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## Article

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**‘We need to confront more forcefully the neoclassical position and show the absolute impossibility of the world it describes!’**



Interview with Thomas I. Palley

Thomas I. Palley is an economist living in Washington, DC, USA. He is currently Senior Economic Policy Adviser to the American Federation of Labor and Congress of Industrial Organizations (AFL-CIO) in Washington, DC. He was formerly Chief Economist with the US–China Economics and Security Review Commission. He has served as Director of the Open Society Institute’s Globalization Reform Project, and before that he was an Assistant Professor at the New School for Social Research in New York, USA. He is also an IMK Research Scholar at the Hans Böckler Foundation in Düsseldorf, Germany, and has his own blog at <http://www.thomaspalley.com>.

*How did you get interested in economics? How did you come into contact with Keynesian and post-Keynesian economics?*

When I was an undergraduate at Oxford, I studied history and economics – which would be a form of joint major in the US – and both have been very important to me. I confess that I was not a very good history student but, over the years, I have become more and more impressed by historians’ methodology which I view as being a little like that of lawyers. One has a hypothesis that one backs with a portfolio of evidence, layer upon layer, all of which is consistent. That’s the approach I have increasingly used when doing empirical economics. I have written two books about the American economy, *Plenty of Nothing* (1998) and *From Financial Crisis to Stagnation* (2012). In both, the theoretical argument is supported by marshalling a broad body of evidence that tells a story and connects all the dots in a consistent persuasive manner. I’m interested in big picture theoretical macro-economic arguments which involve causality and processes operating unevenly in historical time and changing circumstances. That is where econometrics is at its weakest and the historian’s method is at its best.

With regard to economics, I was pretty much a Keynesian from the start. I think Keynes’s principle of effective demand determined output and Richard Kahn’s explanation of the multiplier are absolutely compelling. I also found the Phillips curve to be an intuitive and compelling proposition and, much later, part of my dissertation was on the Phillips curve which has also been a big part of my research. At Oxford, I was completely unconvinced by Friedman’s natural rate Phillips curve which never resonated with me as a description of industrial labor markets. In addition, I recall John Kenneth Galbraith figuring prominently on the reading list and I remember reading the *Affluent Society* and *The New Industrial State*. Not only were they very entertaining to read, but I was

intellectually impressed by them. I think Galbraith really captures something about our present capitalist reality but, unfortunately, his ideas have not really impacted economics.

I have also always had a libertarian inclination and I remember reading some of Milton Friedman's *Capitalism and Freedom* and agreeing about the power of self-interest. Of course, self-interest doesn't produce the wonderful outcomes that Friedman claims it does, but it's still a very important piece of my thinking about economics and in some ways it has been a hallmark of my approach to political economy: the left thinks I am too much on the right and the right thinks I am too much on the left. The neoclassicals think I am too post-Keynesian and the post-Keynesians think I am too neoclassical.

*When did you go to Yale? What were the first steps of your academic career?*

In 1980. Actually my doing an Economics PhD was a two-step process. First, I did an international relations MA program at Yale. It was a two-year program and I was interested in reading some political science, which I had not been exposed to. It was also the time of the North/South debates and I had some interest in development. However, for me, so much of political science is about competing interests at play in the economy. That means you really need to understand economics, which drew me back to doing an economics PhD.

One very important concept I took away from the international relations program is the notion of competing perspectives, which I agree with but does not fit very well with economics. Economists, from both left and right, have a rather universalist view of things that I disagree with. There is a paper I wrote for the *Journal of Post Keynesian Economics* back in 1993 on rational expectations (RE). Post-Keynesians have criticized RE for its incorrect use of probability theory. They are right, but even more important from my point of view is the phenomenon of subjective reality. People assess the world through the models they hold. They hold competing perspectives. That is what my 1993 paper is about and I am still interested in trying to get that concept into economics.

When I finished the international relations program I was lucky enough to be admitted to the Yale economics doctoral program. In economics, it seems to me there are two ways of seeing the world. One is through microeconomics, which places you squarely in the mainstream and the world of competitive general equilibrium theory. The other is macroeconomics, which back then was Keynesian economics. Not only did I have an inclination towards macroeconomics, but I thought that Keynes's view of the world was compelling. The competitive general equilibrium view of the world has always struck me as completely implausible. I remember in my first semester struggling through axiomatic choice theory, axiomatic production theory, and then coming to the general equilibrium model and the first welfare theorem with its claim that competitive markets produce Pareto optimal outcomes. That to me is a completely implausible description of a capitalist economy. I don't care how much you try and make the theory more realistic by adding more and more market failures, the underlying theoretical benchmark model is implausible. Consequently, I chose the route of macroeconomics and have been engaged in macroeconomics research ever since.

*What made you go to Yale and not to Cambridge, with Robinson, Kaldor, and all the others? Did you consider going there?*

I applied to Cambridge and was accepted, but this was a time when the American PhD had great prestige. I thought I would benefit more from an American PhD program, the

classes and the training, rather than the tutorial-centered program they had at Cambridge which I had already experienced at Oxford.

*Do you want to tell us a bit about the PhD program in Yale? We all know that you were mainly influenced by James Tobin.*

Tobin was the dominant intellectual figure in the department, and of course he shaped the macro program. I think he was truly an intellectual giant and he had a tremendous moral stature and standing. In retrospect, what is a bit tragic to me is that the Yale program was deconstructing even as I entered it. Tobin retired in 1983, but already by then neo-Keynesian economics was disappearing – you can see it in the changing content of the *American Economic Review*. In my view, the neo-Keynesians chopped off their own legs, which holds for Tobin too. There is so much of his work that I admire and my research agenda has drawn heavily on his ideas, but Tobin and the neo-Keynesians undercut themselves in terms of overall framework.

On the one hand, they embraced Solow's neoclassical growth theory, which rendered the long-run inconsistent with their macroeconomic model. And on the other hand, they embraced an interpretation of *The General Theory* as a special case based on nominal wage and price rigidities, which undermined their short-run theory of macroeconomics. Back then, Barro and Grossman's general disequilibrium framework was very important. It was a big part of our first semester macro course. It was clear that Tobin bought into both the neoclassical growth model and the general disequilibrium model.

Barro and Grossman basically captured Keynesian macroeconomics and pulled it back into the realm of competitive general equilibrium theory, thereby closing down the distinct macroeconomics view of economics. Barro and Grossman absolutely destroyed Clower's brilliant insights regarding Kahn's multiplier being the result of relaxing quantity constraints. They destroyed it by saying those quantity constraints were only the result of price and nominal wage rigidities. I have to admit that I was initially captured by their view and, when I was trying to find a dissertation topic, I spent a year reading all the competitive general disequilibrium literature – a lot of it written by French economists. Later, I even wrote a paper, a transitional paper in my thinking, published in the *Journal of Macroeconomics* in 1999. It is one of the few times a paper of mine has been published by a reasonable mainstream macro journal. It is a disequilibrium model with Fisher debt effects, but it is still couched in terms of disequilibrium analysis.

The Yale macro program made itself vulnerable by adopting neoclassical growth theory and the price rigidity interpretation of Keynes. It only took Friedman, Lucas, Sargent, and Wallace to come along and say 'well, what happens when we have price and wage flexibility?' That takes you pretty much straight back to classical macroeconomics and neoliberal flexibility policies. The door was rotten and the new classicals pushed on it and found it just came off the hinges.

*And also Tobin's funding from the National Science Foundation was cut off in 1982.*

I didn't know that. I thought he had taken a sabbatical year and gone to Minnesota where Sargent taught, which was a sign of the times. Hearing you say that, I guess the National Science Foundation thought his research program had run out of energy. In retrospect, I think Tobin was attached to the Arrow–Debreu competitive general equilibrium model but, rather than emphasizing microeconomic market failure, he sought to find reasons in monetary macroeconomics why things do not work out as the paradigm predicts.

For instance, there is his famous 1975 paper on Keynesian models of depression with deflation expectations, but without debt. In 1980 he introduced debt effects in his *Asset Accumulation and Economic Activity* – a brilliant book – but it was very late in the game and the new classicals had already effectively won. It is one of the books that have really influenced my thinking, but Tobin never quite escaped from the micro side of general disequilibrium analysis and from the Solow growth model when dealing with the long run.

*How did you move from neo-Keynesian economics, of the Tobinesque type, towards post-Keynesian economics?*

The person who put me in touch with the post-Keynesians was Basil Moore. I was an adjunct economics instructor at Wesleyan University and Basil Moore taught at Wesleyan. Coming out of the Yale school, I had an interest in money and he introduced me to both endogenous money and the post-Keynesian community.

*When was this?*

This was 1985–86.

*Were you still doing your PhD?*

Yes, I was. This made things difficult for me because I was interested in post-Keynesian ideas, but I had to finish my dissertation at Yale, so I had two research projects at once. My first article in post-Keynesian economics was on endogenous money and was published in 1987.

At the time I was still trying to finish my dissertation. I was interested in the need to disaggregate macroeconomics and my dissertation had three essays on multi-sector macroeconomics. One was an empirical essay on Okun's law, showing how the Okun coefficient depends on the composition of the economy and how it is also likely to be asymmetric in ups and downs. The second essay was on David Lillien's sectoral shift unemployment hypothesis which claimed the large rise in unemployment at the end of the 1970s and early 1980s was due to sectoral shifts. My dissertation essay challenged that by showing the sector shift component of unemployment was relatively stable so that the rise in aggregate unemployment was really due to aggregate shocks.

The third essay was on the Phillips curve and it draws on Tobin's wonderful presidential address to the American Economic Association given in December 1971, which is still very relevant today. Tobin lays out a verbal model of a multisector Phillips curve, a sort of disequilibrium Phillips curve. One of the things that Tobin says in the paper is that the Phillips curve is a concept in search of a theory. I think he was absolutely right. In the 1960s, it seems to me people really didn't understand that the Phillips curve was an empirical phenomenon attached to a theoretical model, and it needed a theoretical explanation. Friedman came up with a theoretical explanation, his monetary misperceptions theory of the Phillips curve. Tobin came up with the better idea that some sectors are at full employment while other sectors are at unemployment. As the economy speeds up, inflation, in his famous expression, 'greases the wheels of adjustment.' I provided a formal model of this idea, the key theoretical novelty being that the incorporation of inflation expectations varies by sector according to the state of unemployment. It was published in the *Scandinavian Journal of Economics* in 1994 and it completely anticipates all of the results in the famous 1996 paper by Akerlof, Dickens, and Perry. It's exactly the same reasoning, except that they have downward nominal wage rigidity. In my

model, faster growth of demand put sectors with unemployment on an escalator to full employment, thus reducing the duration of unemployment in those sectors; in the meantime faster aggregate demand growth will cause inflation in sectors with full employment, thus generating an aggregate empirical pattern which is the Phillips curve.

I have continued working on that idea, and I still think it is an important piece of inflation theory that post-Keynesians should embrace. I have always viewed inflation as being context-dependent. You can have conflict inflations as the UK had in the late 1960s and 1970s; you can have demand pull inflation of the sort I just described; you can have imported inflation, as in Latin America; and you can have bottleneck inflation. The implication is we need many different theories of inflation.

*So far you have told us how you were influenced by the neo-Keynesians, but you have not told us why you got interested in post-Keynesian economics; it's one thing to meet someone who is in a school of thought, it is another to start writing within that paradigm.*

Well, endogenous money was the initial reason. I found the theory of endogenous money immediately compelling. I was always interested in credit, and where is credit in the neo-Keynesian model? It's not there. When you read *Asset Accumulation*, Tobin's explanation of why the economy might not adjust is based on Fisher debt effects. Endogenous money was the natural way to make the connection and bring credit into the system.

The other feature that drew me to post-Keynesian economics was income distribution, which is not very present in neo-Keynesian macroeconomics. In the Keynesian consumption function there is some room for distribution effects because of the diminishing average propensity to consume, but that formulation is inconsistent with the empirical data. There is also no class struggle in the Keynesian model. Remedying those features leads to the Kaleckian model. Part of the tragedy at Yale was that Kalecki was not taught. Tobin was very honest in his teaching. He taught rational expectations macroeconomics scrupulously honestly, while also providing a critique. However, no attention was given to the post-Keynesians, except for a bit on Kaldor's 1956 Keynesian theory of income distribution. Actually, I initially attributed to Kaldor what I later learned were Kalecki's ideas. The idea of different propensities to save out of wages and profit, related to a class structure, was immediately appealing. Income distribution matters for aggregate demand, and income distribution matters for how endogenous money gets credit into the system. The two are connected, and debt and income distribution are also connected. Connecting the pieces naturally leads in the direction of post-Keynesian thinking.

*You finished in Yale in 1990?*

I started teaching at Amherst College in 1987 and then I moved to the New School for Social Research, New York, in 1991. At Amherst I was still working on my dissertation. I worked on endogenous money, and I also wrote a paper on the microfoundations of the Keynesian multiplier. I think it is a very good paper but it is published in a completely invisible journal, in the *Atlantic Economic Journal* in 1990.

After that, at the New School, I pursued a two-pronged research program on endogenous money and the Phillips curve, while also beginning to write about inside debt effects. I wrote a paper that was published in 1994 on business cycles resulting from debt effects, debt burdens and transfers of debt service income. I think the first citation of it appeared around 2008 or 2009!

Unfortunately, in the early 1990s there was not very much interest in debt. That was one of the problems I had at the New School. Apart from Ed Nell, and to a lesser degree Willi Semmler, the department had no interest in endogenous money or endogenous finance. Back then, that was a major weakness of the Kaleckian research program pursued by people like Lance Taylor. Lance was focused on the mark-up, income distribution, and the real economy. He did not have an interest in endogenous money or credit. Even Minsky, for whom debt is central, was blind to the aggregate demand effects of debt resulting from debt service transfers. Instead, Minsky has debt operating as a financial constraint on investment spending, a bit like the credit-rationing world of Stiglitz and Weiss. This is not what I was interested in. I was interested in the impact of income transfers between borrowers and lenders.

*The book you published in 1996, was that a result of the courses you were giving at the New School?*

Yes, the book was based on my course on post-Keynesian economics. I needed a book to try and get tenure so I raced to get it done. It's pretty good, but I think the weakest chapter is the one right in the middle where I try to tie everything together. Actually, the program that you (Marc Lavoie) and Wynne Godley accomplished so successfully was what I would have liked to work on. When I was denied tenure at the New School I had to substantially stop doing theoretical economics. My goal had been to try and take the Yale school model – Tobin's 1982 stock-flow consistent model – and add endogenous money and my debt model with debt service transfers. However, I had to stop after I did not get tenure, and from 1997 through to 2005 I shifted to doing empirical and policy economics, publishing in magazines such as *Challenge*.

I was told at the end of 1996 that I had been denied tenure and since a job offer to become the quasi-chief economist at the AFL-CIO immediately came up, I took it and left. My time at the New School was bitter-sweet. On the one hand, I learned a lot and I got a valuable exposure to Kaleckian economics. On the other hand, I was treated pretty badly. I do not think there was much interest in my endogenous-money-debt-research program from the likes of Lance Taylor, David Gordon, and John Eatwell. And then, after the East Asian financial crisis of 1997, John and Lance abruptly changed their minds and took over that program. The East Asian crisis was really transformative for them. They suddenly realized: 'Wow, we need to take debt into account,' whereas in 1996 they didn't have much recognition or enthusiasm for my work. I also think David, John, and Lance wanted to reassemble their Harvard graduate student Camelot community, as they were always trying to hire Sam Bowles and Duncan Foley, whom they eventually got. So my slot was spoken for and I had to move on.

*Since then you have been in Washington doing economic policies among other things?*

Yes. I see myself as being a bit of an accidental tourist in Washington because it is not where I wanted to be. I wanted to have an academic career. I was unable to do so, so I did economic policy as the next best thing. I do not come from a trade union family, but from my understanding of the macro economy I think it is impossible to have shared prosperity without trade unions. They are the absolutely necessary institution for sharing productivity and ensuring workers get a fair share, so it was sort of a natural fit in terms of where my understanding of macroeconomics had evolved.

I was very happy to come to the AFL-CIO. I met Ron Blackwell who has been a very important intellectual influence on me. He really helped me understand and come to

grips with the question of neoliberalism and its effects. Ron really liked my book *Plenty of Nothing*. I wrote it before I came to the AFL-CIO and it was in proof form at that stage. It has also been enriching to get to know people like Jeff Faux who has such a deep understanding of the political context of economic policy. It is very enriching for theoretical economists to engage in policy economics. I have long thought that is a big advantage for mainstream economists. You get new ideas and you get to see new perspectives, new angles on things. I think post-Keynesians are handicapped by not having access to these conversations. That's a nice thing about Germany, where we are doing this interview. Here, the Macroeconomic Policy Institute (IMK) has provided a bridge that links academic economics with policy. Theorists can then work on those issues in a way that is more organically connected and relevant to the real world. I have really enjoyed that aspect of doing policy work.

*What did you learn while you were at the AFL-CIO that had an impact on your future career?*

All the time, I was trying to write and reach people. How do you persuade people? I think persuasion should be a very important part of the endeavor of every economist. It is probably the main thing. Working at the AFL-CIO compelled me to make my work and ideas policy-relevant, strengthened my understanding of the bridge between theory and policy, and forced me to communicate clearly as if I were trying to reach students. Distilling ideas in this way really sharpened my analytical understanding, as well as strengthening my ability to communicate.

*You have worked also for a committee that dealt with China. So what are your views about China today?*

I got deeply into the globalization conversation while at the AFL-CIO. Unions were engaged with globalization long before most academic economists. Working at the AFL-CIO, I saw how there is a dynamic whereby the real world drives the academic conversation rather than the academic conversation driving the real world. Of course, there is also feedback between the two. Academics can help redirect things, but there is a tremendous impetus from the real world.

I got involved with the China Commission through my work on globalization. I was interested in China, or more specifically in China's impact on the USA. There has been a big change since the 1970s. In the 1970s, progressive concern was about the North's impact on the South, and of course that has continued through the Washington Consensus debate. But the situation has also changed significantly. The new system is designed from the North. It co-opts the South; it co-opts their political leaders. The elites of the South join in and, together, elites in North and South have created a system that feeds back to negatively impact Northern workers. Given this, my interest in globalization and China is a natural fit with my views on macroeconomics and my association with trade unions.

*You have also been working for the Soros Foundation for a short period.*

That was for a very short period of time. I was, for a while, the Director of the Globalization Project, but it was really just a title. I was the project. I was an economic adviser of sorts to Soros, but Soros was in New York. I was in Washington, and there was little conversation between us. Instead, he turned to people like Joseph Stiglitz and Jeffrey Sachs who were right next door, more famous, had far superior policy connections, and who did a better job engaging with him than I did.



*Your most recent work, and your most recent two books, are on financialization. How did you get interested in that and what was the motivation to do these two books?*

Well, my interest has been a continuously evolving thing. My interest in endogenous money and debt and the bubble of the 1990s led naturally to an interest in financialization situated in a Kaleckian framework in which income distribution matters. In 2005 I wrote an article on Alan Greenspan in which I claimed he was overseeing the creation of a new pattern of business cycles. That became the take-off for my interest in financialization. Ron Blackwell was also very important here because he helped me understand that financialization is not just about expansion of financial markets: it is also, or perhaps even principally, about corporate control, changing the goals of corporations and how corporations behave.

Actually, while I was an academic I had already done some work on this. I had written a paper on managerial herd behavior published in 1995 and another one on managerial short-termism published in 1997. Both of these were microeconomic papers, reflecting my neoclassical micro inclinations, showing how incentives can be structured to generate certain kinds of behavior.

Gerry Epstein was also very instrumental in raising the salience of the concept of financialization. And then it all began to congeal and make sense. The data is screaming at you that there is something going on, which is why the concept took off. Although many of us were edging towards thinking in these terms in the early 1990s, I did not see just how powerful it was, how transformative it was.

Minsky's followers were also raising the salience of finance by the early 2000s, although his focus was always instability rather than power and income distribution. We are all much more appreciative of Minsky now: I certainly am. In my 1994 paper I grasped Minsky's basic cycle. By the mid 2000s I became aware that his was a deeper and richer theory about a long-run phenomenon, and that this theory provides a very nice vehicle for framing what is going on with financialization. It all came into the open with the crisis of 2008. This long cycle – Minsky's super cycle, as I called it in 2011 – is in his earlier work. But his later work seems to be more and more about his basic cycle – the hedge, speculative, and Ponzi finance dynamic that is too short a time frame. I think that shift of emphasis may have been because he was influenced by and was competing for attention with the neo-Keynesians and Samuelson–Hicks multiplier-accelerator-type models that produce shorter cycles. I think he really was interested in this long-run phenomenon which, by the way, is the reason why we only see financial crises so occasionally. When one reads the later Minsky, it is easy to think that he is dealing with a short-run financial business cycle model. However, his explanation of crisis actually involves many business cycles.

*Given your career as an academic and also as a policy adviser, what is your recommendation to post-Keynesians and also the younger generation to improve the impact of economic policy-making?*

That is a tough question. I would say stay informed and have an historian's perspective on things. With regard to theory, I think we need to confront more forcefully the neoclassical position and show the absolute impossibility of the world it describes. It is not because of market failures or anything like that. The neoclassical model is an ontological impossibility.

Actually, I am hoping to work on this a couple of years down the road. I have been accumulating some notes aimed at a critique of the foundations of neoclassical microeconomics, which have now surfaced as the foundations of what they call New

Keynesian macroeconomics or what I prefer to call New Pigouvian macroeconomics. We need to discredit those microfoundations. New Keynesian macroeconomics engages in imitating Keynesian phenomena while denying the world Keynes was describing. So that is my view of the theoretical agenda. However, it is also a real world agenda because the aim is to change the conversation. Younger scholars should resist just adding an extra wheel to the existing model, which is a tremendously tempting thing to do, especially when you live under the pressure of ‘publish or perish.’

*The interview was conducted by Eckhard Hein and Marc Lavoie in July 2015. We thank Paul Beckta for the transcription.*

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