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

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Interview with Colin F. Camerer, California Institute of Technology

(August 16, 2010)

Colin Camerer was born in 1959 in Philadelphia, Pennsylvania and graduated with a BA in quantitative studies from Johns Hopkins University in 1977, before obtaining an MBA in finance and a PhD in decision theory from the University of Chicago in 1979 and 1981 respectively. Professor Camerer has taught at the Kellogg Graduate School of Management at Northwestern University, 1981-1983, the Wharton School at the University of Pennsylvania, 1983-1991, the Graduate School of Business at the University of Chicago, 1991-1994, and California Institute of Technology, where he currently serves as the Robert Kirby Professor of Behavioral Economics.

Professor Camerer's research looks primarily at the interface between cognitive psychology and economics. This work seeks a better understanding of the psychological and neurobiological basis of decision-making in order to determine the validity of models of economic behavior. His most-cited articles include, 'The Effects of Financial Incentives in Experiments: A Review and Capital-Labor Production Framework,' *Journal of Risk and Uncertainty* (1999), co-authored with Robin Hogarth, 'Overconfidence and Excess Entry: An Experimental Approach,' *American Economic Review* (1999), co-authored with Dan Lovallo, 'In Search of Homo Economicus: Behavioral Experiments in 15 Small-Scale Societies,' *American Economic Review* (2001), co-authored with John Heinrich, Robert Boyd, Samuel Bowles, Ernst Fehr, Herbert Gintis, and Richard McElreath, 'Experience-weighted Attraction Learning in Normal Form Games,' *Econometrica* (2001), co-authored with Teck Hua Ho, and 'Neuroeconomics: How Neuroscience Can Inform Economics,' *Journal of Economic Literature* (2005), co-authored with George Loewenstein and Drazen Prelec. His books include, *Behavioral Game Theory: Experiments on Strategic Interaction* (Princeton University Press, 2003) and *Advances in Behavioral Economics* (Princeton University Press, 2004), co-edited with George Loewenstein and Matthew Rabin.

Professor Camerer was elected as a Fellow of the Econometric Society in 1999 and as a Fellow of the American Academy of Arts and Sciences in 2003.

I interviewed Colin Camerer in his office at the Division of the Humanities and Social Sciences at California Institute of Technology. It was the middle of the afternoon on Monday, August 16, 2010.

BACKGROUND INFORMATION

Bowmaker: What was your attraction to behavioral economics?

Camerer: I went to graduate school in the mid-'70's at the University of Chicago, where I was confronted with an orthodox, highly-rational choice view of economics. But it had one of the few business schools whose approach to psychology formed the early foundations of behavioral economics. This brewing alternative argued that people are neither perfectly rational nor completely stupid, but there may be limits to rationality that are systematic and can be modeled formally. And so it seemed evident to me that it was just a matter of time before the rational choice model would be extended and softened, and that new parameters would come in to form a better picture of human nature. I should also add that I am probably not a dogmatic enough person to be a serious traditional economist [*laughs*].

Bowmaker: As a student, which professors were most inspirational or influential?

Camerer: In graduate school, Hillel Einhorn and Robin Hogarth were my thesis advisors, and they were a study in contrast. Hillel was a very candid New Yorker who had studied industrial psychology, and Robin was a Scottish former accountant [*laughs*]. It was amazing they could write any papers together. I would hand one of them a draft of my thesis, and Hilly would take out all the commas and prepositions, and then Robin would put them back in. It was like a ping-pong match! But they did something in their own work, which I really admire and have often tried to do, which is to think very hard about an important topic and try to find something innovative to say about it. They were relentless. They would meet every day for coffee to update each other on what they had thought since they day before.

Another person who is quite the opposite, but from whom I learned a great deal, is Gene Fama, the finance professor. He left an impression for a number of reasons. One was that in college, I had taken an independent study course from his book *Foundations of Finance*, which had just come out. I worked in the college bookstore and remember literally unpacking the book, and the new-book smell of it. (Proust has his madeleine memory, while I, sadly, have a finance book.) It was exciting to feel like you were in the first generation of people reading that book and learning from it. Fama always struck me as somebody who was not as mathematically brilliant and deep, as, say, Fischer Black, Steve Ross, and others in finance, but he had good ideas, was very tenacious and collected a bunch of data himself. He also knew a million empirical facts. It is true that I now fundamentally disagree with him about most topics, but you don't have to embrace somebody's religion to admire how they do their science.

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

Camerer: One summer when I was in graduate school, I worked for JPMorgan Company as a summer intern. It was intellectual interesting working as an analyst intern, but I had to get up early and wear a tie [*laughs*]. And I had a sense that, as a business school professor, you could have the best of both worlds: work hard on the things you're interested in, and once tenured, have a lifetime deal to do whatever you like.

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

Camerer: One person was Ken McCrimmon. My first job out of graduate school was at Northwestern in 1981, and it was very unusual. It was in the business school there and I was teaching business strategy, which I didn't know very much about. We treated it as a combination of industrial organization economics, game theory and managerial psychology. Ken was very supportive, but also tough.

At the time, the MEDS department (Managerial Economics and Decision Sciences) at Northwestern was also an unbelievable ground zero of game theory. Paul Milgrom was there, and so too was Bengt Holmstrom and Roger Myerson. Those guys were having fun, but they were also very principled and deep in their thinking. I remember sitting in on Paul Milgrom's class on industrial organization theory that was studying a paper by George Stigler on information economics. By today's standards, it would be rejected as a first-year graduate student paper because it didn't have any math, was very naïve about information, and (of course) did not mention behavioral economics. The students were very vicious about it, saying, "I can't believe this got published, and the guy won a Nobel Prize." But Paul pointed out that was actually good. If your paper is so influential that 100 papers come after it that make the original one look anemic, that is known as progress [*laughs*].

And so Paul and Bengt and a number of other people had a lot of wisdom about what they were doing and they also got me very intrigued by game theory. It seemed like it was in need of the psychology of limits of rationality, along with some role for emotions. That was inviting.

GENERAL THOUGHTS ON RESEARCH

Bowmaker: How would you describe the research setting at Caltech?

Camerer: Caltech is very special. The mission statement, as I think of it, is to do work that is innovative, technically difficult, and interdisciplinary, with either an immediate or eventual eye to engineering and application. And so when I am thinking about, say, what the brain is doing when someone is bargaining, I don't have a mind-boggling machine to sell, but the idea that there is something to be engineered and a problem to be solved further down the road informs how I think about it.

Caltech is also very tiny. There are about 25 people in Social Science. On that scale, you either have a Noah's Ark model where you try to have two people of each area of social science, but that is very difficult because it is so fragile, or you pick two or three areas that you think you can stand out in and just leave the others aside. And so we don't do macroeconomics at all, except where it intersects with economic history. Mostly, we do micro theory, experimental economics, and neuroeconomics, and we have some presence in anthropology.

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

Camerer: That's an interesting question. I think there has been inertia in accepting fundamental change in theory that has probably held economics back, particularly at the level of graduate education. My favorite example is prospect theory, which is the most widely cited empirical paper in economics published since 1970.[1] This landmark piece of work is still not routinely taught to first-year graduate students, whereas something that Daniel Bernoulli thought about 300 years ago (expected utility theory) is considered very important. Of course, there has been dramatic progress in something like game theory, which now stands alongside standard economic treatments, but even that was very slow to be adopted. The main ideas began in the 1940s and 1950s and when I was at Chicago in 1977 it was hardly taught at all (only one class on "the core" of noncooperative games).

Bowmaker: Some economists argue that experimental economics informs theory rather than applied or empirical problems? What is your view?

Camerer: I don't agree with that at all. There are a number of strands in which experimentation has influenced design and application. For example, in environmental economics, economists have been doing experiments for decades to try to measure contingent valuations for non-traded goods. And more recently, in auction theory, experiments have been used to test the robustness of different types of auctions. It is very much like if you build an airplane wing and it breaks in the wind tunnel,

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[1] Kahneman, D. and A. Tversky (1979), 'Prospect Theory: An Analysis of Decision under Risk,' *Econometrica*, Vol. 47. No. 2 (March), pp. 263-292.

you don't build the airplane. It doesn't guarantee that you will make the best airplane, but it will eliminate a lot of bad ideas cheaply.

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

Camerer: I am interested in getting the right model of human nature into economics, particularly adding concepts from psychology and neuroscience, but also ones from sociology and anthropology. And related to this is the fact that I have always been very interested in innovative techniques. For example, in order to explore psychology and rational choice, it is very helpful to measure what people are looking at on a computer screen. If they are not looking at something, it follows that they can't be obeying a theory that requires them to have that information. And so we did some early experiments starting in the late '80s in game theory using that eye-tracking technique. Later around 2003 we began to look at activity in the brain and measure all kinds of things.

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

Camerer: I think it is important for the profession as a whole to have people who are like bees, pollinating ideas from here and there, but also to have specialists who are good at taking a suggestion that somebody else imported from, say, psychology and then doing the hard work of formalizing it.

Bowmaker: How about the importance of broad tools?

Camerer: In neuroscience, we have had a fairly lively discussion about the natural division of labor in the sciences. One view is there are theorists who just do theory, and there are empiricists who just test theory, and if there is some useful gain of exchange or complementarity, then that will arise from the process of asking questions at seminars and by reading each other's papers. But often it happens best when it is inside one brain. For example, Herb Simon was very influential in a general way in behavioral economics. He thought about an algorithm as being something that computers and brains both do. He was not the first person to think like that, but if it had not been for those ideas coexisting in him, it might have taken twenty more years for the view that the brain is a computing mechanism to get into cognitive psychology. And so if you had tremendous specialization within the economics profession, and no floating people who are bringing ideas back and forth and using both techniques, it could get pretty stale, and there would be a lot of missed opportunities.

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

Camerer: Despite the profession's best efforts, it has worked out okay for me [*laughs*]. I am lucky to have chosen, and been accepted by, institutions that were pretty adventurous, particularly here at Caltech, but also earlier in my career at the Wharton Business School, where I was in group called Decision Sciences, which was a very eclectic mixture of people doing psychology and operations research.

IDEA GENERATION

Bowmaker: Where do you get your research ideas?

Camerer: Most of them are opportunistic in the sense that there is a combination of a technique and an open question in economics where progress needs to be made. Those are the ones that I enjoy the most. For example, the impetus for our eye-tracking experiments was the fact that many ideas in game theory relating to how equilibrium might be reached, or how players might be reasoning, could be thought of as formal restrictions on what you need to look at in order to evaluate strategy. It seemed to me that if you had a theory that says people look at certain things and then make certain choices, then you could not efficiently make progress without *measuring* both what they look at and what they choose.

In terms of neuroeconomics, I met a few neuroscientists who were very interested in simple ideas in economics, like preference theory. Does the brain have neural activity that you could measure and map onto a number scale that would look like utility? To an economist, it almost sounds ridiculous, because utility does not have to be in the brain. But it *might* be in the brain, and *where* in the brain it is being computed, and *how* computation develops in a person's life cycle could be very interesting. That was an example of a recipe where there was a technology and a field that could contribute to understanding the biological underpinnings of economic choice, and that no one else was doing. And Caltech is a place where we can do such things.

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

Camerer: Many ideas die during the walk from the Faculty Club to here. They don't survive the post-lunch conversation in which I have received that glazed-eye look from someone, or the polite response of, "Well, I wish I had more time to work on that one with you."

IDEA EXECUTION

Bowmaker: What makes a good theoretical paper?

Camerer: I don't have a particular taste for highbrow theory because it has not contributed too much, except, in some case where it sharpens the thought process of graduate students and to help raise the bar on the quality of so-called applied theory. And so to me, the best theory paper is not the one that has the most dramatic math in it, but instead generates the most surprising, non-obvious insight from simple assumptions. This is, to me, the style associated with early Chicago (Becker) and the MIT approach. And, after reading it, if I can't think how to do an experiment or test it with field data, then it has failed as a theory paper because I don't know what it is a theory of. That doesn't mean that theorists have to design an experiment or make it super easy, but if you can't somehow see what the empirical insight might be eventually, then it is not that useful as economics.

Bowmaker: Can you give an example?

Camerer: I will give a positive example rather than a negative one. In one project, we used a tool of Roger Myerson's on Poisson Games.[2] Myerson is in a special, rarified league. He seems to have a magician's touch, particularly going back to his work on the revelation principle, which opened up a

[2] Myerson, R.B. (2000), 'Large Poisson Games,' *Journal of Economic Theory*, Vol. 94, No.1 (September), pp. 7-45.

huge vista into bargaining theory. First of all, I will mention how we used Poisson Games, and then return to why we thought it was so beautiful.

We were interested in a lottery held in Sweden in which people choose a number from 1 to 100,000, and the lowest number that is unique wins a prize. Everyone wants to pick a low number, but if you pick, say, one, and a bunch of other people pick one, you don't win. It turns out that 53,000 people played this lottery every day in Sweden, and it seems impossible to compute the Nash equilibrium in pure strategies. But if you assume that the number of players has a Poisson distribution—it isn't the same number of people playing every day—then Myerson's theory of Poisson Games shows you how to compute the equilibrium. I thought it was very clever because in game theory if you go from a fixed number of players to an unknown number, things typically become impossibly complicated (like in auctions, where you can look around the room and know the number of bidders or log on to a website and not know). Here it is the opposite: a fixed number is impossible computationally. But the Poisson distribution is easy, and it is because of an amazing shortcut called environmental equivalence.

The intuition is as follows. After we got the Swedish data, we ran some experiments, where we recruited 50 people and told them the number of people who were going to play each time. The average would be 27, and we said that if you are "live," you got to play. The Poisson distribution has the property that if you are told you are live this time, on the one hand, you think, "Gee, I'm going to play, which means I should think of myself as competing against 26 others But it also means that since I got picked, the total number is probably pretty high this time, so it could be more than 26 others." And it turns out that those two effects exactly cancel out only for Poisson distributions, which makes a shortcut so you can compute the equilibrium for these games. I don't know how Myerson thought this up [*laughs*]. But it is a very beautiful and mathematically elegant theory paper, which is also a tool to help you think about large-scale systems (he developed it to study voting in large populations).

As an aside, I happened to be giving a talk in Chicago a few years ago, and he had just been working on this topic. We were talking about the Poisson distribution, as I had used it to characterize the number of steps of reasoning people may be doing in thinking about games. And I remember being with him in his office, and he was so excited about Poisson distributions. He was drawing them, and he would instantly rattle off about ten facts about Poisson distributions that I had never known about their structure and what happened in certain ways. His enthusiasm and mastery was charming; it was not something dry and technical. He was in love with those Poisson distributions (*laughs*).

Bowmaker: What makes a good empirical paper?

Camerer: The results should be convincing and not obvious. It is almost like, in reading the paper, your mind goes from, "I don't really quite believe this," to "Yeah, of course."

Bowmaker: Can you give an example?

Camerer: A paper that I like a lot looks at the relationship between colonialism, malaria and economic growth, and it is written by Daron Acemoğlu, Simon Johnson and Jim Robinson.[3] The idea in growth economics is that those former colonies of European countries that fully adopted the country's institutions, such as rule of law, seem to flourish. For example, Singapore is doing great, but Burma isn't. And so the authors go back and say, "Why is that some places adopted those institutions but others didn't?" The argument is that it related to disease. In places where the Europeans could not settle, because of tropical diseases, they were more likely to establish extractive institutions, which remained after independence and hampered economic growth. On the other hand, in places where they could settle, they tended to establish development-oriented institutions, which have contributed to persistent economic growth.

It is a very tricky paper because the data quality on disease is poor, and it is almost a preposterously simplified hypothesis. But I found it very interesting because it was daring, and showed you how something that seemingly is quite non-economic, like disease, might lie right at the heart of this very classic new institutional story about rule of law and trust.

Bowmaker: What makes a good experimental paper?

Camerer: First, what makes a boring experimental paper? A paper is boring if it is too obvious what the result will be. For example, it is easy to think of a game where people don't converge rapidly to Nash equilibrium. And so a good experimental paper should have fruitfulness. This will mean that after reading it, you immediately want to go run another experiment, not because the first one has a flaw in it, but because it raises some new questions. Or at the end of the seminar, everyone is talking about the paper and you can literally hear the buzz of excitement.

Bowmaker: Can you give an example?

Camerer: I will indulge and talk about one of mine [*laughs*]. It is a co-authored paper that was mostly done by Mónica Capra and Charles Noussair.[4] A few years ago, we became interested in economic growth, and Charles, a former PhD student at Caltech, had written a couple of papers on extremely simple growth models with a threshold technological externality. The idea is that agents in experiments have capital, like corn, which they can eat or save. If enough agents pile up enough capital, there is a threshold that takes off, like a technological discovery, and then marginal productivity becomes much higher. Formally, it is a very simple coordination game. If the agents could all agree to save and build up enough capital, they would get across this technology threshold and become super rich. And when we work out the math, it turns out there are two equilibria. There is a poverty trap where marginal productivity is pretty low, so nobody bothers to save, and you never go above the threshold, and then there is a good equilibrium in which you go above the threshold and marginal productivity is super high.

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[3] Acemoğlu, D, S. Johnson and J.R. Robinson (2001), 'The Colonial Origins of Comparative Development: An Empirical Investigation,' *American Economic Review*, Vol. 91, No. 5 (December), pp. 1369-1401.
[4] Capra, C.N., Tanaka, T., Camerer, C.F., Feiler, L., Sovero, V. L. Wang, and C.N. Noussair (2009), 'The Impact of Simple Institutions in Experimental Economics with Poverty Traps,' *Economic Journal*, Vol. 119, No. 539 (July), pp. 977-1009.

In our paper, we were interested in whether there was an empirical set of conditions to take us from equilibrium to the other. And so we tried a bunch of different elements, and it was also one of a small number of papers in experimental economics to allow agents to communicate free form. It turns out that if there is no communication and no political leadership—you can't elect somebody to impose a policy—then you don't get out of the poverty trap. On the other hand, if you have a leadership policy, in which somebody collects your capital, holds it, and makes sure everyone communicates with one another, then you always get out of the poverty trap.

Communication language has been under-studied in economics, but experimentally, it is important for helping groups of people in small-scale experiments solve problems. I am proud of that paper because we introduced a fairly broad paradigm to study the interaction of political and economic factors in a poverty trap setting. And it was the opening step in the experimental economic growth agenda. Unfortunately, we had one rejection at AER [*American Economic Review*] on the grounds that we can't learn about growth economics in the lab, which I think is absolutely completely wrong-headed and it is certain to be disproved someday when people get the money and energy to do really complex experiments. We tried to get money for more but couldn't get even modest grant money from NSF. Anyway, after AER the paper went right to the second journal which handled it well, and especially appreciated the novelty.

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?

Camerer: I hate to start things and not finish them, but I also know a lot about the sunk cost fallacy [*laughs*]. Usually, I will put a project aside and think we will revisit it in a couple of weeks and see if it is really worth returning to, knowing that it will definitely not be the case. I have basically broken up with the project [*laughs*]. But it eases the pain of realizing we made a mistake and that we should just abandon it, which is hard but important to do.

I hope this does not sound too smug, but I don't think we hit too many brick walls, partly because much of the work is hedged in the sense that a viable, say, rational choice explanation or an equilibrium may occur, and if it does not, it is probably because something else happens. And as long as we have a statistically conclusive enough result, we can publish it.

Bowmaker: Do you scrap many projects?

Camerer: Almost everything we do in neuroeconomics involves a scale of three to eight coauthors, and in experiments there are at least two or three. And once students are involved, I feel like we have to finish it for their sake. It is very important for them to see the process through from beginning to end. I hate to not finish and not publish, once we get past collecting pilot data, and it has only happened a few times.

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

Camerer: Within behavioral economics, there has been a rise in middlebrow applied theory and also, more recently, in deeper decision theory. If you go back and read, say, Dick Thaler's early papers, you will see they are written in a literary, pre-math style. You can easily imagine the formalism he had in mind, and often there is some notation, but it is just ideas—great ideas! Economists like

David Laibson, Matthew Rabin, and Botond Kőszegi have come along since and formalized the elements of psychology and carefully checked the logic. That has been very good.

A parallel occurrence is the rise in analysis of field data by people like Stefano DellaVigna and others who are thinking about time inconsistency and limits on attention. That is not a dramatic revolution, but it is percolating along, articles are getting published, and there are tons of interesting topics to work on.

Let me add something about neuroeconomics. One thing that is interesting is that behavioral economics received a small push from cognitive psychology, particularly from Daniel Kahneman and Amos Tversky, as well as Paul Slovic and Baruch Fischhoff. But it was a narrow segment of psychology—judgment and decision making—and we did not get much direct help from those studying, say, attention or emotion. And so since then, behavioral economics has had to take off by itself. But neuroeconomics has been much more collaborative. For example, I have written many more papers with people who are neuroscientists than with psychologists. That rapid kind of collaboration is very unusual in economics, and it has been shocking how well it can work. Of course, it does not always work, but the fact that it *ever* works is amazing [*laughs*]. And for that reason, I don't think it is crucial that neuroeconomics gets much of a foothold in economics departments.

THE WRITING PROCESS

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

Camerer: In 2004, we published a paper in the QJE about steps of reasoning.[5] Ed Glaeser, who is a wonderful editor, forced us to hire a technical editor to write the first four or five pages much more engagingly. I was not going to argue with Ed about how to write a good paper, since he is both an outstanding communicator and was the boss at that point. I thought it was like when someone says you need to go to the gym and you realize after you went and got in shape, as painful as it was, that they were right. And so we hired a woman who writes for applied math journals. She spent hours and hours on the first few pages trying to get the tone just right. That was a very helpful experience to see her slaving over a few crucial paragraphs.

When I wrote a book about game theory [*Behavioral Game Theory: Experiments on Strategic Interaction*], the editor also told me to spend half my time on the first chapter. He said that many people will only read that one. Of course, I was disappointed [*laughs*]. I said, "What do you mean?" and he replied, "Look, you should be happy they read one chapter. That's better than zero. And given that a lot of people will stop on that one chapter, try to make it self-contained. Preview your ideas in there and be catchy." I think Ed had the same thing in mind: people decide whether to keep reading after the first few pages and so, as an author, you should work very hard at the beginning of an article or book.

In neuroscience, we spend hours debating the title because titles are often a self-contained abstract. For example, the title might be something like 'Amygdala Damage Erases Loss Aversion.' You may not believe it without reading the paper, but at least you know what the claim of the paper is! My

[5] Camerer, C.F., Ho, T.H, and J. Chong (2004), 'A Cognitive Hierarchy Model of Thinking in Games,' *Quarterly Journal of Economics*, Vol. 119, No. 3 (August), pp. 861-898.

view is that if you don't know how to title it, then you don't really understand your paper. Part of the reason for the title being important is that the papers are short—around 4,000 words. For a journal such as *Science*, you have a brief window of space to write for a hugely varied audience of very smart people who don't know much jargon. And so you wind up negotiating over this compact expression of what you think is in your paper. By the way, this is a missed opportunity in economics. The titles are often terribly vague!

COLLABORATION

Bowmaker: When you work with co-authors, how do you decide whom to work with?

Camerer: First I ask, "Are they going to do the work?" Recently, I have not taken on too many brand new co-authors whom I didn't know. For most of the empirical projects, there will be a junior student who will be the first author typically and who has the most at stake. And so we try to find a combination of the student who can do the work, but also build up some new skills in the process.

When I work with a senior person, it is somebody whom I want to learn from, and I hope they feel the same way. It is like being in a romantic relationship where both of you think, "Wow! I am so lucky to be with this person. Why are they spending time with me?" And so, in my work, I want to be able to say, "I am really going to learn from this famous neuroscientist. It is amazing that he returned my e-mail, and we are doing this together." And it would be nice if they had a similar appreciation for me.

Bowmaker: Is geographical proximity ever an important consideration when choosing a co-author?

Camerer: It depends. I was in Israel traveling this summer, and I had a Skype call about chimpanzee experiments with somebody back at Caltech and a guy in Japan! But when I was at Wharton School in Philadelphia, I had a co-author, Keith Weigelt, who was in New York. We would each take the train back and forth, hang out and have fun, and also get a lot of work done. But when he moved to Wharton and we were in the same school, we hardly got any work done at all! The synchronization had disappeared. We went from saying, "Okay, we're here for a couple of days. Let's revise the paper" to "We should get together next week…" 52 weeks went by pretty quickly! And so a lack of geography proximity can sometimes be an advantage because it means that if someone makes a visit, you have to be focused on getting things done with them.

Bowmaker: When you do work with co-authors from outside your university, how do you prefer to interact with them (e-mail, phone, or face-to-face meetings)?

Camerer: When we are getting started on a project, we will have chalkboard conversations. But there are a lot of substitutes. For example, a computer science colleague and I have just started thinking about something for a possible grant proposal. We spent about half an hour on his board, and then he took a picture of the work and e-mailed it to me!

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

Camerer: There are two problems: someone not doing enough or someone doing too much. The way I handle it is to have an extremely candid up-front discussion, using some of the language of economics, like residual decision rights. I will say, "You'll be the first author and I'll be the last one, but you may be demoted if I judge you to be free-riding." Students are often shocked that we have this discussion. They often say, "Why don't we just see how it goes, and then we'll decide?" But I always tell them if we do have a problem, it will be much harder to deal with then than now. It's like a prenuptial agreement.

Sometimes a dispute will arise. For example, I have had cases where students felt their work should be rewarded by having their name on a paper. I try to err on the side of inclusion, and I tell them not be scared to have a discussion with me about it, but occasionally I have had to say, "I don't think you've contributed as much as you thought. I am not going to put your name on this. I'll try to make it up to you in this other project."

By the way, all of this also means that we have to think very carefully when writing the first draft of a paper. By that stage, you feel like you have made a commitment to keep someone's name on it.

RESEARCH ASSISTANCE AND FUNDING

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

Camerer: I try to delegate as much as possible to them. I treat the undergraduates like graduate students and the graduate students like young assistant professors.

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

Camerer: It is pretty crucial. If I had almost no money, I would just stop and write a book or develop a course that I have been thinking about.

Sometimes we use the funding process as test marketing of an idea. For example, when I was trying to get money to do large-scale political economy experiments on growth, it was evident from the fact that we failed that there probably was not even an audience in the journals either. I sent in one proposal, and then a revision, expecting it to get rejected but just to hear what people would say in print as reasons for rejection.

I should add that there are a number of cognitive and neuroscientific tools that are not too costly at the margin. For example, the eye tracker that we use costs $30,000, but it lasts a long time, and so the marginal cost is effectively zero. Primates do have high marginal costs because they need to be housed and taken care of. The FMRI [Functional Magnetic Resonance Imaging] machine is mostly a big fixed cost because it is always on and you need physicists and staff people to maintain it and help on techniques.

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

Camerer: Be aggressive. If you want to meet Paul Milgrom. if he is giving a talk at a conference, go up afterwards and introduce yourself. He won't bite. Give your papers to people. I remember I once sent a paper to Amos Tversky, a titanic figure in psychology, but he never replied. Later I ran into him at a conference, and he said, "I really liked your paper." When I told him that I did not think he had even got it, he replied, "Oh, no, you should always send people your papers." The worst that can happen is they think you're being a little pushy and so forth, but often they'll look at it and it goes in their memory bank or they hand it to a student who's interested in that topic. I think it is the same with funding. If you feel like you need senior authors to help get some money, e-mail them.

There are some other common amateur mistakes in getting grants. One is to not read the RFP [request for proposals] carefully. At the NSF, for example, they care about "intellectual merit" and "broader impact." You have to use those *exact* words in the proposal. The second is that you typically have to show pilot data or partial progress in order to clarify what you are going to do, and show that you can do it. Unfortunately, it means you need some seed money somehow to do enough work to get the grant to do the rest. The third mistake is that because the grants have deadlines, usually people rush at the last minute and don't proofread. If it is a multi-person you can tell that two different people wrote two sections, for example, and then you think, "If they can't even converge on the proposal itself, are they really going to collaborate well?" No. Now in our group we usually show the senior grad students and postdocs the proposals at all the stages so they at least get a glimpse.

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?

Camerer: I always learn much more from a neuroscience seminar, which is not as closely related as economics. I find it quite painful to go to many economics talks these days. The ninety-minute seminar based on one paper is an outmoded institution that is designed mainly to help improve someone's paper rather than inform the audience about a general research topic. If I am interested in your paper, I can download it from, say, SSRN, and if you want feedback, there is a refereeing process that guarantees you will receive plenty of opinion about your work.

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

Camerer: It is pretty important. But I do think that academia is meritocratic. For example, Ray Battalio was a famous experimental economist who hated to fly. I met him only once over a 30–year period. But it did not hold back his fame, because he wrote many great papers. Perhaps, when someone went through a mental Rolodex of the top economists in topic X, no pictures of him at conferences popped up in that imaginative process and that kept him from getting picked to do honorary things or serve on editorial boards. But nowadays, you can have a presence on the Web and never have to go to a conference.

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?

Camerer: In the past, I would present work too early sometimes, because I like to think with my mouth. But in the neuroscience world, people keep their cards much closer to their chest. It avoids feuds about uncertain paternity of ideas. For example, when we started doing imaging around 2003, I made a list of the ten big ideas that we should work on. If I heard somebody talking about one of those ideas in a seminar, I would move it up the queue or think, "Oh yeah, I meant to get back to that one. This guy has just given me an idea of how to do it." Then the person whose idea influenced me might think I stole it. There is no way to negotiate the proper credit, plus self-serving bias can cause a lot of heartache. That person thinks he deserves credit for having conveyed the idea such that it improved your paper. But you say, "Well, you did, but I had those notes from five years ago where I had a similar idea." And so the two of you walk away feeling that your combined contribution to the project was 180 per cent [*laughs*].

All of this means that we are now a little more demure about talking about our ideas. And we also have to modulate it. When I visit colleagues, I will sometimes tell them that this is something that we are working on, and so don't steal it. I will say it jokingly, but they understand that if I later find out that they are doing something related to our work, I will have the right to be mad at them.

At the same time, sometimes presenting not-finished work is a blessing. A couple of years ago, I was giving a talk on neuroeconomics at NYU. It was an overview of the field, but I also presented some of our brand new work. One of the editors of *Science* was there, and he came up at the end and said, "I really liked that last paper on public goods. Have you submitted it? If not, we might be interested." After I told him that it was half-done, I ran back to see one of the students who was also working on it. "Hurry up," I said, "Gilbert Chin wants this paper in *Science*!" It still took quite a while, because it was difficult computationally, but it was eventually published there.[6] Clearly, that initial feedback was absolutely crucial, and is one positive case where talking prematurely about some exciting work made all the difference. That is also a positive testimony to how journals like Science work so well. Their full-time editors have time to roam around at conferences and they put a big exploration bonus premium on novel ideas, and they are really smart since they have to know a little about a huge range of science. In economics it is virtually the opposite.

Bowmaker: What are the unique challenges to giving a seminar and how do you overcome them?

Camerer: I once gave a talk at the Board of Trustees at Caltech, and the president of the university, David Baltimore gave me some advice. He said the first third should be interesting to everyone, including spouses of board members, the middle third should be aimed at those with more technical know-how, and the final third should just be for those who know the most. And he also told me that sometimes you have to use jargon. "As a biologist," he said, "I'm not going to make up a new word for DNA." And so if you are talking about price elasticity, that is a useful word for people to know, but you have to walk through it so that they understand it. The hardest part of a seminar, particularly for new PhDs, is stepping back and getting ideas across in plain language.

PUBLICATION

Bowmaker: How do you decide upon the appropriate journal to send your research to? Relatedly, whom do you view as the readership of your research?

Camerer: We spend a lot of time thinking about those things from the very beginning. First, is this an economics paper or a neuroscience one? Once we have figured that out, we decide on the title of

[6] Krajbich I.M.,Camerer, C.F., Ledyard, J.O., and A. Rangel (2009), 'Using Neural Measures of Economic Value to Solve the Public Goods Free-Rider Problem,' *Science*, Vol. 326, No. 5952 (October), pp. 596-599.

the paper and the journal to send it to. For example, if it is economics, I know that the *QJE* likes clever papers that make use of interesting data sets. But, of course, you don't want to become too attached to a particular journal because if the project is not working out, we are back to the sunk cost problem.

The readership depends on who we want to influence. It is usually other academics. Someday I will write something aimed at a popular audience but that is much harder work, quite different than writing for journal readers.

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

Camerer: We are respectful of a referee's time in the sense that we have the view that, just as the customer is always right, the referee is always right. And so, with very few exceptions, if we get rejected at one journal, we will revise it a little bit for the next one based on the reports. I think that is a prudent thing to do. First, you may get the same referees again. It really bugs referees to get the same exact paper to review for a different journal, and see that the authors have completely ignored their first review. And second, if three referees are all saying that your paper is too long, the chances are that three more referees will say the same thing.

Also, I don't like to fight with editors. In fact, I hate it, and editors do too. And there are many other journals that you can try. For example, in neuroscience, *Nature* and *Science* are the main ones, but below those there are a bunch of good ones, like the *Journal of Neuroscience*.

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

Camerer: I am dissatisfied with aspects of it. The neuroscience model clearly works much better. If you installed a number of its features in economics, perhaps that would help. One is that the journal editors are professional editors rather than academics. They are very smart people with PhDs, who have a lot of free time because they are not on committees or teaching, go to conferences constantly, so they know a tremendous amount, and don't have conflicts of interest. They reject papers by Nobel laureates all the time. The second thing relates to paper length. Because the neuro papers are short, you have no excuse to not review it in two or three weeks. And it also means that the reviewing process will not end up with a dramatically changed paper in terms of length and scope. And finally there are two types of journals; ones like *Science* and *Journal of Neuroscience* that contain short, substantive contributions, and others like *Trends in Cognitive Science* and *Trends in Neuroscience* that feature review articles. I like the idea of economics trying to separate more journals into this two-style system. There is a new *Annual Reviews in Economics* series which is off to a great start.

Another problem is that the journals seem to be unwilling to experiment with changes that could be easily reversed if they don't work. For example, there is a new series called *Frontiers* that is only online publication and works very efficiently. One thing that they do is say who the referees were if the paper is published. This means that if accepting a paper is later judged to be something of a mistake, you know what referees are responsible. It also disbands reviewing clubs in which a small number of like-minded colleagues or even friends accept one another's papers and reject all others (or some milder form of favoritism, perhaps even unconscious). If there was such a club you could then see that Mr. A accepted Ms. B's paper and vice versa. When I talked to a couple people about this, they all said "It won't work." It works in *Frontiers*! How do we know it work in economics? It might scare off some referees who are sloppy and afraid to praise a paper that might be flawed, but they should not be refereeing anyway—you *want* to scare them off!

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

Camerer: The worst experience was an experimental paper on dynamic models of savings and consumption that we submitted to the *JPE*. The editor wrote back six months later and said, "We economists don't think that macro models can be tested with individual experiments." We economists?! I am a Fellow of the Econometric Society [*laughs*]. It was clear that the person thought I was a psychologist who had somehow wandered into economics by mistake. Also, it was evident from the title that the paper was an experiment about savings, so the editor was entitled to his or her opinion but should have rejected the paper right away rather than dragging it out six months. I think JPE at that time was also going through a period when they were rejecting a lot of experimental papers, but, ironically, they published a monkey experiment paper around that same time! We then sent our paper to the *QJE*, who liked it, so that was a happy ending.[7]

My best experience was also with the *JPE*. The paper was a field experiment in which we went to a racetrack and made a big bet. First, it was $500, followed by $1,000, and then we would cancel it in the last couple of minutes to see if the markets could be moved. It was so much fun to do, and the punch line of the paper was that the markets were pretty resilient. I was aware that it needed a much deeper theoretical structure to help understand the empirics, but the institutional setting was so complex. Every minute, the crowd was finding out how much money had been bet on eight different horses. To write down a model of that process would just be horrendously complicated and is definitely not what I am good at. I was living in fear that the *JPE* would think the paper made an interesting observation, but was incomplete without an accompanying theory. To my surprise, they were merciful! I give great credit to the editor, Lars Hansen, who accepted it with minimal revision.[8]

TIME MANAGEMENT

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

Camerer: It is deadline driven, unfortunately. I am also a workaholic, so I am constantly checking my e-mail and managing the lab group members. If you are working with people who are self-motivated and independent, I want to always feel like I am taking advantage of them, but that they don't think so [*laughs*].

In the neuroscience model, you are obliged to answer rapidly to things. For example, a former student of mine, who is now at Baylor, has just sent a revised paper to me, and I told her I would look at it within 24 hours. If it means that I have to go to bed an hour late, that is too bad.

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7 Brown, A.L., Chua, Z.E., and C.F. Camerer (2009), 'Learning and Visceral Temptation in Dynamic Saving Experiments,' *Quarterly Journal of Economics*, Vol. 124, No.1 (February), pp. 197-231.
8 Camerer, C.F. (1998), 'Can Asset Markets Be Manipulated? A Field Experiment with Racetrack Betting,' *Journal of Political Economy*, Vol. 106, No. 3 (June) pp. 457-482.

Bowmaker: How do you balance multiple research projects?

Camerer: When something comes back that can be revised briskly, that will go to the top of the queue. But these days, I try not to get involved with brand new projects unless I have a very clear vision about them.

Bowmaker: How do you balance your research and non-research activities?

Camerer: We have a very lean administrative structure in Social Science and Humanities: one division chair and someone who supervises internal faculty issues, including teaching and hiring. And the culture is pretty healthy in the sense that almost everybody wants to finish meetings as soon as possible and get back to their office to do research. The main non-research thing we spend a lot of time on is hiring new faculty. The discussions we have I those meetings are really about our culture and vision for ourselves so they are prolonged and useful. Also, we are small so we cannot afford to make a hiring mistake (with tenure). We have a lot of really wise experienced people in that room when we debate hiring. People also generally rise past their own tastes and don't just choose people like themselves, which is really important. That is the number one disease which makes departments decline.

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

Camerer: I have a four-and-a-half-year-old son, so he is the new major developmental psychology project [*laughs*]. I try to spend a lot of time with him. He is interesting, and travels pretty well [*laughs*]. We have been to three or four conferences with him, and then spent an extra couple of days hanging out. That is another nice thing about academic life.

REFLECTIONS AND THE FUTURE OF ECONOMICS

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

Camerer: There are two big challenges facing neuroeconomics. Doug Bernheim has written a paper in which he argues that if we can understand how the brain works, then we should be able to make fresh, interesting predictions about the relationship between traditionally observable variables, such as prices, and behavior.[9] But, in law, they say never ask a witness a question in the courtroom unless you know the answer. And so I will announce my commitment to that challenge once I am almost done with it [*laughs*].

The second challenge relates to welfare. If the choices that people make don't reveal their legitimate true preferences, because of mistakes or temporary insanity or addiction, then what is the right welfare measure? I must admit that I have stayed away from that one, because I think it is just hard. But if you are willing to admit that there are mistakes at all, which private markets or public regulation might be able to recognize and remedy, then it is likely that some link could be found between mistakes and abnormal or limited brain activity.

[9] Bernheim, B.D. (forthcoming), 'Neuroeconomics: A Sober (But Hopeful) Appraisal,' *American Economic Journal: Microeconomics*.

Bowmaker: At what point in the future do you see IBM or a central bank hiring a neuroeconomist for consulting work?

Camerer: I can see some applications happening very rapidly, say, within five or ten years. But I have a feeling that it will not necessarily be firms telling a neuroeconomist to "build a machine that will make us money." Instead, it might well be non-profit organizations that are more adventurous about, for example, trying to measure consumer satisfaction.

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

Camerer: My weakness is definitely theory. I wish I had spent more time at the beginning of my career learning how to master it. My strengths are that I know a little about a lot of things, and I am a good judge of character in terms of potential collaborators. Early on, I made one or two mistakes. I formed friendships with people whom I thought I could also write papers with. But I quickly realized that does not always work, and that you can't afford to spend time on professional relationships that are not helping your learn and publish.

Bowmaker: Do you have any professional regrets?

Camerer: One regret is that I did not participate in behavioral finance, which is something that I was very interested in at graduate school. I remember Bob Shiller telling me that when he first spoke to people about market psychology, they would look at him as if he were talking about ESP [extrasensory perception]. Back then, it was a very tough field, because people were focused on only a small number of questions, and the data were not plentiful, but I should have persisted with it.

I went back to the University of Chicago as a faculty member for two years. I regret missing some of the intensity of economists thinking about very practical questions, as well as being around a lot of people, all of whom are smart and communicative. But I hated the winter, and I did not like MBA teaching [*laughs*]. Also, I had a feeling that the frontier in behavioral economics would be more related to the technical side of things and that something like neuroeconomics would emerge. Caltech is an ideal place to do that and now we have an unbelievably great group.

Bowmaker: What are your professional ambitions?

Camerer: I don't have many unscratched itches about something exciting to do. But I would like to write an intense monograph of *Neuroscience for Economists*. That could be influential.

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

Camerer: I think economics is unique. What we are especially good at is formalism and using the logic of general equilibrium to understand unintended consequences. Statistically, we are the best social science at inferring causality from correlation and making solid inferences about very difficult questions from data. That is not the case in a whole bunch of other social and biological sciences. But the bad news is that economists, until recently, have been slow to accept computing as an insightful tool for analysis and been unwilling to collect our own data on anything we want to measure. Thankfully, that is beginning to change in practice, though not in core curricula. However, it is still true that in all the economics PhD programs I know of, there are no required courses all Ph.D. students must take in how to produce data. That means students must know a lot about theory to get degrees in economics, but they can get degrees without having any clue about the broad range of ways (including surveys, how governments collect data, experiments) in which theories are actually tested.

The profession is also stodgy and slow to innovate, compared to most other sciences, in how it is organized and run professionally. The way the job market for new PhDs works, use of technology and websites, the nature of professional seminars, slow acknowledgement of the rise of the service and information economy, and the ever-slower frustrating editorial process, are all behind the times on some dimensions. Even worse, there is no mechanism or daring to experiment with big changes. Other fields are creating new online journals and trying out all sorts of mechanisms in lieu of traditional peer review. I think that economists are so well-trained at spotting possible design flaws that it paralyzes them with fear when deciding whether to try out candidate design changes in managing their own profession.

I am extremely optimistic about neuroeconomics. Even if economists voted like a union to exclude it from economics departments that would be fine. It will flourish someplace else, in neuroscience groups, professional schools that are more adventurous about new methods than economics departments, or at unusual places like Caltech.