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Prospects for large-scale financial systems simulation

The Future of Computer Trading in Financial Markets - Foresight Driver Review – DR 14

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Abstract

As the 21st century unfolds, we find ourselves having to control, support, manage or otherwise cope with large-scale complex adaptive systems to an extent that is unprecedented in human history. Whether we are concerned with issues of food security, infrastructural resilience, climate change, health care, web science, security, or financial stability, we face problems that combine scale, connectivity, adaptive dynamics, and criticality.

Complex systems simulation is emerging as the key scientific tool for dealing with such complex adaptive systems. Although a relatively new paradigm, it is one that has already established a track record in fields as varied as ecology (Grimm and Railsback, 2005), transport (Nagel et al., 1999), neuroscience (Markram, 2006), and ICT (Bullock and Cliff, 2004).

In this report, we consider the application of simulation methodologies to financial systems, assessing the prospects for continued progress in this line of research.

Keywords: simulation; modelling; finance; economics; complexity

1 Introduction

The global financial crises of the last half-decade have, understandably, prompted a degree of reflection and reappraisal within the financial sector, government, and academia (e.g., Farmer and Foley, 2009; Haldane, 2010a; Haldane and May, 2011). How might we best prevent (or anticipate, or ameliorate, or recover from) a recurrence of financial catastrophe at the scale recently witnessed? This paper considers the potential role that large-scale simulation models of financial systems might play in achieving this goal.

The remainder of the paper will be structured in four sections. Section two will motivate and contextualize the need for systemic finance models and the requirements that these models must meet if they are to be of use. Section three will briefly outline the literature on financial systems simulation before examining in more detail one case study of financial systems simulation, Darley and Outkin's (2007) model of the NASDAQ exchange. Finally, section four will identify challenges and problem issues, and assess the prospects for progress in financial systems simulation in the short-to-medium term.

2 Large-scale financial systems simulation

Cliff and Northrop (2011) have recently argued that "there is an urgent need to develop major national strategic modelling and predictive simulation capabilities" for the global financial markets in order to better deal with the potential for future financial disaster. Haldane and May (2011) have suggested that our understanding of systemic risk in financial systems could benefit significantly from mathematical and simulation modelling approaches developed within ecology, epidemiology, networks science and other complex systems fields. Farmer and Foley (2009) have argued that a multidisciplinary research effort into agent-based simulation models of financial systems is urgently required, with the aim of developing tools that can be used to inform government policy and the behaviour of investment banks.
Before considering an example of this type of modelling effort more closely, we will examine what we might hope to gain from better models, and large-scale financial simulations in particular.

2.1 Why simulate?

One short answer is that financial institutions are complex adaptive systems (Arthur, 1989; Anderson et al., 1988; Arthur et al., 1997a; Blume and Durlauf, 2006) and that simulation modelling has become the *de facto* standard for dealing with the engineering, management and prediction of such systems. Taken together or individually, banks, mortgage lenders, pension funds, brokers, stock exchanges, regulatory authorities, government departments, etc., represent an ecology of inter-dependent complex adaptive systems (Cliff and Northrop, 2011). That they are *complex* arises as a consequence of the non-linear interactions between these institutions and amongst their parts. This complexity is at the heart of the difficulty that we face in understanding how financial interactions (e.g., particular patterns of investment) combine to give rise to aggregate systemic behaviours (e.g., crashes). That these institutions are also *adaptive* arises from the fact that their component parts (e.g., their people, policies, and positions) change in response to environmental variation through processes of competition, copying, etc. Amongst these changes, those that result in increased success tend to persist, while those that don't do not.

This combination of properties places financial systems firmly in the category of complex adaptive systems, alongside many other biological, social, and socio-technological systems. Whether considering the behaviour of living cells, neurons and brains, whole organisms, animal populations, and social systems, or transport networks, power networks, ICT networks, and cities, simulation modelling has proven to be an indispensable tool (see, e.g., work presented at the conference of the European Social Simulation Association, the World Congress on Social Simulation, and the European Conference on Complex Systems). Consequently it should come as no surprise to see simulation modelling promoted as an appropriate approach to understanding financial systems.

A longer answer to the 'why simulate?' question requires us to break it in two. First, what ends are we hoping to achieve by simulating financial systems; second, why is simulation modelling the most appropriate means to achieve those ends?

2.2 Ends

There is considerable diversity in the intended uses of large-scale financial simulations: from stress-testing financial instruments or assessing the risk inherent in an investment strategy, through understanding the consequences of a change in regulatory policy, to anticipating the onset of a financial tipping point or improving fundamental understanding of the dynamics of the entire financial ecosystem. Here we will consider three kinds of utility that might be found in simulation modelling. The list is by no means intended to be exhaustive or definitive. Rather it is intended to indicate that different demands will be placed on simulation models dependent on the intended aims of the modelling enterprise: prediction vs. insight, situational awareness vs. scenario modelling, local tactics vs. global strategy, etc.

- **The dashboard:** A data-driven, high-fidelity representation of the current state of financial systems that can be used for accurate short-term predictions of, e.g., systemic risk. The dashboard concept (suggested by Doyne Farmer) addresses a need for situational awareness that can inform real-time decision making, whether at the level of governments, regulators, exchanges, and national banks, or investors, fund managers, and individual brokers. Like the regular broadcast of short-term weather forecasts, the model's output would help to guide financial behaviour and avoid unnecessary risk taking.

- **The wind tunnel:** An artificial environment within which to test novel financial instruments, algorithms, strategies and policies. Like Dashboard models, Wind Tunnel models place a premium on accuracy but must also be able to address counterfactual situations and novel scenarios that represent significant potential futures. Like the simulated wind tunnels used by racing car manufacturers, these models are intended to allow their users to explore the consequences of changes in behaviour or strategy without bearing the cost of implementing these changes in the real world. Potential uses might range from assessing the impact of a change in regulatory policy to assessing the properties of a novel trading algorithm.

- **The sandbox:** A playground for learning about systems and how they behave. Like wind tunnels, sandboxes are for exploring possible rather than actual scenarios, but unlike the other two categories, the utility of a sandbox model does not lie in its predictive accuracy but in its ability to generate insights that improve the model user's understanding of the systems being modelled. Here the role of the model is heuristic in that it stimulates new or better ways of thinking about the real world without necessarily making point predictions about the future. Uses might include developing methods to detect the onset of a financial tipping point, or a better understanding of the key factors underpinning systemic risk in a particular market.

2.3 Means

Why might large-scale simulation models be the most appropriate means with which to achieve the ends discussed above?

Simulation modelling is increasingly recognised as a "third pillar" of science, alongside, empirical enquiry (e.g., experimentation or historical study) and rational analysis (e.g., mathematical modelling or logical argument). As a methodology, simulation has particular relevance to the study of large-scale complex adaptive systems because these systems are difficult and expensive to manipulate experimentally and their histories are unreliable indicators of their future behaviour. Moreover, idealisations of the type that are necessary to ensure analytically tractable mathematical models (e.g., mean field approximations, assumptions of equilibrium behaviour or rational action, etc.) often abstract away some of the important features of these systems. For instance, the behaviour of complex adaptive systems can be sensitive to details of their heterogeneous structure, the diversity of agencies involved, interaction across multiple spatial and temporal scales, etc.
By comparison with mathematical models[1], simulations are more readily able to capture the richness of structure and behaviour that is characteristic of complex adaptive systems. By comparison with experimental studies of such systems, simulations are relatively cheap, quick, and flexible. However, while it is true that simulations are powerful tools for science and engineering, we should not overlook some key limitations.

First, model richness or realism is not an unalloyed good. It may indeed be possible to build simulation models that capture much of the richness of the real target system and may even exhibit some predictive accuracy, but what is gained on the swings of richness, is often lost on the roundabouts of impenetrability (Di Paolo et al., 2000).

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[1] It is no longer straightforward to cleanly distinguish mathematical modelling from computational simulation. For our purposes, mathematical models are analytic proofs that typically rely on the manual construction and manipulation of equations, whereas simulations are models that "unfold over time" automatically in the form of algorithms executed by computational machinery.

The more detailed and realistic a simulation model is, the greater the challenge of understanding its behaviour. While very detailed models can be implemented on powerful computers, and their behaviour observed, in order to understand why we see that behaviour, we need to penetrate the model and explicate its workings. In the limit of maximum model realism and richness, we may be presented with a simulation that is a perfect simulacrum of the real target system but is just as difficult to understand. The lesson here is that realism, *per se*, is not something to be striven for in model building. Rather than "realising" the target system, we should aim to "idealise" the system, but in a way that respects its most substantive features, properties, processes, etc. Simulations allow us to model systems without having to make some of the idealisations required in order to make a mathematical model tractable. However, some idealisation will still be required—and desired.

Second, simulations are not experiments. This may seem an odd thing to point out. However, as simulations become increasingly realistic, they tend to take on the status of artificial worlds within which artificial experiments are performed leading, in extremis, some practitioners to imagine that the results of simulations might eventually have the same status as the results of experiments in the real world (Peck, 2004). For these practitioners, simulation modelling is a kind of experimentation *in silico*, taking place in a "virtual laboratory". However, while this vision may be appealing, and while simulations may be very rich representations of target systems and may exhibit realistic behaviour that needs to be systematically explored in order to be understood, such explorations are *not* experiments on the real-world target systems being modelled and their results or findings are *not* facts about these real-world systems. Simulations are not able to deliver empirical facts about the world. They are able to deliver at least two (related) kinds of output: insights and predictions (see figure 1). Whether a model output is a prediction, a forecast, a hypothesis, a conjecture, an understanding, or just an idea is as much to do with the attitude of the modeller as the structure of the model itself and it is a fact of life that shades of grey separate these categories.

Thus the strength of simulation modelling is not that it enables us to build models so realistic that they allow us access to an alternative empirical paradigm, but that it allows us access to an additional *analytical* paradigm. Simulations are flexible enough to allow us to avoid some of the constraints of particular mathematical models. They encourage us to reconceive the target systems and to idealise them in novel ways that can generate novel ideas, insights, hypotheses, and, where our theories are relatively secure, novel predictions (Di Paolo et al., 2000).

From this account it should be clear that simulation modelling is not an alternative to mathematical modelling but is simply an additional modelling tool. Indeed, there is no clear blue water between mathematical and simulation modelling and the two paradigms are best thought of as overlapping and mutually supportive approaches.

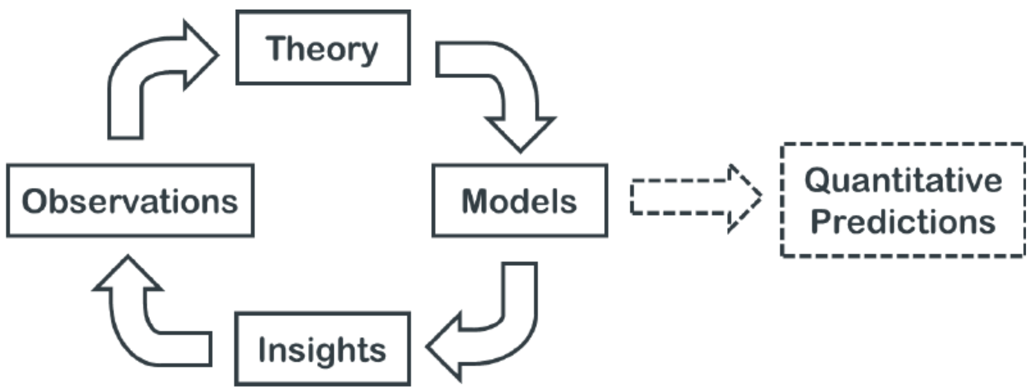

Figure 1: A schematic representation of the role of models in science and engineering that distinguishes between two kinds of model output: insights and predictions. The primary role of a model is often to deliver insights that shed light on a particular theoretical position. These insights are typically cast as hypotheses, the validity of which can be explored through empirical observations (e.g., experiments). The results of these observations may cause revisions to be made to the original theoretical position, requiring new models, and insights, etc. A secondary use of some models is to generate predictions about the world. This may occur where we have strong faith in the theoretical position underpinning the model. Note that there are still further reasons for building models (Epstein, 2008) and also that predictions derived from a model are never certain, even for models that are informed by well-established theories that are true to the best of our knowledge, and "verified" or "validated" against empirical data (Oreskes et al., 1994).

2.4 Why "large scale"?

The phrase "large-scale financial systems simulation" appears to have an immediate and intuitive meaning for those concerned with questions of, e.g., financial stability and systemic risk. However, while the term "large-scale" appears relatively straightforward, the way in which it is being used here is somewhat subtle and deserves some brief consideration.

Clearly the term large-scale is subjective. Largeness of scale for a single day trader will not be the same as largeness of scale for the governor of the Bank of England. While the former might be satisfied by a model that includes only the most proximal factors, the latter will be interested in including far more of the whole financial ecosystem inside their model. One reason for using the term "large-scale" is to indicate that models must be large enough in their scope such that they treat key processes and agencies *within* the model rather than as an exogenous context represented by fixed parameters or structures (if represented at all).

A model of the price dynamics of an entire stock exchange might reasonably be considered to be large scale with respect to a similar model of an ideal market with a pair of traders and a single commodity. However, if one is interested in the systemic risk for a national economy, it will likely be necessary to include multiple institutions and their interactions within a single model. The connectivity amongst the world's financial institutions and economies already implies for some that the utility of large-scale simulations will only be realised when they are cast at a global scale.

However, the scale of a model can be measured in at least two ways: the size of the target being modelled and the size of the model itself. Models of the UK's banking system are larger scale than models of a single UK bank. However, in each case a model cast at the level of individual accounts and individual banking transactions is a larger scale undertaking than one cast at an aggregate level of description. In order to deal with *systemic* financial questions, models will need to be "large enough" in terms of both senses: scope *and* resolution.

A consequence of this approach is that it spans or transcends micro- and macro-economics. Answering systemic questions of the kind that interest financial institutions and regulatory agencies requires both micro-resolution and macro-scope. Successful large-scale simulation models will bridge these levels of description, allowing answers to questions of the form: when and why do micro-interactions of this kind give rise to macro-phenomena of this kind? Often these answers will involve the identification of intermediate meso-level patterns, or organisations.

3 A Case study

It is beyond the scope of this paper to provide an exhaustive or authoritative literature review of large-scale financial systems simulation which is fragmented and distributed over many fields, including, but not restricted to economics, physics, complexity science, mathematics, and computer science.

Interested readers are directed to classic reviews of agent-based computational economics due to Tesfatsion (2002) and LeBaron (2000), and more recent reviews due to Gatti et al. (2010) and Cristelli et al. (2011). Levy et al. (2000) also provide an overview of early work including that by the group responsible for the influential Santa Fe artificial Stock Market (e.g., Arthur et al., 1997b). The econophysics literature is reviewed by Chakraborti et al. (2010) and critiqued by Gallegati et al. (2006). The relevance of modelling work in this vein to policy and regulation is assessed by Thurner (2011). Epstein and Axtell (1996) and latterly Gilbert and Troitzsch (2005) are the standard texts for social simulation more generally.
Rather than attempt another review of these literatures, this section will examine a representative piece of financial systems simulation modelling described in detail by Darley and Outkin (2007). The authors detail the conception, design and analysis of a series of increasingly detailed modelling studies of the NASDAQ stock exchange. The work was carried out between 1997 and 1998 by a small team of modellers working for Eurobios, which was then the UK's foremost complex systems simulation consultancy.

The models were commissioned by NASDAQ in order to better understand the potential ramifications of their proposed move to decimalization. At the time of the study NASDAQ trades were quantized in units of one sixteenth of a dollar. After decimalization was adopted this smallest unit of trade was reduced to one cent. The models explored what impact this structural change might have on the NASDAQ's trading behaviour, making a series of predictions concerning, e.g., price discovery, spread clustering and volume of trade. Since NASDAQ adopted decimalization in 2001 five of Darley and Outkin's (2007) six most important predictions have been supported while the jury is still out on the sixth.

The goal here is not to provide a blow-by-blow account of the models and their predictions, but rather to highlight several issues which were crucial to the success of the simulations.

First, it is important to note that the modelling carried out was not primarily an academic piece of work (although aspects of the studies were written up for publication). The research was commissioned by NASDAQ for the purpose of informing their medium term decision making regarding decimalization. The intention was not to build a general-purpose "virtual exchange", nor to construct "frictionless" abstract models that shed light on academic theories. The provenance of the work can be seen sometimes in the practical attitude taken to the design and analysis of the models.

Second, the scale and scope of the modelling activity is also worth commenting upon. Are these "large scale" simulations? While on the one hand they treat a single exchange in isolation from the ecosystem of financial institutions within which it operates, on the other they do consider a heterogeneous ecology of interacting agents and structures *within* the NASDAQ exchange and cast their model at this micro-resolution of individual traders and individual trades. Whether or not the term large-scale is warranted it is clear that the concerns of the modelling endeavour are "systemic" in that they aim at revealing the system-level impact of what might at first appear to be a negligible change to a low-level market structure.

In order to ensure that the models were fit for purpose, Darley and Outkin sought input and information from a wide range of actors with relevant expertise: from investors and market makers to NASDAQ analysts. Darley and Outkin report that this consultation was detailed and ongoing throughout the modelling process, citing it as a key influence on the quality of the resultant models.

The style of modelling employed by Darley and Outkin is "agent-based". That is, models comprise a population of artificial financial agents simulated within an artificial, idealised stock exchange. While there are many alternative modelling paradigms that might be employed, Darley and Outkin argue that theirs allows for the relaxation of assumptions such as equilibrium behaviour and perfect rationality, while allowing for the exploration of scenarios involving limited information, noisy behaviour, etc.

Interestingly, while the agent-based approach is sometimes characterised as a single paradigm, the simulations reported by Darley and Outkin vary significantly in their structure and function. Initially, the authors report rather abstract and mathematical models of a pair of traders and a single commodity. These are used to explore quite fundamental questions of trader behaviour and to establish the feasibility of the modelling approach. The initial models inform the construction of larger-scale multi-agent systems in which populations of trading agents interact over time, giving rise to aggregate market behaviour. This type of model is the core of the enterprise allowing the authors to make key predictions about the systemic impact of decimalization. However, they go further in exploring the behaviour of realistically calibrated, data-driven models designed to generate results that could be compared directly with the real-world behaviour of the market over a particular historical period. Finally, they carry out a series of a time-series analyses of real market data over the period during which the resolution of the NASDAQ moved from one eighth to one sixteenth of a dollar, using the insights from their models to identify phase transitions in market behaviour that may have arisen as a consequence of this change in granularity. What is instructive here is the full range of uses to which models are put and how the character (design, scale, scope, level of description, etc.) of the models changes to accommodate these different uses.

One key aspect of the core model's design was the inclusion of *adaptive* agents. While the models could be populated by agents that executed *fixed* strategies, either derived from interviews with real market participants, or hand-designed by the modellers themselves, the modellers also allowed for agents that could alter their behaviour automatically, exploring a much larger space of potential strategies. This innovation allowed the models to explore potential strategic responses to the new dynamics brought about by changes such as decimalization. It also ensured a degree of robustness, since the model's behaviour was not reliant on the particular set of pre-specified trading behaviours coded by the modellers. It seems likely that this aspect of the Darley and Outkin modelling approach would be more generally useful in the modelling of any large-scale financial system.

More prosaically, Darley and Outkin's reports make clear the significance of inventing appropriate measures and visualizations of model behaviour. Where target systems (such as stock exchanges) are poorly understood, there will be little prior agreement on which of the system's many properties are critical to understanding its behaviour. How to measure and represent spread clustering, for example, was an open problem at the outset of the study. In some respects, the development of behavioural metrics and visualizations went hand in hand with the design, exploration and revision of the simulation models themselves.

Finally, it should be pointed out that the simulation methodology employed by Darley and Outkin was not intended to preclude mathematical modelling. In fact, to some extent, mathematical treatments were used as a kind of scaffolding to help erect a reasonable simulation (one that agreed with key mathematically modelled behaviours), while also being used to help unpick and understand the behaviour of the models themselves. This last issue deserves to be highlighted, since in some cases the results of Darley and Outkin's simulations, while reported in detail, are not fully explained. In these cases, the model is shown to exhibit some class of behaviour under certain circumstances, reasons for this behaviour are offered, but conclusive explanations are left for future work. Since these explanations could have been fully articulated given the investment of enough time and effort, the lesson here is that getting to the bottom of the behaviour exhibited by models of this complexity is challenging and time-consuming for the designers of the models, and may not be deemed sufficiently worthwhile by the model's users.

What we can take away from the Darley and Outkin studies is a convincing demonstration that it is indeed possible to build simulation models of complex adaptive financial systems, and to derive from them useful insights, understanding and, to some extent, predictions about these systems. More specifically, the studies demonstrate the value of adopting multiple methodologies, combining mathematical, heuristic, and data-driven approaches with particular value to be gained in the use of adaptive agent-based methods. What remains to be seen is whether their approach can scale to meet the challenge of modelling multiple interacting institutions.

4 Prospects

That well-founded large-scale financial systems simulations would be extremely useful if they existed appears incontrovertible. Such models would inform decision makers from top to bottom with the likely effect of strongly reducing the risk of future financial problems of many kinds. They would improve our ability to ensure that regulation and financial best practice keeps pace with technological and financial innovation. They could allow for the real-time management of systemic financial risk, the anticipation and avoidance of financial disaster, and the agile recovery from vulnerable situations. They could guide financial strategy and policy, e.g., allowing an assessment of the impact of high-frequency trading on market stability, or informing the use of "circuit breaker" action to cool down a market in order to avoid a crash.

What is at issue is not the potential utility of such models but rather their feasibility. There is a clear short-fall between the current state of the art in financial simulation and that required in order to deliver the proposed models. What is the nature and extent of this gap? How might it be bridged?

The following sections deal with a number of the most relevant issues raised and discussed during a period of consultation carried out for the purposes of this paper. Many of these issues deserve a much more thorough treatment than can be given to them here. At some stages I will make use of a comparison between the effort to simulate large-scale financial systems and the effort to model global weather (Cliff and Northrop, 2011). While I think the comparison can be a useful one there are obviously some key differences between weather and finance which means that one is rarely a straight analogy for the other.

4.1 Data

Many of the models imagined by proponents of large-scale financial simulation are extremely data hungry. However, the nature of the empirical data required by different modelling enterprises differs considerably. Some of this data is in the public domain, is collected by governmental regulatory bodies, or is already available from vendors for a price. However, for certain types of model the relevant data is either held privately by firms, or is not currently collated and stored at all. In general, the technical challenges and intellectual property or privacy issues involved in the identification, collection, transmission, collation, storage and security of data relevant to large-scale financial simulation are likely to be significant.

Consider a simulation model that reproduces the dynamics of, say, a real-world stock exchange at some relatively fine resolution. To initialise or calibrate such a model would require access to empirical data specifying the trading activity on the exchange being modelled. The resolution at which this data must be captured will depend on the intended use of the model. If the model is a test bed for assessing new trading algorithms, then the data may potentially be anonymised, aggregated, or may even be synthetic to some extent. However, if the model is intended to deliver an understanding of the extent and nature of systemic risk associated with the positions currently taken by traders using the exchange, then it is unlikely that aggregated, anonymised data will be sufficient. In extremis, each piece of trading data must be delivered to the modellers in near real-time and must fully specify the identify of all institutions involved, the stock, price and volume and even more sensitive data about related positions held by the same institutions. As the ambition of the modelling enterprise under consideration increases, such data demands may very quickly escalate.

It might be thought that government funded public-sector modelling initiatives could solve the data problem through passing new legislation requiring that financial institutions take care to collect and store the needed data and giving powers of data access to the government-run modellers. While this might make life easier, it is not a silver bullet. In the United States, legislation has already been passed that will allow a new Office of Financial Research (OFR) to "ask for any data or information it wants from financial institutions in an effort to prevent or mitigate the next financial crisis" (Sivon and Wilson, 2010, p.15). Exactly how the OFR will carry out its data collection is not yet decided, but its legislated powers do not solve the entire problem. For example, they do not grant access to data held by companies outside US jurisdiction, and aggressive pursuit of data from firms within the US has the potential to drive away financial services that are able to relocate to territories with less onerous demands.

Despite these concerns, however, while the aftermath of the recent financial disasters is still fresh in the minds of those involved in the financial services sector there is currently a significant shared will to engage with efforts that may protect organisations and reduce their future exposure to catastrophe. The scale and penetration of the recent problems has convinced many senior figures of the necessity to meet this challenge in concert. While the politics are undoubtedly still daunting, there has probably never been a better time to attempt joint action that includes some kind of central monitoring and modelling.

By comparison, early efforts to model and predict the weather were also hampered by relatively poor resolution data capture and limited sharing of relevant meteorological data. It took a significant time before these efforts evolved into the "global knowledge infrastructure" that weather forecasting relies upon today (Edwards, 2010). Of course weather data is not as intrinsically private or valuable as financial data, but as Edwards makes clear in his book, *A Vast Machine*, the history of any infrastructural development "is marked by struggle". What the data issue makes clear is that without substantive and long-standing engagement from a wide range of financial institutions, a financial knowledge infrastructure on the scale of the worldwide meteorological system will not succeed.

4.2 Global scale

As mentioned in the previous section, the ecosystem of financial institutions and agencies is global and its structural organisation cuts across national boundaries. One consequence of the recent financial problems is an increased appreciation of the connectedness of financial systems. This connectedness has been strengthened by globalisation over the last few decades (itself enabled by technological advances in rapid transit and information and communications technology) and has had a profound effect on the way that the global economy behaves. The multinational scale at which many financial firms operate combined with the high-speed communications links that enable close interaction over vast geographic distances have increased the significance of the coupling between them (Haldane, 2010b).

This raises the bar for financial modelling. In principle, it is no longer adequate to consider individual markets, or sectors of the financial services industry in isolation if one is interested in systemic properties such as financial stability. In practice, there is a recognition that detailed models of the global economy are beyond the state of the art and will be for some time, perhaps forever. However, this situation is not unique to finance and does not prevent progress being made in, e.g., ecology or earth sciences where connectivity and global scale are also combined. In these fields there is an acceptance that a divide and conquer approach is a practical necessity even if the real target system is not strictly decomposable.

Again, the history of weather modelling is instructive where an initial model resolved only two points over Europe, and early US models in the 1950s were restricted initially to limited areas of integration before being expanded to eventually cover the Northern hemisphere. Establishing the boundary conditions for such models was initially a significant problem and a significant source of potential error (Shuman, 1989). Increases in computational power and access to data steadily reduced the error in these models even before high-resolution global models were achieved.

While the same incremental progress is likely within financial modelling, the challenge of spatial scale may prove to be more severe. Two significant disanalogies between weather and finance are relevant here (discounting for the moment that we are not in possession of a formulation equivalent to the Navier-Stokes equations for fluid flow, i.e., we lack a consensually agreed upon fundamental theory of financial dynamics). First, fluid flows are *local* and *low-dimensional*: the weather in London can only influence the weather in New York by propagating that influence via the weather in the places in between. Second the speed with which influence propagates through the fluid medium is relatively *slow* with respect to our ability to sample it. Neither of these properties are true of financial systems where action at a distance through the use of electronic communication is the norm and influence can spread through a global network of automated financial agents at close to the speed of light.

4.3 Modelling people

At the heart of financial systems are people, acting and interacting in large numbers. Given that we would be unable to build a simulation of a single one of these people, it might appear hopeless to attempt the simulation of many hundreds, thousands or millions of them.

The fact that people's behaviour is poorly understood is a serious challenge to modelling many social systems. However, while predicting the minute-by-minute behaviour of an arbitrary individual in a population is perhaps impossible, at the population level people are to some extent predictable (the field of psychology is dedicated to articulating these regularities). In addition to general findings in psychology and sociology, there are three sources of data on human behaviour that could inform large-scale financial simulation. First, behavioural finance and experimental economics has a great potential to deliver relevant evidence regarding trading behaviour and decision making under risk and limited information. To do so, however, it must redirect its attention away from "throwing rocks at rational choice theory" towards empirical study of real financial behaviour. Second, modellers should be able to draw on historical data collected from, e.g., real traders. Reverse engineering behavioural strategies from such data is not trivial but may be possible in some cases. Third, information extracted from experts through knowledge elicitation techniques may be informative, but relies on a degree of self-knowledge through introspection that can be problematic in some domains.

One source of optimism is the restricted nature of the action set of some agents in financial systems. There is a difference between trying to model the people in a philosophy department and trying to model people on a production line. While the people in both systems are equally complex, the behaviour that they exhibit is not. The behaviour of people within financial systems sits at various points between these two extremes: some auction behaviour is extremely constrained, whereas the choices of regulatory authorities are much less bounded.

4.4 Reflexivity

One further disanalogy between weather and finance is especially relevant: reflexivity. Clouds do not pay attention to weather forecasts in order to alter their behaviour in ways that make them money. The fact that people attend to models of their own behaviour and may change that behaviour as a consequence places some unique constraints on social modelling (Lucas, 1976). For topics as politically and financially charged as financial stability, systemic risk, etc., how new information arising from models is disseminated and acted upon is a key factor in whether the modelling effort will have been worthwhile.

Consider a situation in which the output from some state-sponsored or collectively managed large-scale financial simulation is in the public domain. First, wide dissemination of such model outputs will tend to homogenise the behaviour of the relevant financial institutions since each will now be privy to the same information. The resultant reduction in diversity within the system may increase its vulnerability to shocks. Moreover, if such a simulation suggests, say, a change in regulatory policy on the basis that it tends to reduce systemic risk on average, but that this will not be true across the board, this information has the potential to change the behaviour of the financial institutions being modelled, possibly in ways that undermine the conclusions of the model and either prevent the regulatory policy changes from being made with confidence or lead such a policy change to have unpredicted consequences.

Conversely, if the outputs of such a model were to be kept secret, in addition to potentially reducing the extent to which financial institutions voluntarily engage with the modelling enterprise, and preventing some of the potential uses for a simulation facility, this would create a situation of intrigue, speculation, and possibly competition amongst institutions to develop their own modelling resource with which to second guess the moves made by authorities, competitors, etc.

This mixture of logistics, politics, psychology and money is complicated and a clear way forward is unclear. However, to some extent there are precedents for this type of problem in that, e.g., any regulatory body has to engage with the issue that changes in regulation bring about changes in behaviour. In fact, if a concerted attempt to model financial institutions and how they interact draws attention to the role of innovation and adaptation in the behaviour of organisations and individuals, this may be a significant positive outcome.

Consequently, while there are certainly some pitfalls here that mitigate against long-term predictive modelling, the Lucas Critique (Lucas, 1976) and similar concerns are not considered to be show-stoppers for large-scale financial simulation in general, since the “adaptive capacity” of the systems being modelled (i.e., the ways in which systems and behaviour change in response to changes in their environments) will likely tend to be a central concern of the models (e.g., Darley and Outkin, 2007).

4.5 Computational power

It should be clear that significant computational power will be required in order to resolve the large-scale simulation models that are being discussed here. Simply collating and storing the relevant real-time data that would be used to calibrate or initialise some of the models represents a significant effort and demands considerable computational resource and infrastructure. Executing models that are cast at a fine resolution and at a significant, perhaps global, scale will also be computationally demanding.

However, the scale of computational effort required to drive the models being imagined by proponents of large-scale financial systems is entirely feasible using current technologies. Combining large numbers of multi-core processors into computer clusters that are able to communicate rapidly and to decompose and share the computational load imposed by simulation runs is normal practice in many areas of the physical and life sciences.

However, while the demand for raw computational power is not prohibitive, there remain some potential issues surrounding the scale at which large-scale financial simulations are expected to be implemented (assuming for the moment that they can be conceived and designed). Principle amongst these are the challenges of data management and system visualization.

The scale and sensitivity of the data that may be collected from financial institutions and governments presents a technical challenge in terms of ensuring that this data remains secure and up to date, maintaining its integrity and provenance. In particular, there will be complicated issues to resolve before incoming streams of data can be aggregated or fused without compromising the intellectual property rights or anonymity constraints of the data providers.

By system visualization, I am referring to any attempts to present the workings or results of the simulation model for users. Given that these users may range from software engineers and modellers involved in building and executing the models, through analysts and finance experts, to policy makers and regulators, it should be apparent that there is unlikely to be a single best choice regarding which aspects of the model to represent and how to represent them. In general our ability to successfully visualize the behaviour of complex models has lagged behind our attempts to build them (Bullock et al., 2006). If large-scale simulations of financial systems are to have significant impact on our decision making and policy formulation, then the output of these models needs to be intelligible and compelling.

4.6 Underpinning theory

Our failure to anticipate the recent global downturn and the lack of consensus on the best measures to take in coping with it have revealed a short-fall in our theoretical grasp of the way that modern financial institutions behave and interact. This short-fall has the potential to retard the rate at which we can achieve effective large-scale financial systems simulations.

Writing at the end of the nineteenth century, Thorstein Veblen (1898) had already identified that economics lacked a theory of the “economic life process” with which to organise and understand economic behaviour as it unfolded in the real world. In place of such a “systemic” theory, economics had constructed “narrative”, “historical” or “taxonomic” accounts that left economic behaviour divorced from the phenomena studied by adjacent disciplines such as psychology, politics and sociology.

Opinions differ on the extent to which economics and finance remain wedded to a rather restrictive set of neo-classical theories that rely on assumptions of rational choice and efficient markets. However, there is a growing consensus that both fields have failed to keep pace with changes in the real world. While there certainly exists current academic work that addresses systemic behaviour in finance and economics ("the economic life process") outside of the neo-classical assumptive framework it is not mainstream. Those working within this community are under no illusions as to how much work is required before we have a theoretical framework that can explain the behaviour of modern financial institutions. Understanding how these institutions influence one another represents a yet larger challenge given that we are not fully aware of the channels through which they interact.

Since there is a reflexive relationship between theory and modelling it is simultaneously the case that (i) closing the theory gap will be necessary before mature simulation models can be achieved, and (ii) efforts towards building such simulation models will help to close the very same theory gap by driving the construction and testing of new and better theory. This kind of theory gap exists to a greater or lesser extent in all complex systems domains. Such a gap certainly existed at the outset of efforts to simulate the earth's weather system, for instance, and has certainly closed substantially since that time (Edwards, 2010).

Consequently, acknowledging the inadequacy of current theory is not a show-stopper for progress in large-scale financial simulation, but rather might be interpreted as an impetus for it.

4.7 A Grand challenge

Grand challenges are familiar within the physical and life sciences where they have been used to organise significant multinational research effort around a coherent shared aim. Facilities such as CERN and the Large Hadron Collider and less centralised activities such as the Human Genome Programme have successfully mobilised scientific communities, achieving more clearly defined problems, and bringing about research collaborations that often span disciplines as well as research organisations.

Given the scale and criticality of the problems posed by our financial systems, and the multi-disciplinary nature of the proposed solutions, the grand challenge route would seem to be a natural one to take. This is only reinforced when some of the attendant benefits are considered. Notice that these benefits derive from making a serious *attempt* to meet the grand challenge, irrespective of whether this attempt succeeds or not. Consequently, we might imagine one or more grand challenges along the following lines: "Build a useful global simulation of the world's financial ecosystem", "Build a global systemic risk dashboard", "Build a platform for road-testing financial regulatory policy", "Establish a cross-sector research community for innovation in financial simulation", etc.[2]

First, such an attempt would likely help to weaken some of the fortifications that have tended to be erected around the many sub-communities within economics and finance, and also those that separate these communities from relevant research activity in adjacent disciplines. A grand challenge along the lines being considered here would tend to bring about more cross-disciplinary communication and collaboration between fields relevant to the problem of understanding financial systems: economics and finance, but also parts of computer science, mathematics, physics, social science, psychology and complexity science. By the same token, a concerted effort to address the modelling challenges discussed in this paper would necessarily engage academia with associated private sector firms, and government departments, both nationally and internationally.

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[2] Of course the exact specification of such a grand challenge or the mechanism by which such a specification is arrived at is beyond the scope of this paper, but would perhaps be an interesting topic for a meeting of relevant academics and non-academics.

Second, the efforts of these researchers would tend to bring about better data on our financial systems and better infrastructure for collecting, collating, analysing and sharing this data. The direct consequence will be significantly improved situational awareness and decision making. The detection of significant fraud, malpractice or incompetence would also be improved.

Third, as discussed in the previous section, a concerted effort to deal with modern financial systems will stimulate better economic and financial theory.
Of course not every grand challenge announced to the scientific community has triggered an explosion of brilliant breakthroughs and new triumphs. Care must be taken and there is likely much to be learned from past successes and failures. Particular attention might be paid to the “openness” of any grand challenge activity. It would appear likely that the probability of success is increased when the results of independent efforts taking place in parallel around the world tend to be shared readily rather than hidden and hoarded. This situation is made more complicated when there is the potential to make money or perhaps political capital from progress.

A similar issue concerns the ownership of any useful models that might result from concerted activity towards meeting this as yet under-specified grand challenge. Consider that meteorological models have tended to be developed by governments and their results have tended to be disseminated in the form of forecasts that can be considered a public good. Can this model be expected to operate successfully for large-scale financial simulation? Would a subscription model be more appropriate, where payment would earn a user the right to run their own version of the model, exploring specific scenarios and parameterisations that are of most interest? While the logistics of how simulation models might be utilised may appear a little prosaic, the details may have a strong influence on how the models develop and on the degree to which private companies, academics and government liaise and collaborate.

The overall costs of funding an effort along the lines described above would be significant but not outside the range of investment being considered by UK funding bodies[3] or the investment made in other national research facilities. As discussed above, there is also scope for spreading the cost of the enterprise across public and private sector, and thereby integrating the relevant activity of academic groups, the financial services industry and governmental departments in a more joined up fashion. Given the severity and urgency of the overall problem and the relatively small initial outlay required to fund exploratory work in this area, the costs involved should perhaps not be regarded as a significant obstacle.

5 Conclusion

It is certainly the case that financial simulations the equivalent of those used by weather forecasters to generate accurate short-term predictions of global weather patterns, or longer-term climate change, are well beyond the state of the art. However, it is also true that we are within reach of less ambitious simulation models that would significantly improve our ability to cope with the challenges presented by modern finance and that achieving these simulation models will take us a step closer to yet more sophisticated models, and so on.

[3] Consider the recent moves by the Technology Strategy Board to establish Technology Innovation Centres in key areas of research and innovation, or UK Research Council funding programmes targetting the application of complexity science to domains such as energy, healthcare, etc.

The best way forward looks likely to involve interdisciplinary scientific collaboration towards the development and deployment of novel simulation paradigms and new theory. This will require the commitment of significant computational resource and access to unprecedented quantities and kinds of data. Less significantly it will also require investment, likely from public and private sectors. The UK is well-placed to make significant progress in this activity, since it combines a large financial services industry with established and well-respected academic groups in computational finance, algorithmic trading, simulation modelling, complexity science, behavioural finance, and agent-based computational economics. Given this, there is considerable potential for gains to be made through a national effort within the UK. A multi-national initiative between the UK and the US along the same lines has the potential to be game changing.

We should not be blind to the size of the challenge that is entailed in building some of the large-scale financial simulations being imagined. Yet we should not balk at taking first steps in a promising new direction because the path appears to be long and difficult. This is particularly true given that we are likely to gain many benefits well before we reach journey's end, and that staying at home has ceased to be an option.

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