

Book Review

DYNAMICS OF MARKETS: ECONOPHYSICS AND FINANCE

By Joseph L. McCauley, Cambridge University Press, Cambridge, 2004, 209 + xvi pages, ISBN 0-521-82447-8

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The nature of *econophysics* and its relationship with both economics and physics has become increasingly contentious and controversial recently (Ball [2]; Gallegati et al. [3], 2006). The term is quite recent, only neologized in 1995 by Eugene Stanley and then defined by him and Rosario Mantegna (Mantegna and Stanley [4, pages viii–ix]) as a “multidisciplinary field” consisting of “activities by physicists who are working on economics problems to test a variety of new conceptual approaches derived from the physical sciences.” Although physicists have introduced ideas into economics for decades and arguably even for more than a century (Mirowski [6]), this recent movement began in the 1980s, strongly stimulated by interactions in Brussels, Stuttgart, and most influentially, Santa Fe, and has become a full-blown subdiscipline of physics, with its own journals (especially *Physica A*, edited by Stanley) and conferences, if not yet professorial chairs.

A central argument of the econophysicists has been that their foundation in hard physical science makes them more appreciative of empirical realities and less wedded to empty abstract theories that do not explain reality. Much work has particularly emphasized the ubiquity of power law distributions in many financial time series and in some other areas of economics such as firm size and income and wealth distributions. This has set them up to varying degrees as critics of standard economic theory, although their critics such as Gallegati et al. [3] argue that they do not fully appreciate the theories that they are criticizing or the work that economists have done in the empirical area. This has opened up a division among the econophysicists regarding how much they should attempt to integrate their work with existing theoretical economic frameworks versus how much they should assault that theoretical framework and pose econophysics as a full-blown alternative that will (or should) replace it.

Joseph McCauley’s *Dynamics of Markets: Econophysics and Finance* represents the most forceful extended advocacy of this latter viewpoint in print, arguing indeed that economic

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theory is flawed from top to bottom and that at least certain ideas and approaches coming out of econophysics represent the way that economists themselves should go, not just their friendly associates, the econophysicists. Or, if you will, econophysics should simply replace economics as we know it today, a position that McCauley [5] has more recently argued even more explicitly in his reply to Gallegati et al. [3].

This book contains much that is brilliant and insightful. He has read much economics and is not just the garden-variety econophysicist ignorantly wandering into a pile of economic data with this or that favored physics model (although he does have his favored approaches), proclaiming to have the answer that economists do not know or understand while knowing nothing of what economists do know or understand. Certain sections contain clear descriptions of ideas that are not well known, and other parts provide an excellent overview of major developments in econophysics.

I am also sympathetic with much that it contains, as well as with much that the econophysicists more broadly are attempting to do. There are many problems with economic theory in itself, some of which McCauley highlights, although most of these that are reasonable critiques are known to many economists. It is also true that the general complaint of econophysicists, certainly echoed by McCauley, that economists have this tendency to ignore uncomfortable empirical realities that do not easily fit into their theories has much truth to it. In particular, I expect that the repeated pounding by econophysicists regarding the ubiquity of power-law distributions in much of economics will eventually come to be accepted and realized by economists, however they eventually accommodate this (or fail to do so) at the theoretical level.

Nevertheless, this book does have its flaws, with some of the more important ones involving some of its critiques of economic theory. I will run through some of these, but first I will describe in outline the topics and basic arguments provided in the book.

There are nine chapters plus a preface in the book. The first provides a general argument. The second focuses on criticizing neoclassical economic theory. The third reviews several useful ideas from probability theory and stochastic processes, with a special emphasis on asymmetric and stretched exponential distributions, which McCauley sees as especially useful. In Chapters 4 and 5, he shifts to financial theory and criticizes it and its foundation upon standard economic theory, even as he considers the Black-Scholes model to be one of the few in economics that can be considered to be truly scientific in the sense of proposing an invariance principle that is capable of refutation. Chapter 6 is arguably the greatest positive contribution of the book, where McCauley develops his alternative approach to pricing options based largely on exponential distributions. Chapters 7 and 8 can be seen as criticisms of other econophysics approaches, with thermodynamics models the target in 7, and 8 dealing with a broader set of alternatives. The final chapter is on complexity and seems somewhat disconnected from the rest of the book, although it is interesting, if idiosyncratic, in its own right, with McCauley much taken with algorithmic definitions of complexity and critical of efforts to develop an “econobiology” alternative to econophysics.

Probably the most questionable argument in the entire book appears in its first few pages: that true science is the discovery of invariance principles and that the failure of economists to find such principles or to find equilibria in markets that remain constant,

is the greatest problem with economics. This argument resonates throughout the book and is repeated in the final chapter when he criticizes potentially nascent econobiology, noting that while biological systems are clearly very complex, they are difficult to describe mathematically and may be even more lacking in invariance principles than economics, which at least has the Black-Scholes formula. However, the distributions emerging from population genetics strike this observer as pretty rigorous and as well as well-founded empirically, with this field having generated much of the statistical basis for modern econometrics.

In any case, he even has a subsection of Chapter 7 titled, “The challenge: to find at least one stable market.” There are two issues here. One involves simply answering this question. The other is to note the more standard view of economists, which McCauley simply dismisses, that indeed equilibria change over time as their underlying determinants change over time, that economic reality is a sequence of temporary equilibria, an argument McCauley ridicules. While many economists believe that markets are never (or rarely) in equilibrium, few will go along with McCauley’s argument that the fact that equilibria may move over time thereby destroys the usefulness of the equilibrium concept because of the implied failure to adhere to an invariance principle that is involved. Both Walras and Marshall, the founders of neoclassical economic orthodoxy, were fully aware that equilibria move and change and spent much of their time discussing how this happens, even if many find their discussions inadequate. Indeed, the old argument is that even though the equilibria move and the economy may not generally be in equilibrium, they drive system dynamics crucially as “centers of gravitation” around which the dynamics move, Newtonian terminology dating back to the older classical economists such as Adam Smith and Karl Marx. In Chapter 4, McCauley dismisses this sort of notion on the grounds that it requires stationarity, but it only requires stationarity if the equilibrium does not move. This is not a refutation.

Regarding the first point, the stability or instability of markets, I fully grant that unregulated financial markets tend to exhibit high degrees of volatility that lead to the notorious “fat tails” they exhibit in their distributions that are so amenable to the power laws favored by so many econophysicists (even as McCauley disagrees that there are “universal” exponents for such distributions, as argued by some econophysicists). Indeed, it is unclear that either asset markets or foreign exchange markets (a special kind of asset) really do have well-defined equilibria, with McCauley presenting some of the reasons in Chapter 4. Nevertheless, while they may not fulfill conditions for testing that McCauley would demand, there are many nonfinancial markets where prices change infrequently and move slowly over time, especially those for consumer goods where there appears to be some degree of monopoly or oligopoly power.

Automobiles would be a good example, although there are many others. Prices for certain makes are set for a year and do not change at all, although there are individual variations for individual purchases based on local conditions and buyers’ assets and abilities to bargain. As different year’s models are slightly different, arguably they should have different prices, which they generally do, but these prices tend to change slowly from year to year for given makes, especially in real terms. Within a given year they are generally extremely stable, as are those of many other such consumer items. It is hard to argue

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seriously that such markets exhibit any sort of serious (or even noticeable) instability or volatility. One can argue that they are “regulated” in some sense because of oligopoly, but it can be argued that many of these markets are really quite competitive, despite the large sizes of the firms in them. Again, the automobile industry is a good example, and the days when the “big three” dominated and set the prices fairly freely in the US are long past. The world automobile industry is now highly competitive, if still “imperfectly” so, but still exhibits tremendous stability that one does not see in the markets for stocks or commodities.

Furthermore, while there are all kinds of reasons to question the existence or uniqueness or stability of general equilibria on theoretical grounds, as McCauley does in Chapter 2, many individual markets exhibit powerful tendencies to stable equilibria, even as those equilibria may be moving over time. However, it is now known that the characteristics of markets may importantly depend on how they are structured and operate. Thus, it has been known since at least 1962 (Smith [8]) that individual markets organized as double auctions will generally move rapidly to equilibria, even in situations where there are very few agents in the market, even as few as three. This is by now an enormously robust empirical finding that has been replicated many times in economic experiments as well as being observed in the field, but McCauley somehow never mentions this empirical fact at all. His challenge is simply all wet. There are many markets that exhibit high levels of stability, even if many financial markets do not do so, and general equilibria may not do so (or even exist), and even if, crucially for McCauley, the equilibria that they move to so readily may move over time.

Many of the criticisms that McCauley raises against neoclassical theory are ones that are well known to economists, even if they conveniently assume them away on a regular basis in models they use to retain mathematical tractability. A short list of these includes problems with information, optimization, aggregation, integrability, stability, and others. While economists have not clearly solved these problems, especially in an integrated and rigorous manner, many of them are being studied and worked on, sometimes under the rubric of behavioral economics, sometimes under the rubric of heterogeneous agents and complexity, as well as other approaches. Rather than drag through a laundry list, I will simply note three criticisms that McCauley makes that seem to me to be more seriously misguided.

One occurs in the first chapter when he dismisses the Phillips curve analysis of an empirical tradeoff between inflation and unemployment at the macroeconomic level. This dismissal coincides with a criticism of a “Bourbakian” tendency to separate theory from empirics, a point that has some validity. However, he proceeds to dismiss regression analysis on the grounds that it merely shows correlation not causation, a criticism that is relevant to any econometric technique, including those used by econophysicists. What is more annoying is that he presents in Figure 1.1 a supposed set of points for a Phillips curve and then shows an upward-sloping line that is supposed to fit them, declaring that it obviously does not do so. There are several problems here.

The first is that it is not clear that these are real data points, and if they are, what they are or what period they represent. Second is that the usual hypothesis about the Phillips curve is that it is downward sloping rather than the upward sloping he indicates (if he

wanted to show that real data might lead to this counterintuitive result, then he should have made clear he was using real data). Third is that standard regression analysis provides measures of goodness-of-fit, so that one can tell how closely a set of points fits to a hypothetical correlated relationship or not. Finally, the Phillips curve is a highly controversial idea, with many macroeconomists of the new classical rational expectations school rejecting it, or more precisely, arguing that if expectations are fulfilled, it will be vertical (although empirical evidence for this is weak). In any case, its theoretical foundation is at best sketchy, and even economists who think it is downward sloping generally view it as stable only in the very short run. In short, McCauley has set up a straw man here that is not even made out of decent straw.

Another matter appears in Chapter 2 where he draws on work of M.F.M. Osborne to supposedly refute the law of diminishing marginal utility as the foundation of the downward-sloping demand curve (he is a great fan of Osborne's, crediting him with prefiguring the Black-Scholes model and praising him for his advocacy of the lognormal distribution of asset returns, even as this has been largely rejected by most subsequent econophysicists). McCauley poses the problem of "how many green jackets does a consumer want?" He argues that thinking in terms of a continuous downward-sloping demand curve is incorrect because "clearly no consumer thinks this way" (page 21). He then argues that demand curves must be step functions. This is simply the problem of "lumpiness" that is well known in economics. Certainly at the individual level, demand curves will be step functions for bulky items, but this washes out when one aggregates over individuals to the level of the market. Furthermore, the problem is posed to precisely as "green jackets," when in fact one might see more clear cut behavior if one poses it as "jackets" in general. Finally, and most crucially, he does not refute the claim that in the end the demand relationship will be downwardly sloping, even for the individual and even if it does so as a step function. The lumpiness of commodities may refute the individually continuous demand curve, but it does not at all refute the downward direction that the steps will take in such a function for lumpy goods. This is yet another straw man.

Finally, in Chapter 4, there is a critique of corporate finance models of valuing companies and their assets. Much of this critique amounts to noting that there are many different ways to do this, such as book value versus the value of firm equity, and that they almost never coincide in fact. This is well known to financial accountants, if not always to economists. In theory, all these measures should coincide if there were a perfect intertemporal equilibrium. That they do not can be viewed as evidence that such an equilibrium does not exist in fact, but then few economists believe that anyway. There is imperfect arbitrage between these aspects of a firm, often due to liquidity problems that McCauley does mention. In effect, there are distinct markets for these distinct aspects of firms. If one wishes to study them, one must study them separately, which is what is routinely done. Some, such as the book-value market, are likely to be quite stable and slowly changing, much like the market for automobiles. The market for equity is much more volatile, as are all unregulated financial markets. McCauley proceeds to critique many of the standard models for financial markets, such as CAPM and Black-Scholes, and generally I agree with his critiques. The question then becomes whether he provides a solution to these problems.

As noted above, the positive part of this book is in Chapter 6 where McCauley presents his model of option pricing based on the exponential distribution. He argues, accurately I think, that the exponential distribution fits US bond market data better than does the Gaussian distribution. He then provides a model for pricing options based on this. This is well done, but I note some limits, which McCauley seems to be aware of to varying degrees.

One is that the exponential distribution function has a discontinuity. This is overcome by introducing a diffusion term that appears in the relevant Fokker-Plank equation to generate a viable density function. This implies an infinity of solutions. He argues that this can be partly dealt with using a delta hedge strategy and that it is not a serious problem over short time horizons. However, this depends on making an estimate of expected returns. He draws on financial practitioners to argue that this should be somewhat above that implied by the no-arbitrage condition in order to cover “carrying costs,” in short for risks of extreme outcomes. This leads to the problem that the exponential distribution is not all that good for dealing with extreme events, the infamous “fat tails” that undo the Gaussian distribution. McCauley provides ways of estimating the necessary diffusion coefficient, but admits (page 145) that “Our prediction for the diffusion coefficient should be tested directly empirically, but that task is nontrivial.” Indeed.

The bottom line here is that there is no real comparison with alternative approaches assuming different distributions that are non-Gaussian. McCauley dismisses with some reason claims of “universality” regarding power law exponents for distributions made by some other econophysicists, but he does not make a direct comparison between his approach and other alternatives. He may have provided an approach that is somewhat better than standard Black-Scholes, but it continues to have many of the same problems that the standard approach has, ultimately relying on arbitrary assumptions and unable to deal with the most difficult situations. There is a genuine contribution here, but it should not be overstated.

I will not discuss at length here McCauley’s views on complexity presented in Chapter 9 as I have already done so elsewhere (Rosser [7]). I simply note that his algorithmic view of complexity is one among many, with most economists who study complexity tending to use other definitions or viewpoints than the algorithmic one, even as some do focus on that view (Albin and Foley [1]; Velupillai [9]). His critique of “econobiology” very much depends on a particular interpretation of the algorithmic view, and under other perspectives on complexity, evolutionary, or biological approaches may be found to be useful.

There is much in this book that is value. McCauley suggests a number of approaches and techniques that offer promise, if with limits. However, one must be careful of arguments that overstate points from time to time in the book. In any case, it is a stimulating and distinctive look at serious issues in financial economics and econophysics. His views will have an impact.

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