



## Is There an Intellectual Market Niche for Austrian Economics?

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Obviously my answer is “yes,” otherwise I would not have assumed the editorial responsibilities of a journal devoted to social science research from the perspective of Austrian economics. In a recent exchange on this subject published in the *Journal of Economic Perspectives* Sherwin Rosen (1997) argues that Austrian economists made significant contributions to the development of economic thought in the past, and do concentrate on areas of research that other economists neglect (namely, entrepreneurship and innovation). Since Mises and Hayek, however, the Austrian School of Economics has largely failed the market test. In response Leland Yeager (1997) argues that the academic test is not a true “market” test, and, instead, that academic popularity at any moment in time mainly relies on a sort of second-handism in intellectual affairs that is detrimental to the growth of knowledge. Rosen is correct to say that those of us persuaded about the value of Austrian insights need to get our hands dirtier and demonstrate to our fellow economists that there is a pay-off in pursuing different questions (or pursuing the same questions in a different manner). But Yeager, I believe, has the better argument in the sense that progress in science cannot be reduced to a popularity contest, or a counting-heads theory of truth. Unpopular views at any point in time might become essential points of departure in the future—this has been seen in all intellectual affairs, from the hardest of the natural sciences to the softest of the human sciences.

The late Mancur Olson once commented to me over dinner that the problem with neoclassical economics is not the “sins of commission”, but the “sins of omission.” He encouraged me to resist the temptation to criticize other economists for what they do, and simply get on with the work that I wanted to do in economics. His message sunk in, even if I don’t always adhere to it (see Boettke (1997)). There are problems that are not solved, and questions that are not asked. We should make those “sins of omission” potentially “sins of commission” for it is only then that we will find out whether they are “sins” in the first place. That is a worthy goal for *The Review of Austrian Economics*—to become a vehicle for airing contrary opinions and perspectives on economic phenomena.

When contemplating the task of editing an academic journal at this stage in my career, I stumbled across two old *Journal of Economic Literature* articles—one by Oskar Morgenstern (1972) and the other by Joan Robinson (1977). Morgenstern’s article dealt with what he saw as 13 critical problems in economics that had yet to be solved, while Robinson devoted her article to laying out the questions that must be raised and answered by economic research. Despite the obvious advances in game theory and the further development of post Keynesian thought that has transpired since these articles were published, I was struck by

how little progress had been made on a significant subset of Morgenstern's problems and Robinson's questions. Of course, one would have to first demonstrate that those problems and questions are as important as the two authors thought if one were going to critically appraise modern economic thought from this perspective. But what interested me, as it related to the project of *The Review of Austrian Economics*, was the overlap between the two articles. The area of overlap is the contingency introduced into economic theory as a result of treating seriously human desires and beliefs, expectations and imperfect foresight, history and institutions. These are fundamental issues that still require concentrated effort by scholars. Economic theory, for example, still does not possess an adequate theory of the path toward equilibrium, nor have scholars found a satisfactory way to account for the impact of institutions on economic performance.

Treating institutions as constraints retains the determinacy desired of the models, but it does not adequately explain differential performance under alternative institutional settings. Abba Lerner (1946) established that under certain conditions the same marginalist principles developed to analyze capitalist production could be employed to achieve efficient production under collectivism. The controlled economy, Lerner argued, would reconcile liberalism and socialism through welfare economics. Concrete economic institutions would be the same under either system, with disagreements limited to differing assessments of political possibilities. In other words, differences between systems are to be found "outside of the institutional order" that is advocated by either the pragmatic collectivist or liberal capitalist. Since the same principles of optimization apply equally under both systems, the institutional environment fades as an explanatory variable in determining differential performance. When pushed to the logical conclusion, this statement still rings true within the neoclassical framework. Donald Wittman (1995) has recently argued that the neoclassical model of political processes purports to demonstrate democratic inefficiencies, but when pushed to its logical conclusion shows the opposite: democratic bargaining, like economic bargaining, yields the best possible outcome given the constraints. The Lerner and Wittman conclusions follow directly from the logic of neoclassical analysis that in the end nullifies all institutional contingencies: Given the costs and benefits of any situation, the logic of choice results in the "best" outcome available. Various contingencies and imperfections can be introduced in an ad hoc manner, but if the full costs and benefits of any action are taken into account—including the costs associated with achieving an alternative arrangement of affairs—then at any point individuals are doing the best they can given the situation. This style of reasoning has proved useful in solving many problems, but it has also compelled economists to turn their attention away from questions of change, novelty, individual and organizational learning, and institutional adaptation and evolution. Getting inside of these "black boxes" is not a simple task, but it seems to be a necessary one, and one that requires that we approach the matter differently than we have so far. Similarly, recognizing that time's arrow runs in only one direction raises issues of path dependency and hysteresis, and while these issues are being discussed within the mainstream of the profession the question remains as to whether they can be discussed more productively using some alternative framework, rather than trying to force them into a framework that ultimately discounts their effect.

The unsolved problems and the unanswered (and perhaps unasked) questions are most acute in our lingering professional unease with issues related to the nature and causes of the wealth of nations. We venture various reasons to explain why some nations are rich

and others are poor, but even after 200 plus years of asking the question we still don't know. The neoclassical prediction of convergence over time gives way to a recognition of the persistence of a wide discrepancy in per capita income. We have moved beyond the mentality that it is just a matter of "getting the prices right" to one where, more often than not, the answer is "getting the institutions right." Obviously, prices have to be able to adjust freely and thus be "right" and certain institutions are essential to both that task and other myriad tasks required for advanced industrial development, but we still have to figure out how institutions are adopted, or how previously existing institutions mutate to serve the functions required. This remains another "black box" within economic theory that must be opened.

Austrian economics has a long history—tracing back formally to Carl Menger in 1871, but informally to various economists and schools of thought who attempted to get inside the "black box" of processes of change and adjustment, of development and progress, of institutional adaptation and evolution. The uniquely Austrian contribution in this intellectual history was to combine the interest-driven logic of economic analysis with a classical sociological emphasis on social institutions. Menger, for example, both works out the formal properties of the logic of choice, and demands that we apply this logic to explain the emergence of unplanned institutions and overall order. Mises and Hayek developed this approach further in the interwar years and after. This is still *the* Austrian program. It is shared by many other economists and social scientists today—namely, those following a new institutional program of research in economics and political science. There are differences—sometimes quite deep—between Austrian economics and new institutionalism, and they should be recognized and explored, but there are equally as deep shared programatics between the two approaches.

The defining characteristics of Austrian economics consist of: (1) methodological individualism and subjectivism, (2) analytical focus on the agent of change (e.g., the entrepreneur) and market processes of adjustment to change, and (3) an examination of the institutional conditions required for spontaneous order (and the institutional conditions which do not allow for such an ordering to result). Each of these have problems associated with them, and should be open to critical examination. But, these points of orientation in research define the term "Austrian economics." The philosophical justification provided for this research program differ among Austrian economists—such as Kantian apriorism (Mises), some form of critical rationalism (Hayek), Aristotelian essentialism (Rothbard, and perhaps Menger) or something altogether different. These are important philosophical issues to explore. But there is also the more practical issue of exploring the implications for economic understanding of pursuing an Austrian approach independently of how we justify it, and what the payoff will be in terms of theoretical appreciation and historical understanding of economic life.

For the purposes of *The Review of Austrian Economics* the term Austrian will be interpreted in the most catholic sense. This is a forum for research in economics and political economy. As an individual scholar, of course, I have my own biases on what Austrian economics means and where I think the most productive use of intellectual energies should be employed. But it would be an act of extreme hubris for me to think that I, or my team of editors, could determine that for other scholars. Let us pursue our work as best we see fit subject only to the canons of good scholarship. The only standard of judgement for

articles submitted to *The Review of Austrian Economics* will be high quality scholarship, and the expression of ideas in a manner consistent with the journal's mission: to develop the core body of thought in Austrian economics and apply it to address issues of concern in the economics and social scientific profession and—most importantly—to improve our understanding of economic life.

There are unsolved problems in economics, and unanswered or unasked questions. There is an intellectual market niche for Austrian economics and that niche is defined by the unsolved problems and the unanswered questions inside the “black boxes” of human desires and beliefs, of expectations and imperfect foresight, of novelty and change, and of history and institutions. The burden of proof must rest squarely on the shoulders of those who want to do things differently from the mainstream in any discipline, and it will be up to those of us who contribute to the pages of *The Review of Austrian Economics* to demonstrate that solving these problems and asking (and answering) these questions are worth the intellectual energies of economists and political economists. We will have to prove that the payoff is well worth the cost of “daring to be different”—to use a phrase often invoked by James Buchanan.

The unsolved and the unanswered can be found in methodological, theoretical, intellectual history, and applied and historical work in economics and political economy. There are problems in every field of economics—from core theory to the applied fields like monetary economics, industrial organization, public finance and economic development. There are also major historical puzzles that must be addressed in economic and political history—in the 20th century alone the Progressive Era, the Great Depression, the post-World War II era of prosperity, the East Asian Miracle (and its current crisis), the collapse of communism, etc. are subjects of contention.

At present there seems to be a lack of consensus on major issues in substantive economics. The only consensus seems to be on the techniques and style of presentation within academic economics, but even that is fracturing. This is a great opportunity to think differently, to approach something old in a new way, or to approach something new in an old way. The dynamic changes we are witnessing in the world at this century's end—both inside of the scientific and intellectual communities and in the broader world outside of the academy—represent a great opportunity to test the robustness of the “Austrian” economics and political economy research program. Such an opportunity should be grasped with a sense intellectual excitement and seriousness of purpose.

## References

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