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‘Wage repression and secular stagnation are rather close in kind’

Interview with Lance Taylor



Lance Taylor received a BS degree with honours in mathematics from the California Institute of Technology in 1962 and a PhD in Economics from Harvard University in 1968. He has been a professor in the economics departments of Harvard and the Massachusetts Institute of Technology, among other research institutions. He is currently the Arnhold Professor Emeritus of International Cooperation and Development at the New School for Social Research, New York. He has published widely in the areas of macroeconomics, development economics and economic theory.

Usually the first question is: how did one get started in economics? How did you get interested in economics?

That’s an interesting question. I was an undergraduate at Caltech and was allegedly studying mathematics, but I decided I wasn’t that good at mathematics, and at the same time I drifted off into other areas. There was an economics teacher at Caltech named Alan Sweezy, who was the brother of Paul Sweezy. So Paul turned into a Marxist and edited the *Monthly Review*. Alan turned into an, I suppose, left Keynesian. He was denied tenure at Harvard (at least in part due to his politics), had some health issues, and ended up teaching out there in this California Institute of Technology. When I was a senior I took an economics class from him. It was basically reading *The General Theory*, and I think in retrospect that it sunk into some place deep down in my subconscious. Basically I got into economics because of him.

Which year was that?

Well, I graduated in 1962.

And after that?

Then I went off to Sweden for a year and was vacillating between economics and mathematics. I got married, then came back and studied economics at Harvard, mostly doing development economics at that stage.

Did you consider yourself to be a left-wing Keynesian at the time when you got your first job at MIT?

Well, no, I did not at that time. What happened is that I was interested in development and thought I could save the world by working in developing countries, and that kind of thing, while I was studying at Harvard. So I worked on my thesis with Hollis Chenery, who was a well-known development economist at the time. Then, through the old boy

network I got a job in Chile between 1968 and 1970, just before Allende got elected. I learned, more or less, how a highly inegalitarian developing country works and got interested in distribution issues when I was there. Then I came back, taught for a little while at Harvard, and then I got a job at MIT basically through the old boy network again.

Which year was that?

Oh, I can't remember exactly: in the early 1970s. Then my wife and I spent some time in Brazil and got some more lessons about developing countries there.

Still related to this career, at some point I suppose you felt uncomfortable at MIT?

Well, that was later.

I know this was much later. Is that why you moved to the New School?

Yeah, basically, but I didn't move to the New School till the 1990s. In the 1970s I was basically doing computer general equilibrium models because that came out of the Chenery school. I was trying to make sense in terms of macro theory about how I thought developing countries worked. So I wrote a few books basically trying to refine that view.

Let me come to these books. I know you did some work on general equilibrium theory, well, computable general theory.

Yes, not theory but models.

The computable ones. In 1983 you published a book, the title of which was something like Structuralist Macroeconomics ...

Well, the very first book was *Macro Models for Developing Economies*, and that was in 1979.

The 1983 book was the one where you introduced Kaleckian models, claiming that while they were perhaps not valid for the developed world, they were certainly valid for the less developed countries.

Very possible I said that.

The words 'Structuralist Macroeconomic Theory' and 'Structuralist Proposals' appear again in the subtitles of your 1991 and 2004 books.

The latter was the book *Reconstructing Macroeconomics*.

Right, when you were restructuring macroeconomics. Why use this word 'structuralist', is it because it is related to the Latin American structuralist school?

Basically, yes. The idea is that you cannot just describe an economy with production functions, demand and supply functions and maximising this or that, and that instead institutions and history matter. I guess I was thinking of 'structuralist' as a shorthand description of how you try to build that stuff into a model, which also ties back into the computable general equilibrium stuff.

But why not call it Kaleckian? Or was it under the influence of your Latin American experience, so that you were dealing more with inflation theory than were most post-Keynesians at the time?

Yes, probably. I just thought 'structuralist' was a better shorthand description of what I was trying to do.

If someone was trying to put you into a box, would you accept the Kaleckian box, or you would not feel at ease?

No. I guess I'm looking for a broader term. I could be in a Kaleckian box or a Goodwin box.

You talked about computable general equilibrium models. I would like to know, how do you see the relationship between that and the work that you were doing on this in the 1970s, and what you now call stock-flow consistent models or the SAM approach?

Historically, various people were working on what came to be called CGE models in the 1960s. Chenery was doing some stuff with Uzawa. Then Leif Johansen in Norway did a very interesting model, which was basically a CGE model. So I got interested in that. Taking off from some of his stuff, I probably put together the first CGE model that tried to incorporate trade. Then, related to that, the CGE model is essentially a model that starts out with an accounting SAM – a Social Accounting Matrix – with all the accounting restrictions that are built into that, and then you apply additional assumptions to get, if you like, a closed system in high school algebra terms. I was doing that for quite a while, but the additional restrictions that you impose on that accounting framework essentially determine the macroeconomic system, the macroeconomic behaviour of the system. SAMs were invented by Richard Stone in Cambridge [UK] and then they got incorporated into CGEs. Chenery became the research director of the World Bank, and once there, brought over a guy named Graham Pyatt from Stone's Cambridge Growth Project. He brought SAMs with him and so, over a fairly short period of time, SAM and CGE got themselves integrated. You must know more about this than I do. I mean Wynne Godley, for all practical purposes, is working within the SAM framework although you can label it differently, but the consistent accounting is certainly a big part of the system. I've always wondered: was he ever influenced much by Stone?

He knew him very well because they were together in the Department of Applied Economics at Cambridge. He had a great admiration for Stone.

I never talked to Wynne about that.

So, basically, you would say that the difference between computable general equilibrium models and stock-flow consistent models, or SAMs, is in the closure? Would you go this far?

No, I wouldn't. The SAMs were basically, as they were put together, national accounts models. They modelled national accounts systems, essentially expressed the national accounts plus the input–output accounts, in the form of a matrix. You can now fairly easily extend a matrix based on the national accounts to incorporate the flow of funds. It's not easy, it's a big mess as you well know, but the two are in principle compatible. Once you have the flow of funds as a flow table, then it becomes natural to extend it into accounting over time, which leads to the sort of stock-flow approach that Wynne did a lot of work on.

Were there also computable general equilibrium models that tried to incorporate flows of funds?

Oh, yes, in my day I did one that got published some place or another. As Godley would say, a lot of this was also stipulated by Tobin.

Right. Wynne always referred to Tobin's 1980 paper, the one that had four authors, the Backus, Brainard, Smith and Tobin paper. Let me move back to your own work: sometimes one gets the impression, reading you, that everything can be expressed in the form of a two-dimensional system of differential equations?

Well, that's another tendency.

Is that because you feel very much at ease with this method?

Yes, I mean two-by-two diagrams are easy to understand.

They are easier to understand than three-by-three.

Three-by-three gets a little bit complicated. Although three-by-three obviously has more interesting dynamics, and allows for more interesting possibilities, I should say.

Ok. Moving on to different topics, what about the book on Keynes that you wrote a couple of years ago? What was the incentive to write that?

Basically, over the years I got fairly strongly influenced by Cambridge economics in all its various forms, species and subspecies. Then there actually seemed to be a revival of Keynes going on before the awful reaction that later took place. So it seemed like an appropriate time to write a book. I was just trying to think through how I would interpret Keynes 50 years after I read *The General Theory*.

Right, we are going back full circle here, back to the origin of your interest for economics! You have worked advising governments in Latin America and other places, so what is your take on the future?

I don't know, I could tell you various stories about various futures in various places. It's not an answer, but I think it is a little bit implicit in the question. There was this Keynesian revival which lasted less than 2 years. It was a flash in the pan. So the question is what gave rise to this horrible reaction of Olli Rehn and your prime minister [Stephen Harper]. I don't really have a good explanation for that. But the fact that you have really contractionary, regressive income distribution policies all around the world, you would have to ask if that is going to last. And if it does last, then I think the prospects for any region of the world are not terribly favourable.

So you would agree with Larry Summers's speech that we both heard 2 weeks ago in Toronto to the effect that we are stuck in some long-term stagnation?

Yes and no. I mean, Summers is like a chameleon, he comes out with different colours. Secular stagnation was discussed by Alan Sweezy back at Caltech, as he was one of the disciples of Alvin Hansen. Secular stagnation essentially says you have a deficiency of demand. Which is a little bit of what Summers was saying. But, on the other hand, it seems to me from the discussion we had this afternoon, that wage repression and secular stagnation are rather close in kind once you take into account interactions between cyclical dynamics and changes in the distribution of wealth as discussed ages ago by Luigi Pasinetti. Greater wealth concentration leads demand to fall while profits rise, leading to further concentration. That's a pretty potent positive stagnationist feedback that comes straight out of Goodwin cycles.

Well, this brings up a last question. We had a discussion this afternoon about those Goodwin cycles that seem to be profit-led, while on the other hand you have several other researchers who claim that even a country like the US is wage-led. What is your take or explanation on this discrepancy?

I think the explanation is fairly straightforward. Goodwin cycles imply in terms of the dynamics that you have nullclines for capacity utilisation, or u , and the wage share, or ψ , in the (u, ψ) plane. One nullcline has a positive slope, which basically says that as output goes up the wage share goes up. That means that $\dot{\psi} = 0$ along a positively sloped

nullcline. And $\dot{u} = 0$ along a negatively sloped nullcline. In terms of that particular model, then, if you talk about something being profit-led, you are saying that the demand nullcline has a negative slope. There is a high employment profit squeeze when the distribution nullcline has a positive slope (an idea going back to Marx, by the way). And hence you are cycling around those; that's basically the Goodwin cycle. If you start out from a low level of economic activity then, coming out of a recession, the wage share is going to fall, because of the presence of overheard labour or because of a rise in productivity or something like that. When the (u, ψ) trajectory hits the $\dot{\psi} = 0$ nullcline, the labour share will start to rise, cutting back profits and effective demand. Eventually the trajectory will cross the $\dot{u} = 0$ nullcline at the top of the cycle. So you get a counter-clockwise cycle, which you see in David Kiefer and Codrina Rada's piece that was discussed this afternoon. Now, if you use single-equation techniques, there is obviously an identification problem in the sense that you've got two curves, and if you try to fit a single equation model to those two curves one signal is typically going to come out more strongly than the other signal in terms of the econometrics. And what the Kiefer and Rada paper shows, in terms of the econometrics, is that the positive slope of the output versus the wage distributive curve is going to be the more robust. So a single-equation regression will give you something which you could label wage-led, but I think it's really profit-squeezed.

The interview was conducted by Marc Lavoie in April 2014, during a workshop held at the New School. The transcript was prepared by Brett Fiebiger.

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