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‘The vigorous critique of the neo-Kaleckian or post-Kaleckian growth model is a measure of its success’

Interview with Marc Lavoie



Marc Lavoie is a Senior Research Chair of the University of Sorbonne Paris Cité, located at the University of Paris 13, and he is a member of the Centre d'Économie de Paris-Nord (CEPN), France. Between 1979 and 2016 he taught at the University of Ottawa, Canada, where he is now an emeritus professor. He received a 'doctorat honorifique' from the University of Paris 13, France, in 2015. He is one of the managing editors of *Metroeconomica* and of the *European Journal of Economics and Economic Policies: Intervention*, and he is an IMK and FMM fellow. His latest work is *Post-Keynesian Economics: New Foundations* (2014) – an exhaustive account of post-Keynesian economic theory – for which he received the 2017 Myrdal Prize of the European Association for Evolutionary Political Economy.

How did you get interested in post-Keynesian or heterodox economics and how did you get in contact with post-Keynesian economists?

I was a student at Carleton University, in Ottawa. I had chosen to go to this English-speaking university because I feared that I would be frustrated by not getting enough classes in French at the University of Ottawa, which is a bilingual university. At Carleton I knew everything would be in English anyway. In my fourth year, there was an honours seminar which was run by Tom Rymes – T.K. Rymes. Every week or so there was a different topic, Friedman, Keynes, Patinkin, Jensen, Coase ... As I recalled elsewhere (Lavoie 2013), Rymes spent two weeks on the Cambridge controversies, but we got so confused that we all thought he did not have a clue what the Cambridge controversies were all about; only later did I discover that he had written a whole book about them (Rymes 1971)! Rymes also talked about Kaldor's, Robinson's and Pasinetti's growth models, and that is when I got attracted to post-Keynesian economics.

This was still your undergraduate studies?

Yes indeed, and in 1976 I had decided to go to Europe to do my graduate studies. I tried to get a scholarship from various countries, and finally got one to go to the University of Brussels, where it turned out they specialized in general equilibrium theory; in the meantime I found out I had been accepted at the Canadian residence of the *Cité internationale universitaire* in Paris, so I figured that since my lodging problem was solved I might as well go to Paris without the scholarship, and benefit from the fact that there was much better fencing in Paris than in Brussels. So I went to Paris.

So the choice of universities was not guided by what you learned with Rymes, but was guided by your main or second occupation as a national team fencer? Or did you become a good fencer after your move to Paris?

Well, I participated at the Olympics in Montreal in July 1976, so Paris was after the Olympics. I did not know then that most Canadian students who were serious about economics would go to good American universities. I wanted to go to Europe essentially because of the fencing.

When you were in Paris, how does the story continue with you becoming one of the leading post-Keynesian economists of our time?

I was in Paris between 1976 and 1979. My former tutorial supervisor at Carleton University, Gilles Paquet, had given me the telephone number of a young university lecturer, his name was Frédéric Poulon, a French Circuitist. I met him and he encouraged me to go to Paris 1. I had interviews at Paris 2 and Paris 1; Paris 2 did not look very friendly, and at Paris 1, I met the director of the macroeconomics master's program there – Pierre-Yves Hénin. He looked rather unfriendly as well, until I told him that I had read a book by Bent Hansen (1970), on the basics of general equilibrium theory, at which point he became very enthusiastic about my candidacy, telling me right away that I was accepted. It turned out that in this master's program, Hénin was the only teacher who was neoclassical. He was into Keynesian disequilibrium theory of the Benassy and Malinvaud type, whereas all the other professors were truly into Keynesian economics.

Who was the professor who pushed you most during this period in France?

My supervisor for the master's thesis was Bernard Ducros, but because he was giving advice abroad, he was often being replaced by an assistant professor – Alain Parguez. Alain was quite a character when he was teaching and so he was fun to watch, and I got along with him and Frédéric Poulon. We would go and jog together around the *Cité internationale universitaire*.

Who was your PhD supervisor and what was the title of the thesis?

It was Bernard Ducros and the title of the PhD thesis was: Investment, finance and income distribution.

This was already in a more post-Keynesian vein?

Yes, it was completely in the post-Keynesian vein, or what Giuseppe Fontana would call the monetary theory of production. It was influenced by the French Monetary Circuit school. My parents were living in London at the time, so I would often go and see them, and go to the bookstores there. I bought a lot of books that were classics like *The Accumulation of Capital* by Joan Robinson (1956), the book by Alfred Eichner (1976), *The Megacorp and Oligopoly*, Minsky's (1975) book called *John Maynard Keynes*, the book by Jan Kregel (1973), *The Reconstruction of Political Economy*, and Paul Davidson's (1972) book, *Money and the Real World*. I brought all these with me on a one-month trip to Italy, in Padova, in 1977. I went there, again just for fencing, because the club in Padova had three young sabre fencers who were on the verge of making the Italian national team and they were about my age. I was on my own and, besides training, I had a lot of free time, so I carefully read all those books.

So this was the period when you still were very active in fencing?

Yes, by 1977 I had won the Canadian national championship three times in a row, and had been on the national team since 1973. I remained on it until 1984.

When did you finish your PhD in Paris?

The thesis defence was in May of 1979.

And you got your first job in Ottawa?

Yes. I had sent my CV to various places, General Motors Canada and a few of the big banks in Canada, and I had applied to a couple of universities, one in a small town called Three-Rivers and the other one in Ottawa. Apparently I was ranked second at both universities, but when I arrived back in Canada my aunt told me that the Chair from the Economics Department at the University of Ottawa had kept phoning and asking when I would arrive. What happened is that they had sent me a letter saying that I had not been chosen, but then the candidate who was ranked number one was an English-speaking Canadian coming from a Quebec French-speaking area and the Dean was from the same area. Because he was frustrated that this candidate did not speak French at all, the Dean sat on the contract until the guy took some other offer. The Chair told me that if I could talk for one hour in front of the departmental members, for sure I would get hired. This was a replacement position, and then two years later four positions simultaneously became open. I was hired on one of those tenure-track positions, at the same time as Mario Seccareccia who had arrived also on a replacement position in 1978.

Your work in these times, as far as I know, was on endogenous money and your first publication in English was in the 1980s?

My first publication in English on money was in 1984 in the *Journal of Economic Issues*, but my very first publication was in 1981 in a journal called *Canadian Taxation*, which was a law journal. My piece was a critique of the double taxation argument. My first publication in French was in 1982 – it was very similar to that 1984 publication on endogenous money.

So this was even before Basil Moore published his book, but he had a few articles published on the topic before.

He had his book in 1988, *Horizontalists and Verticalists*, and he already had a couple of papers by 1979.

Had you been in contact with Basil or other post-Keynesian or heterodox economists during this time?

Basil asked me to read the whole manuscript of his 1988 book before it got published. At the time, in 1980, there were six heterodox economists out of about 16 or 18 members in our Department. There was Mario Seccareccia, and there was also an older fellow – Jacques Henry – who at the time was around 50 when we were just 25 or 27 years old. Henry was into Sraffian economics, but under our influence he became interested as well in the Kaleckian and Fundamental branches of post-Keynesian economics. At the time we invited a lot of heterodox economists, like Ed Nell, Alfred Eichner, Vicky Chick, Sheila Dow, David Levine, Luigi Pasinetti, Bertram Schefold, Anwar Shaikh; all these people came to give presentations at the Department.

And you also went to the Trieste School in that period – when did you go there first?

Yes, I only went once to the Trieste Summer School, in 1984, right after my participation in the Olympics in Los Angeles.

That was the period when you got personally linked to the post-Keynesian network as it was in the earlier 1980s?

Yes, we would go to the annual meetings of the Eastern Economic Association that several European post-Keynesians would attend at the time. In 1982 or 1983, the meeting was in Washington, and this is where we met colleagues like Heinz Kurz and Harold Hagemann, who were celebrating the ninetieth anniversary of Adolph Lowe.

When did you get tenured?

In 1984. I applied for tenure in the fall of 1983. It was a very hard battle because there had been a huge change of atmosphere between 1979 and 1983. Suddenly the Department was becoming much more neoliberal and not interested in having more pluralism. The new Chair recommended against tenure, and so did the Departmental Teaching Personnel Committee and the new Dean, but the Faculty Teaching Personnel Committee, which included members from sociology, psychology and political science, recommended in favour of it. Finally, the decision-making body at the top granted tenure. When Mario Seccareccia went for tenure a year later, he got a refusal from the chair but support from the dean, so his case flew more easily.

So in a sense right from the start, not looking at the first four or five years, you were an embattled minority at the University of Ottawa.

Yes, at the same time we were the young guys who were really doing lots of things, being the entrepreneurs of the Department.

But you did not manage to enlarge and get more colleagues to Ottawa and to make it a centre for post-Keynesian economics.

No. We could get people to come and visit. But when it came to tenure-track positions, we were handcuffed, and quickly things kept getting worse.

Coming back to your work, you started out with endogenous money and contributed many papers in this area. When did you think that you had to diversify a bit and also enter other areas of post-Keynesian theory?

From almost the very beginning I gave a course on growth theory. It was linked to my PhD dissertation. I wrote a book in French that was published in 1987 on post-Keynesian growth theories. The book also dealt with measures of technical progress and the Cambridge controversies. Thus from very early on I was interested in growth theory, but in conferences I would always make presentations related to monetary economics. In 1987, after I had written the book, I discovered the neo-Kaleckian model of Bob Rowthorn and Amitava Dutt. These neo-Kaleckian models provided answers to some of the questions that I had been asking myself in the book.

This means in the book you do not have Kalecki?

I did have a graph of a short-run Kaleckian model as presented by Don Harris and Tom Asimakopulos, with overhead labour costs. But I did not have a variable rate of capacity

of utilization in the long-run model. What happens when there is an increase in aggregate demand or effective demand? Are we going to come back to the normal rate of utilization? My thought then was that the higher rate of utilization could only be a temporary phenomenon. I was exactly in the same situation as Joan Robinson, who was talking about changes in the rate of utilization in the short run, but who was still stuck at the normal rate of utilization in the long run. The only possibility I came up with was that this normal rate of capacity utilization could itself become endogenous and become permanently higher. I was frustrated because I realized that other people had found an interesting solution, and that I could have accessed it before finishing the book.

And then you started your own research on Kaleckian growth theory?

I remember that the first time I presented a Kaleckian growth model I said: my name is Marc Lavoie; it is the same Marc Lavoie that you know, but I am going to talk about growth theory, not endogenous money!

What was this first paper?

In fact I do not think it was a paper as such. I was writing my 1992 book, *Foundations of Post-Keynesian Economic Analysis*, and several of the papers that got published on growth theory arose from the book. I used the book as a launching pad, so to speak.

So when did you make the decision to write the Foundations book?

I wrote the book during a full-year sabbatical in 1990–91. At the same time I was learning how to use the computer. I wrote it in about a year. The book had no footnotes because I did not know how to introduce them with the DOS word processor. The book was based on my teaching in that period.

With Mario Seccareccia and Jacques Henry we created two new courses in our Department which were both called post-Keynesian economics. The first one, the fourth-year-level course, was on money and effective demand, and the second one, a graduate course, was on prices and production – that was the Sraffian course. I was mainly teaching the money and effective demand course and that became the core of the 1992 book.

This was already in the early 1990s. What came after that?

In the mid 1980s I started getting interested in the economics of ice hockey. Alone or with colleagues I wrote a number of papers on hockey salaries and entry discrimination in hockey and even in baseball. In the mid 1990s I also wrote two books on ice hockey. Of course, that kept me busy for a while. I kept writing on monetary economics as well.

Then came the integration, which is what I got very much interested in, of your expertise on money and on growth and income distribution: the 1995 paper in Metroeconomica, and I think there was already a paper two years earlier, in a book edited by Mongiovi and Ruhl?

Yes, I was thinking of how to link interest rates with growth models, and those two papers came out from the thinking that I had in the book. And I must say that if the *Metroeconomica* paper looks good, it is because I got a lot of help from one of the two referees, I believe he was Gallegati or Delli Gatti. He made a few proposals that turned the paper into something that was neat to read.

When did you get interested in this stock-flow consistent modelling? We are jumping now a bit ...

Not that much! In 1996, Anwar Shaikh came to Ottawa, because his parents lived in Ottawa. He told me that there was this fellow who was at the Levy Economics Institute who was doing really interesting work and that I should read his newly issued working paper. That paper was the first true paper of Wynne Godley on stock-flow consistent modelling, with figures and all the equations. I read it and I thought, wow, indeed this is really interesting! I remember telling my friend Mario Seccareccia that this is the kind of work that we should be doing, as we were getting tired of talking in circles about endogenous money and all that. Of course, we did not do anything for a while. Then – I have told this story many times – Godley published a modified version of that working paper in the *Cambridge Journal of Economics* in the Summer of 1999. In the fall, the four of us – Mario Seccareccia, myself, T.K. Rymes, who was still teaching at Carleton University, and Colin Rogers who had come from Australia for four months – had our own workshop where we would discuss recent books or articles. I had suggested the Godley article, but then it turned out that we could not reproduce one of the equations of the paper, so I sent an email to Wynne Godley – the first time I had ever communicated with him – saying we are four full professors and we cannot solve the equation, and he answered: ‘I know why you cannot get it solved, it is because there is a mistake in the equation’. By the way, I missed an opportunity to meet Wynne Godley much earlier because I went to Cambridge for a month in 1985, and someone, whose name I will not tell, advised me not to meet Wynne Godley. I had said I would like to meet Nicholas Kaldor, Wynne Godley and Tony Cramp, who was into monetary economics. About Wynne Godley, I was told: ‘do not waste your time meeting this old ignorant fool’. At the time I was a young scholar of about thirty years old, so I did not dare push this any further.

Coming back to 1999, you wrote this email to Godley?

Yes, Wynne was very enthusiastic when he got the email, because at the time he felt there were very few people taking interest in his work, so we invited him to give a lecture at the University of Ottawa, and he did. This was in December 1999. Then, a bit later, I was writing papers on growth theory, trying to take into account capital gains, and I was having a hard time doing so. I had submitted the paper, the referees were not happy about the way I was treating the capital gains, so I contacted Wynne Godley, asking for help, and he said ‘this is a piece of cake for me’. He suggested that I meet him at the Levy Economics Institute, so I stayed at his place for one week. We worked on the equations, we did the simulations and so on, and that became the paper that was published in the *Journal of Post Keynesian Economics* in Winter 2001–02.

This is the ‘Kaleckian models of growth in a coherent stock-flow monetary framework: a Kaldorian view’, when your collaboration started?

Exactly. Then, at around that time, Wynne sent a draft version of the book that he had been thinking about for at least ten years. He asked me if I wanted to collaborate with him on this manuscript and I said yes, that looks really exciting. I remember going to the Levy Institute for a week again and telling everybody that I will be doing this with Wynne Godley, and people making strange faces. I understood later that Wynne had been completely unsuccessful in achieving something with two previous co-authors, and they thought I would be the third one to fail.

How was the collaboration with Wynne Godley?

The collaboration went extremely well because we had exactly the same vision of economics, whether we were talking about monetary economics, production, pricing, even monetary economics in an open economy. We both had the idea of the compensation thesis in a fixed exchange-rate regime, that is, a surplus in the balance of payments, and hence an increase in foreign reserves, will not generate an increase in the money supply because the central bank will reduce the other components of its assets. It took us a lot of time to finish the book because we were rarely in personal contact. We communicated mostly by phone or email. To finish the book I went to the University of Cambridge, at King's College, and spent a full week with Wynne. He also stayed at King's College during the whole week, and so we managed to tidy up all the details in 2006. In between we also wrote a few papers, with different models.

So the book came out in 2007. But before this book, you had done a short book, an Introduction to Post-Keynesian Economics?

Yes, I wanted to write a short book in French that was based on my courses on post-Keynesian economics, and I had submitted a full outline to two different publishers in France, receiving no answer whatsoever. I then contacted my friend Gilles Dostaler, the well-known specialist in the history of economic thought, who was highly regarded in France, and he told me that this was good news because he had himself been asked to write a short book on post-Keynesian economics but had too many other things to do. Thanks to his recommendation, the book got published very quickly, in 2004. The English translation came out in 2006, and this was followed by translations in Spanish, Chinese, Japanese, and now in Korean. Afterwards, when the book with Wynne Godley was barely finished, Mario Seccareccia and I were offered the chance to write the Canadian version of the Baumol and Blinder first-year textbook, so that got me again very busy for another two or three years, until about 2009–2010.

With hindsight, was it worthwhile to do this first-year textbook?

I think it is a good textbook. There was no resistance in introducing post-Keynesian economics in the micro part, but in the macro part it was more of a battle, and we had to backtrack on a number of issues because the criticisms from the five or six readers of each chapter were too strong. The book never did well on the market. Funnily enough, it was the micro book which did better, but never enough to have a second edition. With hindsight, yes, it was a bit of a waste of time, because we did not have enough of an impact.

Then came the big effort to do the second edition of the Foundations of Post-Keynesian Economic Analysis. When did this start?

I had a contract with Edward Elgar to write the book, which I was supposed to deliver in September 2009. I had accumulated notes, but only started on it at the end of 2012. I finished the manuscript in the fall of 2013, and it got published in 2014.

Ok, you are a quick writer!

Well, for some of the chapters I used the previous material, but a lot had changed over 20 years about some of the topics, for instance on monetary economics; also I created a new chapter on the open economy. Yes, it was intensive work: I was getting up early in the morning and would work on the book most of the day.

Let us turn to more policy-oriented work, the project you did for the International Labour Office, the ILO, with Engelbert Stockhammer and several others. How did this come about?

People at the ILO asked if I could make a presentation and then a year later they asked me to preside this team on wage-led growth. Since the team was composed of Europeans, except for me, I thought it would be best for Engelbert to be the co-president of this team and so the two of us did it. We had this small conference at the ILO, and the book came out in 2013. I think the project has been successful, it has some influence in some countries. At least, I know that in South Korea the book is having an impact.

You have been in the midst of at least two big battles in post-Keynesian economics. On the one hand, the Accommodationists or Horizontalists versus the Structuralists debate in monetary economics, and, on the other hand, the controversies surrounding the Kaleckian growth model and whether the degree of utilization can be an endogenous variable beyond the short run. What are your views on the outcomes of these two big battles?

Tom Palley believes that everybody now is on the Structuralist side. My impression is rather the opposite. I have the feeling that people have recognized the advantages of the Horizontalist story, especially with central banks now doing explicit interest-rate targeting, but to be ecumenical I would say that we have reached a compromise which makes everybody happy. I think it has been realized that what the Horizontalists were talking about is the short-term rate of interest which is controlled by the central bank and therefore is exogenous in that sense. The Structuralists, however, were looking more at the long-term rate of interest, which indeed can move around because of changes in liquidity preference. I think that this is how the two views get reconciled, and of course I would advise students not to go into this controversy any more because I think it has been settled. On the Kaleckian model, things are more complicated, meaning that there are still many papers coming out, criticizing this or that aspect of the neo-Kaleckian or post-Kaleckian growth model. The vigorous critique is a measure of its success, with a lot of young researchers amending it one way or another. Maybe the mistake was to speak of long-run equilibria; perhaps there would have been no controversy if from the beginning we had called them medium-run equilibria.

You have been involved with stock-flow consistent modelling as the co-author of the main book on that topic (Godley/Lavoie 2007), but the current fashion now seems to move towards agent-based modelling, or to synthesize stock-flow consistency with agent-based modelling (ABM). What is your view on that, what perspectives do you see, and can we expect anything path-breaking in this area?

First, I think Wynne Godley would be excited to see how popular the stock-flow consistent approach has become. So his efforts and perseverance were vindicated. On agent-based modelling, I am more positive than I used to be about how useful it can be, especially if it is tied with stock-flow consistency as we define it. Yes, there may be emergent properties that can be obtained from agent-based modelling that one cannot get from a strict macroeconomic model. Perhaps there is a way of getting our propositions through to the mainstream thanks to this ABM approach, which is probably more acceptable to at least the dissident orthodox economists.

Let us come to the final question: What is your view on the current state and prospects of post-Keynesian economics, both in terms of the content and the areas of research we are covering, and also its survival or its development as a specific school of thought in economics?

It is clear that a very important topic now is the one linking ecological economics with growth and employment. The proof of the pudding is that a lot of our students are now

interested in this issue, some of them using stock-flow consistent methods or agent-based modelling. About the future of post-Keynesian economics, I think as everybody else does, that life for a post-Keynesian in academia is still very difficult, but there are a lot of young scholars who are into post-Keynesian economics and this is very encouraging. In conferences, perhaps 15 years ago, you would see a lot of people of my generation, but now there is a large majority of younger scholars. Not all of them will manage to get a position in academia but a lot of them will find room in important institutions like central banks or international organizations. Therefore, I am relatively optimistic that at least some of our policies will be implemented one way or another.

Thank you very much.

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

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