David Card (University of California, Berkeley)

David Card was born in 1956 in Gueph,(Canada) and graduated with a BA in economics from Queen's University, Kingston in 1978 before obtaining a PhD in economics from Princeton University in 1983. He has taught at the Graduate School of Business, University of Chicago, 1982-1983, Princeton University, where he was a Professor of Economics between 1987 and 1997, and the University of California, Berkeley, where he currently serves as the Class of 1950 Professor of Economics.

Professor Card's research interests include welfare reform, immigration, the effects of the Medicaid program in the US, pension incentives and retirement, labor supply, education, minimum wages, strikes and collective bargaining, evaluation of social programs, unemployment, and wage rigidity. His most-cited articles include, 'On the Covariance Structure of Earnings and Hours Changes', *Econometrica* (1989), co-authored with John M. Abowd, 'The Impact of the Mariel Boatlift on the Miami Labor Market', *Industrial and Labor Relations Review* (1990), 'Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States', *Journal of Political Economy* (1992), co-authored with Alan B. Krueger, 'Estimating the Return to Schooling: Progress on Some Persistent Econometric Problems', *Econometrica* (2001), and 'Skill-Biased Technological Change and Rising Wage Inequality: Some Problems and Puzzles', *Journal of Labor Economics* (2002), co-authored with John E. DiNardo. His books include, *Myth and Measurement: The New Economics of the Minimum Wage* (Princeton University Press, 1995), *Handbook of Labor Economics*, co-edited with Orley Ashenfelter (Elsevier, 1999), and *Poverty, the Distribution of Income, and Public Policy*, co-edited with Alan Auerbach and John Quigley (Russell Sage Foundation, 2006).

Professor Card's academic awards include the John Bates Clark Medal (1995) and the IZA Prize in Labor Economics (2006), the leading award for labor economists. Among his honors, Card was elected as a fellow of the Econometric Society (1992) and the American

Academy of Arts and Sciences (1998). He has served previously as co-editor of the *American Economic Review* and *Econometrica*.

I interviewed David Card at his office in the Department of Economics at the University of California, Berkeley. It was the early afternoon of Thursday, August, 19, 2010.

BACKGROUND INFORMATION

What was your attraction to economics?

Originally, I was a science undergraduate. But my girlfriend through college was taking an economics class, and she was having some trouble with the textbook's chapter on elasticities of demand *[laughs]*. I started reading it and thought it was quite informative. I grew up on a farm and there's a puzzle in the agricultural businesses: Why is a good year for farmers really a bad year? It's basically because the elasticity of demand is less than one. And so reading about that was quite enlightening and I went through half the textbook over the next few days. Given that I was also probably not going to be the greatest physicist of all time, I decided to switch to economics.

As a student, which professors were most inspirational or influential and why?

Because I got into economics so late, I had to take the classes that didn't have any prerequisites. And so I ended up taking classes in labor economics and in income distribution from two relatively young guys, Michael Abbott and Charles Beach. They had just finished their PhDs at Princeton, and I got interested in the set of topics that they worked on. Both of them were advised by Orley Ashenfelter, who, ultimately, recruited me to Princeton and became my thesis advisor.

Why did you decide to pursue an academic career?

I like to wake up late in the morning *[laughs]*.

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

Orley Ashenfelter. My first job was at the Graduate School of Business at Chicago and then, luckily for me, Orley had a job offer at MIT, and he wanted to hire some young assistant professors in labor economics at Princeton. He convinced them to hire me back there. We are still friends. In fact, he's visiting here right now. I've been very strongly influenced by him.

I've had a lot of other colleagues and graduate students whom I've worked with and learned a lot from. The first person I worked with closely was a guy at Chicago called John Abowd, who's now at Cornell. When I went to graduate school, I didn't really know that much about statistics. I had a certain level of training, but it wasn't particularly deep. John had more of a background in statistics, and we got working on a project that was fairly successful in the end, and I learned a huge amount from working with him on it.

GENERAL THOUGHTS ON RESEARCH

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

In the '70s, Berkeley was a very theoretical department and its reputation was built up on that. But various things have happened over the course of the last 30 years, and it's now probably more known as an applied place even though we still have a number of very strong theorists, and always trying to get more.

It's different at other places. I was at Princeton for a very long time, and it had always had a very theoretical economics department. It became better known as being applied in the late '80s and early-'90s with the loss of some top theorists, but at this point it's probably theoretical again. Departments do come and go.

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

The value of a well done, pure piece of theoretical research is more long-lasting. If you figure out something like (Paul) Samuelson did in his *Foundations* book, or like Arrow and Debreu did in the '50s, those things influence the way people even *think* about problems for many, many years. Because the economy is a moving target, I think it's hard for much of what we do as empiricists to have the same influence. On the other hand, the ability to write an important, deep, and fundamental contribution in theoretical economics is pretty small. Maybe there'll be some breakthroughs, and people will come up with new ways to think about problems, but right now I think it's easier to do something on the applied side.

How would you describe the dialog between theory and empirics in economics?

There's quite a wide divergence of opinion on how those two should be blended. There are those who think that most research should be more theoretical and that applied work should be relegated to the government or researchers outside university, and others who believe that almost all theoretical work is, at this point, not particularly useful. Those are pretty extreme positions. In my own department, all opinions are valued and we don't have any big fights.

But we are in a period when there isn't a new thing in theory, like in the late '80s when there was incredible excitement about reformulating macroeconomics, along with a resurgence of interest in game theory. When I was at Princeton, Hugo Sonneschein was turning out two or three PhD students a year for almost a decade and they went on to revolutionize game theory. There isn't anything like that right now, and I think that makes it difficult both for students and for researchers. What are we supposed to be doing? Hopefully, something will come along and rejuvenate things.

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

Well, it's possible that I don't have a research agenda *[laughs]*. I work on problems that are interesting at the time and ones that I think I can make some progress in answering. But most problems in economics, especially in labor economics and applied micro, have been around for a very long time, like how to infer something about consumer preferences or the way in which markets work. The difficulty is more in getting either a dataset or a method to try and present a new answer to that question. For example, I've been getting interested again in working on the old problem of unemployment, now that there's a lot of it *[laughs]*.

Looking at unemployment is much better than it was in the '80s because of improvements in the data and so it's possible that some new insights will emerge.

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

I'm not sure there's a single answer to that question. I think each person has to figure out what suits their own interest. One issue for some people, as they get a little bit older, is that the thing that carried them through tenure and promotions, and quite a few good papers, gets mined out and then they have to think about how to adapt it or move on.

Would you say having a broad set of tools is more important?

You can learn new tools. There are a lot of new tools, like dynamic programming, that have been refined, developed, and made more feasible by computing. The basic structure of dynamic programming was understood in the '60s and '70s, but since then people have figured out how to implement estimation schemes when you assume that agents are following dynamic programs with uncertainty and some information processes and so on. I think that's a pretty exciting area of work. But it's not something I learned at graduate school; I've had to pick it up and follow along.

Versions of econometric methodology have changed. When I was a graduate student, maximum likelihood and linear regressions were the state of the art. And often the maximum likelihood methods would be combined with very parametric models of the way in which people behave, but now we are doing research that is much less parametric and much more flexible. That's another example of where you have to pay attention to what's going on in the literature and figure out whether you can use this new method to look at a problem that's been bugging you for 20 years.

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?

I think that the pressure on young people today isn't much worse than when I was an assistant professor. But the standard for tenure now is very strongly emphasized when you are first appointed and then reappointed mid-career. Everyone talks about whether or not you have your five or six papers in the top journals. It is true that there are more and more people all around the world trying to become professional economists than there were in the '70s and '80s, and they're all aiming to get their papers published in the *QJE* and *AER*, so it is extremely competitive. And what this means is that young people today have to be very careful about not undertaking a project that doesn't yield something with relatively high probability in a year or two. On the other hand, it's still the case that most of the innovative new work is coming from junior faculty, so it's hard to say that that's really hurt anything too much. Maybe advisors are helping people pose questions and get started a little more.

In terms of whether there should be a difference, a former colleague of mine embarked on a project in his early 40s that involved collaboration with a bunch of people who weren't economists. A number of my other colleagues said, "That's outside of economics. Why is he doing that?" But I thought, "That's exactly what you're supposed to do when you're tenured." And so there are quite diverse opinions on this issue. But I do think that there isn't only one way to succeed in a career as a researcher. You just need to find your own thing at each point in time; something that you want to do and that you're good at.

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

Sometimes when I get a referee report, I think not *[laughs]*. But I would say I've managed to be successful despite some of my own limitations, and so I certainly don't feel like I can complain relative to lots of other people who have worked hard and maybe not been as successful.

IDEA GENERATION

Where do you get your research ideas?

(*Long pause*). I don't really know. Sometimes there are questions that have been hanging out there in the world for a very long time. And I see myself as having a list of those questions at the back of my head and someone then creates a new dataset or a new theoretical model that makes me rethink them. A good example would be the huge debate about whether health insurance makes people healthier. The difficulty is the research design for tackling that issue: rich or better educated people have more health insurance, and, of course, they're healthier. But a couple of years ago, I started thinking about the Medicare program, which provides very generous, universal health insurance to all Americans once they hit 65 years old. I've been combining my thinking about that system with the regression discontinuity method, which has been very widely adopted in economics in the last decade. As you see more applications of a method, you end up saying, "Oh, I can think of another problem for it." That's what I've done.

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

I always say that most projects are at their peak in the third hour of the discussion with your co-author, because it's only going to be downhill from there *[laughs]*. The modeling won't be as clean, the data won't be as good and the results won't be as decisive. But once I have the project's beginning and end in my head, I can usually sit down and do it.

IDEA EXECUTION

What makes a good theoretical paper?

My feeling is that the better theoretical papers are extremely simple. Although there might be some elegant math someplace, it isn't usually rammed down your throat when you read the paper. And the authors may have stripped away quite a few institutional features and made some crude assumptions, which arguably means that the thing they're working on doesn't ever totally apply to anything, but it will give an insight into a wide range of problems. I do tend to prefer more abstract theoretical modeling.

Can you give an example?

A paper that I've always liked is (Leonid) Hurwicz and (Hirofumi) Uzawa's 'On the integrability of demand functions' [*laughs*].[1] It's very heavy duty, but it's basically saying, "Okay, this is a problem in solving a differential equation. Let's find out what conditions under which that differential equation can be solved, and let's just blast it." That's what

[1] Leonid Hurwicz and Hirofumi Uzawa, 'On the integrability of demand functions', Chapter 6 in *Preferences, Utility and Demand*, 1971, edited by John S. Chipman, Leonid Hurwicz, Marcel K. Richter, and Hugo F. Sonnenschein, New York: Harcourt Brace Jovanovich.

Samuelson set out to do in *Foundations*, but he didn't quite nail it. It's pretty interesting how a relatively straightforwardly posed question could take so long to answer.

What makes a good empirical paper?

There are many kinds of empirical papers. The first kind attempt detective work - they're trying to figure out why something is the way it is. They are useful and I like them. A nice example that I saw looked at the effect of the civil rights law that desegregated hospitals on the drop in infant mortality among African-American children in the '60s. It showed that if you look at black/white infant mortality, it has a constant pattern, then there's a big gain in the '60s and not so much after that. The paper was written by a couple of my former PhD students, and it is the best forensic piece of work I can think of. Unfortunately, it's never been published, because the students got into an argument with each other.

Another kind of hypothesis-testing paper is when somebody conjectures that something is true. For example, one of my best-cited papers is a very simple one on the Mariel boatlift (*see his biography*). I was trying to see the effect of over 100,000 people coming very quickly to Miami early in 1980. I got the idea from an undergraduate student at Princeton, who had grown up in Miami and talked to me about the boatlift. He then moved on and got a job, but I thought it was worth pursuing. And so all I did was just collect up the data and compare Miami to a bunch of other cities pre- and post-boatlift. The paper is not published in a great journal, but it gets a lot of cites because it's very straightforward.

A third kind of paper that I like a lot is when somebody poses a theoretical model of a behavioral channel, like demand or a reaction to a phenomenon, and then somebody else tries to figure out a way to find a setting where that is identifiable from all the noise in the system, and estimates how important that behavior is. A classic example would be attempting to estimate demand functions. The problem is that you very rarely get exogenous changes in prices. Actually, you don't often have any variation in prices across people, which makes it a mess. And so I really appreciate those kinds of papers, and I would say that's what people are trying to do today in IO more than in labor.

You are an economist who has reported one or two controversial findings in his papers. When you find a result that you realize is likely to be controversial, how does this affect how you write the paper?

You have to be aware that, in most people's minds, theory tests the data. That's just a true fact that I learned from John Abowd when I went to Chicago. And so if you present a finding that is contrary to a theoretical result that someone holds hold dear and near to their heart, the best you can expect is that they'll say, "Well, you gave it a good shot, but you got unlucky - your errors were such that you didn't get the right finding." They can't find anything that you did wrong, but they're not going to tell you, "This overturns my way of thinking about the world." In my opinion, no-one's view in economics is overturned by any single paper, or even any collection of papers. And so the best you can do is what I would call 'professional work.' This means no errors and no obvious omissions. In a lot of papers, it's surprising how many times somebody has made an error that could be quite fundamental, or has missed something, or just didn't spend enough time trying to figure out what was really going on. If you have a controversial finding, the first thing everyone's going to assume is that you did one of those things. I'm the same! When somebody presents a result that I think doesn't make a lot of sense, I assume there's a very good chance they screwed it up [*laughs*]. You must realize that a bunch of very smart people, who have very strong incentives, are going to pore over your results and try and figure out what you did wrong. And if they can find something, that'll be a note or a comment in a journal, which will be extremely embarrassing for you, and they'll be the heroes of the day. So I don't want to publish a controversial result and have it be *possibly* the case that somebody could spend less than, say, three years on the project and find an error [*laughs*]. And I make sure that I've reported enough of a range of my results so that no-one can poke around and tell me, "You say the coefficient is 3, but here's a specification showing that it's 0.1." A final thing is you shouldn't be too confident that your controversial result implies anything general. All you can report is, "In this particular circumstance, what we found is…" Don't say that your results are anything other than what they are.

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?

In applied work, a brick wall can arise if you start working on a project with the idea that some proposition from a model you had in your head, but never wrote out, was true, and then you look at it more carefully and realize the empirical design is much less informative about that theory than you thought. For example, for almost 10 years, I was working on a very complicated welfare experiment being conducted in Canada. Women who had been on welfare for a while were being offered a short-term incentive to go back to work. I was not involved with the design of the project, but I was called in once it was in the field. And so I got together with a friend of mine, who was a specialist in dynamic empirical modeling, to take a look at the experiment. I said, "There's something wrong with the way we're thinking about the problem." And then one day I realized that it needed to be put into a search theory framework. Once I posed it that way, the whole thing made a lot of sense and it was a very successful project for us; the paper got published and people liked the interpretation.[2] What was most amazing was that all the time we'd been working on the project, we'd been thinking about it incorrectly. It was an inherently dynamic incentive that was offered, and we were looking at it from a static point of view. That was an example of a breakthrough; of necessity being the mother of invention *[laughs]*.

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[2] David Card and Philip Robins, 'Do Financial Incentives Encourage Welfare Recipients to Work? Evidence from a Randomized Evaluation of the Self-Sufficiency Project', in *Research in Labor Economics,* Vol. 17, 1998, edited by Solomon Polachek, Greenwich Connecticut: JAI Press.

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

I usually send it to a lower tier journal. I've got lots of papers in those journals. Sometimes you start a project and you know that even if it's outstanding and well done, it's going to go to a specialist journal like *The Journal of Labor Economics*. Other times, you start a project with high hopes, but it turns out to be less decisive, and so it has to move down the food chain. That's just the nature of the business.

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

When I started, most projects would have a pretty explicit theoretical front-end, and sometimes the best ones would then map that directly into the empirical approach using, say, maximum likelihood. That was like my thesis. But then some time in the '80s, it became less and less important to have this well worked-out theoretical framework. In some cases, people were focusing on extremely straightforward questions with much more emphasis on how credible and carefully identified were the empirical results. You might call it the research design revolution. But in the last 10 years, there's been a backlash, and for almost all of my PhD students, I really emphasize the importance of having a well-posed theoretical model.

The other thing that has changed is that when I was doing my PhD, Princeton was the very best place for empirical labor economics. It was a producing a steady stream of people, and maybe once in a while somebody would come out of MIT or Harvard. Now there are seven or eight places that are turning out pretty decent empirical labor economists. But the field itself is not very big, and it has also diffused. In fact, most PhD advisors will tell you they haven't trained a true labor economist in 20 years *[laughs]*.

THE WRITING PROCESS

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

Writing the introduction. If I have to, I can write a 25-page paper overnight, but I won't be happy with the introduction. I never like it and I always change it. I don't know why; it's just my problem.

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

I've worked with co-authors who are better or more facile writers, and I've tried to imitate them.

Who proof-reads your writing?

Just me and my co-authors. I don't take it to an editor. A few years ago, I wrote a book with Alan Krueger on minimum wages (*see his biography*). Alan was working in the government when we were finishing it, so I had to do the whole final draft. And I worked with an editor. Editors can fix awkward writing or repetition and suggest more interesting adverbs and adjectives, but what they can't do is fix the entire tone of an article, and that's probably the thing that's hardest to imitate. A really good writer of an empirical labor economics article writes in a 'stripped down' way, where the technical details are suppressed or put aside. A sociologist or a beginning graduate student in economics should be able to read almost all of it.

COLLABORATION

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

Most of the time, I work with people whom I know fairly well. At this point, they tend to be younger people, but I don't work with my graduate students very often. Some advisors do, but I don't think it does the students any good. They need ownership of their own projects.

Is geographical proximity ever an important consideration?

I actually like working with people who are not in the same time zone (*laughs*). If you're in a hurry, you can get a lot done. For example, if you're working with somebody in Europe, you can work a combined 24 hours a day. And I work late at night, so if my co-author is on the East Coast, that can work out pretty well too.

What would you say are the biggest challenges associated with collaborative work, and how do you overcome them?

I've probably written around 50 co-authored papers. Only once have I had to quit working on a project with a co-author because he and I didn't see eye to eye. If you're going to work with me, most people know that I have certain preferences and feel very strongly about certain things. Sloppy mistakes drive me nuts! And I hate crappy computer code. I'll hear people talk about working with a co-author and the results keep changing. If that happened to me, I would shoot the co-author (*laughs*). I can't deal with moving targets. If I think that the person is not somebody who's going to nail it, then I do the empirical work myself. The guy whom I mentioned earlier had very strong priors about what the results would be. Every time we'd do the results, I felt he was molding them into this prior rather than looking at them and saying, "Well, there are some things that are working this way and others that are not. Maybe there's something else going on." He was too rigid; he wanted to estimate this one equation to get this one coefficient and that was going to be the paper. That's not how I write my papers.

RESEARCH ASSISTANCE AND FUNDING

How do you use undergraduate and graduate research assistants?

The problem is that training graduate students to be research assistants in empirical work is really painful and slow. Undergraduates are better. People tend not to understand that most of what we have to do as researchers is just crap work. Yesterday, for example, I spent the whole day in the library going through historical volumes. It's very hard to get graduate students to do something like that, or collect data, code it carefully, and then put it a spreadsheet. They don't pay attention, or put in the hours, unless they're a co-author on the project. But an undergraduate thinks it's fun. In fact, I've just hired a guy whom I ran into at a small college. He's in-between finishing his undergraduate degree and starting graduate school. He'll work for me for a year.

How important is funding for getting your work done?

Hugely important. These days, to do anything empirically, you need to have access to specialized data. Yesterday, a graduate student told me he wanted to do a project on heart attack patients. That data will cost around $9,000.

To the extent that you're doing anything remotely interesting computationally or theoretically, you constantly need to upgrade your computers as well.

You also need the time to work on a particular project and not something else, which usually requires some kind of funding. And so I spend a huge amount of my time trying to write grants.

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

Boy, I wish I were better at it. I've had lots of projects that I thought were really promising but took three revisions to get to funding stage or never even got funded. I served on the NIH panel for many years, and I'm part of a "college of reviewers." I think the reviewing process is like the refereeing process – it's getting crazy. You send something to the NIH and it's a bunch of people sitting around saying, "Oh, I don't like that equation." They never step back and ask whether it is generally good or bad or whether the person has a sound track record or not. They want to control the whole damn thing.

SEMINAR PARTICIPATION AND NETWORKING

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

If you can successfully identify them, it's probably better to go to the ones that are not closely related to your work, because that's where you're going to get the best new ideas. But I often think you can do well by just reading the paper. I like to go through papers in the *AER* or *QJE* by people whom I've heard are doing interesting work. That's useful.

How important is professional networking to success in research?

It's important. The first reason is that you have a better sense of what questions are being asked in a particular area. And the second reason is you can also figure out the current topics and angles in that same area. For example, if you could write a paper right now looking at the effect of asymmetric information on somebody's behavior, it'll be publishable. I don't know why, but that topic's in the air. Of course, once in a while, somebody writes a path-breaking paper that just says, "Okay, forget all that – let's work on this instead." While that is incredibly useful, it is also really hard to do.

How does the researcher without extensive networks succeed?

I think they have to look at the work that is being done by the assistant professors at the top 10 schools. That will give them a sense of what are the current, interesting topics.

COMMUNICATION OF RESEARCH

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

That's an interesting question. I think what people do nowadays is they get the project done, show it to a few friends, present it to a few selected audiences, try and figure out how to revise the paper as strongly as they can, and then submit it. Because of the competition to get into the top journals, there's a premium to novelty, combined with the fact that no one ever reads a paper twice. In the past, I've submitted a paper that's been completely trashed by the referees, thought about it a lot more, and then revised it into a much better one. The problem is by that stage it's already been rejected by the initial referees and everybody says, "Oh, we saw that paper. It's a piece of junk." You've got one shot with a top journal and you've got to try to make it go there. Otherwise, it will sink like a stone (*laughs*).

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

John Abowd once explained to me that every talk is a job talk, and so you should think about every seminar that way. It's yours to lose. I do find it amazing how people don't prepare enough in terms of pacing themselves during a seminar. It's really important that you don't get up there and bullshit your way through the first few slides for 45 minutes and waste the audience's time. I've been in a situation more than once where a department is thinking about making an offer to a senior person, who then gives a talk that kills it. From a Bayesian perspective, a seminar should not provide any new information, but it seems to be inevitable that it does. I think it's partly because we view someone as like a reformed alcoholic, who at any moment could fall off the wagon and become irrelevant. I don't think many 45 or 50 year-olds are aware of how people are very concerned that you are no longer what you used to be (*laughs*).

PUBLICATION

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

Most of my papers get rejected at least one or two times. Usually, it's very annoying, but don't belabor it too much. You'll always hear, "Try it at a different journal," but in my experience, that doesn't help. If it's not going to make it at the *QJE*, then it's not going to make it at the *AER* either. Younger people tend to look at the referee report and pretend, or think to themselves, that there's something in there that's saying why their paper didn't make it, but oftentimes that's not really why. The reason was because it's not quite interesting enough, or not quite decisive enough, or not quite fitting in with the scheme of how things are going in the field, or it doesn't seem plausible even though it looks superficially okay, or the research design is maybe decent but not super strong.

The "revise and resubmit" is the most important stage of the project. It's your job to address every single thing that's raised by the referee and nail it. If you speak to Larry Katz, who's been editing at the *QJE* for more than 10 years, he'll tell you that that's the kind of paper that gets in. Somebody takes the comments seriously, follows the advice, tries to nail down the loose ends, and even fesses up if there's a problem by saying, "There's one thing we can't figure out. But we're going to rewrite the paper to acknowledge it." When I was editing the *AER* in the mid '90s, I also noticed that that's how the successful guys would respond. Those who were less well-trained would send back only a few pages of comments and the referees would then be alienated, which meant the end of the paper. I learned a lesson.

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

Can I take the Fifth on that? *[Laughs]* I guess it's like what people said about democracy: it's not a very good system, but it's better than the alternatives.

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

One time years ago, I wrote a paper with a guy about the interpretation of strikes in the 1880s. We sent it to the *QJE*, even though it wasn't quite good enough, got it back and came up with a much better way to pose the problem. The *QJE* still didn't like it, and so we submitted it to the *Journal of Labor Economics*, where it was accepted as is.[3] That was my best ever experience. Most of the time, it's nothing like that. Sometimes, the editors, for example at the *AER*, will say, "We've got to fill up the last 25 pages of the journal, and we'll let you send it in as a shorter paper." That's a bitter disappointment, and certainly leaves a bad taste in your mouth, because you've just won the booby prize.

I try not to aim my papers too high. I wrote a paper a while ago with some colleagues, and they really wanted to send it to a top journal. I knew it had no chance, but I couldn't talk them out of it. We sent it in and it got promptly canned! Normally, I win the argument and

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[3] Card, D. and C.A. Olson (1995), 'Bargaining Power, Strike Durations and Wage Outcomes: An Analysis of Strikes in the 1880s', *Journal of Labor Economics*, Vol. 13, No. 1 (January), pp. 32-61.

say there's no point in submitting this paper to a top journal; it's just not going to work. I think if you send a well-executed and well-written paper to a second-level journal, that's something they will like. They get lots of papers that are interesting but screwed up, and so with your paper the editor will say, "Oh my God, here's one I don't have to worry about too much." That's helpful.

REFEREEING AND EDITING

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

Editors appreciate good referee reports. You can tell when somebody has understood the paper, spent time with it, digested it and put into a form. There are cases when you can see that a paper is way better because of a particular referee. It's probably useful to accept a refereeing job just for that reason. It can also be good experience if you're able to abstract back to your own work. But sometimes, you'll say, "I could see how to fix that paper. Why can't I see how to fix mine?" (*Laughs*)

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

One thing is that you're only making enemies. People hold it against you for a very long time that you rejected their paper. And so you don't want to be getting into that position unless you're capable of handling that level of negativity. You have to reject some of your friends' papers, former students' papers, and older, well-respected people's papers. You have to cajole referees to send reports, and you'll be disappointed that people say that they're going to do things and then they don't. I've never been a department chair, but I think it would be very similar: you'll be largely disappointed with your colleagues, and you end up feeling like you only bring bad news (*laughs*). It's true that you do bring some good news as an editor, but when you tell somebody you've accepted their paper, it's not like they send back an effusive note, saying, "Of course, you accepted my paper. It was the greatest genius contribution since Milton Friedman!" You only get the blowback, as I used to call it, which can mean that when you reject a paper, you'll get an e-mail from the author later that same day saying the most amazingly negative things.

Most people think that editors have an agenda or a predisposition to like certain kinds of papers or certain people. And so if their paper gets rejected, their first reaction is that you're not being fair. And the second reaction is that the judgment was made on completely incorrect grounds, like something was argued to be a weakness in the paper, but it's not true. I learned from Orley when he was the head editor at the *AER*. If somebody complained, he would say, "Okay, give me a list of a couple of people, and I'll choose one of them off your list and one off another list, and we'll send out your paper for refereeing again." The *AER* had a process to let people calm down, but these days 95 percent of papers are rejected at the top journals and so there's not much positivity.

I do have a funny story. At the time when I was editing the *AER*, we refereed every single paper; there were no desk rejects. My wife was my assistant. She has a PhD in music, but even she could tell that at least half the papers were not going to make it, but we would still send them out to referees. One paper came in that was about the artificial economy in a computer game where people buy avatars, which are scantily dressed women. At the end of the article, there were a couple of pages of pictures of the avatars. I needed somebody to referee the paper, and so I sent it to a very serious Mormon who I'd known for many years. He said, "You only gave this to me because of the scantily clad women at the end." But I had also read the paper and I told the author that we couldn't publish it. About two weeks later, I got a call from National Public Radio. The reporter said, "We're doing a story on artificial worlds and avatars…why did you reject that paper?" I started laughing. I said to my wife, "You're not going to believe what's going on here!" I told the reporter I couldn't discuss the reasons why it got rejected, but I might have at least mentioned the last couple of pages of the paper [*laughs*].

TIME MANAGEMENT

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

My wife and I don't have any children, so I spend a much longer time working than most people probably do: a minimum 60 hours a week. But I waste more time at work than most! There have been articles published on why Americans don't take vacation and Europeans do, and I think it's partly because we tend to get 'on the job' leisure. Ideally, I come in at around 10:30, go home for dinner around 7:00, and then come back and work until midnight in the office. At weekends, I work at our house in Sonoma.

How do you balance multiple research projects?

I'm no good at that. I always do one thing at a time. I prefer to get really into one project, have it all in my head, remember exactly what I'm doing, and then finish it and move on. I hate multitasking.

How about the balance between your research and non-research activities?

I have a lot of PhD students, so I probably spend more than a day a week on them. I teach one undergraduate class. It's a mathematical version of intermediate micro theory. I've taught it for many years and it's very straightforward. But I hate teaching graduate classes, because it's so much work. For my labor economics course, I spend many hours the night before preparing every lecture. I've never gotten to the stage where it's easy, and I find it very stressful.

How do you balance your personal and professional lives?

I probably don't spend enough time in my personal life, because we don't have children. But it's okay. My wife used to teach at Columbia when I was at Princeton, so we could only spend weekends together. Now that she's retired, I count on her to do a lot of stuff around the house. And so relative to what most people would put up with, I'm way over the boundary *[laughs]*.

REFLECTIONS AND THE FUTURE OF ECONOMICS

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

At the beginning of my career, one important set of findings showed that labor supply elasticities are relatively modest. Many, many people, especially macroeconomists, continue to reject that hypothesis, but my reading of the latest round of research, which is mostly being done by public finance economists, suggests it's still true. In fact, I think if you were to take the point estimates from what we thought we knew in 1985 and look at what's coming out today, you would say that we were right all along.

In the 1990s, a huge amount of research started to work on education. In the early part of decade, Alan Krueger and I wrote a paper that tried to estimate the effect of quality of education on earnings (*see his biography*). That topic had been talked about in the 1960s, and I think our paper was one of the first to bring it back to the table. Now, it's a huge issue, and the findings have implied that it's pretty hard to change the quality of education. My interpretation is that there aren't any obvious free lunches. Everyone says, "Good teachers matter." Yes, but we haven't figured out a way to get good teachers and keep them. Do we have to pay them more? All we do know is that there is an effect of school quality and it is like any other investment we can make. For example, there was a big controversy up until the '80s about whether going to school more increased your earnings or whether it was just you went to school more because you were more able. That's a topic in Gary Becker's book, *Human Capital*, from the early '60s. And then starting in '89 or '90, there was a whole band of research being done on the issue, mostly by students and colleagues at Princeton. I contributed a couple of small things around the edges, and I think that work has shown that, in fact, if you take a typical person who's going to drop out at twelfth grade and push them another year, it's not going to be, perhaps surprisingly, a worthless investment. That's another important finding.

What are the main challenges facing your research fields?

In labor economics, a major problem right now is that the newest lines of work are using more complicated administrative data where you can follow individuals over time as they change employers and you can match them with their employer. That kind of data is not available in the United States, so the research frontier in labor economics is moving offshore very quickly. I've written a few papers using data from Austria and Italy, and many of my students have written papers using data from Germany, Sweden, and Norway. When I started my career, the entire labor economics field was centered in the United States. If we don't do something soon, that's going to be lost.

What are the strengths and weaknesses of your own research?

I tend not to write a series of papers on the same thing over and over and over again, which means my research doesn't have a synthetic view. And people might say my papers are also quite negative, because I often find that such and such program or policy doesn't work. But I don't think there are many mistakes in my papers relative to the average. And my topics and approaches are pretty diverse too.

Do you have any professional regrets?

I'm probably going to spend the last 10 years of my career in a public university, when public universities are going down the shithole *[laughs]*. I suspect that will turn out to be the most difficult thing that would have happened to me. The future for public universities is pretty tough, and we're going to struggle here for a long, long time in trying to maintain quality. Whether we'll succeed is very unclear because it's increasingly difficult in the United States. It's amazing that we have public education here, let alone public higher education - people are not very public oriented in this country. You're an immigrant like me. You must feel that to some extent too.

Do you have any professional ambitions?

Retire as soon as possible. I don't want to be one of those guys who hangs on forever and everybody makes fun of. You see them at the faculty club with crumbs on their beard *[laughs]*. I think you should do your thing and get out.

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

Relative to the rest of academia, we've done extremely well over the last 30 years. I hope we maintain being able to attract incredibly bright kids into our PhD programs, because that's the number one thing. If you talk to somebody in sociology, for instance, a big concern they have is that the quality of students entering the PhD programs today is not the same as it was in the '40s, '50s and '60s. We rely on getting future Robert Solows into our programs; really brilliant people who can see things a new way and once in a while change things.

Every field should be judged by its 28 year-olds and 30 year-olds. But I don't know that in economics we're doing a great job of nurturing them in quite the right direction. If they're all working on Wall Street, that's tragic, because they're just stealing money from each other. If they're talented and interested in doing something that is positive and engaging, it is our job as professors to help them as much as possible…and old guys like me should get out of their way (*laughs*).