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‘People will come searching for us post-Keynesians if the political atmosphere is encouraging’



Interview with Robert A. Blecker

Robert A. Blecker is a Professor in the Department of Economics, American University (AU), Washington, DC, USA. He is also a Research Associate of the Economic Policy Institute (EPI), Washington, DC, USA and a Fellow of the Forum for Macroeconomics and Macroeconomic Policies (FMM). He has published widely on open-economy post-Keynesian models, North American integration, the Mexican economy, global imbalances, export-led growth, and trade theory and policy. He serves on the editorial boards of the *European Journal of Economics and Economic Policies: Intervention* as well as the *International Journal of Political Economy*, *International Review of Applied Economics*, *Investigación Económica*, *Metroeconomica* and *Review of Keynesian Economics*.

The first question we would like to ask is: how did you get interested in heterodox economics and post-Keynesian economics in particular? What were your first contacts with post-Keynesian economists? Who got you interested in the subject?

My first exposure to any kind of economics was probably a bit unusual: I read Thorstein Veblen's *Theory of the Leisure Class* (1899) for a high school history course in the early 1970s. I was really captivated by Veblen's concepts such as conspicuous consumption and pecuniary emulation, and I was persuaded by his general view of consumption as a means for people to express their positions in modern social hierarchies. When I started my undergraduate studies at Yale in the fall of 1974, I signed up for an economics class expecting it to be like what I now know was the institutionalist approach of Veblen. Instead, I found myself in a course on neoclassical microeconomics in which we learned the theory of the utility-maximising consumer, how markets were optimal and how the consumer was sovereign. Because I had read Veblen, I already knew that this was nonsense.

Fortunately, I had a somewhat open-minded professor, Raymond Powell, a scholar of comparative systems who exposed us to other points of view (for example, he had us debate markets versus planning) in the introductory courses. Although he was very neoclassical in microeconomics, Powell was a solid mainstream Keynesian in macroeconomics (after all, Yale was the home of James Tobin). After that introduction to mainstream economics, I began searching for alternatives. I started by reading more economists in the institutionalist tradition, like John Kenneth Galbraith, then I read some in the neo-Marxian tradition, such as Paul Baran and Paul Sweezy, and eventually I read some of the original work of Karl Marx.

In my second year, I took David P. Levine's courses on the history of economic thought and Marxian economics. Later, I also took a graduate seminar with Levine, in which we learned about the Cambridge school and what were then called the 'neo-Keynesians'. In that course, we read the works of economists such as Joan Robinson, Michał Kalecki and Josef Steindl, among others. In addition, Stanford professor Donald Harris visited Yale in my senior year. I took his course on 'The Theory of Capitalist Development', in which we studied Marxist, Schumpeterian, and what I later realised were post-Keynesian perspectives (although the term was not much used at the time).

So you then went to Stanford University to study with Donald Harris?

Yes, I went specifically to work with Don. I arrived at Stanford in the fall of 1979 when the heterodox economics program there was probably at its peak. There was an older professor named John Gurley, who had previously been an orthodox monetary economist and then became a Maoist; he taught Marxist economics to undergraduates, but didn't do any research. Duncan Foley had been at Stanford in the mid 1970s, but he had been denied tenure about a year before I arrived. That left Harris as the only professor doing serious theoretical work in heterodox economics, or 'political economy' as we called it at the time. Many of the Stanford economics professors were quite conservative or reactionary, partly as a result of the influence of the Hoover Institution, but there were others who were intellectually interesting and innovative scholars especially in economic history and micro theory. And Don did a terrific job of compensating for his isolation at Stanford by bringing in a steady stream of visitors (including Paul Davidson and Hyman Minsky, among many others) to make presentations in our political economy seminar.

Don was a very inspiring teacher and mentor. He taught a graduate course on theories of growth and distribution, based on his book *Capital Accumulation and Income Distribution*, which had just been published in 1978. We learned a lot about the Cambridge capital controversies, and also about comparisons of neoclassical, neo-Keynesian and neo-Marxian approaches to growth theory. Don was also interested in economic development and Latin America, which was a plus for me.

Coming to your PhD work, in Stanford you then specialised in international economics?

Yes, partly, but I also studied other fields. We had a field called 'alternative approaches to economic analysis', which consisted of Harris's course on growth and distribution plus a course on economic thought. I took that field plus international economics (trade and finance). For my third field, I went outside my department and did a minor in political science, where I studied international political economy with Robert Keohane and political economy of development with Richard Fagen.

There were a few other graduate students interested in alternative economics?

Actually, there was a large cohort of such students at Stanford at the time. We were a very disproportionate part of the graduate student population in the late 1970s and early 1980s – easily around a fifth to a quarter of the students. For example, Steve Fazzari, Tracy Mott, Maria Floro and Nilüfer Çagatay were part of that cohort, just to name a few. But by the mid 1980s, the heterodox programme began to shrink. I think the attitude of the admissions committee was different, and also the interests of the incoming students were different. Of course, it was the Reagan era, so it was a very conservative time. After I left Stanford in 1985, I heard that the few remaining students in alternative economics were being ostracised by their classmates.

It was Harris who got you interested in Kaleckian models?

It was Harris and Levine (I first read the original works of Kalecki in Levine's seminar at Yale). What we would now call neo-Kaleckian models (for example, the papers by Bob Rowthorn and Amitava Dutt) were starting to come out in the early 1980s, but some fore-runners of those were Don's article in the *Australian Economic Papers* in 1974 and Tom Asimakopulos's paper in the *Canadian Journal of Economics* in 1975. Those were very inspirational papers for a lot of us. Also the Thirlwall model of balance-of-payments-constrained growth came out in 1979, which added a new twist to the Kaldorian tradition of export-led growth analysis, and Don steered me to the work of John Cornwall who emphasised that tradition. I had also studied structuralist development economics, which led me to the work of Lance Taylor – who was doing essentially neo-Kaleckian modelling but called it 'structuralist macroeconomics'. So all of these ideas were 'in the air' at the time.

The early neo-Kaleckian models were called 'stagnationist' because they implied that a higher profit mark-up or profit share would lead to lower rates of utilisation and growth, in line with Steindl's 1952 theory of stagnation in monopoly capitalism. Or, in the terminology that has become more common recently, they implied that both demand and growth were 'wage-led'. However, the Marxian and Robinsonian sides of my training made me question whether higher unit labour costs and lower profits might not be so good for capitalist accumulation. As an international economist, it occurred to me that higher labour costs could have adverse effects on the trade balance. By assuming a flexible mark-up rate that was influenced by relative labour costs, I was able to construct an open-economy neo-Kaleckian model in which the negative effects of higher labour costs on net exports could possibly outweigh the positive effects on domestic demand, and this could reverse the stagnationist or wage-led outcome.

In my PhD dissertation, I tried to combine this neo-Kaleckian model of an open economy with a Thirlwall model incorporating a balance-of-payments constraint. The referees at the *Cambridge Journal of Economics* later advised that I should take out the Thirlwall part and keep the neo-Kaleckian part, in what became my 1989 *CJE* paper. Meanwhile, Amit Bhaduri visited Harvard and Stanford while he was in the US in the mid 1980s, working with Steve Marglin at the former and Don Harris at the latter. I saw some early versions of what became the Bhaduri–Marglin model presented at a seminar at Stanford around 1985 and later at the Trieste summer school in 1987. I could see that they were focusing on the investment function, and the advice I got from them was to concentrate on the open-economy aspect, which was less important in their story.

So there was a clear division of labour between Blecker and Bhaduri–Marglin in the late 1980s when both papers came out. Then, after a period of four or five years, the empirical work on these papers got started. Has there been anything related to your paper in between? Because at the time the reference was usually to Bhaduri–Marglin (1990), not so much to your paper which was published one year earlier.

There's no doubt that the Bhaduri–Marglin model received more attention. They were better known internationally – both were already top scholars in heterodox economics, while I was a 'newly minted' PhD – so their work received instant recognition. Nevertheless, some scholars in the field (such as Lance Taylor and Amitava Dutt) did recognise and cite my work, and a few of my former AU graduate students went on to do innovative work in neo-Kaleckian modelling (for example, Stephanie Seguino, who adapted it to construct gendered macro models). At my end I continued to publish various neo-Kaleckian, post-Keynesian or structuralist models of open economies throughout the 1990s and

2000s, including work on North–South trade (Blecker 1996), the international saving–investment correlation (Blecker 1997) and gender wage gaps (Blecker/Seguino 2002).

Do you think that the reason the Bhaduri–Marglin model had more success is that you still had the rate of profit as one of the variables in the investment function, which is always hard to measure, whereas they had the share of profits?

Yes, Bhaduri and Marglin did come up with an analytically very neat model, which emphasised the magnitude of the profitability effect on investment compared with the propensity to save out of profits as the key determinant of whether demand is wage-led or profit-led (for a closed economy with no saving out of wages). They also came up with the clever terminology of ‘stagnationist’ and ‘exhilarationist’ regimes to distinguish the wage-led versus profit-led cases, respectively.

Although it was theoretically clever and convenient, Bhaduri and Marglin’s focus on the profit share may ultimately prove to have been somewhat of a distraction. After all, income distribution is endogenous, and the real question is how the underlying social forces, structural changes and policy variables that affect both distribution and demand cause changes in endogenous variables such as utilisation, growth, employment, real wages (or the wage share) and so on. My work along with that of various others shows that the direction of these effects can vary considerably depending on the source of a ‘shock’ to the system and other feedbacks (for example, financial and technological).

Once the empirical literature estimating neo-Kaleckian models developed some robust findings in the 2000s, I think some of the advantages of my approach became clear. First, many studies had trouble finding large or significant effects of the profit share on investment, which is not surprising since most theories of investment do not imply that this variable belongs in an investment function. A post-Keynesian investment function inspired by the work of Hyman Minsky (of the type often estimated by Fazzari, and which I used in a study of US manufacturing investment in 2007) would include an accelerator effect, a cost-of-capital term and a realised cash flow variable, but not the profit share *per se*. Second, by about ten years ago it became clear from the empirical studies that the decisive factor in determining whether most countries have wage-led or profit-led demand is usually the impact of labour costs on net exports, not investment. So I think my approach has achieved more recognition in the past decade as it provides a better foundation for understanding these empirical findings.

When did you go to Washington to American University?

I started at AU in 1985 and I have been there, except for sabbaticals and other leaves, ever since.

Were there any heterodox colleagues with you at American University over those more than 30 years?

Yes, the AU department is very pluralistic, although only one colleague (Alan Isaac) has done research specifically in the post-Keynesian tradition. At the time I arrived, the AU economics department was divided into two camps: political economy radicals and mainstream economists. The political economy camp included everything from Institutionalists (like Jon Wisman and the late Jim Weaver) to orthodox Marxists (like John Weeks). My colleagues also included one founder of URPE (the Union for Radical Political Economics), Howard Wachtel, and other URPE members such as Robin Hahnel and John Willoughby. Over the years the department has evolved, for example through the arrival of several Feminist economists (Maria Floro, Mieke Meurs and the late Barbara Bergmann) and many

other good colleagues, and we are no longer divided into two warring camps. On the whole, AU has been a very congenial place for me.

You also did some more empirical and policy-oriented work on open economies?

I did a lot of work for the Economic Policy Institute (EPI), a progressive think-tank in Washington, DC, in the 1990s on topics such as the US trade deficit, NAFTA (North American Free Trade Agreement), and trade policy and global finance more generally. I wrote a book for the EPI in 1992, in which I critiqued the then-popular ‘twin deficits’ hypothesis that blamed the US trade deficit primarily on the budget deficit. EPI later commissioned me to write a book, *Taming Global Finance* (1999), in which I surveyed the theories and debates about financial liberalisation, the causes of financial crises and reform of the ‘global financial architecture’ as of the late 1990s.

Coming back to the other half of your PhD project, so to speak, what happened to your work on the balance-of-payments constraints and the attempted synthesis of that with the more Kaleckian approach?

Let me leave the balance-of-payments-constraint aspect aside for a moment. The second part of my dissertation was actually an empirical study of how profit mark-ups were squeezed by import competition in the US steel industry and what were the consequences for investment in the sector. My thinking on this topic was especially influenced by Alfred Eichner’s book, *The Megacorp and Oligopoly* (1976). Eichner’s model essentially depicted a closed economy, pre-financialisation, and ignored international competition.

By the time I was doing my research in the 1980s, import competition was limiting the ability of big oligopolistic firms to charge the kind of mark-ups they had been setting previously. So, the second half of my dissertation was a case study at a micro level of what I was modelling at the macro level in my neo-Kaleckian work. In my regression analysis for the steel industry, I found that there was a structural break when imports achieved a certain threshold level in the US market in the mid 1960s, after which either a decrease in import prices or a rise in the import penetration ratio had a significant negative effect on the profit mark-ups of US steel producers.

When was this published?

The core study came out in the *Journal of Post Keynesian Economics* in 1989, and then a second paper came out in the *International Review of Applied Economics* in 1991. The latter paper combined various elements from Kalecki, Steindl and Eichner’s models of oligopolistic firms – extended to an industry facing international competition – to create a picture of cumulative causation (in this case, cumulative decline) in the US steel industry. Thus, if lower-cost imports come in and are more competitive than domestic products, they squeeze the profit margins of domestic firms, which are then less able to finance investment. If firms invest less, they fail to increase their productivity and that feeds back to further worsen their competitiveness, which then invites more imports and so the cycle gets worse.

And what happened to the balance-of-payments-constrained-growth work?

I came back to that approach several different times in my career when it seemed like the right framework for addressing questions I was interested in.

In the early 1990s, I found Thirlwall’s theoretical approach helpful for fleshing out my ideas on how the increased US trade deficit reflected a loss of competitiveness of US-based industries. Using the Thirlwall model as a foundation and combining it with the assumption

of mark-up pricing, I represented a lack of competitiveness as a situation in which, in order to balance a country's trade at a given relative wage, the country's growth would have to be constrained to be below a full employment growth rate (the so-called 'natural rate of growth'). Otherwise, the country would have either a continuously falling relative wage or an increasing trade deficit over time. I first presented this conceptual framework at a conference at the Federal Reserve in 1993, and later I published an extended version of it in the *JPKE* in 1998.

Then, in the late 1990s and early 2000s, I got interested in the hypothesis of a 'fallacy of composition' as a limitation on the prospects for successful export-led growth in large numbers of developing or 'emerging' countries. I developed an extension of the Thirlwall model, combined with the Almost Ideal Demand System of Deaton and Muellbauer (1980), to model a group of developing country exporters all competing for the same, limited markets in the advanced economies. This was published in a post-Keynesian conference volume in 2002. Then, in empirical work co-authored with Arslan Razmi (a former AU PhD student who became a professor at the University of Massachusetts, Amherst) (Blecker/Razmi 2010), we found econometric evidence showing that countries whose export prices or real exchange rates shifted unfavourably relative to other, rival exporters suffered reduced GDP growth in addition to slower export growth.

More recently, I published two papers co-authored with a Mexican post-Keynesian colleague, Carlos Ibarra (Blecker/Ibarra 2013; Ibarra/Blecker 2016), in which we extended the Thirlwall model to incorporate imported intermediate goods that are used in export production and re-evaluated the evidence for how trade liberalisation, NAFTA and other policy shifts and global events (for example, the rise of China) impacted the balance-of-payments constraint in the Mexican economy.

On the theoretical side, I had long wrestled with how to reconcile the Kaldorian model of export-led growth with the Thirlwall model of a balance-of-payments constraint in teaching these models to my students at AU. I presented a paper on this topic at the FMM post-Keynesian summer school in Berlin in 2009, a revised version of which appeared in the *Oxford Handbook of Post-Keynesian Economics*, edited by Geoff Harcourt and Peter Kriesler, in 2013. Then, my involvement in debates over the validity of the Thirlwall model and the role of relative prices (or real exchange rates) in the growth process for open economies led to an invitation to write a review of these debates for the *European Journal of Economics and Economic Policies: Intervention* in 2016.

What is your current main focus of research?

I am continuing to think about the wage-led versus profit-led growth issues. If most economies are profit-led, as claimed by those who have found 'Goodwin cycles' in the data, why isn't the world economy doing better at a time when income is being redistributed to profits almost everywhere? Wage shares are falling and yet most economies (except in Asia) are growing slowly or experiencing long-term stagnation. My argument is that the Goodwin cycle result is really a finding (at best) about short-term business cycles, not about longer-term growth trajectories; it harks back to the 'profit squeeze' theories of business cycles of the 1970s. I wrote about why I think demand is more likely to be wage-led in the longer term in my contribution to the *ROKE (Review of Keynesian Economics)* symposium on the Bhaduri–Marglin model in 2016.

Suppose you have a two-equation model of growth and distribution, with an aggregate demand function and some kind of distributive curve. If they are sloping the way the neo-Goodwinians say – that is, in wage share and utilisation space there is a downward-sloping aggregate demand and an upward-sloping distributive curve – then a long-term decline in both growth and the wage share has to come primarily from a downward shift in aggregate

demand that dominates the shift in the distributional relationship. We've had a lot of things that have depressed aggregate demand in the last two decades, including shifts toward inflation targeting in monetary policy and austerity or balanced budget rules in fiscal policy. Also, several studies have found that financialisation depresses aggregate demand, especially investment.

I think people telling the neo-Kaleckian story have been thinking the other way around: There has been a shift in the distributional relationship against labour, and the economy has moved down and to the left along an upward-sloping aggregate demand curve. That's certainly another possible story. But I think a lot of the empirical work has not adequately distinguished whether what's being found are changes in the equilibrium levels of the variables as opposed to shifts in the underlying relationships. And I do think that, oddly enough, we post-Keynesians may not have emphasised macro policies enough in terms of their effects on distribution in addition to demand or growth.

I'm also working again on NAFTA issues, since Donald Trump has launched a renegotiation of this trade agreement while threatening to withdraw the US from it. I am working with two Mexican co-authors (Juan Carlos Moreno-Brid and Isabel Salat) on why the neoliberal policy regime enshrined in NAFTA did not lead to the convergence of the Mexican and US economies, as had been promised by its advocates, but instead contributed to greater inequality and long-run stagnation in both countries. And in a longer-term project that is going all too slowly, I'm working on a heterodox macro textbook with Mark Setterfield of the New School.

This leads to two final questions. What is your view on the current state of post-Keynesian economics? And what is your recommendation for the younger generation?

Well, these two questions are very much linked. I think we're at a kind of a crossroads or in a contradictory situation. On the one hand, we know we are constantly under attack in academia. We have trouble getting hires of people like ourselves, especially at the more elite institutions. We can't get published in the top journals. So we are always feeling marginalised. Yet at the same time I feel this tremendous excitement and energy, which comes from the fact that there is a younger generation developing heterodox ideas in economics and there is an international spread of those ideas especially in Latin America.

I also believe that the long-term trajectory has a lot to do with politics and political cycles. The revival of all kinds of radical and heterodox economics without question goes back to the 1960s – to the political upheaval, the anti-war movement in the US and the student uprisings around the world. That was when students began demanding to have radical professors. Also, many professors got radicalised at that time. People like Duncan Foley, Steve Marglin and Sam Bowles started out as mainstream economists and they changed because the political environment changed and they began to have critical thoughts about what they had been taught and started to work in new directions. So, a lot depends on the political environment.

If people start searching for solutions to today's economic problems, they're going to find us, the same way that our generation found role models in people like Baran, Sweezy, Davidson, Minsky, Cornwall, Eichner, Bowles and various others. People will come searching for us post-Keynesians if the political atmosphere is encouraging.

And do you think that post-Keynesians are well prepared if this political option comes up? Or what would you consider to be areas of research, which would need to be further developed?

I think this sort of preparation is already starting to happen. Younger post-Keynesians and heterodox scholars need to deepen themselves in quantitative methods of all kinds. What

we do in our empirical work is not so different from what the ‘mainstream dissenters’ (as Marc Lavoie calls them) do. It’s just econometrics. But we need to learn the more advanced tools and use the best techniques. And it’s more than econometrics now. It’s also agent-based modelling, stock-flow consistent modelling and the use of large micro-level data sets to research macro questions. These are all very exciting methodologies. And this is already happening. The younger people are taking the ideas that our generation (and previous generations) developed and extending and transforming them with all these methods into new areas or directions. For example, at this summer school we’ve seen presentations that are building post-Keynesian specifications of macro and financial relationships into models that also look at climate change, sustainability and emissions. I think that’s very exciting work.

I am also encouraged by the fact that many mainstream economists are now taking up our ideas, including links between distributional equity and growth, the stagnating impact of increased monopoly power and even endogenous money (although, unfortunately, usually without citing us). But we have something to offer that the mainstream doesn’t. Although the mainstream dissenters have poked considerable holes in orthodox theories, by showing how they don’t hold in practice, they lack an alternative unifying framework which is something that we can provide. We also have to keep building our institutions, like the FMM, the Brazilian Keynesian Association, and the various organisations of heterodox economists in the US and Canada. It’s really vital to have journals like this one, as well as the *CJE*, *ROKE*, *JPKE*, *Metroeconomica* and various others. Overall, I’m cautiously optimistic. But it’s a struggle as it always is.

Okay, well thanks a lot Robert.

This interview was conducted by Eckhard Hein and Marc Lavoie in Berlin in August 2017, during the 6th FMM International Summer School on Keynesian Macroeconomics and Economic Policies. We thank Ryan Woodgate for the transcription.

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