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‘I see my research and my teaching as trying to understand the world in which we live, to paraphrase Keynes’



Interview with Steven Fazzari

Steven Fazzari is the Bert A. and Jeanette L. Lynch Distinguished Professor of Economics at Washington University in St. Louis, Missouri, USA. At this university, he is also a Professor of Sociology and a Resident Fellow of the Weidenbaum Center on the Economy, Government and Public Policy. He is also a fellow of the Forum for Macroeconomics and Macroeconomic Policy (FMM). Fazzari obtained his PhD from Stanford University, California, USA. His research has focussed on the link between finance and macroeconomic activity, and on the financial determinants of investment, in particular. He is also widely known for his recent work on causes and consequences of the Great Recession, which focuses on the interaction of rising financial fragility, inequality and household consumption. In recent years, much of his research has studied demand-led growth, with particular emphasis on how the demand side leads the dynamics of supply.

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

I grew up in southern Wisconsin with reasonably conservative parents. I thought I would go into science, because I was a smart kid in high school and was good in maths and physics. Sometime in my second year in college at Stanford, however, it became pretty clear to me that I was not cut out for sitting in the laboratory. So, I thought maybe I should take some economics. My first economics course in 1975 was taught by Duncan Foley. He introduced mainstream neoclassical microeconomics and Keynesian macroeconomics. Then, Duncan presented Marxian and more heterodox economics, which stimulated my interest. But the class that really changed the direction of my life was taught by John Gurley. He was a mainstream monetary economist in the 1950s and 1960s. But during the Vietnam War, he became radicalised and developed interests in Mao. He taught a course called ‘Challengers to Capitalism: Marx, Lenin and Mao’. I thought this way of thinking really helped me to understand the real world, and I was captivated by it. But my engagement with these ideas was still reasonably shallow and I was not yet fully committed to studying heterodox economics. This even remained so when I took a more advanced class with Donald Harris that introduced Sraffa, the Cambridge capital controversies and heterodox growth theory. Perhaps I wasn’t yet intellectually mature enough to appreciate the real-world relevance of that level of abstraction. Well into my junior year of college, I was not really sure about where I should head. I next moved to computer science and took a lot of programming classes. As an aside, this was in the 1970s, and about

20 years later it occurred to me that if I had followed that path in what was to become known as ‘Silicon Valley’, I could have become rich! But I have no regrets. The key moment that led me to graduate school was a job interview in the fall of my senior year with a firm that was doing classified work for the military. The interviewer was cagey about the actual content of the work, but I inferred after the interview that they were probably interviewing me to help program the nuclear arsenal of the United States. This was quite a shock, and it helped me decide to stay at Stanford for graduate studies and work with Donald Harris. It was really a last-minute decision, just a few weeks before that application deadline, and many random things could have put me on a different path.

What kind of impact did the economic situation of the mid-1970s have, the first deep crisis of post-war capitalism?

Looking at it from where I am now, I see my research and my teaching as trying to understand the world in which we live, to paraphrase Keynes. But in the beginning, my interest was more theoretical and conceptual – for example, about the determinants of income distribution. I really started following economic events when I got to Washington University in the 1980s.

For your PhD, you worked under the supervision of Donald Harris. Did anybody else impress you in that period?

Since Duncan Foley was forced to leave (for Barnard College, New York) and John Gurley was near the end of his academic career, it was pretty much Donald Harris. There were other open-minded people, but Don was my mentor.

Were your main lines of research already present in that period?

Yes, I would say that in this period I became convinced of two things: one is when businesses expect to sell more, they produce more, and the second one is, when businesses have more money, they invest more. These two simple points largely define my entire research career. Regarding the first, I remember sitting at my desk in my little apartment on the Stanford campus and writing down some equations – and I rediscovered Harrod’s (1939) warranted rate of growth. That became the motivation for my dissertation, that is, expected demand leads to actual production and the resulting dynamics can create knife-edge instability. I published just one paper out of my dissertation on expectations and the warranted rate of growth (Fazzari 1985). However, these ideas continued to percolate in my mind and I have returned to this research program over the past decade.

How would you compare your research to the now very fashionable Sraffian supermultiplier models?

With Piero Ferri, Edward Greenberg and Anna Maria Variato, I have a paper in the *Review of Keynesian Economics* (Fazzari et al. 2013), where we rediscovered Serrano (1995). Working through Harrodian dynamics and thinking about what would contain the instability, we came up with this idea that some autonomous demand would put a floor under the Harrodian instability. It would not necessarily require growing demand; any level of autonomous demand would prevent that knife-edge collapse to zero in the simple Harrod model. And then the question is, what happens if autonomous demand grows? I was working through this idea and then it turned out that I had the supermultiplier model, which seems to me a fairly compelling framework for thinking about how the demand side can drive economic growth.

What was the next step in your career when you had finished your PhD at Stanford?

The Stanford degree was a useful asset in the job market, and I was hired at Washington University in St. Louis. I know now that, behind the scenes, Hyman Minsky pushed pretty hard for my candidacy, but I also seemed to have convinced the open-minded, but mainstream, members of the faculty there to hire me. I think this was in part because I was talking about expectations and interpreting the Harrod warranted rate with rational expectations, a topic that was popular in the early 1980s.

What was your relationship with Hyman Minsky?

Hyman was a very important person in my life, both personally and professionally. I remain close to his family. He is the person that made the connections with Ferri, and also with Greenberg, who was a wonderful colleague at Washington University. Hyman was a kind of mentor in an intellectual sense, in terms of encouraging me to move forward with my research program. People that knew Hyman know that he was not a person to really sit down and give you very concrete advice about career details. He was always thinking about big ideas. My first publication is a co-authored paper (Fazzari/Minsky 1984), one of Hyman's very few co-authored papers. At roughly the same time, Hyman also published a joint paper with Ferri. Piero and I had a bit of a debate about which one of us was Hyman's first co-author. What I regret is even though I was talking to Hyman almost daily, and I was reading some of his work and teaching some of his ideas, I did not really appreciate the deeper power of the financial instability hypothesis until after his passing. This basic idea is that stability is destabilising; tranquil conditions can lead to changes in behaviour and necessarily lead to more risky financial conditions, eventually leading to a financially induced crisis. Now, this is a major theme of my teaching and I have used it productively with my former student Barry Cynamon in a set of papers that explore the dynamics leading up to and through the US Great Recession (Cynamon/Fazzari 2008, 2015, 2016, 2017).

Another big theme of yours is the determination of investment and the role of investment finance.

Yes, I got into this line of work with Tracy Mott. Tracy was also working with Donald Harris toward his PhD at Stanford in the late 1970s, and he was interested in Kalecki's (1937) principle of increasing risk as a determinant of investment. Don insisted that Tracy have an empirical part of the dissertation. In those days, working with big data sets was difficult and cumbersome, but I had this computer science background from my undergraduate years. Since Tracy and I were friends – actually, he was the teaching assistant during my undergraduate studies – we agreed to work together. This empirical work then became the foundation of my work on finance and investment. Our joint paper in the *Journal of Post Keynesian Economics* was my first paper in this area (Fazzari/Mott 1986/87). Then I pursued some ideas with one of my graduate students, Michael Athey, which came out in the *Review of Economics and Statistics* (Fazzari/Athey 1987). That was a time when many people were interested in the issue from the asymmetric information perspective. The paper with Athey connected asymmetric information models with empirical evidence on cash flow and investment. That got the attention of Glen Hubbard and Bruce Petersen, and we started working together (Fazzari et al. 1988). Bruce and I continued with a series of successful papers on this topic (e.g. Brown et al. 2009; Fazzari/Petersen 1993). Bruce is a great friend, a fantastic economist and a very solid empirical researcher. I owe him a lot; he taught me a lot. That work over more than 20 years remains very rewarding.

There is one question I (Eckhard Hein) asked you after one of your presentations on Harroldian instability and autonomous demand several years ago: what is the role of finance and debt dynamics in your autonomous demand-driven growth approach and the (in)stability of the warranted rate of growth?

In terms of the post-Keynesian work on demand-led growth and supermultiplier models, I think it is important to link to finance. I have not quite gone there myself. I know you have done some good work in this area (Hein 2018; Hein/Woodgate 2020). People are looking at these issues now and, like you have said, this is becoming a popular approach. So, connecting the investment side of the supermultiplier model to financial issues seems to be very worthwhile. Also, I think it is becoming clear, even though the abstract models tend to have constant rates of growth of autonomous demand, in practice, this is not realistic. I think the supermultiplier framework is useful in organising our ideas, but then the real question becomes what drives the dynamics of autonomous demand. Take, for example, debt-financed household consumption as an engine of autonomous demand in the US prior to the Great Recession, which then led to over-indebtedness and the financial crisis.

I fully agree with this idea, and you have been a main contributor on inequality, indebtedness and the US economy, which then led to the 2007–2009 crisis and also to the stagnating tendencies which we have seen since then.

Yes, this has been an important strand of research, which is still ongoing with Barry Cynamon. We were thinking about financial fragility of the household sector leading up to the Great Recession (Cynamon/Fazzari 2008). I think the first time I really talked about it publicly was in a lecture for Washington University alumni in early 2007. People were simply underestimating the risks created by household indebtedness. In later work, Barry and I connected these dynamics with rising inequality (Cynamon/Fazzari 2015, 2016).

This is where your link with Minsky's work becomes relevant again?

Yes, for sure. I think the household debt issue really does fit the basic Minskyan idea. Falling interest rates and deregulation led to more and more household debt, and this drove demand on an upward trajectory, but also created rising financial fragility – and finally a crisis. I remember expecting the 2001 downturn in the US to be worse than it was due to high household debt. Looking back, the household consumption engine of debt financing just kept powering right through the early 2000s. So the big investment shock from the bursting of the tech bubble only caused a mild recession. But eventually the 'Minsky Moment' for the household sector came in the middle of the decade. We then used this framework to explain the stagnant recovery after the crisis. Having lost the stimulus from household borrowing, the US went through a period of stagnation, never to recover to the output path it was following before the Great Recession. This insight actually then links with the supermultiplier work, trying to tie together this empirical historical thread of research with more abstract modelling. It also connects the dynamics of autonomous demand to financial conditions.

Let us come to the current crisis. How do you view the current situation in the US with the Covid-19 crisis and now rising rates of inflation?

I believe a basic Keynesian perspective serves very well. We had the Great Recession and the subsequent stagnation, which can be explained well by the post-Keynesian autonomous

demand-led growth perspective and the associated rise and fall of household debt. Going back to the beginning of my professional career in the early 1980s, I believe that demand has constrained US output up until the pandemic hit. But the pandemic created some macroeconomically meaningful supply constraints. It was a lockdown that led to a massive fiscal response, which, in my view, was very effective in generating a quicker recovery than I actually expected for the US economy. But inflation is now a new issue. Since I began teaching in 1982, inflation had been on a downward trend. It had been pretty stable and relatively unimportant for several decades. I have to admit being a bit perplexed by the whole inflation situation. My current thinking now suggests what I am calling a disruption theory of inflation: very simply, the pandemic created a huge disruption in the economy in terms of shifting demand across sectors with different kinds of supply constraints, all of which, from a simple mainstream microeconomic perspective, would generate relative price changes, likely very big relative price changes. Big relative price changes need not cause inflation if nominal price declines offset price increases, but that has not happened. So the result has been inflation, but more as a shift in the price level than a permanent rise in the inflation rate. This was a big disruption, but I would still expect this inflation to come down as supply constraints relax and demand patterns return to normal. I think the US Federal Reserve is playing with fire by raising interest rates, possibly driving the US economy into a recession.

You do not see any indicators of a price–wage–price spiral at work?

I think there is a certain logic in that idea, but so far nominal wage increases are far behind inflation. Real wages are falling, profit rates have risen and there is actually credible work out there that some businesses are taking advantage of the situation to raise mark-ups. Again, there may be some permanence to that, but it is more of a level shift in prices than a long-term rise in the inflation rate.

You have been at Washington University in St. Louis for a long time and have been active at your university, too. What has been your experience with this kind of work?

I am now in year 41 at Washington University, which is hard for me to believe. Many years ago, when Hyman Minsky and I were sitting in some airport on our way to the Eastern Economics Association conference, Hyman told me ‘Do not get pulled into administrative work. Stick with research, this is the path you should be on’. But I did not follow that advice. So, I have done a lot of administrative jobs at the university over the years. I was chair of the economics department for six years, and I somewhat regret not pushing that department in a more heterodox direction. I think that it would have been difficult given what the university required. They were open minded in terms of the direction of work, but they wanted mainstream journal publications on a CV for hiring. I was fortunate that my investment and finance work met this requirement. After my term as chair, the economics department went in a new classical direction. I ended up being somewhat disconnected with the department after 2005, but in a friendly way. This experience encouraged me to establish more external connections with the post-Keynesian community, going to the Knoxville, Kansas City and Levy Institute conferences in the US, and then also to the FMM conferences in Berlin.

More recently, there was an interesting development regarding the sociology department. Washington University had a controversial sociology department in the 1960s. It was heavily influenced by the Vietnam War, and it was apparently a rather conflictual place. Then, partially because of that somewhat contentious history, the department shrank, and around 1990 the decision was made to close it. This decision left a real

gap in undergraduate education and the University's research breadth. Several attempts were made to fill this gap, and finally, about ten years ago, a new dean came in who was very supportive of the idea of bringing back sociology, because issues of social inequality are so critical. I was a member of the dean's academic planning committee at that time. During one of the committee's meetings, the idea came up that an inside person, who was familiar with the institution and who knew how to hire faculty, could start the department, relying on advice from sociologists outside of the institution. I walked out of that meeting thinking that this was an interesting idea and an important mission for the University, and I suggested that I might be interested in leading the effort. Within a few months, I was appointed as the inaugural chair of Washington University's new sociology department, a job I held for five years. During this period, we hired 11 sociologists. It has been a huge success – it is a great department with wonderful people, and I remain very active with them. Now, I have a joint appointment as Professor of Sociology and still work with the sociology faculty to continue the new department's development. But I am still an economist.

Let us finish with our usual final question: what, in your view, are the current challenges for heterodox or post-Keynesian economics and what are your recommendations for the younger generation?

To see rising global interest in heterodox and post-Keynesian economics in the younger generation, and to meet folks from around the world coming together at this FMM conference, has been a great experience for me and it is very encouraging. I think for young people the motivation is to explain the world in which we live. This has been my motivation, and I feel like, in a sense, I am an 'unlikely radical'. I was not planning to overthrow the bastions of power, but I just strive to explain and improve the world we live in. I think my broad advice for the next generation is to keep that focus. I am encouraged by the work that I see in post-Keynesian economics. I have been captivated by research on the supermultiplier growth models. I think this is moving in a really interesting direction. There are lots of interesting questions, like those we talked about earlier, linking finance and really understanding the dynamics of demand. I think this is maybe less of a 'let us run a regression' kind of exercise, but rather to get a sense of how the history works. For example, look at the household financial boom and how that affected demand dynamics in the lead-up to the Great Recession. Of course, I am also encouraged by more formal, ongoing empirical work. For example, I am currently working with a great graduate student, Alejandro González. His empirical skills are really good and applying them to post-Keynesian issues, and then to put facts out there is important (Fazzari/González 2023). Anybody can criticise a model, but if you have empirical evidence supporting more heterodox views, then that opens some doors. In addition, we have started to see post-Keynesian ideas make their way into public debate. It is striking to see Stephanie Kelton on the front page of the business section of the *New York Times* or the *Wall Street Journal*. Of course, these newspapers are criticising Modern Monetary Theory, but seeing that impact on the public discussion is a positive surprise to me. I did not think I would see this in my career. So, I am encouraged.

Okay, let us stop here with this very optimistic outlook. Thank you very much Steve.

This interview was conducted by Eckhard Hein and Gennaro Zezza in October 2022 in Berlin during the 26th Conference of the Forum for Macroeconomics and Macroeconomic Policies (FMM). We thank James Lubwa, Berlin School of Economics and Law, for the transcription.

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