

Review of Alan Kirman's *Complex economics: individual and collective rationality*. The Graz Schumpeter Lectures. London: Routledge, 2011, 272pp.

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Alan Kirman's latest book is an interesting but preliminary account of the developing tradition of complexity economics. The main goal of this, as most preliminary programmatic statements, is to motivate itself in opposition to existing programmes. In this case the target of critique is mainstream economic theory for its failure to address central economic problems, notably the ongoing financial crisis.¹ Those familiar with Kirman's work will recognize this as a continuation of the theme: "the economic crisis is a crisis for economic theory" (Kirman 2010). The focus of the critique is the individualistic assumptions of mainstream economic theory, which abstract away direct economic interactions. As an alternative, Kirman offers a socialized view of economics which grounds economic organization in social interaction. This leads to the rather radical message that organization and not efficiency should be the key concern of economics.

Providing a fair assessment of this line of argument from a philosophical perspective is difficult for a number of reasons. First, it is primarily a book by an economist for fellow economists. It is a critique of mainstream modelling and an alternative developed largely through discussion of Kirman's previously published models. Though methodological principles are central to motivating the critique, their justification is not the primary focus. Second, as a rather exploratory series of separate lectures rather than a fully developed programme, it suffers from some omissions and inconsistencies. Kirman is quite upfront about the preliminary nature of the argument. He does not pretend to test his principles relative to a fully developed exemplar, instead using the principles to suggest the steps one *might* take to improve rather simple models.

¹ I will use the modifier 'mainstream' to refer to the equilibrium-centric, maximizing representative agent framework that Kirman criticizes.

These two difficulties mean that the most interesting arguments from a philosophical perspective are not always precisely and explicitly articulated. This situation demands a rather unorthodox review. First, I will give a brief summary of the main methodological arguments. Second, I will discuss more speculatively the issues this raises in the use of the principle of realisticness. As a final stylistic note, it is worth emphasizing that none of the difficulties I outline should be taken as implying inaccessibility to a philosophical audience. The book is well written and assumes limited previous technical knowledge of either mainstream or complexity modelling approaches.

In motivating the complexity economics programme, a good portion of the book is devoted to a critique of the mainstream approach. This is well executed and quite even-handed, but the arguments are not new. Primary among the charges is that modelling assumptions are selected not on any empirically defensible basis but to save a system of mathematical formalisms (see the criticisms made by other ‘heterodox’ traditions, such as Nelson and Winter 1982). There are three main targets in this respect: rationality, independence, and impersonal market interactions. It is argued that there is empirical evidence that economic agents possess quite limited rationality, their choices are socially conditioned, and the details of their direct market interactions are essential to market outcomes.² The main force of the critique is that mainstream models are unable to provide traction on actual economic phenomena because they utilize problematically unrealistic assumptions. In line with his other recent work, Kirman reiterates that these failings of economic theory have concrete and immediate implications: economic theory cannot explain or even accommodate the financial crisis. In summarizing qualitative and forensic accounts of the crisis, he argues that they refer to entities—trust, contagion, networks—which are absent from and indeed inconsistent with mainstream models. The story of this failure is used to support the intuition that economics should ‘start from’ empirical facts rather than axioms.

Kirman then faces the problem of all critiques of the mainstream approach: providing a viable alternative. The alternative proposed is an ‘interactionist’ approach, a combination of institutional economics and

² Part of this is a reiteration of Kirman’s argument against the representative agent approach and for the importance of the representation of direct interactions between agents (see Kirman 1992).

a formal basis in network models. The institutional side consists of a commitment to incorporating the social and interactional factors which constitute economic behaviour. In line with this, there is a movement from a choice-centric framework to a social influence-centric framework. Unlike much institutionalist work, there is a focus on formal network models of the emergence of organization from interaction. This relates to Kirman's focus on the *emergence* of institutions from repeated interactions rather than only an analysis of existing institutions. Though this moves the institutional focus towards individuals, the methodological argument is that the focus should be on investigating the *interactions* of individuals rather than the individuals *per se*. A corollary is that the unit of analysis is the population level distribution of choices made rather than reasons for particular choices by particular agents.

A good example of the promise of the approach is a novel treatment of the aggregation problem. Here, Kirman argues for breaking down the symmetry assumption regarding the properties of individuals and aggregates: the properties of individuals aggregate via market mechanisms to give markets emergent properties that individuals lack.³ Significant empirical support is provided through detailed study of the relationship between individual and aggregate demand in particular markets (mainly the Marseille fish market). While individual demand is quite divergent from standard rationality assumptions, aggregate demand is similar to standard assumptions. This emphasizes the possibly synergistic contribution of such studies to mainstream theory: realistic micro foundations are not always disruptive to standard market analysis.

From a philosophical perspective there are two interesting questions which are more raised rather than settled by Kirman's analysis. First, what is the principle which drives the critique of existing practice? Second, how does the proposed alternative manage to avoid being subject to similar criticism by this principle?

These questions require some reading into Kirman's approach as they are not explicitly discussed. It is obvious that the principle at stake relates to the importance of empirical evidence in evaluating models (and theories). This is evident in Kirman's fish market models, which

³ It is worth noting that this is formally supported by research in computer science on multi-agent systems, where the asymmetry between agent and collective properties is well established.

are constructed on the basis of trading data and observation. There are also statements about being interested in “how markets actually work” (p. 127) and “how agents actually form forecasts and modify them” (p. 139). Moreover he takes it as problematic that the view of theorists diverges from the view of market participants and regulators. Most tellingly, he argues for the development of more realistic versions of his models and presumably takes it to count for (against) the less realistic version of the model if its results are confirmed (disputed) by the more realistic version of the model.

My interpretation of this is that Kirman is relying on some form of realisticness principle: the degree of accordance between model assumptions and empirical evidence is relevant to model evaluation. Mainstream models are criticized for being very unrealistic and a more realistic approach is proposed. The role of the realisticness principle in criticism is relatively straightforward. It is the second question, of how his alternative avoids these charges, that is likely to be subject to stronger questions. I raise three of these questions here and try to interpret the direction Kirman implicitly takes. First, whether the sort of realisticness that Kirman is proposing can be defended against charges of vacuousness and impracticability. Second, how realisticness as a goal can be aligned with the significant and self admitted unrealisticness of Kirman’s research strategy. Third, how the realisticness principle can play a crucial role in some of his modelling exercises but very little role in other exercises.

The position Kirman is trying to establish is that mainstream theory is unrealistic (insufficiently realistic) and that his alternative has a chance of avoiding this by being realistic (sufficiently realistic). The most immediate critique of any approach based on realisticness is to object to it as an impracticable standard which is therefore vacuous. The structure of this sort of argument is that: a) criticizing a theory as unrealistic commits one to the principle that the more realistic is to be preferred to the less realistic, b) the logical implication is that the perfectly realistic is the most preferable, c) there is no perfectly realistic model, d) therefore the unrealisticness critique holds against all models and is self-defeating (see Friedman 1953, for a classic statement). To avoid this type of argument Kirman needs to make an argument along the lines of: a) there are degrees of realisticness, b) (for some purposes) certain degrees of realisticness are acceptable but others are not, and/or c) *ceteris paribus*, more realisticness is desirable but this

trades-off with other desirable properties of models (simplicity, analytical power, and so on). There is strong evidence that Kirman accepts a), as he only uses the term 'realistic' with relative qualifiers such as very unrealistic and more realistic. While further clarification of the argument is needed, a qualified view of realisticness has potential to be practicable and non-vacuous. To defend this position more development is needed of an account of degrees and respects of realisticness and the role of realisticness in a multi-dimensional evaluative scheme.

A clearer account of partial realisticness may also provide some traction on a second consistency problem: how the realisticness principle as a goal can coexist with unrealisticness in research strategy. For example, on what basis can Kirman assert the importance of realisticness regarding interactions, but not regarding agent properties? On the one hand, Kirman argues that the details of the interaction structure need to be represented more realistically. On the other hand, he argues that actual agent motivations and internal choice mechanisms can be abstracted away (i.e., represented as 'zero intelligence' or random strategies) as long as the distribution of choices that such agents make is consistent with empirically observed distributions. As it is difficult to see the latter as realistic, there is need of an argument justifying why the realistic representation of interactions is relevant but the realistic internal representation of agents is irrelevant and can be at least temporarily ignored. Here, it seems most natural to define relevant realisticness in terms of the scope of the questions Kirman is interested in. Mechanistic arguments might be useful here: researchers often investigate one level of a multi-level mechanism hierarchy realistically, relying on parallel investigations into other levels to eventually produce a more complete realistic synthesis (see Craver and Alexandrova 2008). The strategy of limiting the scope of required realisticness to interactions might then be a justifiable provisional measure. This strategy, if it is in fact being used, needs further justification. In particular, the problem of separability needs to be addressed: how reliable are the separate analyses if the levels of analysis are causally connected? This discussion indicates some interesting analytical ground regarding the compatibility of the desirability of realisticness in general with strategies which provisionally accept a significant degree of targeted unrealisticness in practice.

A final question is raised by the different realisticness standards which models are held to. In examining the different models presented in the book, some (notably the models of fish markets) seem to fit into Kirman's idea of 'starting from' empirical knowledge, but others have a much more tenuous link. Why do realisticness constraints only seem to be in force regarding certain models? Examining the different models on offer suggests that Kirman differentiates the realisticness requirement according to the purpose or target of the model. The different modelling exercises can be divided into case based (fish markets), experimental (human subject game research), stylized (stock markets), and model-model analysis (generalization of the Schelling segregation model). To take case-based, stylized, and model-model purposes, these seem to have strong, limited, and nil realisticness requirements, respectively. To be consistent with the realisticness principle (that the more realistic should be preferred), there is a need to clarify the role of unrealistic models. Another interesting analytical project would be to attempt to make space in a generally realistic approach for a productive role for unrealistic models (e.g., for hypothesis generation; conceptual and computational development; as a simplified but generally consistent summary of a realistic model; and so forth).

These issues would be interesting to develop further for those who share what I interpret as Kirman's dual intuition. First, that qualified forms of realisticness get to the heart of the important issues in the debate over the empirical foundations of economics. Second, that complexity economics (and related heterodoxies) have made important initial steps towards the goal of realisticness.

Overall, this book is an interesting read that raises many challenging philosophical problems. It succeeds as an outline of an interesting alternative direction to the mainstream approach and as a summary of the arguments against mainstream assumptions. As is to be expected in an early stage research program, the motivation of the alternative is better developed than the alternative itself. Like many realisticness-based critiques of mainstream economics, it struggles to enunciate a qualified realisticness that has critical bite while remaining practicable. In this, it presents an interesting challenge to philosophers of economics to develop more sophisticated views of qualified realisticness which admit partiality, provisionality, and progressiveness. This project will undoubtedly need to address prominent views on realisticness in economic models, such as Lehtinen and Kuorikoski's (2007), and

Mäki's (2009). Refocusing modelling from equilibrium to organization seems to be fertile ground for this discussion for those who find the claims of greater realisticness intuitively appealing but in need of much work.

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