

Conversations with an Eminent Labour Economist: Thomas Lemieux

Thomas Lemieux (TL) is a Professor at the Vancouver School of Economics at the University of British Columbia (UBC). He completed his PhD in Economics at Princeton University in 1989. He is a Fellow at the Royal Society of Canada and the Society of Labor Economists. He has written extensively on wages and income distribution and has contributed significantly to advances in econometric techniques including regression discontinuity. He was interviewed at the Asian and Australasian Society of Labour Economics (AASLE) conference in December 2017 by **Rob Bray (RB)** from the Centre for Social Research and Methods at the ANU.

RB: What advice would you give to a young scholar starting off a career in labour economics?

TL: Spending quite a bit of time being on the other side, the editor's side, looking at papers, maybe I am more optimistic than others, but I think really what matters is to work on interesting questions. That's often the first thing we look at when we receive papers at journals.

This is important because obviously publishing in journals is the most important part of a young scholar's career. It is often what's going to determine where they end up, where their career is going to go. I think often there's a sense that it's more the latest fashion of the day or doing the more technical that is important, but I think it's important to keep in mind – at the end of the day – that what most economists are interested in is seeing some interesting work on important questions. Also, the interesting and important questions should be answerable with data, especially in the field of labour economics. Often that's the big challenge. But I would say, at the same time, that over my own career the growth in the data that's available for research has been very impressive.

David Card (2017)'s talk at the AASLE conference provides a good illustration of this, with him explaining that even the way we think about the labour market depends on the data available. That is, before we had good and detailed data about firms we couldn't really think so much about the role of firms and this affected the type of models we had in mind.

So I think – yes, what makes for a good paper, a good research contribution, is when you have an interesting question that you can actually answer with some data. And often it's some new data that's better in one dimension or another than what people have done before.

RB: Do you think there are some big questions that we avoid. For instance, If we go back, say to the 1920s, questions such as the theory of wages received a lot of attention by the economics profession, but we don't hear much about that today.

TL: Yes, it's true, but it also depends on how we define big questions. I think as in many other fields, and you see that definitely in the sciences, as the body of work and research keeps growing and growing, often the contribution will become a little narrower. So, at the same time compared to what was being done back then it's true it's probably narrower. But now you can still cut bits and pieces of interesting questions and give a more substantive answer because we have data to answer those questions.

But do you know what? In the field of labour economics I get the sense that over the last four, five or ten years lots of important questions that have been ignored for a certain period of time are coming back.

Probably part of the reason is that there was a period where, in the terms of research, a huge focus was put on using experiments or natural experiments, or making sure that we have this very convincing source of variation to answer different questions. I think the issue was that for some of the bigger questions we couldn't really think of any way of either running an experiment or having an interesting natural experiment. As a result people started, maybe not ignoring, but paying less attention to, some of these questions. For instance, thinking about some of the questions about the connection between macro and labour, given lots of the macro questions are happening at the whole economy level, it's hard to think of the natural experiments that you can use to answer some of these questions. So I think that's why, at some point, the search for these compelling research designs had a real impact on the field of labour economics and often labour economists started working more on education, health, or on crime, because there they could find these kind of interesting natural experiments.

However, I feel that over the last 10 years, and partly because of better data coming online, as I mentioned for example with David Card (2017) and firm data, we are in a position where we can start making first steps in understanding how the labour market really works. I think also, what Americans call the great recession, has also played a role. I've noticed it in the US because suddenly, from about 2009, not only was unemployment a very big issue, but also long term unemployment, while it was traditionally not so much an issue. Suddenly we started seeing much more research about unemployment and asking what explains these long unemployment spells: 'Does it have to do with the structure of the US unemployment insurance program?'; 'Does it have to do with some scarring of the workers?'; and so on.

But I would say, overall in labour economics, people have been going back to some of the core questions that have been left aside a little, 'because we have this neat natural experiment'.

RB: The way you are talking about labour economics it seems you see it mainly through the lens of empirics ... what's the role of theory?

I think during the 1980s and 1990s, there was actually lots of progress made on the theory side and lots of it had to do with leaving aside the standard competitive model and thinking more about the employment relationship, the role of contracts and so forth. Bengt Holmstrom recently got a Nobel Prize and lots of his work was very innovative labour theory, bringing in incentives, imperfect information, etc. But, I think the connection between that kind of theoretical work and empirical work still hasn't been fully established. Actually, some of my own work looks at performance, big contracts, and mode of compensation and things like that. So I'm always intrigued by how we can connect that to these models. However, I would say that what most likely happened is that over the last 20 or 30 years so much more data became available. It's a little bit like when you talk about technological change and how it affects the nature of work. I think in our field the big technological change was that there were so many computers and they are way more powerful. You can have huge databases and there is more and more data coming on line.

So I think the big reason why labour economics, certainly over recent years, has been highly empirical is that, although we can keep improving theory, we don't have this big technological revolution that would help push the frontier in terms of theory.

Also, our field has always been more empirical than most other fields of economics and it's not surprising that most of the work in labour continues to be empirical. But talking about this data revolution, it is not just in labour economics. Many other fields that used to be much more theoretical are now much more empirical, because they now have data that can be used. So to me it's not so much that theory is dead, so to speak, but if you're a young researcher interested in labour economics, it tends to be that the direction in which you have more chances of making a new interesting contribution is often finding a new set of data.

RB: One of the big questions which has re-emerged is inequality. This raises two issues: One is why it has re-emerged? Second, is are we so certain that we actually have to do something about it?

TL: As you say, equality is certainly one of the big questions coming back. I clearly remember when I was a graduate student, when I started in graduate school in my PhD in the 1980s, no one was talking about inequality. I was at Princeton where Alan Blinder had done some work on the income distribution and inequality. He had written something in the late 1970s and early 1980s saying that the only remarkable thing about inequality is that how stable it is, 'it's not changing or anything'. Then there was a big recession in the early 1980s, 1981 to 1984 in the US, and then suddenly around 1986, remembering that back in those days it always took a couple of years before the micro data would become available, some people start noticing, 'oh, it looks like inequality is growing'. I remember many distinguished labour economists at the time being interviewed by the New York Times, or whatever, and they said, 'oh you know, it's just the impact of the recession: lots of job loss; less skilled workers;

manufacturing suffering more but, you know, once the economy comes back we'll be fine'. Then two, three years later people realised that it was not coming back and inequality kept growing. And within a few more years, that's by 1990, it was the new big question in labour economics.

The growth in inequality in the US, and this emerging research focus then generated lots of interest, in the UK in particular, because they then started noticing inequality increasing in their countries. So I think economists got interested in inequality again because inequality was suddenly growing.

Inequality research is also a good example of a topic where many different approaches have been used. Some of the most influential work on inequality is very descriptive. Picketty and Saez (2003), for instance, have had enormous influence. They talk a little about explanations but much of what they have done is spend an enormous amount of energy and effort in developing collaborations with co-authors all over the world who collect new data and see what's happening in different countries over the long run. It is actually an interesting case where you have a mix of highly descriptive work. With other work, in cases like the minimum wage, for instance, in the US in particular, where you have some variation across the states, researchers use a little more of a natural experiment or difference in difference approach to try to assess the role of that particular factor. But still what I think is interesting about inequality is how all kinds of different approaches, including very descriptive work, has been pursued and lots of this highly descriptive work was published in some of the best journals in the economic profession. I think this is not only because it's Picketty and Saez but because it's a big question. In that case just being interested in the question and knowing what's really in the data became something that was very interesting.

RB: Turning to the second part, about whether we should be concerned about income inequality?

TL: Well, the question of whether inequality is bad, and do we need some? It's true that early on, typically the more Chicago - type people would say 'well, you know, it's important to have incentives in the system, to have returns to scale and to have rewards to effort'. From that point of view we need some inequality. I don't think anybody is really debating the basic point that you need some incentive, although we can certainly argue about what is its importance. Even if you're running a firm, what's the role of purely monetary incentive versus other issues in the way you run your business and get people to like their work and contribute. But leaving that aside, there is no question that some incentives are needed in the system, but this is only part of the issue. Rather it becomes a case of using the kind of framework that has been put forward by people like Atkinson and others saying, okay, but we also have a social welfare function.

Essentially an argument where the main reason why inequality is bad is just that if you give one more dollar to a very rich person it doesn't have any impact, while it could make a big difference for people at the lower end. I think at the end of the day it's mostly that – unless you have a good reason why inequality is good, say for growth or for incentives. However It seems to me that most of the growth that we've had in inequality, especially in the US, has really not increased the incentive effort, nor has

it contributed to overall growth. For example, even back in the 1980s the return to education was quite large, along with the gains from career progression if you were doing well. So the idea that by paying CEOs ten times more than they were in 1980 is really going to change behaviour, that is quite unlikely.

RB: So we should be listening a bit more to some of the other fields of economics as well?

TL: Yes. When I was a young professor at one point I was asked to teach a course on income distribution covering subjects such as income distribution, inequality and poverty, and then I actually spent quite a bit of time reading more of the public economics and welfare literature - people such as Atkinson and others - because essentially I knew nothing of that from my training. I don't think that has changed very much. I think it's one of these things it would probably be good for every economist to know a little more about, but at the same time from our point of view, when we look at inequality, partly it's just a more substantive way of saying that inequality is bad.

To me what I find more interesting is actually going down to the explanation for growth in inequality because I basically don't think that this inequality is doing anything good in terms of growth, or putting in more incentives so that people can be more entrepreneurial. Questions about the bargaining power of workers and the role of labour market institutions and unions; and the labour share, actually a much more central issue. It's something else that when I was in graduate school I didn't hear much about, but then when I was teaching my inequality course as a young professor, I started reading some older work from earlier in the 20th century where this whole question of income distribution, probably because of data limitation, was all about how much goes to capital and how much goes to labour in a very aggregate way. Once again that's a question that people really stopped thinking about for the same reason, 'well, the only remarkable thing of the labour share now is how flat it has been'.

But now that labour's share has been declining it has been raising lots of the big questions. I have not worked on that very much so I don't want to venture too far on the explanation. However, it certainly seems to be a sign, while there may be some other explanations too, that the changes in the bargaining power of workers are so large that we're even detecting it at the aggregate level. I think it's also related to one of the reasons why I find what's happening to the top 1 percent interesting because early in my career when there was a paper about chief executive pay, I felt that was very boring work and not that particularly interesting as it was only a small fraction of the workforce. But then inequality started increasing to the point that the share of total national income going to the top 1 percent was becoming quite important. That's when I started thinking that it's actually important to understand what's happening to these very high income workers because even at the aggregate level there is an important impact. So then when you actually combine the facts that the labour share is declining, and that among labour an increasing fraction goes to the 1 percent, it means that the share of national income going to the lower 99 percent has really fallen quite drastically.

I think again that's a case where, because of these big changes that we're now seeing, it has become again a hot question to understand. And in one sense maybe that's the right way to go. When big things happen in society we should actually redirect our research effort to this.

RB: One last question. Looking at the citations of your most cited paper, DiNardo, Fortin, and Lemieux (1996) - the DFL paper - suggests very gradual build up over time, do you think this was because of slow permeation of ideas or is it because there's now been an explosion of interest. And are there any issues for young scholars who write papers and no one cites them?

TL: The story of that paper is interesting. I mean what I often tell young scholars that now that I'm a well-established person, I will write a paper and then I get invited to present it at many places so I will present that paper many times. But with the DFL paper I was only invited once to present it, to a seminar at the University of Michigan. I guess part of that was because at the time I was just a new scholar, so people didn't really know me. Then it was almost at random that someone asked me, 'do you want to come and present?' So yes it's my most cited, and probably the paper I'm most proud of, and I almost did not present it.

While the pattern of citations in part reflects the way Google Scholar works I would say that at the time it was viewed as a bit of a controversial take on that question because the key papers that were written in the early 1990s about inequality were really very conventional. They were about supply and demand, arguing 'so what's happening in inequality must be that demand for highly paid workers is going up, and here we go'. I mean, if you look at a paper I really like, and which was extremely influential, by Katz and Murphy (1992), it's basically just that the supply of highly educated workers is growing and if the return to education is going up despite that, it must mean that demand is going up even more, and we're done – I am exaggerating a bit.

In the DFL paper we started looking at the data in a different way. At the time people would focus on wage differentials, so we said let's just plug in the distribution and then – then we saw that for men in the US: 'oh there seems to be a spike and the minimum wage seems to have a real effect'. I remember, at this moment we said, 'well, if the minimum wage has a large impact for men what is it going to be for women'; so then we just got the data for women and plugged the distribution in and essentially the distribution looked like a triangle. You have the minimum wage and then it declined. When I saw that I thought, 'oh boy, that's going to be – that's going to be a good paper, I'm probably going to get tenure and everything, because we really found something important.

But you know, I think at first, because people thought the explanations were already there, 'it's supply and demand' there was not much attention. We had a few people such as Richard Freeman, of course, say, 'oh no, you should use bargaining power, it can be important', and people would say, 'oh yeah, yeah, Richard, it's probably a part of the explanation', but just part of it. At the time it was not a conventional explanation. Eventually, however, people realised that, yes, it's a big part of the story.

That is also an example of work which was successful because we were looking at things differently, really trying to look at the whole distribution instead of just specific wage differentials.

There is a methodological part of the paper too – essentially we suggested this way of constructing counterfactuals doing these ‘what if’ exercises. For instance, ‘what if the unionisation rates were still as high as the early 1970s’. Actually that part – the methodological part - also caught on and I think part of the reason why the citation has been going up over time is not purely for its contribution to the labour inequality literature but also the method.

It’s actually an interesting case, and has a bearing on the question of advice to young scholars. When I was head of my department at the annual meeting with all my young colleagues, I was telling them about the importance of going to conferences and making sure their work was getting to be known. I said, of course, another way of getting work to be known is to give seminars, but the problem is that to give seminars you need someone to invite you. So I told them, well, you can still invite more senior people to present at UBC and then you can get to know them and tell them about your work and then they can invite you back to their department. So that was my little tip. Of course I tell them I understand that you cannot expect a young scholar to get five, ten invitations a year and then I just give this example and say actually my best paper was almost never presented because I was the new person.

References

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