

TRANSCRIPT

The Work Goes On

Guest Host: Janet Currie

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Orley Ashenfelter:

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Janet Currie:

Welcome to [The Work Goes On](#), a podcast from the [Industrial Relations Section at Princeton University](#). For today, I'm your host, [Janet Currie](#), the Henry Putnam Professor of Economics and Public Affairs at [Princeton University](#). This podcast series of conversations with leading thinkers and practitioners is creating an oral history of an entire generation of industrial relations experts and labor economists whose contributions to their fields have been absolutely extraordinary.

So, it is only fitting that our guest today is [Orley Ashenfelter](#), the Joseph Douglas Green 1895 Professor of Economics at [Princeton University](#), and the person who originated this podcast. He is renowned for his foundational work in labor economics, econometrics, and law and economics. Orley, welcome to [The Work Goes On](#).

Orley Ashenfelter:

Well, it's very nice to be on the other side of the microphone. Thank you.

Janet Currie:

Well, you're welcome. Let's start by talking about your background. Where did you grow up?

Orley Ashenfelter:

I'm a native Californian. I was born in San Francisco just after the Second World War started. My mother was a nurse at St. Francis Hospital in San Francisco, and that's where I was born.

My mother was from North Dakota. She decided that it would be wonderful to move back there from California. You can imagine what a change that would be. She was from there and I think the idea was that nurses were supposed to bring back husbands who could be in these small North Dakota towns.

So, we moved back to North Dakota for a couple of years, which I really loved. And then, the small business my father had started didn't work out. And, we ended up in the San Diego area where he went back to work.

He went in an aircraft factory, in fact, in that case. And, I really grew up in Southern California. It wasn't until I left to go to graduate school that I was ever out of California.

Janet Currie:

So, I know you went to Claremont McKenna College for your undergraduate work. What was that like? And, how did you get from there to be doing graduate work at Princeton University?

Orley Ashenfelter:

It's a very interesting thing. I was just at our, believe it or not, 60th reunion of my college class, which only had 89 students in it. At that time, it was Claremont Men's College. It was all male. There was, luckily, a women's college next door so that the straight people could actually find someone else.

The unusual feature of it was it was small. It was quite new. It'd been started in 1947, so it was only 17 years old. It's hard to imagine going to a college that young.

But what was really special about it for me is a professor, named Orme Phelps, who many people had been influenced by. In fact, several people who went to this small college ended up doing graduate work, typically at Berkeley, but not always. One was someone you know, Al Finegan, who went to University of Chicago for his PhD.

So, he had several students that he kind of guided toward graduate programs. He knew most of the people in the field. He had a textbook that was sold pretty well, a small college town. It made him pretty decently well off.

And he was a big influence on me, not just me, but quite a few other people. I was doing labor economics. And as an undergraduate, he guided me towards what he thought were the places where the field was strong.

So, he knew about Lloyd Reynolds at Yale and Fred Harbison at Princeton, and John Dunlop at Harvard. Berkeley actually had, far and away, the biggest group. But I had it in my mind, I guess, to go to an Ivy League school.

Janet Currie:

So when you got to Princeton, I understand you worked with Al Rees?

Orley Ashenfelter:

It's very hard. I had this discussion with someone the other day who was trying to do an intellectual history of the origins of people that were Ph.D. students at Princeton. And, it's not easy to do because back in those days it was very flexible. Who was your advisor was a little unclear.

In this particular example, Scott Cunningham asked me about Jim Heckman. Jim lists as his advisors, three people. However, they're completely different from the list of people he has in the footnote of an article in the *American Economic Review*, where he lists five people as his advisors, none of whom are in the three, I don't think, that he lists on the Wikipedia page. So, it's a little unclear.

Originally, I really had an interest in what Bill Bowen was doing. He and Finegan were doing this massive book on labor force participation. And I worked on that, in fact, with an economist, eventually, Dan Rubinfeld. We did a lot of the computing that went behind that giant book.

So, I was working with Bill, but Bill was a fast mover. He'd already moved toward administration. And he recruited Al Rees, and Al came along just at the right moment for me. He was just a wonderful man to work with.

It's hard to describe what I called him. There's this phrase, the greatest generation. Al was definitely a part of that. There's something about a kinder bunch of people and Al was extremely kind and very helpful in many, many different ways.

Janet Currie:

Hmm. It's interesting. A theme of what you've been talking about so far is how one person can have such an influence on your life, first your undergraduate mentor, and then maybe Al at Princeton. I think one thing that's interesting about that is you yourself have trained a huge number of graduate students over the years. Many of them have gone on to be quite successful in their own rights. So, that made me wonder what your opinion is about whether there's some kind of secret sauce for being a great PhD advisor.

Orley Ashenfelter:

That's interesting. My undergraduate experience was really formed by this one guy. But as a graduate student, there were quite a few other people that were influential. Dick Quandt had extremely important influence on obviously all people that were trained in quantitative methods. He was pretty understanding of people's flaws. I still see him. He's still around.

So, I think at Princeton it was a little bit more, not so much a single individual as a group of people, who could influence you. I think the reason it worked out so well with the graduate students that I was involved with, including you by the way, is there was kind of a style of research in the air that I had gotten my hands around and I think several other people were interested in it, too.

And because of that environment really, which I was perfectly happy to be a part of, and also the insulation. So, we were kind of insulated in a way. If you remember your time in the old Industrial Relations Section, you're sort of insulated from people who thought that everything you did was crazy. Somehow, that kept everybody focused on something that was more substantive.

You're right. There were many. In fact, the name of one of them just came up in a podcast that we dropped, Ernie Stromsdorfer's podcast, in which he mentioned Nick Kiefer.

I'd completely forgotten about how Nick Kiefer started working on training programs because of Ernie. But, yeah. He was an early graduate student, too.

Janet Currie:

Yeah. So, going back to your thesis, it was about racial discrimination and labor markets?

Orley Ashenfelter:

Yeah.

Janet Currie:

How did you become interested in that topic?

Orley Ashenfelter:

You know, that is an interesting question. Someone asked me about my thesis topic in a podcast the other day. And I said it was about essays on racial discrimination, which of course when I wrote that thesis, which was in 1966 to '70, it was not a topic economists talked about very much.

Becker had written this book, "Economic Discrimination," which was completely ignored. And the whole idea that racial differences existed, in the popular mind, it was important. The 1964 Civil Rights Act really changed things.

I've never quite understood exactly why I found it such a fascinating subject. I think partly because there hadn't really been any good empirical work on the topic. Becker, for example, his book is really a theoretical book. When he does a little empirical analysis; it's from a Ph.D. thesis someone else did at the University of Chicago.

I was interested in it from the very start and interested in how it could happen, how it could exist, and especially how to measure it. I spent a long, long time actually doing that, over the years. And ultimately, it became something that's used in court cases. It's not acceptable anymore, but it was a little unusual.

I think also, Rees, despite the fact he was chair of the Department in Chicago, he was kind of an old line, union-oriented Socialist in a way. So, I think racial differences didn't make him very happy.

Growing up in California probably was another factor. Racial differences were not... Even though there's some discussion of it, people didn't really see those as such a big deal. It's always been a mix of Japanese Americans and Chinese Americans and Black Americans and everything else. So, maybe, I was a little more surprised at the fact that there seemed to be differences that were well documented.

Janet Currie:

So, you also spent time at the U.S. Department of Labor as the Director of the Office of Evaluation. Can you tell us about that and how it influenced your subsequent work?

Orley Ashenfelter:

That was a great moment for me, and I think it had an influence on the whole profession. It really came about very indirectly because of George Shultz, who is a wonderful man, a Princeton grad, by the way, came out of the Industrialization Relations Section, not as a Ph.D.

George had been the first Secretary of Labor in the Nixon administration, extremely, really, really high quality people that he brought in. The subsequent president of Northwestern University was in one of the administrations. The Solicitor General was a man named Dick Schubert, who became president of the National Red Cross.

There was a big controversy at the moment about training programs and whether or not they had a negative effect on people's incomes, rather than a positive effect, and it was circulating in Washington at a time when people paid attention to evidence.

The negative effect came about because someone had looked at the change in the income from prior to going into the training program and compared it to afterward.

As we now know, some people call it the "Ashenfelter dip." There's this very complex process by which people's earnings vary prior to the time they go into a program. So, trying to measure the effects of training on earnings is extremely difficult without a randomized trial. In fact, it's the reason I ultimately argued for randomized trials in trying to test for the effects of training programs.

I was recruited by someone who was to become the Secretary of Labor there. And the idea was, they wanted to have someone who would be very quantitatively oriented and would refocus the Department of Labor on evaluating their programs.

So, they set up an office, basically. They gave me a tax. I was able to tax the agencies to provide funds for this central office, hard to believe now. We were tasked with trying to evaluate all of the

department's programs, that meant the Occupational Safety and Health Administration. "Was it worth what we paid for it in terms of lost output, the minimum wage law?," and so on.

And I actually worked on all those topics, or brought in people like Bob Smith at Cornell to work on occupational safety and health, who worked on it.

So, we actually did get started with a kind of overall program evaluation. At the time, people would talk about evaluation in two different ways. One, the overall effect of the program. The other was how well it actually functioned, in other words, whether it had bureaucratic problems. We were to focus on the first, not the second, even though I appreciated the second.

So I became a bureaucrat, a real GS-17, pass the test, full-time bureaucrat, and brought along with me a guy named Dave Smith who had been an undergraduate at Princeton, who did a lot of computing with me. And what really attracted me was that they dangled something in front of my head, not just this bureaucratic job, which of course seemed like a useful thing to do.

But the fact that a guy named Farber, not Hank Farber our colleague, but a different Farber, had come from the Social Security Administration and brought with him a way to link trainee data, data on individual trainees, to Social Security records, so you could actually find information on their income before and after training.

And they dangled in my face, basically, that I could have access to these data and try to figure out what the effects of training programs were. It was irresistible, just irresistible.

Janet Currie:

That's remarkable, actually. I think it's another way in which you're a pioneer, just using these kinds of linked data, which of course now people think of as a routine type of thing. But then, I guess it was really-

Orley Ashenfelter:

It was really rare. It was extraordinary. In fact, it was the very beginning of really big data, that was the linking of data across agencies. And then just prior to that, I had used data from the Survey of Economic Opportunity.

In my dissertation I wrote about unions and Black workers. And we didn't actually know, believe it or not, how many workers were Black or White, who belonged to unions. And, that's because that question had never been asked in a CPS before. So, that was the first time we even had access to that data, and that information was coming out in a very straightforward way.

But, yes. You're right. The administrative connections, now, I mean, everybody does that. That's absolutely right. I did figure it out. At least I wrote, in fact, back in a paper while I was at the department, that was published in a very obscure place, *The Proceedings of the Industrial Relations Research Association*. I-

Janet Currie:

I've questioned that, too.

Orley Ashenfelter:

Yes. It's so obscure that it doesn't even have the same name any more. People have asked me for copies of that. It doesn't seem to be digitized. It's very difficult. And, I'm actually arguing for randomized trials

because the training programs are such a complex... Developing a control group for them is a very complex problem. I argued for randomized trials, but I did my best with what I had. I can tell you that.

Janet Currie:

Of course, you and your students have become known for developing methods of causal inference through focusing on natural experiments and the development of difference and difference techniques. And so, I think starting with the US Department of Labor, you've been telling us a little bit about how that happened. Can you expand on that?

Orley Ashenfelter:

Yes. I think the atmosphere came about because my generation of economists learned how to do econometrics pretty well. We had these famous textbooks by Jack Johnston and Art Goldberger, which really revolutionized the way that economists do empirical work.

And I loved Goldberger's book. The only mathematics I ever really liked was matrix algebra. Suddenly I could see, "Wow. This is great. Matrix, I love this."

The interesting thing was, though, that people were so sloppy in many, many papers when they tried to deal with instrumental variable estimates, or two-stage least squares estimates, or whatever. They were so sloppy that often they never even stated what the instruments were.

Today, that would be considered completely outrageous. But, I can show you papers, I will not name any names, where it was just quite unclear that anybody really was taking any of those estimates seriously. And, that was something that just didn't seem right to everybody.

And I think that at that point people had been so sloppy, they just finally learned a little bit about how to do these things. They had become so sloppy, and they didn't really quite understand what they were doing in the first place. So, the result was that you had a lot of attempts at causal estimates that were really terrible. They were not convincing. There was also some element of... You could say I've seen this change recently.

Minimum wage was a good example. There had been a controversy in the 1940s between Dick Lester, who was on the Princeton faculty, and Fritz Machlup, who ultimately joined the Princeton faculty, and George Stigler, who was at Chicago. And, they took it as a matter of pure theory that a minimum wage would have to reduce employment.

Now today, we realize that even in pure theory, thanks I guess to Joan Robinson, if there's monopsony in the labor market, which a lot of people think there is now, that that's probably not necessarily true anyway.

But there was a sense in which if you didn't get the right result, you didn't get published. And for years, George dominated the *Journal of Political Economy* and had a ludicrous influence on some really terrible papers that claimed to find effects. So, most of us got interested in the credibility of the estimates.

Now, you might ask, "How did that happen?" Well, that came from the government. When you're in the government and you actually make a presentation, you can't expect a bunch of bureaucrats to listen to some sloppy discussion about instrumental variables. They don't even know what that means.

In fact, that's how I came across the idea of talking about difference-in-differences. It was the government, really. Difference-in-differences is so simple. If you have the right data, you can set it up so that it's a very complicated estimator, but you can also explain it.

You subtract twice. Everybody can add, subtract, and multiply and divide. The credibility part really came from the Department of Labor. I got to the point where I felt that, first of all, you needed to understand really what the first stage of the problem was.

So, one of the papers I wrote early on was on the minimum wage. It wasn't about the effect of the minimum wage. It was about, "Was there a minimum wage?" People don't realize this, but there's no penalty for violating the minimum wage. Well, if there's no penalty, why would anybody do it?

And in fact, first time offenders don't face any penalty. Second time offenders do so you don't violate it twice. And of course, nowadays, I think, lawyers will be smart enough to tell you that you probably shouldn't violate it unless you're going to do it in a flagrant way.

So, a lot of that came from the Labor Department, too. There was this emphasis on credibility. You needed that in order to convince people that the work that you had done should actually influence policy.

Janet Currie:

A couple of times it's come up talking about theory and sort of the role of theory and labor economics, for better or worse. I think some people might be surprised to realize that a lot of your work is actually grounded in theory. So, for example, your Frisch Prize winning paper about unemployment integrates labor supply and consumption choices into a model in which unemployment is treated as a constraint on worker choices.

So, clearly you're not against theory. Can you talk a little bit about what you think the appropriate role of theory is in labor economics?

Orley Ashenfelter:

I fell in love with the model of consumer demand. And then, the expansion of it to labor supply was just so obvious. And also, it can be used for many other things. Intertemporal aspects, including savings, can be taken into account.

Actually, I would much prefer to have a simple theory that you can then use to guide your empirical work. The problem is that if the empirical material doesn't fit with the simple theory, and I'm afraid that it very often in the labor market does not, you have to do something to figure out how to deal with that. And I always think that when it comes down to it, if one of them has to give, it's going to have to be theory.

Theory can be changed. The data, if they're done correctly, it's very hard to change. So, I love a little paper where you can write down a really simple model. Well, a good example is a paper that I thought really kind of borrowed the idea from my colleague, Bobby Willis.

But it was a little paper about competition, franchise no poaching agreements, and how you can actually try to use a little bit of a theory to try to explain what the impact of a group of firms, not all the firms, agreeing not to poach would be on the effect of concentration in the industry. That's a very nice little way to examine the issue.

Complicated theories are harder to deal with. And I always had a lot of trouble with game theory, because too much of it just doesn't really fit the simple, empirical findings that we have in the world.

Janet Currie:

So, you've mentioned a couple of things, like your early interest in discrimination, that maybe give a clue about how you got interested in law and economics, or even the example you gave about whether the minimum wage is even really binding or not.

Can you talk a little bit more about how you became interested in law and economics and your role in doing things like founding *The Journal of Law and Economics*?

Orley Ashenfelter:

Well, you really stated the reason. So many of the policies involved have to do with the law, that it fascinated me. But there was another factor at work which is, I was asked, in the late 1970s, to join a program to teach econometrics to judges. Now, this is a formidable task.

Judges, they become lawyers because they don't want to do econometrics. And this law and economics program, it was started by a man named Henry Manne, who had it for many years.

It's become quite controversial because the law and economics group in the law field are sometimes considered always to be conservatives. It's not really true. There are many that are not. But nevertheless, the case, it's kind of got that connection.

So, what happened was, and this is another aspect of the credibility. I was hired basically to teach econometrics to judges, quantitative methods, give them some idea.

And in fact, the reason it happened is because the judges themselves wanted a course. I would do a one-day course in the basic ideas. And then, subsequently the judges wanted an advanced course.

And the reason for that was because statistical methods had come into the courtroom in a big way because of discrimination cases.

So, they were seeing giant arguments over statistics in their courtroom. And, they were feeling like they were not able to really understand what was going on.

They actually wanted this course, and the course got started. And then, I started presenting material in it. Now, when you talk to judges, you need to be right. These guys are masters.

In fact, I'll never forget. I was at one of these meetings when a judge who actually was a graduate of Princeton, I didn't know it at the time, we were in the [inaudible 00:25:54].

He said to me, "Well, I'm around Princeton quite a bit." "Oh," I said, "really? Why is that?" "Well, I went to Princeton. Oh, yeah. I'm there a lot. I'm pretty much there twice a year."

"Oh, really?," I said. "Why?" "Well, I'm the chairman of the Board of Trustees." So, I thought, "Well, this is a little impress... It's my boss."

So, I realized right away, "This is not just an ordinary judge." He was from San Francisco. And I think he was chair, maybe he wasn't. But, he was on the Board of Trustees. So, I said to him, "Well, let me ask you, then. There's been this discussion about how we're brainwashing judges. And, what do you think of that?"

He looked me right in the eye and he said, "Orley, I've been brainwashed by the best, and you're just not in that league." And I thought, "Well, I think we know how hard I can do. I better do something that's credible."

So, after that, I tried in fact, as best I could, I would often lecture from the old program where they originally did randomized trials, MDRC, did these randomized trials with some programs, try to help people to get back into the labor market.

And, I'd often use those. They loved that kind of stuff. Even now, I've taught it again. One of the great examples to use is the very first COVID-19 trial. It's a beautiful example of a pure randomized trial.

Almost all the problems that you could face don't exist. There's no other treatment out there. You have to worry about contamination. Pretty much, all the problems are solved.

So, that's how I got involved, really, in trying to be more credible. I felt I had to teach them. And then of course, you can't just teach your undergraduate students any differently than you teach judges, so that ended up being in the class, too.

Janet Currie:

Hmm. Very interesting. I wanted to ask you about your role. I guess you've alluded to the atmosphere in the Industrial Relations Section and so on, but you were the Director of the Industrial Relations Section for almost 30 years.

So, presumably you had a big hand in creating that atmosphere. So, I was thinking about what your goals were in that position and whether you feel that you realized them.

Orley Ashenfelter:

That's a very good question. You know my administrative style, which is to say not too much administration, try to have very good people and everything will work out fine. I think we did have a big influence.

The number of people who, as you mentioned, I mean, it's really an astonishing number of people who had a big influence on the field of labor economics. But I think actually more generally in economics, what the Industrial Relations Section really provided was a kind of a... We used to call it the cone of silence, like the old "Get Smart" program, and funding.

So, although we did, early on, raise money outside, a lot of the time it was endowment funding that would permit us to not have to worry about where the funding came from for research. So, it kind of provided an umbrella of both funding and an intellectual experience that was very satisfying.

And, my goal was pretty much always to try to help students develop their skills and succeed. It was the old line about, "How, if I just had 10 percent of the income of all my former students, I'd be a wealthy man." I could do it with half a percent, actually.

That was, I think, probably that feeling. I don't know how you feel about it. My understanding is you're quite an adept dissertation supervisor yourself and have had many good students, Janet.

Janet Currie:

Yeah.

Orley Ashenfelter:

I think that, not only the style of the research, but also the style of the teaching, kind of carries over to other people, too. I've noticed it with so many people all the way from Hank Farber, who's had a ton of great students, Jim Heckman, who's had wonderful students. You've had many good students, many, many people. Joe Altonji, lots of others who aren't just fine in their own research, but take teaching seriously and take their graduate students very seriously.

Janet Currie:

So, Orley, I think that might be a good place to stop. And, I just want to say it's been an absolute pleasure talking to you today.

Orley Ashenfelter:

Thank you. You're a fabulous podcaster.

Janet Currie:

Well, thank you.

So our guest today has been [Orley Ashenfelter](#), the Joseph Douglas Green 1895 Professor of Economics emeritus at [Princeton University](#).

I'm your host, [Janet Currie](#). Thank you for listening.

Announcer:

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