Pragmatic socio-economics: A way forward?

Many authors, including myself (Clapham, 2009), have argued that housing is a very complex field that defies simple explanation. This acceptance drives any housing researcher either to accept that they are going to provide only a partial analysis that has to be combined with insights derived from other perspectives for a more complete picture, or, alternatively, towards a holistic form of analysis in order to capture this complexity. The intention in the paper by Lux and Sunega in this issue falls into this latter category. The authors aim to replicate the success from the inclusion of elements of psychological thinking into behavioural economics by adopting the same approach to insights derived from sociology. The example they use is to build on the existing economic literature on access to tenures that sees them as substitutes and the outcome of entry to a tenure as a rational choice made by individuals after an analysis of the costs and benefits to them of entry to each of the tenures. The paper cites the extensive literature that shows that this process is not the predominant one in practice and that people sort themselves into their preferred tenure category on the basis of income and tenure preferences and norms. This is not a new insight and one of my first research projects in housing 34 years ago showed this outcome (Clapham D, Kintrea K and Munro M, 1987). The approach in the paper is to adopt the sociological concept of social norms as applied to tenure decisions and to incorporate this into neo-classical economic models of tenure choice in order to shed light on issues of house price increases and volatility.

The empirical example chosen by the authors seems to show a pragmatic approach to using the appropriate tools to apply to a specific situation and this results in an analysis that sheds light on some aspects of the issue, whilst hiding others. The adoption of only one concept, whilst ignoring others that are closely related to it, is bound to result in a partial picture. There needs to be care in lifting concepts from one discipline to another without a clear appreciation of the compatibility of the different underlying assumptions that form the fundamental base of the concepts. The authors accept this point, and, in the beginning of the paper, propose an overall approach that they label *pragmatic socio-economics*. The aim is said to be to follow the lead of behavioural economics which has adopted some concepts from psychology. The fusion between economics and some areas of psychology has been relatively easy because of the shared values and positivist orientation of both. However, there has not been the same 'borrowing' of concepts from sociology and the analysis of the authors shows why this has been the case. They seem to blame the lack of fusion on sociology and lay out a number of problems that they perceive that have led to this situation. Interestingly, they do not do the same exercise for neo-classical economics. The problems they identify are 'an inability of sociologists to overcome methodological and theoretical objections made by housing economists with regard to sociological theory fragmentation, the nature of sociological surveying and constrained ability to generalize the results across different cultural contexts.' There is no reference here to the problems that economists need to overcome in order to engage with sociology and so improve their analyses. It should be noted that these problems do not occur to anywhere near the same extent in the aspects of psychology adopted in behavioural economics. In other words, economists tend to adopt concepts when their underlying assumptions match their own.

It is worth examining some of the supposed barriers to the adoption of sociological ideas and concepts identified by the authors. One is the insistence in many elements of sociology of the embeddedness of economic behaviour in diverse institutional, network, welfare, and social contexts. Adoption of this (in my view correct) viewpoint is argued to significantly constrain the possibility for theoretical and empirical generalization upon which mainstream rational-choice economics and cognitive psychology are based. I agree with this analysis, but would conclude that it means that the claims of economics to universal applicability are unfounded and should change.

A second barrier is said to be the adoption in sociology of the view that actors are guided by social groups and institutions, meaning that their behaviour cannot be explained using the "narrow" concept of economic rationality and the methodological individualism (individual freedom of choice) of mainstream economics. In my view, this is an accurate observation, but economics should overcome its denial of cultural and social factors in society. The individual freedom of choice in economics is an ideological and value-based position rather than an accurate view of the nature of societies. Therefore, if economists wish to improve the validity of their analyses they need to take these factors into account.

A third barrier is said to be the that 'data from surveys of attitudes and qualitative sociological research (declared preferences) are not viewed as relevant by economists who collect and analyse quantitative behavioural data (revealed preferences)—this creates a significant methodological divide between these two disciplines.' Again, this is an accurate observation, but each method has its own strengths and weaknesses and should be judged on its contribution to a specific research aim rather than ruled out in principle. There are many problems with imputing preferences from behaviour because of the many influences that may impact on behaviour.

The authors are correct, in my view, to argue that these three barriers (and the other two they identify) are serious and make integration difficult. But they point a way forward that places little emphasis on the need for economists to abandon their misplaced assumptions and ideologies. They argue that: 'Relinquishing our ambitions in social theory may, paradoxically, contribute to a more solid interdisciplinary bridge between sociology and major housing economics. It means that sociology should respect the achievements and robustness of mainstream rational-choice economics, as well as acknowledge that attempts to substitute it with sociological theory have so far failed.' To me this seems a very one-sided analysis and that much more effort could be spent in exposing the inadequacies of mainstream economics and its urgent need to change.

In their attempt to 'embed' the economic analysis of tenure choices, the authors use the concept of housing regime to be able to categorise countries. This is a reasonable step to take, although there has been a recent questioning of the regimes approach (see Stephens 2020 and commentators). Further, the authors adopt the concept of social norm and apply it to tenure choice. In a very revealing note they state that: 'Norms are not favorite terms for economists because they, by definition, constrain individual freedom of choice.' Note that it is not that norms are an inaccurate view of human behavior, but that they contravene the belief system of economists with its base in free-market ideologies.

The authors claim to have overcome the difficulties of acceptance of the concept of social norm by simplifying it in order to achieve what they term 'sufficient' generalisation. In their own words: 'We followed the concept of embeddedness of economic behavior in an informal normative (social) context but, at the same time, our focus allowed for sufficient generalization because it pragmatically limited complex social and institutional worlds to one important factor of economic decision-making, one social norm that may easily be generalized, operationalized, and included in microeconomic empirical research.' The obvious question that occurs to me is why the aim of generalisation is so important that it is seen to be more important than accuracy. Why not take the generalisability of findings as a question to be empirically determined and only undertaken to the extent that it is empirically justifiable? Why not accept that markets can take different forms and so need to be analysed individually before generalising?

One reason that the concept of a social norm is so challenging for economists is said to be that it infringes the concept of rational choice. And quite right too! Who can possibly hold to that concept except economists blind to the reality of human behaviour? Behavioural economists have already shown the value of strongly redefining the concept. The importance of social norms should be the death-knell for this ideological construct. The authors attempt to follow the path of behavioural economists by arguing for a partial suspension of the concept in the situation they are examining through the idea of a 'rationality bias'. But how much bias has to be shown before the concept is abandoned in its entirety?

The justification that the authors give for accepting the view that renting and owner-occupation are perfect substitutes is very weak. In this view, people do make rational decisions based on cost and utility based on the assumption that tenures, with all their differences of rights, social status and so on, are treated as the same in people's decisions. So much housing research shows that this is decidedly not the case. The authors' argument that if it was to be rejected it would mean that costs would have to be rejected as an element considered in the purchase of other goods such as tractors. Of course, it is possible that elements, such as status, do play a part in the choice of tractors as in other goods. But why is it so difficult to accept that a house is different from other goods?

Despite these misgivings about the theoretical approach put forward in the paper, the empirical results are interesting and are congruent with sociological research. The findings are that social norms of tenure are very strong and are transmitted through family networks. This much we already knew. What is interesting is that when applied to models of house price change, this can explain the persistence of inertia in housing systems and the existence of house price volatility. They repeat the previous findings that the higher the share of private rental housing in a country's total housing stock, the lower the housing price volatility. The share of private renting in a country is significantly and negatively associated with both house price volatility and price growth. In other words, where there is not a substitution affect between private rental and owner-occupation, there is a greater likelihood of price increases and volatility. Although interesting for an economic audience, this finding is not a shock for others and has been registered before as the authors point out in their literature review and linked to questions of increased financialisation. As the authors admit, 'our findings do not capture other sources of potential housing price volatility that have appeared more recently, such as increasing financialization and global investment activity in the residential market. The transformation of housing from home to wealth and from wealth to investment asset may produce new market instability, which remains open to other research in this field.'

The aim of Lux and Sunega to integrate sociological and economic thought on housing markets is a very brave and worthwhile one. It is brave because of the significant differences between the two disciplines. The use of the specific example of social norms around tenure choice works well, but does not really lead to findings that are novel and could not be found through sociological enquiry alone. The general theoretical justification for the position they adopt of 'pragmatic socio-economics' is less useful in my view as it goes too far in accepting the ideologies and value bases of mainstream economics. Until these are challenged and amended, the prospect of meaningful integration is slight.

References

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