A reply to Rosser and Kirman

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Reply to Book Reviews

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Let me start off by thanking Alan Kirman and Barkley Rosser for their careful reviews of my book *Behavioural Rationality and Heterogeneous Expectations in Complex Economic Systems.* Their beautifully phrased reviews nicely capture the essential features of the book and raise a number of interesting and important points for discussion.

The book shows how instability arises through nonlinearities and interactions of heterogeneous expectation rules and how this leads to Poincaré's notion of homoclinic orbits and chaotic dynamics. The book also emphasizes the empirical relevance of non-rational expectations dynamics, especially in *positive feedback* systems. Where does this positive feedback come from? Alan Kirman points to a very important observation about human behaviour also made by Poincaré, when he wrote his report for Bachelier's thesis developing the random walk hypothesis: ``*they have an inherent tendency to act like sheep*''. Individuals coordinate their actions, and this is not necessarily irrational. In positive feedback systems, it is in fact better to follow the majority.

In his review, Alan Kirman raises a number of points that merit further discussion: (1) ``the wilderness of bounded rationality", (2) coordination of beliefs that are collectively ``wrong", (3) gains versus forecast accuracy, and (4) non-stationarity and structural breaks. Let me discuss these points in a slightly different order, starting with (2).

The learning-to-forecast laboratory experiments (the last chapter of the book) show that in positive feedback systems, subjects coordinate their actions on the "wrong'' price, that is, on a price very different from the homogeneous rational expectations (RE) price, and these beliefs are (almost) self-fulfilling. As Alan Kirman notes, this goes back to an old (theoretical) literature of learning, e.g. Bray (1982), Kirman (1983) and Woodford (1990). Hommes (2013) relates *almost self-fulfilling equilibria* in positive feedback systems to Soros' ideas of reflexivity and fallibility (Soros, 2013). Research should focus on which of these almost self-fulfilling equilibria are empirically the most relevant and are learnable through coordination of a large group of individuals. Laboratory macro experiments, where aggregate behaviour is shaped by many individual decisions, are extremely useful in this respect. I would conjecture that *simplicity of the patterns of these equilibria* is crucial here. Coordination on *simple almost self-fulfilling equilibria* seems more likely in a large group of individuals than learning to believe in complicated erratic stochastic sunspots. What experiments have shown is that, in simple lab environments, coordination on a converging path to equilibrium, on an oscillating path around equilibrium and also on bubble and crash dynamics are possible through learning in the lab, especially in positive feedback systems.

That brings me to the "wilderness of bounded rationality". Should we discipline the models we build by a priori restricting them to a limited class in order to prevent us from getting lost? Or is this discipline, which macroeconomists have comfortably associated with "sound microfoundations", illusory, as Alan Kirman puts it, and should we only be disciplined by what we observe? A promising general strategy to tackle the ``wilderness of bounded rationality'' may be to consider models with simple (linear)

heuristics. For example, in a recent paper Hommes and Zhu (2014) follow this route and propose *behavioural learning equilibria*, where a homogeneous representative agent uses the *best univariate linear* forecasting rule in a *higher dimensional linear* world. The simple one-dimensional forecasting rule is mis-specified, but the best rule within this simple class is obtained by pinning down the two parameters by observable quantities: the sample average and the first-order sample autocorrelation of the rule must coincide with empirical observations. This leads to an equilibrium concept different from RE, with a representative agent using a simple one-dimensional linear heuristic in a higher dimensional linear world that correctly forecasts the first two moments of the distribution. Hommes and Zhu (2014) show that these equilibria typically exhibit much more persistence and excess volatility than RE, consistent with empirical data. Similar to this research strategy, one could allow for heterogeneity, e.g., as in the Brock-Hommes framework, with a class of simple (linear) strategies with endogenous (nonlinear) switching based upon relative performance. Coordination on such behavioural learning equilibria, very different from the RE equilibria, seems likely in positive feedback systems. Policy analysis based on these kind of behavioural models characterized by almost self-fulfilling equilibria could yield important new insights, very different from those obtained under RE, on how to manage and stabilize a macro economy with many boundedly rational individuals who coordinate their actions onto almost self-fulfilling non-RE equilibria.

This brings me to the third point. Is forecast accuracy the right economic incentive in the lab experiments to reward the subjects? Wouldn't the gains or profits generated by that forecasting rule be a better or more realistic reward? As Alan Kirman puts it: ``If I gain a lot by using a rule which forecast a modest increase in a price when a larger increase occurred should I downweight this rule?'' This is a crucial point, that certainly deserves more attention in future research. As discussed in detail in Chapter 8 of the book, in the learning-to-forecast experiments subjects are professional forecasters, whose only task is to provide forecasts. All other economic tasks than forecasting, such as producing, buying, selling, etc., are computerized in these experiments, based upon a given model or theory, and typically derived from utility or profit maximization principles. This has the advantage of being a laboratory test for forecasting behaviour *only*, ceteris paribus, and forecast accuracy is the natural success measure in such an environment. Alan Kirman raises the important question of what would happen in a more realistic setting where subjects engage in both forecasting and other economic activities. They then not only have to forecast accurately, but they also have to *learn to optimize* their other economic decisions, e.g., how much to buy or to sell. In order to study individual *optimal behaviour* in the lab Marimon and Sunder (1993) have proposed *learning-to-optimize* experiments complementary to learning-to-forecast experiments. It then becomes natural to measure the performance of subjects in terms of gains, utility or profits and reward them accordingly. Recently some learning-to-forecast and learning-to-optimize experiments have been run by Bao et al. (2013, 2014), where both tasks had to be performed and the rewards are profits and/or utility. This extends the results of chapter 8 and it appears that convergence in negative feedback experiments becomes slower, while bubbles in positive feedback experiments become more severe. Apparently, learning-to-optimize may be even more difficult than learning-to-forecast.

A final point raised by Alan Kirman concerns the fact that reality is characterized by non-stationatiy and structural breaks and that our models and experiments do not (always) take this into account. Structural breaks are indeed very important and may act

as triggers to a crisis. In the laboratory one typically builds a stationary environment for repeated macro experiments. There are some exceptions however, for example, Bao et al. (2012) ran learning-to-forecast experiments with (unanticipated) large exogenous shocks and studied how forecasting behaviour would respond. Negative feedback systems remain rather stable and, after each large shock, the new equilibrium is quickly restored. Positive feedback systems are again very different. Unanticipated shocks do not lead to sudden changes in forecasting behaviour, but the system adjusts rather slowly and then overreacts to the new equilibrium situation. This shows that in non-stationary positive feedback systems learning is even harder and occurs "too late" and in the end overreacts. For systems with structural breaks, laboratory experiments are well suited to study individual and aggregate behaviour, with the frequency of structural breaks under the control of the experimenter. [1]

Barkley Rosser's review presents a nice and rather detailed summary of each of the chapters in the book. He points to a number of topics and ideas that he would have liked to see added to the book's content, such as catastrophe theory, or topics he would have liked to see discussed in more detail, such as fractal basin boundaries. In addition to these methodological issues Barkley regrets the absence of macroeconomic applications. I fully agree with these observations and to my defense I can only say that I should have finished this book already years ago and did not want to delay it any further by extending its contents to a number of other important, but in my view not essential topics. I chose to keep the mathematical methods balanced with economic applications. I believe all methods discussed in the book should be part of the toolbox of an economist using dynamic models, but I certainly agree with the view that the list could have been longer. Off course, there are much more complete overviews of nonlinear dynamics in economics, including some of Barkley's own books, particularly Rosser (2000), including an extensive and still up-to-date discussion of catastrophe theory.

As a minor point Barkley Rosser mentions the relatively short Chapter 7 on empirical validation. This certainly does not reflect a lack of interest in econometric testing of the models. On the contrary, I believe empirical testing of nonlinear models in general and heterogeneous agent models in particular is crucial for a more general acceptance of this approach. Luckily, there is a rapidly growing empirical literature, surveyed recently by Chen et al. (2012). In a recent paper Hommes and in't Veld (2014) have extended the empirical results of Chapter 7 and they estimate a 2-type switching model with fundamentalists versus trend-followers using quarterly data of the S&P500, 1950-2012. They estimate the model around two different benchmark fundamentals in finance: the dynamic Gordon model and the Campbell-Cochrane consumption habit model. In both cases they find statistically and economically significant behavioural heterogeneity, explaining the amplification of the dot-com bubble and crash as well as the recent financial crisis. This further illustrates the empirical relevance of heterogeneous expectations models to finance.

The limited attention to macroeconomics is perhaps the most serious omission of the book. Nevertheless, I would claim that the methods presented in the book are very

important for dynamic macro modelling. Barkley Rosser suggests to include some early nonlinear business cycle models, e.g. Hick's nonlinear trade cycle model or Goodwin's nonlinear accelerator, that would have served well in illustrating some of the complex nonlinear phenomena. An even more important omission in my view is the recent work on the New Keyenesian macroeconomic models with heterogeneous expectations. Paul DeGrauwe's recent beautifully written book *Lectures on Behavioral Macroeconomics* applies the Brock-Hommes heterogeneous expectations framework to the NK-setting and obtains interesting and plausible empirical results. At CeNDEF in Amsterdam Domenico Massaro's recent PhD thesis (Massaro, 2012) follows a similar path and presents a theoretical frictionless DSGE model with heterogeneous beliefs (Anufriev et al., 2013), a microfounded NK-model with heterogeneous expectations (Massaro 2013), an estimated switching model with forward-looking fundamentalists versus backward looking naïve expectations (Cornea et al., 2012) and a learning-to-forecast laboratory experiment in the NK framework (Assenza et al., 2012).

The fact that so many new results and applications to macroeconomics have appeared in such a short period after the publication of the book shows that this area is a hot topic for research with high potential for financial and macroeconomic modelling and, ultimately, for better policy analysis.

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