

ECONOPHYSICS AND ECONOMIC COMPLEXITY

J. Barkley Rosser, Jr.
James Madison University
rosserjb@jmu.edu
<http://cob.jmu.edu/rosserjb>

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I. Introduction

This paper will focus upon the confluence of two strands of discussion and debate that have been developing for some time and their interaction and mutual implications. One involves the nature of *economic complexity*, how it is to be defined, what is the best way of thinking about it, both theoretically and empirically. The other is the question of the nature and relevance for economics of the recently developed sub-discipline of *econophysics*. Debates over both of these strands have become more intensified in recent years, and this observer sees the debates as having relevance for each other.

We shall proceed first by considering the recent debate within economics over the concept of complexity (Israel, 2005; Markose, 2005; McCauley, 2005; Velupillai, 2005; Rosser, 2007a), which has featured most particularly a resurgence of the idea that only computationally (or computably)¹ based definitions of complexity are sufficiently rigorous and measurable to be useful in any science, including particularly economics, although also presumably physics and its peculiar recent offspring, econophysics, as well. This resurgence has in particular involved criticism of more dynamically based definitions of complexity that have tended to be used more widely and frequently in economics over recent decades. While the arguments of the advocates of the computational approach have some merit, it is argued that their criticisms of the dynamic approach are in some ways overdone.

Then we shall move to the separate strand of debate that has involved the nature and relevance to economics of the recently developed sub-discipline of econophysics (Mantegna and Stanley, 2000; Ball, 2006; Gallegati, Keen, Lux, and Ormerod, 2006;

¹ The more widespread *computational economics* focuses more on issues related to specific programs used to solve certain economic problems. *Computable economics*, neologized by Velupillai (2000), refers to a deeper focus on the logical foundations of computability of certain solutions in economics, although clearly there is some overlap between these two approaches.

McCauley, 2006; Rosser, 2007b; Yakovenko, 2007). In particular, while econophysicists have made strong claims about the superiority of their approaches, even going so far as to argue that econophysics should replace standard economics as such (McCauley, 2004), the critics have argued that there have been some serious flaws in much econophysics work, including ignorance of relevant work in economics, inappropriate use of statistics, excessive and unwarranted assertions of finding universal laws, and a failure to provide adequate theory for the models used. Again, as with the complexity debate, points made by both sides are found to have some reasonableness to them.

Finally we shall consider how these debates come together. A particularly striking episode provides a way for us to see their link, that of the failure of the Sornette (2003) and Sornette and Zhou (2005) models to predict financial market crashes as was claimed would be the case. One of the solutions that has been posed for the debate over econophysics has involved proposing collaboration between economists and physicists in this line of research. This has been spreading in fact. However, as warned in Lux and Sornette (2002) and described in more detail in Lux (2007), use of inadequate and inappropriate economics models in this collaboration can lead to problems. More particularly, Sornette (2003) and Sornette and Zhou (2005) relied upon a conventional neoclassical model assuming a homogeneous rational agent. Rather what would be more appropriate would be the use of dynamically complex economic approaches involving heterogeneous interacting agents, as for example used by Gallegati, Palestrini, and Rosser (2007) to model exotic phenomena such as the details of financial market crashes.

II. Relevant Views of Economic Complexity

While as of a decade ago (Horgan, 1997, p. 305) Seth Lloyd had compiled 45 distinct definitions of “complexity,” many of these are variations on each other, with many of them related to concepts of computability or difficulty of computation or minimum algorithmic length. Some are quite vague and not particularly amenable to any scientific application or testability. However, even compressing related definitions together into single categories and eliminating those that are effectively vacuous, there remain a substantial number of candidates for scientific application, with many of these having been used by economists. Rosser (1999, 2000, 2007a) has argued for the use of a dynamic definition originated by Richard Day (1994). This is that a system is dynamically complex if it does not converge endogenously to a point, a limit cycle, or an explosion or implosion. Clearly there are terms in this definition that are themselves controversial and calling for definition, especially the troublesome concept of endogeneity, which can be extremely difficult to distinguish empirically from exogeneity. Also, some consider convergence to a limit cycle to be sufficient to constitute such dynamic complexity,² while others would argue that convergence to any even numbered, finite periodicity of a cycle not to be dynamically complex. Nevertheless, this definition does contain many models that have been of interest to economists, including the “four C’s” of cybernetics,³ catastrophe, chaos, and heterogeneous agents type complexity.⁴

It has generally been argued that such dynamic complexity necessarily involves nonlinearity of the dynamical system underlying it. Clearly nonlinearity is not a

² It has been known for some time that for many economic growth models, changes in certain control parameters can lead to limit cycles through the Hopf bifurcation (Magill, 1977).

³ Cybernetics arguably had two distinct threads, cybernetics proper initiated by Norbert Wiener (1961) and deriving from mathematical logic and biology, and system dynamics initiated by Jay W. Forrester (1961) and deriving from engineering and computer science, with both emphasizing the roles of positive and negative feedback in dynamical systems. See Richardson (1991) for further discussion.

⁴ Rosser (1999) has labeled this latter type as “small tent” complexity, while calling the broader concept that contains all four of these as being “big tent” complexity.

sufficient condition for such complexity, as there are plenty of nonlinear growth models that follow smooth expansions without irregularity in an asymptotic convergence on infinity. For that matter, it is well known that some systems that are complex, for example deterministically chaotic, for certain parameter values will be well-behaved and simply converge on a single point for other parameters. A simple example is the quadratic logistic, given in Equation 1, which converges on a point for low values of its tuning parameter, a , but exhibits increasing complexity as this parameter increases, passing through a sequence of period-doubling bifurcations until reaching a zone of aperiodic oscillations of a fully chaotic nature (May, 1976). However, it has long been known that such endogenously complex dynamics can arise from coupled linear systems with lags (Goodwin, 1947; Turing, 1952), although the uncoupled equivalent of such systems is indeed nonlinear.

$$x(t) = ax(1 - x) \tag{1}$$

Another approach is to draw on the original dictionary definitions of the word, “complex.” These emphasize the idea of a whole that is composed of parts, with this taking a variety of forms, and with some of the oldest usages in English referring to the complex whole of the human who is composed of both a body and a soul (OED, 1971, p. 492). This can be seen as implying or hinting at the concept of *emergence*, generally seen as a whole, or a higher-order structure, arising from a set of lower level structures or phenomena or processes. Thus, the physicist Giorgio Parisi (1999, p. 560) refers to a system being complex “if its behavior crucially depends on the details of its parts.” Crutchfield (1994) has emphasized this in a computational context, Haken (1983) has done so using the concept of *synergetics* (also a dynamically complex concept), and

many in biology have seen it as the key to the principle of self-organization and the evolution of life from single cell organisms to what we see around us now (Kauffman, 1993). It is also arguably the key to Herbert Simon's (1962) concept of hierarchical complexity, in which sets of distinctly operating sub-systems are combined to form a higher-order operating system.⁵

At this point the question arises regarding the closely related word, “complicated.” Many have viewed these as being essentially synonyms, including von Neumann in his final book on automata (1966). However, as Israel (2005) emphasizes, they come from different Latin roots: “complex” from *complecti*, “grasp, comprehend, or embrace” and “complicated” from *complicare*, “fold, envelop.” The OED (1971, p. 493) recognizes this distinction, even as it sees the closeness of the two concepts. Thus, “complicate” is seen as involving the intertwining together of separate things, with the word also apparently appearing in English initially in the mid-1600s at about the same time as “complex.” The key difference seems to be that complicatedness does not involve the emergence or appearance of some higher-order whole. It is simply an aggregation of a bunch of distinct things, tangled up together in a way such that they cannot be easily disentangled.

In terms of economic models and applications, the term “complicated” would seem to be more suited to work that has been claimed to represent *structural complexity* (Pryor, 1995; Stodder, 1995). What is involved in these works is considering the many interconnections that exist within economies, the indirect connections that suggest the old adage “everything is connected to everything else.” While this may be true in some

⁵ Arguably related to the Simon view of hierarchical complexity is the approach of Chomsky (1959) in his analysis of grammatical hierarchies. This has been taken up more recently by Mirowski (2007) in an argument about the evolution of emergent market forms, which we shall consider later in this paper.

sense, it does not necessarily imply a higher-order structure or system emerging from all this interconnectedness. Thus, Pryor describes the US economy and its many sectors and the many surprising and interesting links and interconnections that exist within it. But, in effect, all he shows is that a fully descriptive input-output matrix of the US economy would be of very high dimension and have many non-zero elements in it.

This brings us to the broad category of Seth Lloyd's that has the most definitions that can be related to it: computational or algorithmic complexity. While there had been some economists using various versions of this as their method of approach to studying economic complexity prior to 2000 (Lewis, 1985; Albin with Foley, 1998), it has been more recently that Velupillai (2000, 2005) along with Markose (2005) and McCauley (2005), among others, have pushed harder for the idea that computational complexity of one sort or another is the superior definition or approach, based on its greater degree of rigor and precision. It is arguably Velupillai who has done more to pull together the strands of this argument, linking the various notions of Church, Turing, Tarski, Shannon, Kolmogorov, Solomonoff, Chaitin, Rissanen, and others into a more or less coherent version of this view of complexity, especially as it relates to economics.

Velupillai (2005) lays out a development that ultimately derives from the Incompleteness Theorem of Kurt Gödel, whose implications for the existence of recursive functions that can be solved in finite time, that is computed, were understood initially by Alonzo Church (1936) and Alan Turing (1936-37) in what is now known as the Church-Turing thesis. Their arguments underlie the most fundamental definition of computational complexity, that a program or system does not compute, cannot be solved,

goes into an infinite do-loop, does not halt (the so-called *halting problem*).⁶ It must be noted that neither Church nor Turing discussed computability as such as programmable computers had not yet been invented when they were writing.

However, besides this basic definition of computational complexity as being that which is not in fact computable, another strand of the argument has developed at an intermediate level, the idea of measures of degrees of complexity that fall short of this more absolute form of complexity. In these cases we are dealing with systems or programs that are computable, but the question arises about how difficult they are to solve. Here several alternatives have competed for attention. Velupillai argues that most of these definitions ultimately derive from Shannon's (1948) entropic measure of information content, which has come to be understood to equal the number of bits in an algorithm that computes the measure. From this Kolmogorov (1965) defined what is now called *Kolmogorov complexity* as the minimum number of bits in any algorithm that does not prefix any other algorithm that a Universal Turing Machine would require to compute a binary string of information. Chaitin (1987) independently discovered this measure and extended it to his minimum description length concept. His work linked back to the original work by Gödel and would serve as the inspiration for Albin with Foley as well as Lewis in their applications to economics.

These and related definitions suffer from a problem pointed out by Velupillai: they are not themselves computable. This lacuna would be corrected by Jorma Rissanen (1989) with his concept of *stochastic complexity*, which intuitively involves seeking a model that provides the shortest description of the regular features of a string. Thus,

⁶ For a sophisticated overview of the halting problem as the essence of computational complexity, see Blum, Cucker, Shub, and Smale, 1998.

Rissanen (2005, pp. 89-90) defines a likelihood function for a given structure as a class of parametric density functions that can be viewed as respective models, where θ represents a set of k parameters and x is a given data string indexed by n :

$$M_k = \{f(x^n, \theta): \theta \in \mathbf{R}^k\}.$$

(2)

For a given f , with $f(y^n)$ a set of “normal strings,” the *normalized maximum likelihood function* will be given by

$$f^*(x^n, M_k) = f(x^n, \theta^*(x^n)) / [\int_{\theta(y^n)} f(y^n, \theta(y^n)) dy^n], \quad (3)$$

where the denominator of the right-hand side can be defined as $C_{c,k}$. From this *stochastic complexity* is given by

$$-\ln f^*(x^n, M_k) = -\ln f(x^n, \theta^*(x^n)) + C_{n,k}. \quad (4)$$

This term can be interpreted as representing “the ‘shortest code length’ for the data x^n that can be obtained for the model class M_k ” (Rissanen, 2005, p. 90). This is a computable measure of complexity based on the earlier ideas of Kolmogorov, Chaitin, and others. It can be posed by the advocates of computational complexity as more rigorous than the other definitions and measures, even if there is now no longer a clear division between what is complex and what is not using this measure.

Advocates of this measure have especially ridiculed the concept of *emergence* that is emphasized by so many other views of complexity. It is seen as vague and unrigorous, a revival of the old British “emergentism” of Mill (1843) and Lloyd Morgan (1923) that was dismissed in the 1930s on precisely these grounds. McCauley (2004) identifies it particularly with biology and evolution, arguing that it is unscientific because it does not involve an invariance principle, which he sees as the key to science, especially

as practiced by physicists.⁷ Rosser (2009) provides a response drawing on bifurcation theory and theories of multi-level evolution to this argument of the computational school, as well as noting that at least the dynamic definition provides a reasonably clear criterion to distinguish what is complex from what is not, which is useful for a wide array of models used by economists. We leave this argument for now, but shall return to it later.

III. Econophysics and its Controversies

It can be argued that what is now widely viewed as econophysics originally came out of economics and went into physics, in particular the idea of power law distributions, which was first studied by Vilfredo Pareto (1897) in regard to income distribution. This would pass mostly into statistics and physics and reappear occasionally in the 1960s and 1970s in the work of such people as Mandelbrot (1963) on the distribution of cotton prices⁸ and Ijiri and Simon (1977) on the distribution of firm sizes. Its revival in the 1990s would largely come through the efforts of what have been labeled “econophysicists” since this term was neologized by Eugene Stanley in 1995 (Chakrabarti, 2005, p. 225), with Mantegna and Stanley (2002, viii-ix) defining econophysics as “a neologism that denotes the activities of physicists who are working on economics problems to test a variety of new conceptual approaches deriving from the physical sciences.”⁹

⁷ McCauley also argues for the superiority of the “complete surprise” approach of Moore (1990) over this emergentist view.

⁸ This work of Mandelbrot would lead to his later development of the concept of fractality, with multi-fractality arguably a form of econophysics modeling, although it has been more published in economics journals than in physics (Mandelbrot, 1997; Mandelbrot, Fisher, and Calvet, 1997; Calvet and Fisher, 2001).

⁹ For further discussion of the historical and definitional issues, see Rosser (2006).

Also, what is now viewed as the competing, conventional economics model of lognormal distributions of various economic variables was first introduced in a mathematics dissertation by Bachelier (1900), only to go into use in physics in the study of Brownian motion, with physicists such as Osborne (1959) being among those advocating its use in economics to study financial markets, where it would come to reign as the standard tool of orthodox financial economics. This going back and forth reflects a deeper exchange that has gone on between economics and physics for at least 200 years, with a similar exchange also going on between economics biology as well.

So, standard economics has tended to assume that distributions of economic variables are more often than not either Gaussian normal, or some simple transformation thereof, such as the lognormal. This became entrenched partly because of the ubiquity of the Gaussian assumption in standard econometrics, and also its convenience for parsimonious forms of such useful formulae as the Black-Scholes options pricing model (Black and Scholes, 1973). That financial returns exhibit leptokurtotic fat tails that are not well modeled by normal or lognormal distributions, as well as other economic variable distributions, has only recently come to be widely recognized, and is still not as known among economists as it should be. This opened the door for the use of power law distributions and other distributions that are better suited to the representation of such fat tail phenomena, with econophysicists leading the way for much of this reintroduction and analysis.

A generic form of a power law distribution is that introduced by Pareto (1897), which can be given by N as the number of observations above a certain limit x , with A and α being constants:

$$N = A/x^\alpha. \quad (5)$$

Canonically, the sign of a power law distribution is that it will be linear in a log-log representation of the given data. A special case of the Pareto is when $\alpha = 1$, which gives the Zipf (1941), which was first suggested as explaining the distribution of city sizes. A variety of other distributions have also been used to study economic variables that exhibit leptokurtosis or skewness, with some that do not exhibit quite as fat, fat tails as the Pareto including the Lévy (1925), used by Mantegna (1991) in an early effort at econophysics.

The application of such distributions to economic data has proceeded apace in recent years with an explosion of studies. The most extensive studies have been done on financial markets, with good overviews and selections to be found in Chatterjee and Chakrabarti (2006) and Lux (2007). Another area of fruitful study has been on wealth and income distributions, recalling the original ideas of Pareto, with good overviews and selections to be found in Chatterjee, Yalagadda, and Chakrabarti (2005), and Lux (2007). Also, the findings of Zipf on city sizes have been generalized by more recent studies (Gabaix, 1999; Nitsch, 2005), as well as those of Ijiri and Simon on firm sizes (Stanley, Amaral, Buldyrev, Havlin, Leschhorn, Mass, Sanlinger, and Stanley, 1996; Axtell, 2001). Other topics have also come under the purview of econophysics methods including growth rates of countries (Canning, Amaral, Lee, Meyer, and Stanley, 1998) and the distribution of research outputs (Plerou, Amaral, Gopkrishnan, Meyer, and Stanley, 1999).

Now as recounted in Ball (2006; Gallegati, Keen, Lux, and Ormerod, 2006; McCauley, 2006; Rosser (2007), the debate over econophysics has erupted quite vigorously recently after this long period of fruitful development. Again, the main

complaints of Gallegati, Keen, Lux, and Ormerod in particular were four: a lack of awareness by econophysicists of relevant economic literature (with resulting exaggerated claims of originality), poor statistical methodologies, excessive claims of finding universal laws, and a lack of proper theoretical models to explain the empirical phenomena studied. McCauley has responded to these charges by arguing the lack of invariance laws in economics, noting especially the problem of identification in the law of supply and demand, with the superiority of physics methods claimed.

Rosser (2007b) in effect combines the first and last and the middle two of these complaints into two broader arguments, theoretical and empirical. Regarding the empirical arguments, these seem quite strong for many studies. Thus, there has been considerable reliance on “eyeballing” figures rather than using rigorous significance or other statistical tests. This has coincided with the claims of universality of coefficients estimated, when these do not seem so universal.¹⁰ Thus, while some have claimed to have found universal coefficients on income or wealth distribution, there is considerable evidence that these vary from country to country and across time within a single country, as argued by Kuznets (1955), who saw inequality tending to increase in a society as it initially industrializes (as for example China now) and then becoming more equal in its distribution in the more post-industrial phase (as in much of Western Europe). Furthermore, there were the failed forecasts of stock market crashes based on Johansen and Sornette (2001) that would be made by Sornette and Zhou (2005), of which more will be said in the next section.

¹⁰ However, there may be some phenomena for which universality may hold, such as certain sizes for exponents on trading volume and trades per time unit (Gopakrishnan, Plerou, Gabaix, Amaral, and Stanley, 2001), although these claims have been disputed (Farmer and Lillo, 2004).

The theoretical issue is more difficult. Gallegati, Keen, Lux, and Ormerod focus on the fact that most of the models used in econophysics only hold for pure exchange models, which they argue with some reason are of limited interest, given the importance of understanding and analyzing production. They express their sympathy for the criticism of orthodox economic theory that the econophysicists provide, but in effect argue that criticism is not sufficient, that theoretically sound alternatives are needed. Many of the models used by econophysicists draw on statistical mechanics, and these have been adopted by a number of economists before the econophysicists really got going, even if the limits of their conservation assumptions were understood (Föllmer, 1974).

In this regard, Rosser (2007b) offers a possible solution, albeit one proposed by an economist (Foley, 1994). This is the use of certain entropic formulations. Now the Gibbsian model that Foley introduces is mostly applied for pure exchange situations with conservation. However, his framework does allow for both production and exchange. His solution of “entropy prices” is not one of general equilibrium, but one with a statistical distribution of behaviors coming out of the model, relying on an assumption of entropy maximization. Assuming n agents of r types k with offer set A^k , $h^k[x]$ is the proportion of agents of type k who achieve a particular transaction x . Multiplicity of an assignment is the number of ways n agents can be assigned to S actions, with n_s being the number of agents assigned to each action s and is given by

$$W\{[n_s]\} = n!/(n_1! \dots n_s! \dots n_s!). \quad (6)$$

This measure has a measure that is its informational entropy, which can be maximized subject to feasibility constraints, which if non-empty yield a canonical Gibbs (1902) form

$$h^k[x] = \exp[-\pi x] / \sum_x \exp[-\pi x], \quad (7)$$

where the vectors $\pi \in \mathbf{R}^m$ are the entropy shadow prices. This model has some peculiarities, including an assumption that all possible transactions have equal probability and with also the possibility of negative prices (although such are sometimes observed in the real world, especially when environmental costs are involved, as for example with real estate). Of course, it must be noted that this is a formulation not only put together by an economist rather than a physicist, but it is one that is based on informational entropy rather than the thermodynamic variety, and hence not really physics.

IV. What Kinds of Economists Should Physicists Collaborate With? A Cautionary Tale

Now a common refrain that comes out of these debates and discussions is that the solution to these various problems is for economists and physicists to collaborate on studying these matters. This certainly has appeal and sounds reasonable. However, there is a rather serious caveat that must be recognized at this point. It matters seriously what kinds of economists and what kinds of models they bring to the table in such collaborations. This is because the statistical mechanics models that underlie most of these empirically useful econophysics formulations implicitly assume heterogeneous agents. Hence, they are most suited to being paired with more heterodox economists who are accustomed to using such models and thinking in such terms. While physicists may not think there is much going on here, the hard fact is that established economic orthodoxy has long assumed that one can explain economies while assuming that agents are both homogeneous and rational. The evidence supporting these assumptions has been

weakening for a long time, but these assumptions remain powerful in the minds of many economists.

It so happens that in recent work we have a rather astounding example of how econophysicists can go wrong when they do not follow the advice of the more heterodox schools of economic thought. This is the case of the failed forecasts by Sornette (2003) and his various coauthors regarding stock market crashes. What is most ironic here is that Sornette had engaged in a study in collaboration with Thomas Lux (2002), which should have warned him away from the path he pursued. This study examined the predictions of a power law model derived from the rational stochastic crashing bubble model of Blanchard and Watson (1982). They found this model not to fit the empirical data well. However, when he was not coauthoring with Lux, Sornette continued to develop models that relied upon this failed framework. It is not surprising that while they were able to replicate the trajectories of several past bubbles and their crashes, when it came to predicting out of sample crashes on ongoing bubbles, they completely failed (Lux, 2007). What was the problem?

The basic problem is that indeed the Blanchard and Watson model is an orthodox model that assumes homogeneous rational agents. What is involved is that when a bubble starts (presumably through some stochastic shock not modeled), the homogeneous rational agents know that the bubble will crash suddenly at some point in finite time. It is also the case that as the bubble proceeds, the probability of this crash will increase, and this probability will be common knowledge among all these rational agents, who also happen to be risk-averse. Therefore, in comparison with an exponentially rising bubble that might simply go on forever, as is may be possible in certain circumstances with

overlapping generations models (Tirole, 1985), these bubbles must not only rise faster than that and must accelerate their rate of increase. This is because these rational, risk-averse agents will demand a risk premium to cover the probability of the crash. As the bubble proceeds and the probability increases, the risk premium must also rise, which provides the stimulus for the rate of rise to accelerate. The mathematics are such that the rate of increase will go to infinity (and the bubble itself will also go to infinity) in finite time. It is this time of infinite explosion that became the basis for forecasting crashes in actual bubbles. However, as already noted, while able to mimic some past bubbles, Sornette and his collaborators failed to forecast future crashes.

Now, one possible explanation for him being attracted to this model, besides its “respectability” in established economic theory, was that he was not out of the usual statistical mechanics background from which most econophysicists have come. Rather he has been a student of geophysics and of earthquakes, with crashes thus being modeled as similar to earthquakes. Thus, he was more open to these more standard economics models of homogeneous rational agents.

Furthermore, while nearly all models of rational bubbles and crashes involve a rapid rise in price, followed by a sudden crash back to the fundamental, this is not what most historical bubbles look like. Kindleberger (2000) has shown that the vast majority of actual, historical bubbles, including most of the famous ones, follow a different pattern, with a peak being followed by a period of gradual decline for a while, a “period of financial distress,” which is then followed by a crash. Also, some bubbles just decline gradually, after having gone up rapidly. These three patterns are shown in Figure 1.

[insert Figure 1 here]

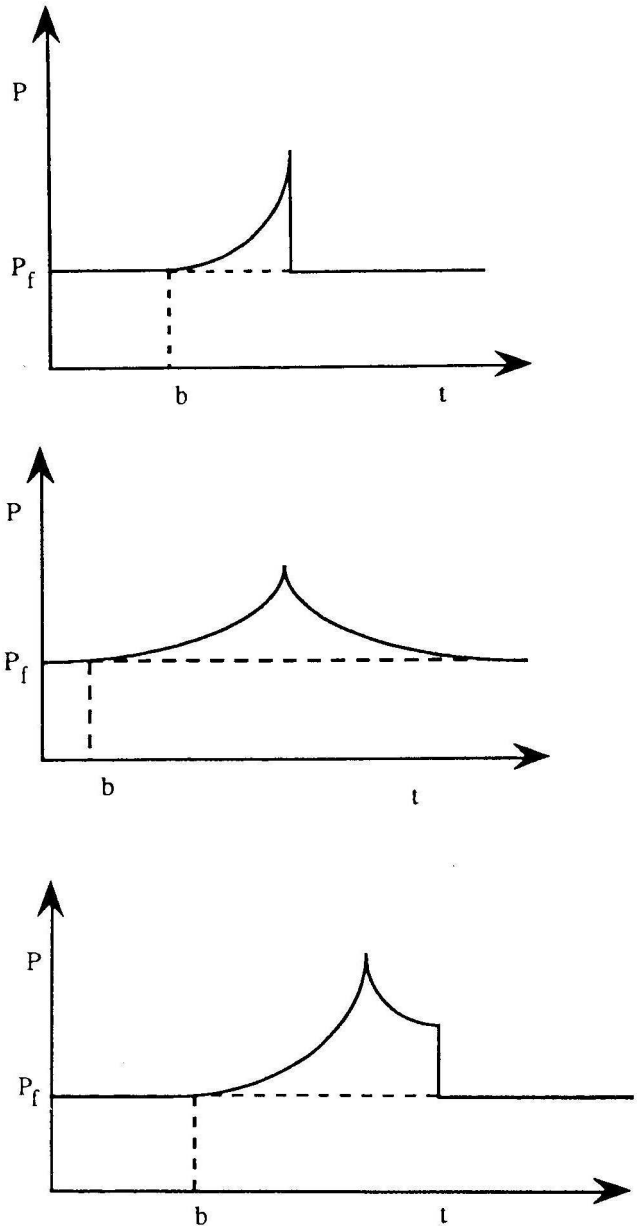


Figure 1

To date there has been only one model that has been able to show this most common of historical patterns for bubbles. It is the model of Gallegati, Palestrini, and Rosser (2008), which is a model of heterogeneous interacting agents, which is based on a formulation of a model originally due to Brock and Hommes (1997). Crucial parameters in this model determine the degree of interaction between agents (“herding”) and also the

willingness of agents to change their strategies. An additional feature, which resembles real world situations, is that of financial constraints. The combination of these elements allows for the pattern to appear described by Kindleberger, which in turn draws on earlier discussion by Minsky (1972).

Hence, econophysicists should seek to collaborate more with economists whose theoretical approaches are more in tune with the behaviors implied by the models used by the econophysicists. These economists are those who are the followers of the “small tent” complexity approach of explicitly modeling heterogeneous interacting agents who are not necessarily rational. Thus, dynamic complexity can be seen to be very compatible with what is implied by many econophysics models.

V. Final Observations

At this point I wish to draw together the various themes of this paper in a newer view that is emerging in economic and transdisciplinary discourse. While some advocate that physics per se must conquer all (McCauley, 2004, 2006) and others advocate emergent evolutionary approaches, with the ongoing conflict between computational and dynamic approaches to economic complexity, intersecting with the rise of heterogeneous interacting agent models and their consonance with much of the rising econophysics movement, I see these threads potentially coming together. A possible approach for this emerging synthesis is the model of markomata recently proposed by Mirowski (2007). In his view, market systems are evolving entities themselves that are fundamentally algorithms. Their evolution leads to the emergence of higher order market systems that embed the lower order market systems in a Chomskyian hierarchy. Futures markets

embed spot markets, only to lead to options embedding futures, and ever more complicated derivatives embedding options. While the system may appear to be more efficient and able to spread risk, it may be doing so at the cost of increased fragility and reduced resilience (Geithner, 2006; Brock, Hommes, and Wagener, 2006).

The markets may exhibit the power law behavior associated with econophysics as they consist of heterogeneous interacting agents. The model combines computational and dynamic notions of complexity with hierarchical ones, as it also combines both physics and biology concepts. The newer understanding of the economic system will involve a greater transcendence of our traditional disciplinary and intellectual boundaries than we have been used to in the past, just as the ongoing evolution of market systems and the ever-increasingly complex nature of their dynamics and evolving fragility challenges our understanding in the real world.

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