

"As if" theorizing in heterodox economics?

Comments on "Implicit Microfoundations for Macroeconomics" by Ian Wright

by *Duncan Foley*

Leo Model Professor, New School for Social Research and External Faculty, Santa Fe Institute

Ian Wright's paper offers excellent observations on the methodology of macroeconomics and incorporates an exemplary review of recent empirical work on the statistical distributions of key economic variables, such as distributions of firm size, firm growth rates, profit rates, wage income, wealth, and real GDP growth rates. Wright's paper is a valuable contribution to the (as yet far-from-mainstream) revival of interest in a statistical microfoundations for macroeconomics along with work by Aoki and Yoshikawa, Yakovenko, and others.

The central theoretical contribution of this paper is an agent-based simulation model representing a highly stylized set of interactions of an imaginary economy. The claims of the paper are centered on a comparison of the steady-state statistical behavior of the model economy with the real-world data mentioned above. With one important exception (real GDP growth rates) the model distributions tend to bear a strong resemblance to the real-world data in their shape.

The model itself bears little relation to the institutional realities of real capitalist economies. Based, following a series of papers in the econophysics literature, on the concept of a conservation of "money" that bears no relationship to the actual payments or credit systems of evolved capitalism, the model makes very approximate assumptions about agent interactions, which in turn bear little relation to the complexity of real labor, credit, and output markets. The model's assumptions about firm formation and dissolution, labor contracts, and spending decisions of households and firms seem to be motivated more by their analogy to models of physical systems than to reflect any attempt to understand what actually goes on in real capitalist economies. For example, all the agents in the model (given the money conservation assumption) are effectively liquidity constrained. While liquidity constraint is clearly an important phenomenon in real capitalist economies, the assumption of universal liquidity constraint is a somewhat peculiar first approximation (and may be responsible for the inability of the model to produce empirically realistic distributions of real GDP growth rates). Markets and market prices such as interest rates, wages, and output prices, play essentially no role in influencing individual agent decisions in this model. This is a refreshing alternative to the dogmatic insistence of mainstream economics on attained market equilibrium models with no statistical variation, but perhaps goes too far in the opposite direction.

Milton Friedman opened the gates of modern mainstream dogmatic adherence to attained equilibrium models with his famous essay on methodology published in his *Essays in Positive Economics*. (A representative excerpt is available at <http://www.marxists.org/reference/subject/philosophy/works/us/friedman.htm>.) This essay puts forward a confused and half-baked account of a type of philosophical positivism that was already discredited in the literature of philosophy of science at the time Friedman wrote it. Friedman's position is often summarized as claiming that it

doesn't matter in science whether the assumptions of a theory are correct, or bear any relation to the phenomenon it purports to explain, as long as it yields "correct predictions". Friedman intended this essay to authorize the use of institutionally unrealistic models assuming constant clearing of perfectly competitive markets as tools for analyzing the much messier reality of real capitalism. We know how that has turned out.

How different is Wright's method from this? Again we have a highly stylized model which on the face of it does enormous violence to what we know about institutional and behavioral reality. The model yields predictions (different ones from the mainstream attained equilibrium models, to be sure) that are in some ways strikingly confirmed by empirical observation. Just what, if anything, can we conclude from this, and where does this point future work in this vein?

My own guess is that the empirical successes of this type of model are generic. (Wright himself alludes to this possibility.) In other words, any one of a large class of models that include this one would produce rather similar statistical results. My reason for thinking this is that statistical physics shows that a wide variety of concrete phenomena can be modeled by a small range of canonical cases (drift-diffusion processes with reflecting or absorbing barriers, for example) and appropriate choice of key parameters. If this guess is correct, it implies that the details of Wright's model behaviorally and institutionally, including the problems of conservation of "money" and randomized transactions prices) are not crucial to his results. But if this is true, it is vital that we know how it works explicitly, and can identify reliably the specific features of this class of model that give rise to each of the empirical successes it claims.