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'If you are convinced that post-Keynesian economics is a good way of thinking, get on with it'

Interview with Victoria Chick

Victoria Chick started her career in 1963 at University College London (UCL). She became Professor in 1993, retired in 2001, and is now an Emeritus Professor of Economics at UCL. She is a world-leading scholar of Keynes and is well known for her work on economic methodology and monetary theory. Her 1983 book *Macroeconomics After Keynes* was translated into four languages. She has published over 100 journal articles and book chapters. Chick served on the council and executive committee of the Royal Economic Society, as well as on the governing bodies of UCL and the University of London, and on the editorial boards of several journals, including *Metroeconomica* and *EJEEP*. She is an advisor to Rethinking Economics, the student organization advocating change in the economics curriculum.

Why did you go into economics and where did you start as a student?

It was just a series of accidents; there was no plan. As an undergraduate at UC Berkeley, I started out in science. It was so sexist, a woman just could not survive – they hounded you out. Somehow, I ended up in economics. No high ideals, such as benefiting the world – none of that.

You went to Berkeley, you took the standard classes in economics and some of the other fields, and after your graduate studies, what happened?

In my junior year, I had one teacher who was very inspiring. (I had some other good ones too, people like Howard Ellis.) He did not allow us to read anything; we figured out microeconomics from scratch for ourselves. I found it exhilarating. I worked very hard, and I loved it. Unfortunately, while I and another student benefited from this, the others got left behind. I do not think the experiment was a success, except for the two of us. He suggested that I go to graduate school. It was the Eisenhower recession, and I said OK – nothing else was happening: jobs were scarce.

And where was this graduate school?

Also in Berkeley, and then I went to LSE, to the London School of Economics.

Why did you go to England then?

Well, why not?

There are many famous graduate schools in the USA…

But most of the good universities in America at the time did not admit women or made it very difficult for them.

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Which year was that?

1958. When I was head-hunted for Barnard College as I was retiring from UCL, I met a woman who would not let her daughter go to Harvard. She said to her, 'No daughter of mine is going to the library by the back stairs'. That is the kind of institutional sexism that was rife at the time. By contrast, the University of California, which was founded in 1868, less than 20 years after the gold rush, admitted women on the same terms as men in its second year of existence. I was not used to the kind of palaver dished out at Harvard and would not stand for it. Most of the East Coast places were ruled out.

We will come back to this issue. You then found a job at University College London; was that also luck?

Yes, definitely. Somebody came up to me and said, 'There is a job going at UCL. Applications have to be in by Friday'. So I went to ask Richard Sayers for a reference. He said, 'In principle yes, but I have never heard you give a paper'. I replied, 'As it happens, I am giving one this afternoon, if you are free'. He was free, he came and wrote a letter. I had an interview, and I got the job. Luck played a large role.

Which year was that?

I went on the staff in the autumn of 1963. It would have been early '63 or late '62.

I have read somewhere that back in December 1971 you were at the conference that Joan Robinson gave at the American Economic Association. How did you move from perhaps a more mainstream economist towards a more heterodox one?

You've got it right. When I started teaching, I taught what was being taught everywhere. Then I started playing with models, IS–LM in particular, and it just fell apart in my hands. I thought, 'This does not make a whole lot of sense, so let's go back to what Keynes had to say for himself'. And that was the beginning of my shift. By that time, I had met Jan Kregel and discovered that the kinds of ways in which I thought were not unique. It was nice to know that I was not completely alone. We talked about a few people who thought in the same way. We used to call them 'the good guys'.

I still do in my classes!

Very good! Now you know it has an historical precedent. Jan told me, 'Be sure to go to the December 1971 AEA meeting in New Orleans', which I did, and it was a stunning event, especially Joan Robinson's Ely lecture (Robinson 1972). The partitions between two huge lecture halls were pushed back; the room was enormous. Joan was rather wonderful. You must have read the essay, and you know how good it is. When she got a standing ovation, she said to Galbraith (who was chairing as AEA president), 'Don't be deceived; they are treating me like a pet'. It was at these meetings that Paul Davidson convened a reunion of 'the good guys', which Joan attended. We had to find a name. She suggested *post-Keynesian*, meaning after Keynesianism. Very few people actually know that, which has caused a lot of confusion. But that was the meaning she gave to the name.

Yes, I remember reading Fred Lee (2000), who showed she had some correspondence with Alfred Eichner before that meeting. They were trying to identify a name that would please both sides of the Atlantic.

John King, in his *History of Post Keynesian Economics* (2002), superb book though it is, does not know that. For him, post-Keynesianism started with the death of Keynes.

In 1988 you founded, with Philip Arestis, the Post-Keynesian Economics Study Group, which has now become the Post-Keynesian Economics Society. How did that happen?

Philip came to me and said, 'Let's found a study group'. It was his idea; I am not going to take credit for it. Indeed without his optimism and general bluster in filling out the ESRC [Economic and Social Research Council] form with magical numbers, it would never have happened. The ESRC had established a whole set of study groups at that time. The Money study group, which is now the Money Macroeconomics and Finance group, and a lot of study groups that have gone by the wayside – the Political Economy study group, for example.

That was in 1988 from what I gather from Arestis et al. (2002). At about the same time, there was the creation of the Review of Political Economy*. Were you involved in this as well?*

Yes, I was on the editorial board. Then one day in 1993 we were all sacked – a palace coup. Whether it was engineered by John Pheby or by the publishers remains obscure. I'm sorry the original logo, designed by Sheila Dow, has disappeared. It was a good message: approaching a problem from many different angles.

I think the first issue came out in 1989, just one year after the creation of the Post-Keynesian Study Group.

It was very much the initiative of the late John Pheby, who was also behind the Great Malvern conferences. Did you go there?

I went there twice, and once I was on the same train as you were with Philip Arestis. Let's talk more about your own work, your first book, The Theory of Monetary Policy *(1977). In the light of the current monetary policy, can you see any link between what you were writing then and the way central banks behave today?*

It is pretty outdated, that book, but it was tremendously exciting writing it because I began to work on monetary economics, lecturing on it and so on, and ideas began to consolidate in some sort of blocks of ideas. On a trip to France, just driving to the countryside, I began to stack these blocks up, with a coherent structure, and then I found the keystone.

Which was?

I do not remember now; I wish I had kept an intellectual diary. But one of these blocks of ideas made the whole thing hang together. I was really excited about that because it was a different way of looking at monetary theory. David Laidler came up to me at a conference and said, 'Vicky, do you think Milton Friedman knows what he is doing or does he just land on his feet?'. I asked, 'What is the subtext?'. I had gone through the permanent income hypothesis in the book, including David Laidler's contribution to that debate, and had pointed out that when you use permanent income the IS–LM curves shift in the classical direction, away from the Keynesian direction. David said, 'I did not realize what I had done until I read about it in your book'. He was just doing economics, the way people do, without realizing the political subtext. I thought that was very funny.

Yes. The two of us first met in 1981 when you came to a small one-day conference organized at the University of Ottawa when you were on a sabbatical at McGill University. At the time you were working on your second book, Macroeconomics After Keynes *(1983). What was the main message of that book?*

I do not like 'main message' questions. The main purpose of the book was to scrub all the Keynesian, Bastard Keynesian, creations away and say, look, this is the structure of Keynes's

General Theory, and it is not what we teach and what we think; it is something quite different. It was an exercise in restoration if you like.

But you also took an historical approach, did you not?

Yes, of course I did, because Keynes did too. That was part of the restoration: an historical approach and also a sequential approach which even now you do not get much of.

Very few people have taken this path. There was a paper by Paul Dalziel, from New Zealand, using the sequential approach.

I know his work, yes.

Could one say that this book was influenced by your previous meeting with Hyman Minsky?

I knew Hy when I was a graduate student. If you read the preface of my book you will find a little sentence – let me see if I can recover it … 'Hyman Minsky tried to teach us *The General Theory*, but I did not see the point then. I wish to thank him for trying and apologize for being so obtuse at the time'. That's all true. With what I know now, I think I know why I didn't 'get it'. Although there was quite a variety of understandings of economics in the department (Berkeley) at the time – there were Marxist and classical types, neoclassical types, and Minsky – all discussions were at the level of theory, not method. But Hy was so attuned to differences in method, as well as to the politics and the ideology of various schools of thought, that I believe he thought everybody else understood these things. I certainly did not. He said nothing about method, why Keynes was so distinctive and why he stands out even now as unique in his method. I do remember an exam paper in which the word 'expectations' appeared in every question. But the fact that neoclassical theory assumes perfect certainty was never mentioned. Why were expectations important? Why were they missing in the neoclassical school of thought? But we heard nothing of that. Hy had read Keynes's *Treatise on Probability*, though he never wrote about it and didn't talk nearly enough about it.

Do you tie these ideas about expectations to radical or fundamental certainty?

Sure. If you allow for time (in contra-distinction to the static analysis favoured by neoclassical theory), by definition you are uncertain, and anybody who writes models which assume that you have certainty or certainty-equivalence to the end of time is just wasting public resources, in my view.

Well, there are a lot of people wasting public resources!

I know, it is quite terrifying.

A few years ago you wrote a paper on an evolutionary view of banking, which was published in 1986. I think I can say that it is one of your papers that has been cited most often, but at the same time maybe one of the most controversial. How do you relate this evolutionary view of banking to the events that we have had over the past 10 or 12 years?

I would relate it to something even bigger than that, and that's the role of history in economics. Because institutions evolve, and when they do, there comes a time when your theory does not work any more and you have to retool and bring it up to date to fit the new circumstances. That's a perfectly general point, but of course the same thing happens in banking. I've taken that paper forward: I think there are five or six papers in the same vein. There is no question that banking has evolved massively, and there are good reasons for it, logical reasons why these things happen. Where did derivatives come from? You increase banking competition and that squeezes profits; the first thing the banks did was ditch all their liquid assets because they do not give enough return. Then they needed liquidity; how are they going to get it? They take their illiquid assets, bundle them up into derivatives and sell them so they become marketable, which is not quite the same thing as liquid, but it is part of it. Goodness knows what little wrinkle they will think of next, because there will be other innovations like that. Those who don't like my approach argue that theory should be independent of history – a position I find incomprehensible.

If you were to give a name to the stage at which we are now, what name would you give?

I guess I did give names to those stages, at least for a while.

But how would you characterize the present stage?

I would characterize the present stage as dominated by mega-banks – far too big, too big to fail, and still not really in charge of risk. The banking system is not being repaired very well. It has been patched up rather than repaired. A central banker's view must be extraordinary: all the banks relied on their central banks for liquidity. The originate-and-distribute model is a marketization of the functions of banking, but there were reasons why banks, as specialized institutions, were able to function as they did once they earned people's trust. Now they have lost that.

Talking about money, there are some proposals which are popular, in particular in England, like sovereign money or 100 per cent reserves. What is your view about these proposals which are not exactly the same but have some similarities?

Let me tell you a little story about Minsky which will amuse you. He was taught at Chicago and other places, such as Harvard. At Chicago he was a close student of Henry Simons and so was Milton Friedman. Henry Simons as you know was a 100-per-cent-reserve-banking man, and it is fascinating to me that, filtered through these two radically different personalities, Friedman picks up the idea and promotes it. While Minsky says, 'I know that banks are unstable, but their flexibility gives us economic growth and I do not want to challenge that by hampering them in this way'. Same problem, the inherent instability of banks, but very different policy conclusions. I would be with Minsky on that, if banks were still lending for productive purposes, which they are not, now. I think that only 6 per cent of the UK banks' loan book has to do with production in the economy; all the rest of it is mortgages and loans to purchase other kinds of assets. So we have asset-price inflation. Have you looked in the real estate agents' windows while you are here [in London]? It's worth doing. But be strong!

It is the same in Paris!

I can imagine. Part of the trouble is that the capital adequacy rules of the BIS [Bank for International Settlements] encourage mortgage lending. Mortgage loans count as less risky than loans to the productive sector, because of their collateral. This is just starving the real economy of the kind of lending that Minsky was talking about.

Yes, the BIS rules have many consequences that were not forecasted, unintended consequences.

Because we do not actually know their thinking. We cannot say 'unintended consequences', though. It is implausible to me that they could intend such a thing, and to you too. Certainly, it has had disastrous consequences, in my way of thinking.

I do know at least one person from the BIS, Bill White. He is quite despaired by the behaviour of banks. I do not think he wants to encourage instability.

Sorry, we're talking of the direction of credit, not instability as such. Of course he doesn't favour asset-price inflation, nor would any of the BIS people if spoken to individually. But get them in a room and they say, 'We must avoid risk, and something that has good collateral should have a lower risk rating than the thing that would actually pull us out of the hole, which is encouraging a productive economy'.

What about modern money theory (MMT or neo-Chartalism). Have you had a look at their works?

Yes, sure. I think one of the things they did, and I wish post-Keynesians around here had said it, was to point out that government spends before the taxes come in. That is exactly parallel to investment preceding saving. It is not even rocket science, but no one had really said it. But it is another thing to say that, therefore, there is no limit on government expenditure. This proposition may be true in the little framework that they are using, but if you broaden it out to the economy as a whole, of course there are constraints on government spending, like the balance of payments. It is a question of how big your framework is, whether you see those constraints or not.

Yes, I think they are quite reluctant to seriously consider the issue of the balance of payments. This is the main complaint from the colleagues from Brazil that I know: the lack of attention to the issue of the balance of payments. What they seem to be more concerned about is whether these government expenditures would lead to inflation; they are aware that it might lead to demand inflation.

Like most things, what we need to do is take the very valuable part of what they have to say and push them on those other matters to broaden out their framework.

A related question: perhaps you know I like to put people into boxes. There are some people who consider that modern money theory is not part of post-Keynesian economics. Is that also your view or you would say, 'No, obviously MMT is one segment of post-Keynesian economics', as I think it is.

Partly in and partly out, but more in than out, if I am really pushed.

Why is this so?

Why this ambiguity? Partly because of this narrow framework where, first, I think post-Keynesians ought to think systemically on an issue like money. Money is part of macroeconomics; it is not a microeconomic thing. They do not seem to acknowledge that. And partly because of their work on the employer of last resort, which they tack onto this monetary theory. It does not really follow, although it is worthy in itself.

No, it should not be a compulsory part of modern money theory.

It is a separate issue. I think combining these elements reflects a lack of integration of their system in their own mind. It comes back to the same point about not thinking sufficiently systemically at the macroeconomic level.

Let's come back to the beginning of the interview when you mentioned how difficult it was for women when you started to be in science or even to be in economics. One question of *concern to a lot of us is the small number of women economists within the post-Keynesian school.*

Better than in the neoclassical field, I think.

Is it? You really think so?

I do not actually know.

I was going to ask: why do you think there are so few women in post-Keynesian economics?

There are not that many people of either sex in post-Keynesian economics in the first place. We have to have numbers on proportions to be on sure ground. We are not, either of us, on sure ground, are we? Certain aspects of post-Keynesian economics have an appeal which neoclassical economics lacks. Post-Keynesian economics and Keynes's economics always have a tension between two or maybe more different ways of looking at things, and you have to hold these things together. You can think about something microeconomically and macroeconomically at the same time. You get different answers but you can think about them at the same time. Women are less likely to think very dualistically: black and white, right or wrong. To think in terms of complementary attributes, logic and not illogic, but emotion, perhaps, or intuition. Thinking in terms of complementarities has something to do with the socialization that we experience as children, and we also think very much in social terms. The idea of atoms running around, not saying hello, is not part of our social make-up. I would also say that, on the whole, post-Keynesians are nicer people. This is a very strange statement to make but I am going to make it. If you go to a neoclassical seminar, people interrupt all the time; they never let a speaker finish. They are there to show off how clever they are; they are not interested in listening to the speaker. In post-Keynesian seminars nobody does that. If they want to say something, they wait until the end and then ask questions in a civilized manner, politely. It is a different social framework.

So you think that this social framework within post-Keynesianism should be more attractive to women than neoclassical economics?

Yes.

As a subsidiary question: any young stars among women post-Keynesians that strike you?

Well, I do not know many stars of either sex because I do not teach any more, but I would say Daniella Gabor, who is wonderful on shadow banking – dead bright, so clever – she is worth watching. She got a chair very young and I am very pleased.

There is the question that we always ask at the end of these interviews: any advice that you would give to young post-Keynesian economists, either people who are doctoral students or young professors?

I should hate to be in their shoes, because the structure of academic economics now is so vicious and excludes heterodox economics so vigorously that they are going to have a hard life. No question about it.

But on the positive side?

If you are convinced that post-Keynesian economics is a good way of thinking, a positive and productive way of thinking, get on with it. What else can you do? I was extremely lucky coming into the profession when heterodoxy was tolerated, and now they cannot do anything about me. They view me as a mad woman in the attic, not really an economist at all. Follow what you think is right, and that makes you happy.

Exactly. That makes a very good conclusion: to work on what makes you happy. Many thanks.

Thank you.

This interview was conducted by Marc Lavoie in London in December 2018. We thank Camille Lafortune for the transcription.

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