I am grateful for the insightful attention paid by the commentators to my article in this volume, “Economic Theory and Social Policy: Where We Are, Where We Are Headed.” Pondering these comments has led me to think perhaps more deeply about the issues involved. Let me begin by summarizing my argument, somewhat revised in light of the comments.

1. The economic system characterized by the interaction of competitive markets regulated by powerful national governments and local communities is a complex dynamical system.

2. The Walrasian general equilibrium model, which is taught in virtually every PhD-granting economics department in the world, provides the simple structural equations underlying the complex dynamics of the market economy.

3. There is no plausible alternative to this model that can serve as a starting point for analyzing the complex dynamics of market economies. There is not even a suggestion of an alternative structural model, to my knowledge.

4. Until Antoine Mandel and I provided agent-based simulations of general market interaction and provided analytical proofs of stability using evolutionary game theory, there was no acceptable model of complex market dynamics. We showed, however, that complex market dynamics flow from simple structural rules, with a strong tendency for market economies to approximate market-clearing equilibrium, but with considerable tendencies for large aperiodic excursions from equilibrium (high volatility).

5. Contemporary macroeconomic theory is largely powerless to model market dynamics because it models rational expectations and a representative agent rather than adaptive expectations and large numbers of heterogeneous agents.

6. Any acceptable theory of complex market dynamics we may develop in the near future will have the model of market interaction initiated by Walras as its structural starting point.

Many of the insights provided by the commentators are in agreement with these points, are elaborations of them, or complement them. I shall focus here only on points made that find fault with one or more of the above points.

David Sloan Wilson’s analysis is perhaps the most seriously in contrast with my argument. “Out with the old, in with the new,” he proclaims. His most telling point is that traditional economics has been used extensively to promote free market ideological hostility to state regulation. He does not deny that this is not a bias flowing from the theory itself, but rather from ignoring the half of traditional economics that explores the nature of market externalities and their regulative correctives. Nevertheless, he believes the facility with which neoclassical economic theory can be distorted to further free market ideology is a good reason to abandon it.
I maintain that a scientific theory should be abandoned only when it has inherent uncorrectable flaws. Otherwise it should be maintained, corrected where possible, and where suitable corrections have not yet been developed, its flaws should be freely and openly admitted. That is the current state of neoclassical economics, where principles of complexity and evolution are tools for correcting its weaknesses. I should stress that as far as I know, the weaknesses of neoclassical economics, when supplemented by the findings of behavioral economics (especially concerning adaptive dynamics and non-self-regarding preferences), do not foster politically biased or ineffective social policy recommendations. Note that none of the commentators suggests that my preferred economic models give poor policy advice. They assert at best that others use economic theory opportunistically to give bad policy advice. This is of course correct.

I might remind the reader that evolutionary theory has the same problem as economic theory: people misuse it to give bad advice. Evolutionary theory has been misused to promote eugenics, atheism, theories of racial inferiority, Social Darwinian survival of the fittest, and genetic determinism. It does not matter how frequently and carefully we refute these claims, they refuse to die. There is no more justification for abandoning evolutionary theory for this reason than there is for abandoning economic theory.

David Wilson adds that the excessive mathematization of economic theory “has crowded out” evolutionary approaches such as agent-based computer simulation. I believe, by contrast, that economics needs more, not less, mathematics to deal with complexity and dynamics. Economists do learn a lot of math in their five or so years of graduate training, but not nearly as much as physicists, for example. Moreover, there are many more economists today than in the past, and there are thus more opportunities for specialization in which economists who study complexity dynamics acquire specialized knowledge not shared by all economists. There is no need for “crowding out.”

Finally, David Wilson argues that neoclassical theory is the product of physics envy, a theme famously espoused by Philip Mirowsky, as in his 1991 book More Heat Than Light: Economics as Social Physics, Physics as Nature’s Economics. I have never understood this critique because economic models are nothing like physics models. All of modern physics, at least since the development of quantum mechanics, are based on Lagrangian and Hamiltonian techniques, as well as conservation laws flowing from the symmetries of Lagrangians and Hamiltonians. There are no parallels in economic theory (or in evolutionary theory, for that matter). Neoclassical economics is “classical” in the sense that, like much of modern physics, it is based on the eighteenth- and nineteenth-century principles of the differential calculus and smooth dynamics on differential manifolds. However, considerations of complexity dynamics have invaded physics in quite the same way as it is now invading economic theory. This no more involves sweeping away the “old physics” than it means sweeping away the “old economics.”

Nestled within Robert Axtell’s thoughtful remarks is his denial of my assertion that real-world market economies are generally nearly in equilibrium, thus rendering comparative static analysis based on the general equilibrium model practically irrelevant. My assertion is based on the notion of “quasi-equilibrium” in complex dynamical systems, as developed in my work with Antoine Mandel. A quasi-equilibrium is a dynamic state in which there is constant low-level stochasticity in all markets, with the mean standard error of the absolute value of excess demand in all markets being very small, but with occasional large excursions from this state due wholly to complex local system resonances. In this framework, considerable volatility in a few markets, such as the stock market in many
countries, is possible and indeed is observed. Indeed, Mauro Gallegati, our coauthors, and I, and others, have shown that when we add a financial market to the general equilibrium model and use adaptive expectations, we find exactly such behavior in some financial markets.

I, of course, share Eric Beinhocker’s skepticism concerning the basic model of contemporary macroeconomics, the so-called Dynamic Stochastic General Equilibrium model. This model, however, has nothing in common with the general equilibrium model defended in my article. The train of thought in Beinhocker’s exploration of the scientific status of the general equilibrium model is certainly worth exploring, but it quickly moves us into the intricacies of epistemology and the philosophy of science. My own take on this is that philosophical critiques have never successfully undermined a theory favored by scientists. Philosophy has influenced scientific thinking (e.g., Mach’s critique of Newtonian physics was an inspiration to Einstein in formulating special relativity), but so has art, music, and even religion. Philosophy of science is an important field, but it in no sense guides or disciplines scientific activity.

Both the strengths and limitations of the general equilibrium model flow from its massive abstraction from institutional detail. Deploying a model of generalized market exchange requires constantly reminding ourselves not to go beyond the model’s limits. For instance, Samuel Bowles and I have shown that when one drops the model’s assumption that all market exchanges are costlessly enforceable, one can understand why there is positive unemployment in equilibrium, why the interest on capital is not determined purely by supply and demand, and why firms that supply quality goods are generally quantity-constrained (Bowles and Gintis 2000).

To appreciate the scientific value of the general market interaction model, we must abandon Popper-type notions that the validity of a theory is reducible to the accuracy of its predictions. Many models in science are simply so fundamentally predicated on abstraction from “details” that they must be evaluated not by themselves, but on the fruitfulness of the more detailed models that they tend to generate. The general equilibrium model is hugely successful in this sense, and the successful models it generates virtually always involve complex dynamics.

I am reminded of the basic models of de Sitter, Friedmann, and others in contemporary cosmology, based on the assumption that the universe is homogeneous and isotropic on sufficiently large scales. These models are absurdly simple (although their solutions are not) and of course they assume there are no such things as stars, galaxies, and clusters of galaxies. They thus assume away all the things we really care about. Yet harnessing real-world lumpiness to simple, radically reductivist, cosmological models has been extremely productive, and is the basis for all of modern cosmology.

I am in total sympathy with Paul Ormerod’s remarks. There are many unsolved problems in general equilibrium theory, the most looming being how to integrate financial markets, and more generally interperiod exchange, into an equilibrium model. It may be ultimately impossible. It is even difficult to introduce money into the model because money has no value in a market-clearing equilibrium, although in our quasi-equilibria money inevitably arises spontaneously through the complex market exchange process.

I am also in sympathy with Ulrich Witt’s frustration with contemporary economics’ ignorance concerning and devaluation of evolutionary dynamics. This is in sharp contrast with the discipline’s embracing of behavioral economics, despite its strong critique of the Homo economicus agent that characterized economic theory for a century or two. This acceptance proves that economics as a discipline is wide open to new ideas, especially if they have a solid empirical support. For this reason I do not accept that we must reject general equilibrium theory because it biases researchers against dynamics. I rather think that we will
need another decade or two of solid results of complexity economics before this field will be integrated into economic theory.

I want to close by noting that despite my love of complexity and evolutionary theory, I do not believe we have yet discovered fundamental principles that are contrary to the standard social policies recommended by influential economists based on standard neoclassical theory. In this sense behavioral economics is far ahead of complexity economics. I look forward to an equally bright future for the latter.

WORKS CITED
