

An Interview with Duncan Foley

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This interview was conducted as part of the special issue of the New School Economic Review honoring the legacies of Professors Duncan Foley and Anwar Shaikh and the broader tradition of political economy at the New School for Social Research. The interview took place on October 27, 2022. The transcript has been lightly edited for clarity.

Maybe we can start with you talking a little about how you came to the field of political economy.

Well, that's pretty much my whole life. I sort of backed into the field of economics when I was a college senior at Swarthmore College. I was an economics major and a math minor, and the one idea I had about my career was that I didn't want to be a college professor. That was the only very clear notion that I had. I was thinking of possibly a career in the foreign service, or maybe in politics in some way, or since I'd done some writing and playwriting, that was another possibility. But of course there was the draft. When I graduated I had the hope that I was going to marry a foreign national, and that meant from the point of view of the foreign service that I would have to wait for at least a year to actively go into the foreign service. So I decided to spend that year studying economics because I'd been offered admission to the Yale economics program. So I got into economics in a somewhat backward way to begin with, and was trained very much in a neoclassical, general equilibrium theory way of thinking. I've written some about this in an essay that's in a series called "Makers of Modern Economics," and particularly about my experiences, at Yale and then at MIT.

I started teaching at MIT in the Fall of 1966, which was when the Vietnam War was escalating and like many people I got kind of radicalized. I started thinking hard about just what kind of system we have and what the relation between politics and economics is, and also began becoming increasingly restless with the type of economic theory that I was teaching, which at that time was pretty much straight general equilibrium and neoclassical economics. At that time I tried to read Marx because I understood clearly that Marx was trying to address the question of the relation between economics and politics in the kind of society that I was living in. But I did not succeed very well, which is very typical, I think. For people who have a neoclassical economics education, it's very difficult to understand even the basic points about Marx's theory.

And that was more or less where things stood until my wife got a job at Stanford University, and I moved out there. At Stanford there was a group of graduate students who thought of themselves as radical and in some cases as Marxist economists, and also a very remarkable group of faculty: in the Economics department, Bridget O'Laughlin and Donald Harris, who was a visiting professor, and Shelly and Renato Rosaldo in the Anthropology department. They were thinking about these questions of economics and politics in a very sophisticated way. In that atmosphere I got a lot of support to reread Marx and study parts of it and to understand it better. I would say that when I went to Stanford, I was probably thinking I was on my way out of economics altogether. I was losing interest in it. And it was really the political economy perspective that renewed my interest and regenerated the energy that I have working on economics.

So I would say, that's where that came from. It was from that period that I wrote two papers that I think were pretty central to developing my own thinking about that. One was the paper that appeared in the Review of Radical Political Economy on the transformation problem, and the whole question of values and prices, which was reissued in their fifty year retrospective with an article I wrote looking

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back on that. And the other was a paper that appeared in the *Journal of Economic Theory* on Marx's theories of the circuit of capital that tried to use some modern mathematical techniques to formalize some aspects of Marx's theory of the circuit of capital. Those experience, consolidated my feeling that there was a lot of useful and fruitful work to be done in applying modern analytical tools to classical political economy.

You said you were initially thinking about giving up economics altogether because you felt like the neo-classical framework you were working within was not worth pursuing. And this was because it didn't apply or have explanatory value for the questions you were interested in or ... ?

You could think of it that way, or you could think of it in terms of how much progress I thought you could make within the constraints placed by the neoclassical perspective. I've given that a lot of thought over the years, and my views on it have probably changed some. But at that time, for example, what I was most dissatisfied with in general equilibrium theory was the poor treatment of the role of money. And I had become, after trying to work on that fairly extensively, pretty convinced that you needed some other perspective to make much progress on that kind of issue. So it was more a question of feeling like there were diminishing returns to research work within those constraints. The more I think about it as time goes on, the more I think that it's very important to think of the constraints issue. What's important about neoclassical economics and general equilibrium theory is not so much what it addresses, which is the process of competition, and allocation of resources, and all those questions, which are perfectly reasonable questions, and I have some real intellectual interest in them. But it's what it leaves out that in my view tends to make it less interesting. What it tends to leave out is the broader context in which the economic process takes place, especially the capitalist economic process. And above all it leaves out class. The neoclassical economists tend to have a very narrow view of what's endogenous to economics. For example, taking population as a given, whereas it's pretty clear that over a longer term, decisions about fertility and also consequences for mortality are highly endogenous to the economic process.

So I think it was the sense I had that you could broaden out, and the sense I had that for a variety of historical reasons there were a number of issues in political economy in general and Marxian political economy in particular, that were ripe for treatment with more modern mathematical tools but that no one had thought through very carefully.

I don't know if you are familiar with this concept from Schumpeter of economic "vision"?

Yes, yes, I am. And you mentioned it in your email so I gave some thought to it. I would say the other big intellectual theme over my whole career, was, when I started, in the late 1970s and early 1980s, to apply mathematical dynamics, especially dynamical systems theory to these political economy problems. I became aware of what's called the "complex systems" point of view about methodological issues, which goes quite a bit beyond social science and has probably penetrated social science only to a limited degree even today. But I would say that supplied the other piece of the puzzle for me. If you think of it in Schumpeterian vision terms, it began to make sense to me to think of the economy and social interactions in a broader sense as cases of complex systems which have their own very particular methodological problems. They're not completely impossible to analyze, because a complex system has this property of self-organization, which has a kind of reproduction element to it, but they also can be extremely endogenously unstable and are subject to things like regime changes in terms of the kind of observations that they produce.

I would say that was very important for me, and it actually kind of closed the circle because it led me to reread the classical political economists, Smith particularly, and then Marx, as in their own way trying to think of how you could say something substantive about as complex a system as the capitalist economy. And the long period method—their idea that you have to focus on phenomena that appear only as statistical averages over long periods of time—in my view, corresponds to those self-organizing elements of the complex system. If you look at it that way, then you see that what they are talking about, in their own way and without the elaborate mathematical machinery of modern systems analysis, is an approach to complex systems.

So I would say that's my vision: that the capitalist economy is a complex system, and if we're going to learn anything about it, one has to learn it from looking at these self-organizing properties.

We're describing this issue in honor of you and Anwar as themed around "the New School tradition of political economy." And I'm curious if you think that there is a distinctive "New School tradition" in political economy and if so, what characterizes it in relation to classical political economy in general?

I have my own feelings about that which actually in some ways are rather different from what I sense many people would say. So let me tell you what I think, and people can shoot it down if they don't like it. I tend to look at it in a little bit longer view, as you get, say, reading Judith Friedlander's history of the NSSR or the Graduate Faculty. And there's no doubt that there is a New School tradition, and it's also related to progressive politics. The people in the 1910s and 1920s who started the New School tended to think of themselves as progressives, and tended to be instinctively hostile to conservative and traditional ways of thinking about social science and social policy. But they, in my view, did not have a very well conceived, informed, specific program of political economy in particular, although they tended to be dissidents from what they saw as the orthodoxy of social science in the 1910s and 1920s. They weren't sold on any particular alternative like Marxism or other radical or critical approaches to social science. And a second point is that if you look at the array of people that were involved, they were all also interested in empiricism, in trying to use more sophisticated quantitative methods to apply to social science theory. And in that respect they were perhaps different from at least some strands of political economy, which has in some respects had a limited ambition to do quantitative research.

So I think that the New School seen in this broader perspective oscillated in its own way between being more liberal or centrist and being a constant gadfly and critic of bourgeois social science from the outside. For example, in the late 1940s the New School was pretty much aligned with the liberal anti-communist establishment, and that might not be exactly what we think of as coincident with the tradition. There are major figures in social science, people like Jacob Marschak and Franco Modigliani (who got his PhD at the New School), who undoubtedly are innovators methodologically. But I don't think you would think of them as part of a specifically left wing New School tradition. And as far as I can tell, that was probably true, or up through the 1960 and 1970s: that the surviving members of the university in exile, while they were very critical thinkers, did not have a very clear programmatic left wing approach to economics and social science.

The figure who seems to have changed that was Heilbroner, who was a persistent left wing liberal critic of received mainstream economics and wrote very eloquently. He was such a marvelous, wonderful writer, and he had a huge influence on people's thinking. But I don't think that it was, again, that he had formulated a very clear and political alternative to the mainstream ways of thinking. He was probably the leading member of the department in the 1960s and 1970s when radicalism hit, when radical economics started, and that happened not just particularly at the New School. There was a movement that spread all across the American economics establishment, especially among younger graduate students and younger faculty. I think Heilbroner was more aggressive than most in being willing to experiment with more extreme or radical points of view, and therefore he'd hired people like Ed Nell and Anwar Shaikh and David Gordon, who were more committed to forming a coherent and articulated specific alternative. So I would say that's a second phase of New School political economy, and it's important to realize its relation to the first phase, the more general critical phase from before the 1970s.

As far as I can tell, the New School itself was never sold on this and that's been an issue for the Economics Department throughout. I moved to New York in 1977. I had known several of the people, Anwar Shaikh among them, before I moved to New York. But once I moved to New York and was teaching at Barnard, I was in a position to attend some talks and seminars, and so forth. And some graduate students would come up to Barnard to talk to me about what they were interested in. So I started to have some insight into what was going on in the department. As far as I can tell, the whole graduate Faculty- as it was called at that time- had become fairly principled in applying a Marxist approach to such social science. That was true in Anthropology, Sociology, I think, and Economics, not so much in Psychology which has always had a somewhat distant intellectual relation to the other departments.

Jonathan Fanton in the 1980s made- with Ira Katznelson's help- a very focused campaign to de-Marxify the Graduate Faculty. As you would predict this led to huge levels of internal conflict: bad feelings, resignations, in some cases the complete collapse of departments for several years, and their inability to function. The Economics department did not experience the worst levels of that, but it was impacted. And one of the reasons that it didn't was that the figures who were brought in by Katznelson and Fanton to moderate or change the political economy tone were Lance Taylor and Alice Amsden, and also people like John Eatwell. Now, in my view, this was a very fortunate choice. These were very fortunate choices for the New School because these were very, very high-performing intellectuals who were very productive, and had their own new ideas. Alice Amsden completely revolutionized, I think, the understanding of what really went on in the case of the East Asian "economic miracles." Lance Taylor has completely revolutionized our understanding of problems of inflation in Latin American economies and issues like that. So there was a lot of intellectual energy that was brought along with the levels of conflict that were involved. That was more or less where things were when I came on the scene, largely as a result of two major developments: one was David Gordon's untimely death and the other was John Eatwell's decision to return to Cambridge. So I think I was hired- improbably enough- to try to fill both of those empty spots in the department.

So, yes, there is [a distinctive New School tradition of political economy]. I think that the bigger tradition for the New School is one of broad left wing, open, intellectual and critical debate about social science. And then within that grew up a specific tradition, more focused around radical Marxist ideas, in the '70s, '80s, and '90s, that continues today.

Now it looks to me as though the department is asking- I mean we've talked about this over the last 10 or 15 years frequently- "Where do you go now?" Especially after the demise of the Soviet Union and the development of neoliberalism and globalization, exactly what is the content of our political economy going to be? And I still think we're groping to find answers to that question. I don't think it's very clear at this point.

Over the last 10 or 15 years, you say there's uncertainty about the orientation of the department. But do you think that the work coming out of the New School's Economics Department has distinct approaches or shared tendencies compared to the other heterodox departments with significant political economy contingents even in this period?

I think there are some very distinctive things about this more focused New School Economics tradition, which I would characterize as being committed to a very high level of analytical rigor and sophistication and modeling, and towards linking modeling to empirical investigations. And I think that's influenced almost every other part of political economy on a world scale to a considerable degree. Anwar points out, and I think he's exactly right, that it's amazing that a department as small as the New School has been able to have the kind of larger impact on people's thinking that the work has had.

But there was a peril in what you might call schismatics: the tendency for people on the left to emphasize the points on which they disagree rather than the points on which they agree. And that struck me in the '80s and '90s as particularly counter-productive, because of the fact that as a whole heterodox economics or critical economics occupied such a very tiny niche in the whole broader picture of social science and economics. It was more to the point to spend as much time on emphasizing the things on which people agreed—for example, the importance of the distribution of income as a fundamental element in understanding political economy—rather than to focus too much on some technical disagreement, like how you close models exactly and whether there really is a long-run capacity utilization constraint. Those technical questions are perfectly reasonable to pursue but if they get in the way of the more important points then I think they need to be deemphasized. Overall, I tend to see the New School—partly by over the last 20 or 25 years having such a remarkable group of graduate students and creating a very fertile core of younger scholars who have gone to a lot of different places— as contributing to the development of a more quantitative, more analytical, more rigorous, but still quite critical analysis.

You mentioned the importance of complex systems in your own thinking and I'm wondering if you think that is something that is part of a shared orientation.

Well, it was the unifying principle for my personal teaching in the department. And I think I

persuaded some people to take it seriously and to start to think that way, but not everybody. So sure, I guess if I pull the camera back a little bit and look at a more wide angle view of the situation, I would say it's an important piece of what we've learned over the last 25 years but it's not necessarily the most important piece or the dominant piece or anything like that.

The other thing about the complex systems point of view is that I sense that many people have an instinct for it. It corresponds to the way they think the world is or how they feel the world is, because of their own personal experience, and because of the history that they've lived through. But the level at which they formalize that, or bring that into a formal framework is very different. For some people it's natural to begin to think about that in mathematical terms, or in terms of stability, and the instability of mathematical systems and things like that. For other people it remains much more instinctive and intuitive: the sense that there's something wrong with the idea that the whole system can be described by a bunch of simultaneous market clearing equations or whatever it might be, which I think is also a very healthy thing and connections can be made there.

The final thing we wanted to ask you about is what future work you're excited about. Are there particular research trajectories for yourself or in general that you're excited about the potential of?

There are a lot of things to be interested in and excited about, because I think we're in a position where there's a lot of unanswered questions that can be answered, or at least partially answered using these newer approaches and techniques. I tend to let the students that I work with come up with the specific problem areas and I have learned an enormous amount from them and in some cases can help them see how to apply these analytical techniques to the particular problem that they're interested in.

At the moment there are two things that I'm most interested in. I've been trying to bring some closure to a longer project which is to rethink the foundations of statistical analysis from a Bayesian and information theory point of view. There was a manuscript that's circulated within our department a couple of times, but Ellis Scharfeneker and I are taking it up again and trying to form it into a somewhat coherent whole. So that's what I'm working on mostly right now.

I also think that we're at a point where some very deep questions about what it means to have a left-wing politics in a mixed economy have to be addressed. One of the things that struck me was that the collapse of the Soviet Union and the Stalinist project, which many people predicted would lead to a complete eclipse of Marx as an intellectual factor and so forth, in fact marked a renewal of interest in critical class-based analysis of capitalist society, especially for a younger generation and amongst students. There's much more interest in studying Marx seriously today and since the 1990s than there was in the 1960s and 1970s even, which I think is heartening and good.

But at the same time there's a tendency for people to think of themselves as Marxists or socialists without realizing that there are some big gaps in that program as a practical political program and I would like to spend some more time thinking through those gaps. For example, what are the practical alternatives for some kind of fundamental reform of capitalist social relations? Stalinist central planning is not the only one and in my view not even the most interesting one for a variety of reasons. But there are a bunch of other ideas that were tested out to some degree and developed historically especially in the 1960s to 1980s in Eastern Europe: worker-controlled basis for enterprises, various versions of market socialisms that didn't involve private ownership of capital and so forth. And I would be very interested in trying to push more people's understanding of those alternatives as a basis for some practical political work.

I know you have written a little along these lines already, about potential alternative economic structures, work that is more prefigurative or prescriptive say, rather than analytical or descriptive about capitalism. But is this sort of suggesting a pivot to a somewhat different orientation? It seems like there has been much stronger emphasis within political economy - maybe especially at the New School- on critical analysis of existing capitalist economic structures, and not so much on a role of political economists in themselves directly developing ideas of alternative structures, even if people hope that their work will in some way be beneficial those projects.

I tend to see the two things as pretty closely connected, and I think over the last 25 or 30 years there's been a growing awareness of the source of social problems—say, inequality among them—and also various forms of persistent discrimination, on gender and ethnic and other bases. There's been a

real growth in understanding of how closely intertwined those phenomena are with specifically capitalist social relations of production and the way capitalist economies work. And I think that's an excellent thing.

But it's one thing to say "Well, it's capitalism that's the problem" and maybe it's even true in some historical perspective. But when you say capitalism is the problem, some people are going to hear you as saying, "Well, the way to solve the problem is to change or get rid of capitalism, or at least alter or reform it in some fundamental ways." And that's the dimension that I think we haven't done as much work on, or haven't given as much thought to. But it seems to me it's very logically connected with the first dimension, because the first dimension is like the diagnosis of the disease, which is the first step towards understanding what's going on but what you really want to do is try to do something to cure the disease and that's often a much more difficult thing to figure out. I think it is especially in the case of capitalist social relations because many of the ills of capitalism are very deeply intertwined with the functional aspects of it.

Capitalism really is a way of guiding human effort—not with anywhere near the precision suggested by general equilibrium theory—but in general it does work by creating incentives and thereby directing human behavior towards some kind of a homeostasis or self-organization. But it's exactly those things that produce the ills of capitalism, especially inequality. If you're going to have incentives, you're going to have to have unequal outcomes. And if you start trying to suppress unequal outcomes, you're also inevitably going to suppress their function in signaling how to allocate common resources and educational resources and a whole bunch of stuff like that. So I think that's the area where I would like to see myself moving over the next few years.

Well, thank you again for taking the time to talk to me about all of this. I think that basically covers everything that we wanted to ask you about. Is there anything else that you would like to be included in this? Or any concluding thoughts?

Let me just finish up with one thing, since you asked about the tradition of political economy at the New School and so forth. From my personal point of view the overwhelming direct experience has been a human one. It's been an incredible chance to meet and work with this amazing group of people who have been the New School graduate students over the period that I've been here. It attracts a community of people who are in my view quite unusual and quite rare in society, and really exciting to experience and work with. So if we're going to end on any theme I would like it to be my feeling of enthusiasm for what you might call the sociology of the Economics department at the New School as an intellectual and educational enterprise.