 million in grants and needs to constantly show that he is not wasting it.

We have two teenage girls. When they were younger, I asked them, "What do you say to your friends when they ask what your mom and dad do?" They said, "That you guys just sit in front of computers all day long, staring and typing." Now that they're older, they say, "It's something about the economy and space and fluids ... some boring stuff." [*Laughs*].

On weekends, I'll sometimes bring the kids to the Fed to do their homework. Many people think that I'm abusing my children by doing that, but they're fine. All I've done is turned them off to being economists [*laughs*].

REFLECTIONS AND THE FUTURE OF ECONOMICS

Bowmaker: What have been the most important findings and contributions in your research fields during your career?

McGrattan: If you took the stock of papers in macro when I was a graduate student and compared it to now, you would say, "Oh, my God, we've advanced so much." The Keynesians were writing down investment or consumption equations using partial equilibrium analysis, but now we try to get the general equilibrium effects, if there are any. Bob Lucas and Tom Sargent's introduction to their

‘After Keynesian Macroeconomics’ article is brutal but gorgeous.¹⁰ They had to beat up the Keynesians, but we can’t get away with that now, because the New Keynesians aren’t that different from the real business cycle theorists.

I could also tell there had been an enormous change when Ed wanted to do a practice for his Nobel seminar in Stockholm. He brought in some younger colleagues and started talking about how we don’t do systems of equations anymore. They were thinking, “What are you talking about? We write out the first-order conditions and then we’ve got a system of equations that we solve.” What he meant was in the past, somebody gives you the ‘I’ (investment) equation, somebody else gives you the ‘C’ (consumption) equation, and then you make a system. I said, “Ed, stop. You need to talk more explicitly about how we now write these models as fully articulated economic systems.” He didn’t realize he had to explain that, which was very interesting to see.

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

McGrattan: Strength: it’s quantitative. Weakness: more micro has to come into play.

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

McGrattan: I’m not sure about the profession. But my colleagues here at the Fed have completely shaped me—they’re the best.

Bowmaker: Do you have any professional regrets?

McGrattan: None. There’s a line out of a Disney movie: “I’m a lucky bug.”

Bowmaker: What are your professional ambitions?

McGrattan: My professional ambition is to always remain in the trenches. I don’t want to be a general because their job is boring. Tom Cooley once said to me, “Ellen, you need to become the President of the Minneapolis Fed.” I told him how much I would hate it, and then Tom Sargent said I could just do it the Barbra Streisand way: hold only a few concerts that are so good that you build up a huge following. But I couldn’t see it. I love being the dork sitting in the office and thinking about questions. That’s so much more fun than being the general.

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

McGrattan: I am very optimistic. The only thing that I’m worried about is that we’re getting a little stale. We need people saying, “You guys have it all wrong,” which is why I want to be in the trenches. With the financial crisis, people have been saying that we’re ruined. I think that’s crazy; we

¹⁰ In light of the economic difficulties facing the US economy in the 1970s, Lucas and Sargent describe the predictions of Keynesian theory to be “wildly incorrect” and argue that, “The task now facing contemporary students of the business cycle is to sort through the wreckage, determining which features of that remarkable intellectual event called the Keynesian Revolution can be salvaged and put to good use and which others must be discarded. Though it is far from clear what the outcome of this process will be, it is already evident that it will necessarily involve the reopening of basic issues in monetary economics which have been viewed since the thirties as ‘closed’ and the reevaluation of every institutional framework within which monetary and fiscal policy is formulated in the advanced countries.” (Robert E. Lucas, Jr and Thomas J. Sargent (1978), ‘After Keynesian Macroeconomics,’ in *After the Phillips Curve: Persistence of High Inflation and High Unemployment*, Boston, MA: Federal Reserve Bank of Boston, pp. 49–50).

knew all along that there's a lot to work on. We must remember to remain humble because there's always going to be the next revolution.