Econophysics and the Social Sciences: Challenges and Opportunities

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1. Introduction

The discipline of econophysics is now some 15 years old. The purpose of this article is to consider the challenges which it faces in gaining broader scientific acceptance in the social sciences and especially within economics. First of all, however, we discuss briefly some of the main successes of the discipline.

As noted in [1], the main area of activity for econophysics has been financial markets, a natural area for physicists to investigate given its terabytes of well defined time series data. The evidence for the fat-tailed distribution of asset price changes noted in [2] has now been established beyond doubt as a truly universal feature of financial markets. A genuinely original and very important contribution of econophysics, using the technique of random matrix theory, has been the discovery that the empirical correlation matrix of price changes of different assets or classes of assets is very poorly determined [3-6, for example]. This latter point undermines Markowitz portfolio theory and the capital asset pricing model, still regarded as powerful and valid theories by many economists [for example, 7a and b].

Another active area of empirical investigation for econophysics has been industrial structure and its evolution. As with financial markets, large amounts of generally reliable data are available in this area, too. It should be said that some of the econophysics literature is perhaps less original and/or well established than physicists might appreciate. Strong evidence on the right-skew distribution of firm sizes, for example, has been both available and well known in industrial economics for many years [8,9 for example]. A more distinct finding by econophysicists is that the variance of firm growth rates falls as firm size increases, the seminal paper on this being [10].

But despite this impressive record, econophysics has made little impact on the social sciences, even on economics itself, the name of which discipline is of course part of the 'econophysics' description.

An indication of this was given by the initial conference of the Institute for New Economic Thinking (INET) held at King's College Cambridge in April 2010 (the location was symbolic, King's being the college of Keynes). INET is sponsored by a $50 million donation over a five year period by George Soros. A wide range of economists were invited, but no econophysicist was on the list[1]. The efforts which the leading members of the community had to make to get a single person invited are documented at http://www.unifr.ch/econophysics/. Eventually, Doyne Farmer was invited. But econophysics as a discipline has clearly made very little impact on the thinking of economists who realise that new intellectual approaches are required.

[1] The current author, Ormerod, has extensive connections with econophysics and was invited in the original list, but as an economist and not as an econophysicist

2. Econophysics and the mind set of mainstream economics

It is important to stress from the outset that economics has become very resistant to new ideas, especially those from other disciplines. Much of this arises from the rules of behaviour which agents are postulated to follow, a crucial point which we also discuss further in section 5. The point illustrated here, however, is the closed mind set of many economists.

The dominant paradigm in macroeconomic theory, for example, over the past 30 years has been that of rational agents making optimal decisions under the assumption that they form their expectations about the future rationally - the rational agent using rational expectations, or RARE for short.

Rational expectations do not require that an agent's predictions about the future are always correct. Indeed, such predictions may turn out to be incorrect in every single period, but still be rational. The requirement is that on average over a long period of time, expectations are correct. Agents are assumed to take into account all relevant information, and to make predictions which are on average unbiased. Deviations from perfect foresight in any given period are an inherent feature of this behavioural postulate, but such deviations can only be random. If there were any systematic pattern to the deviations, the agent would be assumed to incorporate the pattern into his or her expectations. Again, on average over a long period, such expectations are correct.

It will be apparent that the theory is difficult to falsify to someone who really believes in its validity. Even the most dramatic failure to predict the future, such as the 2008 financial crisis, can be explained away as a random error. A rational expectations enthusiast can still continue to maintain the correctness of the theory by simply assuming that over some (theoretically indeterminate) period of time, on average agents' expectations prove accurate.

An assumption of the theory is that, as part of the set of information being processed, the agent is in possession of *the* correct model of the economy. Indeed, on the logic of the theory itself, if the model being used to make predictions were not correct, the forecasts would exhibit some sort of bias, some systematic error, and agents would realise that it was wrong.

It might reasonably be argued that it is difficult to subscribe to the view that agents understand the correct model of the economy given that economists themselves differ in their views as to how the economy operates. For example, in the autumn of 2008, many prominent American economists, including a number of Nobel Prize winners, vigorously opposed any form of bail-out of the financial system, arguing that it was better to let banks fail. Others, including decision makers at the Federal Reserve and Treasury, took a different view entirely.

The response of the academic mainstream has been to insist that there have been strong moves towards convergence within the profession on opinions about macroeconomic theory. By implication, anyone who takes a different view and is not part of this intellectual convergence is not really a proper economist.

For example, the American Economic Association launched in January 2009 a new journal, *Macroeconomics*. It turns out that the academic profession believes it has reached a broad consensus. The first issue carries an article by one of the world's leading academic macroeconomists, Michael Woodford, entitled 'Convergence in Macroeconomics: Elements of the New Synthesis' [11].

The first and most important part of the new synthesis is that 'it is now widely agreed that macroeconomic analysis should employ models with coherent intertemporal general equilibrium foundations'. Incredibly, Woodford's article was published in January 2009. I suppose it was written at some point during 2008, but even so the West as a whole was already in recession in the middle of that year.

Olivier Blanchard is the chief economist of the International Monetary Fund, and here is what he had to say in August 2008 in an MIT working paper entitled 'The State of Macro' [12]: 'For a long while after the explosion of macroeconomics in the 1970s, the field looked like a battlefield. Over time however, largely because facts do not go away, a largely shared vision both of fluctuations and of methodology has emerged…… The state of macro is good.' The state of macro is good! In August 2008, just a few weeks before the financial crisis almost brought capitalism literally to a halt!

To be fair to Blanchard, he did then express some reservations, but these were largely technical in nature, and he did not challenge the fundamental idea of rational equilibria. On the contrary, he concluded 'macroeconomics is going through a period of great progress.'

On any reasonable scientific criteria, these models were falsified by the financial crisis of 2008/09. Blanchard himself conceded as much in a later paper [13]. But for the profession as a whole, these rational agent equilibrium macroeconomic models are still highly regarded. One of the sessions at the Cambridge INET conference, for example, was mainly devoted to papers arguing that the economic recession offered a great opportunity. The new ranges explored by the data (i.e. the deepest recession in the West since the 1930s) would, it was claimed, enable the models to be developed and calibrated more accurately.

It is very hard in the face of such attitudes for different approaches to gain any sort of traction with most economists. Even the work of many Nobel Prize winners *within* economics itself is ignored by the mainstream, a situation which is impossible to imagine within the discipline of physics.

For example, the prize in 2000 was awarded to the micro econometricians Heckman and McFadden. Heckman in his Prize lecture [14] states that ''an important empirical regularity is the diversity and heterogeneity of behaviour [of agents]'. Yet a great deal of mainstream theory still uses the single 'representative' agent[2] to proxy the behaviour of the entire economy.

Daniel Kahneman and Vernon Smith shared the prize in 2002, for their work in psychology and experimental economics. Kahneman's summary of the entire corpus of this empirical work is: 'humans

[2] A devastating mathematical critique of this concept was provided over 20 years ago by Kirman [15]

reason poorly and act intuitively' [16]. Yet as Smith points out [17]: 'Within economics there is essentially only one model to be adapted to every application: optimization subject to constraints due to resource limitations, institutional rules and /or the behavior of others, as in Cournot-Nash equilibria'. And here is Edmund Phelps, 2006 winner: 'After some neoclassical years at the start of my career I began building models that address modern phenomena. At Yale and at RAND, in part through my teachers William Fellner and Thomas Schelling, I gained some familiarity with the concepts of Knightian uncertainty, Keynesian probabilities, Hayek's private know-how and M. Polyáni's personal knowledge.' [18]. Each of these concepts is diametrically opposed to the view of the economic mainstream as to how the world operates.

So the lack of impact which econophysics has had within economics is to a substantial degree due to the attitudes of the economics profession itself.

This in turn has implications for the strategies to be followed by econophysics in order to gain wider scientific acceptance. Whilst there are some genuinely open minded mainstream economists such as John Sutton at LSE and Marcus Miller at Warwick (to name two prominent academics in the UK context), in general it will be very hard to get economics itself to take econophysics seriously. The strategy of attempting to engage with economics should not be ruled out, but it is unlikely to prove successful.

3 Self-imposed restrictions of econophysics

Some of the reasons for the lack of a wider impact of econophysics are the restrictions which much of the discipline imposes on itself. These relate both to the nature of the problem which are addressed and to the other disciplines with which econophysics seeks to engage. In this section, we discuss the former and return to the latter in section 5.

As noted above, a great deal of the work by econophysicists has focused on financial markets and industrial structure. Both these areas are fortunate to have very large data sets containing reliable data. Although such data are not the outcomes of controlled experiments, in terms of their quality and quantity they resemble the type of data to which physicists are accustomed.

The development of information technology is expanding the areas in which such data can be found, the structure of the Internet and the various social networking sites being obvious examples.

But most data in economics, and indeed more generally in the social sciences, is simply not like this at all. The data series are short and contain substantial amounts of noise. In general, the data are not collected for the purposes of scientific analysis, but are often the by-products of government activity. So, for example, estimates of GDP, the total output of an economy, are built up from literally hundreds of different sources. Some are reasonably reliable, such as the net amount of Value Added Tax on consumer purchases, which can be used to derive estimates of the amount of spending in those sectors of the economy where the tax is applicable. Even here, there may be problems in sectors, especially those involving drugs such as tobacco and alcohol, because of tax evasion. Others, such as estimates of the self-employed, may be more difficult to estimate reliably. Yet others, such as measuring the output of the non-traded public sector such as defence, rely upon rather arbitrary accounting conventions. An idea of the complications involved in constructing estimates of national economic data, even for a sophisticated developed economy such as the UK, can be obtained from [19].

There will also typically be only a small number of observations by scientific standards. Continuing to use the example of GDP, until the mid-19th century most Western countries were still dominated by the agricultural sector which had been the main sector since time immemorial. It was only during the second half of that century – somewhat earlier for the small number of countries such as the UK and the Netherlands – that they came to have the characteristics of modern developed economies. An enormous amount of work has gone into constructing estimates of GDP for Western economies back into the 19th century (for example [20]), but we have at most 150 annual observations. It is not possible to obtain anymore.

It is perfectly reasonable for an individual econophysicist to decide that he or she does not wish to work with data of this nature. But this limits rather sharply the social and economic issues which can be analysed. Again, the areas where large amounts of reliable data are available are expanding, but this is not yet the case with many problems which are of interest to economists.

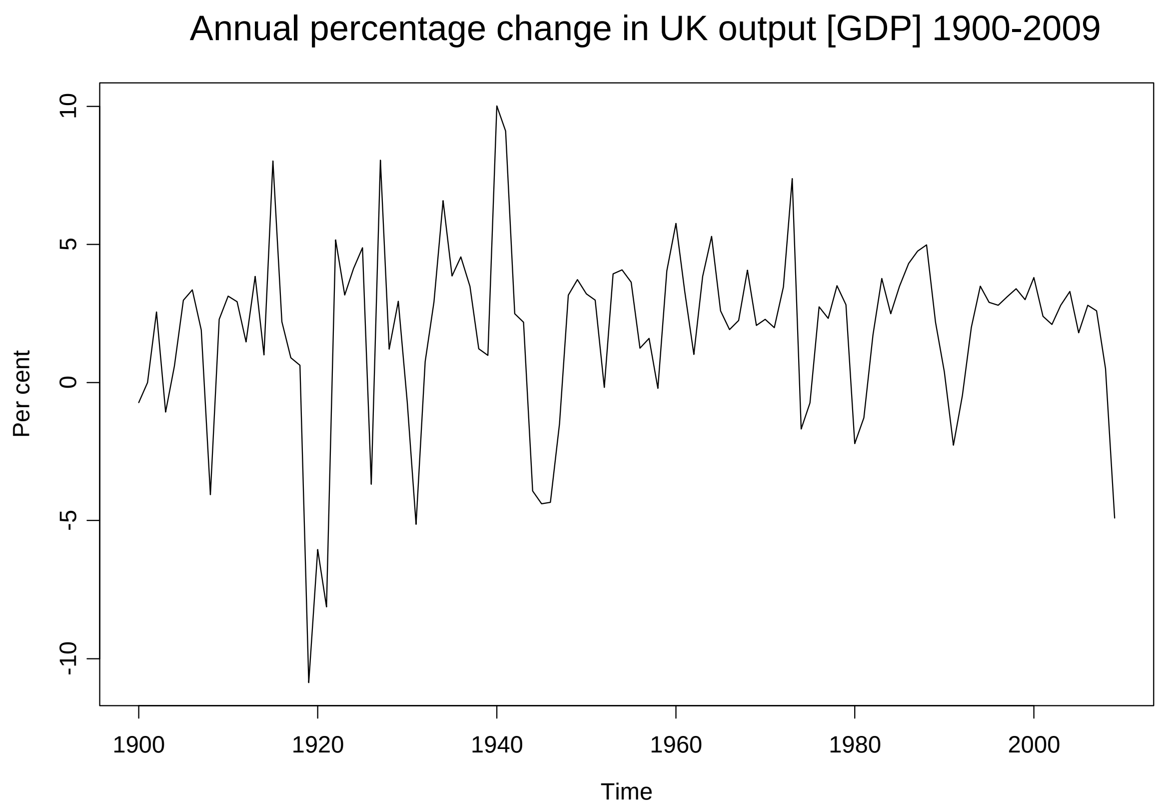
As the recent crisis has shown so clearly, financial markets are important. Further, apart from the scientific attraction of being able to work with huge amounts of high quality data, there has undoubtedly been the lure for econophysicists of the possibility of making substantial amounts of money by analyzing these markets. There is absolutely nothing wrong with this motive. But it is hard for econophysicists to appreciate that the detailed kind of analysis which they carry out is seen within the discipline of economics as a rather specialized activity. Many students can graduate from top universities without having taken a course in 'financial economics', which may in any event be something which the economics faculty has simply hived off to the business school.

Economics deals with a very wide range of human activities. For example, following the seminal work of Nobel Prize winner Gary Becker in the 1960s and 70s, economics now presumes to investigate social issues such as racial discrimination, crime, family structures and drug addiction. It is obviously impractical to list the entire set of areas which are of interest to economists, but the investigations of much of econophysics are confined to a tiny subset.

But for illustration I offer two very difficult problems where I believe econophysics can make a real contribution. Within macroeconomics – the study of the economic system at the aggregate level – there are two features of the market-oriented developed economies which distinguish them from all previously existing societies. Both of these are understood at best poorly by mainstream economics.

First, the slow but steady growth in output over time which averages between 2 to 3 per cent a year per head of population. It is this long-run growth in particular which distinguishes capitalism from all other forms of social and economic organisation in human history.

Second, the persistent short-term fluctuations in output around this underlying slow growth. From time to time, these fluctuations are severe and output actually falls for a period of time, before growth is resumed. The chart below plots annual GDP growth in the UK since 1900. It is entirely typical of the Western economies.



Rather confusingly to physicists, economists often refer to these fluctuations as the 'business cycle'. The data do not obviously follow a cyclical pattern, and this is simply the descriptive term used within economics for these short-term fluctuations.

Given that these fluctuations are persistent both over time and across countries, they represent a serious challenge to a view of the world based on the concept of equilibrium. Further, they are at present a hot topic, given the falls in output which took place across the West in 2009.

Econometrics essentially attempts to replicate the history of series like this, fitting n-dimensional planes to the data and a set of variables which purport to explain it. But this is literally mere 'curve fitting' and lacks scientific foundations.

Models are needed which are based on the actions of agents from which empirical features of the system as a whole emerge, exactly the approach of econophysics. Factors to be reconciled with the model include the properties of the data in the time and frequency domains, the distributions of the size and duration of recessions, the wait time between recessions (see for example [21] for some general features of Western economic recessions).

4 Weaknesses of econophysics

In 2006, with a group of economists sympathetic to econophysics, I published a paper entitled 'Worrying Trends in Econophysics' [1]. Although some econophysicists interpreted it as an attack on econophysics, this was not our intention at all. We stressed in the paper the achievements of econophysics and our admiration for the open-minded way in which the discipline addressed problems. We were observing econophysics as economists who realise that a profound paradigm shift is needed in economics, and suggesting ways in which econophysics could become more effective.

In general, the worries we expressed remain valid and there is no point repeating them in full here. One point we made, however, is that economics is not a completely empty box, and a familiarity with some economic theory would be useful to any econophysicist seeking to break out of the world of financial markets. The work, for example, of Zhang and his colleagues and associates at Fribourg shows how strikingly original work can be carried out in areas at the heart of economics (to take just one example, [22]), but for which knowledge of existing economic theory is an essential prerequisite.

There are two related points which are worth emphasising. From a purely tactical standpoint in terms of gaining a wider audience receptive to econophysics, these do act as a deterrent to economists who are at least willing in principle to examine the work of econophysicists.

Physics as a discipline does not require a great deal of knowledge of statistical theory. A theory is developed. Experiments are carried out to see if it conforms to the evidence. Both the theory and the experiments may present formidable intellectual challenges. But in general, once the experiments are carried out and the discipline is satisfied with how they are done, it is obvious if the theory is wrong. The results of the experiments do not usually require refined statistical analysis in order to confirm this.

The situation is quite different in the social sciences. Social science is not in the position of Einstein who, when asked what he would think if observations failed to confirm his prediction on the perihelion of Mercury, said, 'I shall be very surprised'. In social and economic science we have difficulty judging whether results of observations are surprising or not. In part this relates to the small sample noisy data discussed above, in part to the non-replicability of events actually observed in the human world.

As a result of this, many social scientists have extensive training in statistical theory. As it happens, there are many more issues in the social sciences which sophisticated statistical analysis has *failed* to resolve than there are which it has succeeded. The point being made here is a subtle one. Statistical analysis has shown itself to have serious limitations in resolving problems in the social sciences. But the knowledge which many social scientists have of statistics means that they tend to be dismissive of any work which contains statistical analysis which is not very sophisticated.

By far the most notorious example of this is the tendency for econophysicists to publish papers which claim to have discovered empirical power laws. There are certainly examples where this is valid. But there are far too many papers in which this claimed relationship is, to a trained statistician, obviously not true.

Perline [23] offers a detailed critique of the claim that power laws characterise many data sets in the social sciences. I recommend this article very highly to econophysicists. He notes that findings are often represented as though data conformed to a power law form for all ranges of the variable of interest. Perline refers to this ideal case as a *strong* inverse power law (SIPL). However, many of the examples used by Pareto and Zipf, as well as others who have followed them, have been truncated data sets, and if one looks more carefully in the lower range of values that was originally excluded, the power law behavior usually breaks down at some point. This breakdown seems to fall into two broad cases, which Perline calls here *weak* and *false* inverse power laws (WIPL and FIPL). WIPL refers to the situation where the sample data fit a distribution that has an approximate inverse power form only in some upper range of values. FIPL refers to the situation where a highly truncated sample from certain right-skew (and in particular, "lognormal-like") distributions can convincingly mimic a power law. His paper shows that the discovery of Pareto–Zipf-type laws is closely associated with truncated data sets. Further, through detailed analysis of some reported results, he concludes that many, but not all, Pareto–Zipf examples are likely to be FIPL finite mixture distributions and that there are few genuine instances of SIPLs.

The problems of truncation in data sets are particularly acute. For example, as Perline observes 'it is in the nature of things the low end, or very commonly, all but the upper tail, of many kinds of data is hidden because of definitional fuzziness and the difficulties associated with measurement below some threshold. At the same time, it is frequently the high end that is most important or most likely to capture our attention'.

The second main point in this section is closely connected to the above. Power laws have an important status in the physical sciences, and so it is natural that econophysicists should try and discover them in the human world. However, for social scientists power laws have no special significance. They are one of a number of non-Gaussian distributions. And it is the distinction between Gaussian and non-Gaussian which the social scientist regards as important, not whether a non-Gaussian distribution is of one particular form rather than another.

The constant attempt to represent data as conforming to a power law does a considerable disservice to econophysics in terms of it being taken seriously by social scientists, and especially by economists. The discovery that non-Gaussian outcomes are 'normal' rather than the Gaussian is of enormous importance. But social scientists may disregard such findings if these outcomes are invariably claimed to be power laws, especially when by simple inspection they are obviously not.

5 The gulf between economics and econophysics

The fundamental building block in economics is the agent. The agent is usually an individual, but the word is applied to the decision making unit in the context which is being analysed, so 'agent' can also mean a company, an institution such as a regulator or a government, or even a whole country.

The standard economic approach ascribes considerable intelligence to agents in the decision making process, both in terms of the information they gather and the rules they use to process it. The 'null model' of agent behavior in economics is that of the rational agent, which is assumed to have complete information about the decisions which need to be made, and makes the optimal choice given the agent's (fixed) tastes and preferences.

This may be modified by assuming some or all of the agents have incomplete information (for example [24]). But even in these situations, the decision making rule is still based upon the principle of maximizing, i.e. taking the optimal decision on the basis of the information available. The agents in such contexts are usually described as being 'boundedly rational', but the bounds relate to the amount of information which they have rather than to their decision making rule given their information. Great store is also set in economics on agents gradually being able to learn optimal behavior.

The most important challenge to this approach comes when decisions do not depend not on omniscient cost-benefit analysis of isolated agents with fixed tastes and preferences, but when the decision of any given agent depends in part directly on what other actors are doing. In such situations, which are probably the norm rather than the exception in social settings, not only do choices involve many options for which costs and benefits would be impossible to calculate (e.g., what friends to keep, what job to pursue, what game to play, etc.), but the preferences of agents themselves evolve over time in the light of what others do.

In complete contrast to economics, the 'null model' of econophysics is the particle model of agent behaviour. Particles of course act at random, by definition cannot act with purpose or intent, and cannot learn. It is rather literally a 'null model', deliberately assuming as little as possible, in order to identify the most general characteristics of collective human behavior.

This contrast gives rise to a massive gulf between economics and econophysics. On the one hand, in economics the null model of agent behavior assumes complete rationality and optimizing behavior, which is then modified to make it more realistic by restricting the set of information which is available. On the other, in econophysics the null model assumes literally zero intelligence, a postulate which can then be modified by giving agents limited abilities to gather and process information.

An important example of 'close to zero intelligence' behavior is copying, or, to give it a more refined description, social learning. The phenomenon of 'social learning' –learning through observation or interaction with other individuals - occurs widely in various forms in the animal kingdom [25]. Natural selection is now believed to favour social learning strategies, mechanisms that specify when agents copy and who they copy [26].

There is in fact a large body of evidence to suggest that agent behaviour in many social and economic contexts is much closer to the null model of econophysics than it is to that of economics. Much of the literature in other social sciences such as psychology, sociology, anthropology suggests that this is the case. With anthropologist Alex Bentley, I have a lengthy paper in press [27] on the various modifications to the 'particle model' of econophysics which might prove useful, and which also contains more evidence for the assertion that behavioural models in which agents have highly imperfect knowledge and processing abilities often have more realistic bases than the behavioural models of economics.

The implication of the above is that there is a fundamental difference of view between economics and econophysics on the fundamental building block of economics, namely agent behaviour. It is this which acts as a real barrier to acceptance of econophysics models more generally within economics itself.

There are other areas in the social sciences which are much more amenable to the general approach of econophysics. The European Social Simulation Association and the North American Association for Computational Social and Organisational Science in particular are organisations many of whose members are sympathetic to models in which agents follow relatively simple rules of behaviour. Their primary focus is sociological, but many interesting issues are social rather than economic. More generally, there is interesting agent based modelling taking place in both anthropology and geography, where there are many important questions to analyse. The International Association for Research in Economic Psychology and the Society for the Advancement of Behavioral Economics have as their primary focus the empirical analysis of actual behaviour, rather than the a priori theorising which characterises much of economics. But valuable insights on the rules to be used for agent behaviour could be obtained by econophysics liaising with these groups.

In short, there are many other social sciences to which the methodology of econophysics can more easily relate than it can to economics. But in order to develop meaningful scientific collaborations, econophysicists will have to make an effort to find out the interesting work which has been done in these areas.

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