

A Look at Subjectivism, Intelligibility and Economic Understanding: Essays in Honor of Ludwig M. Lachmann on his Eightieth Birthday

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In his preface to this volume, Israel Kirzner “draw[s] attention to the altogether special contributions made by Professor Lachmann to the creation and growth of the Austrian Economics Program at New York University” and to his “crucially important part in the remarkable revival of interest in Austrian economics” (p. viii)¹ in the profession as a whole. This is altogether correct. Lachmann’s long and distinguished written contribution to the praxeological school and especially his arrival on the scene at New York University (NYU) were absolutely pivotal; in the absence of the role he played, there is a great question as to whether or not there would have been any Austrian revival at all.² Professor Lachmann functioned as a gadfly. With his iconoclastic views and his continual criticism of all received Austrian axioms and doctrines, he served as a constant challenge, spur, and inspiration—not only to his colleagues and students at NYU, but also to all those such as myself who had the good fortune to come into contact with him.

This is why it is all the more highly unfortunate that a book committed at least in part to the renaissance of the Austrian school makes, with just a few honorable exceptions, so little contribution to that estimable goal. While the Lachmann *Festschrift* is indeed partially dedicated to the recent rise of the praxeological school of thought, it is devoted to an even greater degree to the life and work of Professor Lachmann himself. As a result, it is not so much a praxeological book as a Lachmannian one. Kirzner, in his preface, says as much: “Despite Ludwig Lachmann’s participation in this renaissance of Austrian economics, the present volume is not a collection of Austrian papers” (p. viii).

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Review of *Subjectivism, Intelligibility and Economic Understanding: Essays in Honor of Ludwig M. Lachmann on his Eightieth Birthday*, Israel Kirzner, ed. (New York: New York University Press, 1986).

However, the present volume has been widely hailed as the hermeneutical response to Austrianism, as “the best thing written since *Human Action*,” and as the ultrasubjectivist new paradigm or model, the path to be followed by the “new” Austrian movement. This, as we shall see, is unwarranted. As well, it is important to put to rest even the more modest view that “This book can be recommended to serious students wishing to know what Austrian economists are thinking about today.”³

On the contrary, this book is a collection of highly heterogeneous essays, the overwhelming majority of which have nothing at all to do with praxeology. Some are written by mainstream economists in almost complete ignorance of the Austrian tradition; as a result, their views are at best irrelevant to this school of thought. Included under this rubric are the contributions of Boland, Hicks, Langlois (who discusses game theory), Lewin, and Torr (who grapples with the Rational Expectations and Ricardian schools, but treats neither from an Austrian perspective). The institutionalists are represented by Perlman, who attempts no less a task than to equate Austrianism and the German Historical school, against which Menger and Mises waged a decades-long intellectual battle. The hermeneuticians (Ebeling and Lavoie) undertake no less in behalf of their philosophy with equal success. Then there are those who uphold the post-Keynesian system (Egger and Kregel), without, unfortunately, deigning to reply to the Austrian critique. There are as well the nihilists (Fehl, High, and Strydom), whose goal is to deny the validity of equilibrating forces. Fehl, for example, goes so far as to claim that not only is the market order impossible, it is undesirable too (p. 79).⁴ There are also the obscurantists (Addleson and Shackle) about whose essays it is impossible to say anything coherent.

Quite apart from the fact that most of the book is devoted to non-Austrian viewpoints, there is also the shortcoming that many of the tracts are simply fallacious, even on an elementary level. Egger, for example, appears to call into question the notions of time preference, degrees of roundaboutness, and structure of production, likening them to “primitive concepts which have brought Austrian theories of capital and the business cycle much scorn” (p. 64). He does so without defending this position against the mainstream Austrian analysis, which sees these concepts as a basic building block of praxeology. Boland, a methodologist, appears not to recognize any incompatibility between Walrasianism and market process (32–33). Strydom conflates subjectivism and market process. And Rizzo and O’Driscoll reprise their book (1985) without taking into account Baird’s many fundamental and telling criticisms.⁵

Out of a total of twenty-three papers, only six are both relevant to Austrian economics and free from multitudinous and egregious errors: those written by Boehm, Garrison, Hutchison, Kirzner, Mittermaier, and White. These authors are the honorable exceptions; they make real and important contributions to praxeology. Their work may truly be taken as indicative of what Austrian economists are thinking about today.

Space permits a detailed analysis of only a small proportion of this material, and it is to this task that I now turn. I begin with two articles that are highly critical of Austrian economics.

High

Jack High's "Equilibration and Disequilibrium in the Market Process" is an attempt to "defend Lachmann's thesis that the marketplace is a disequilibrating as well as an equilibrating process" (p. 112). The article is an interesting one, but cannot successfully maintain that thesis.⁶

Professor High gives an accurate and insightful account of the evenly rotating economy (ERE), as utilized by Mises, Rothbard, and Kirzner. He asks: "If the market is always moving toward an evenly rotating state, why doesn't it ever get there?" and cites the traditional Austrian answer: because "the end state is always moving," due to exogenous changes in tastes, technology, and resources (p. 113). But our author rejects this, on the ground that it unnecessarily and artificially limits the scope of economics. On the contrary, asserts High, these phenomena are endogenous: "We know that an open market for ideas will lead people to change their value scales, that the prospect of profits will induce entrepreneurs to seek out new technologies and uncover new resources" (p. 113). Further, to take these changes as exogenous to the market "is to take facts, the knowledge of which should be explained, as given" (p. 114), something Hayek forcefully and eloquently warned against.

The problem with this line of argument, however resourceful and inventive, is that it multiplies the scope of economics way beyond all reasonable bounds. To be sure, changes in tastes, technology, and resources "should be explained," but why must this be done by economists? At present, knowledge of consumer tastes is the domain of psychologists, physiologists, or market researchers; for analysis of technology, we defer to the physicist, chemist, or engineer; nor does the mere armchair economist delve into the arcane mysteries of resource discoveries and development, wisely leaving these matters to the geologist. I am as receptive as the next dismal scientist to the expansion of our turf (see Becker [1971, 1976]; Bernholz and Radnitzky [1986], but this idea of High's would appear to be imperialistic to a degree so far undreamt of.

Nor is this savaging of the intellectual division of labor the only drawback in the scheme proposed by Professor High; it also has a serious logical flaw. He cites Lachmann as stating: "To speak here of 'random shocks' would be to profess ignorance where we have knowledge" (p. 113).⁷ Of course we have knowledge of tastes, technology, and resources. But this is only as *laymen*, not professionals. Using this as a criterion, we know *something* about *everything*.⁸ It is therefore not merely the case that tastes, technology, and resources happen to be exogenous to economics, as per the views of those stick-in-the-mud

Austrians Mises, Rothbard, and Kirzner. High is logically tied to the rather amazing claim that there simply could not be anything at all exogenous to economics! According to his reasoning, even a flood, a tornado, a hurricane, or a volcano would not be exogenous to economics. This would be to profess ignorance where we have knowledge. For do we not know something about floods, tornados, hurricanes, and volcanos? We most certainly do. In addition to the knowledge that meteorologists can provide about these phenomena, we know that they exist.

As part of his overall thesis, Professor High next makes the claim that “the same active mental processes which are taken to adjust to change once it has occurred [entrepreneurship], will also originate change” (p. 115). In this, High is influenced by Schumpeter’s views of entrepreneurship, namely that the profit-seeking businessman will destabilize the economy. Unfortunately, however, he nowhere mentions Kirzner’s utter and complete evisceration of Schumpeter on this point. This is indeed puzzling, since High’s footnote 7 cites this literature, so he can hardly be unaware of it. All that could be done in refutation of this viewpoint would be to recite the brilliant Kirznerian analysis. Since this is readily available, and it is beyond the ability of the present author to improve on it, I shall content myself with referring the reader to it. I must note, however, that it is highly disappointing that the author of the chapter under discussion chose to defend Schumpeter against Kirzner without specifically addressing the points made by the latter against the former.

High places great emphasis on the fact that “money, monetary calculation, business firms, and advertising emerge and persist in the market.” This, in his view, “belies the claim that the market is a strictly equilibrating process” (p. 117). But such occurrences are readily explained by the existence of exogenous forces that continually keep the market in a state of flux and render our knowledge obsolete. This is why we still have need for these institutions that promote and economize on information—despite the fact that the market is strictly an equilibrating process.

Surprisingly, after fighting mightily against equilibrating forces as a demonstration “that order will emerge from decentralized decision making” (p. 117) and as an organizing tool for economists, Professor High seizes upon profit-seeking behavior for this very purpose. Our author gives an insightful and eloquent account of how the entrepreneurial search for wealth helps in the development of the complex economy, and he even contributes to a determination of the extent of the division of labor. But this is still highly problematic, at least on the theory he propounds, because it is difficult to imagine a more equilibrating phenomenon than the desire to capture profits.

Further, we could use the very arguments employed by High in criticizing the Austrian view, in opposition to his proposal that profit seeking is disorderly. For example, why is it that profits have not by now been completely competed away? High could not reply that this is due to exogenous shocks to the system,

for he is committed to the view that there are no such things. Moreover, what about the destabilizing (read: disorderly) aspects of profit seeking? Why has Schumpeter been so suddenly and unceremoniously been shoved out the back door?

I have interpreted High as attempting to defend the Lachmannian “kaleidic” hypotheses that there is no market tendency toward equilibrium. According to an alternative view, the implicit thesis underlying this chapter is that economics should be radically redefined. Austrianism should be limited to a study of the evolution of institutions. All notions of equilibrium are erroneous and should be eschewed, to the extent that they imply an end state. Instead, economics should focus on describing patterns of evolution, with the Misesian monetary theory serving as a model.

If this interpretation is correct, there are several additional difficulties with High’s views. So radical a restructuring would make praxeology unrecognizable; even assuming the notion to have merit, why not define a new discipline (call it Highism or Lachmannianism) separate and distinct from Austrianism? There is also a failure to make a sharp enough distinction between Walrasian general equilibrium, which is inapplicable to reality, and a tendency toward equilibrium, which is not. Most important, we must underscore the Misesian point that all action is inherently aimed at achieving equilibrium.⁹ The economic actor purposefully tries to remove a felt uneasiness, not to increase it. As well, Mises’ monetary theory, just like the rest of his system, was praxeological, not evolutionary.

High’s article is a welcome one in that it thoughtfully tries to show that disequilibrium is an integral part of the market. If the Austrian edifice is to be built sturdily, it will have to withstand such onslaughts as directed to it by this author. Thanks to him, it is now in a better position to do just that.

Lavoie

Professor Don Lavoie sets himself the herculean task of showing that Mises was really a closet hermeneutician. The first arrow in his “Euclideanism versus Hermeneutics: A Reinterpretation of Misesian Apriorism” is the claim that there is no strict dichotomization, in Mises, between theory and history.¹⁰ And Lavoie’s evidence for this rather dubious point? The fact that strict dichotomization between one thing and another is not equivalent to their isolation. He illustrates this by showing, quite correctly, that theory and history “are absolutely necessary for one another” (pp. 193–94).

But this will not do. First of all, this is a straw-man argument. No one has ever claimed, least of all Mises, that the absolute dichotomy between theory and history implies that they must always and ever be kept completely apart. On the contrary, Mises (1969) should serve as strong evidence that the two can and indeed have been brought together. Second, let us argue from analogy:

in like manner, we could say, there is a strict logical dichotomy between normative and positive economics, and a biological distinction to be drawn between male and female. Yet it would be ludicrous to demolish the argument that therefore each of the members of these two pairs ought to be held in complete isolation from one another—if for no other reason than no one has ever made any such bizarre claim. We must therefore reject this view of Lavoie's and insist that for Mises, not only does there exist a deep chasm between theory and history, but that the two concepts, no matter how different, are still complementary.

The next shot across the bow of the good ship Mises is Lavoie's "analysis" of praxeology and apriorism. Consider his charge concerning "the antagonism to empirical work" (p. 195). The problem, here, is an equivocation. Given that for Mises, economics is a logically deductive system, akin to mathematics, no empirical finding, in and of itself, could possibly overturn a praxeological conclusion. Naturally, under these circumstances, a "hostility" to empirical work would appear to arise. But this is true only if the regression equations are interpreted as *testing* an a priori law. On the contrary, there was not an aversion to empirical work per se, as shown by Mises' warm regard for economic history as providing an illustration, not a test, for the theoretical findings of economics.

Lavoie also misinterprets, indeed confuses, "dogmatism and rigidity . . . and [a] confident air of completeness and apodictic certainty" (p. 195) with the Misesian methodology of praxeology. The former is a psychological state, the latter a philosophical category, and no direct translation from one to the other is justified. That is, one can be a deductive economist and yet hold all one's findings in a timid, tentative manner. And the same goes for mathematicians, who also practice an apodictic calling. Alternatively, one can be dedicated to positivism, falsifiability, and empirical testing, while yet being personally obnoxious, dogmatic, rigid, and deaf to all criticism. No facile deduction from the economic methodology employed by a scholar to his psychological state is thus justified.

Lavoie then criticizes Mises for holding the view that "No historical account can ever cause us to go back and reconsider our a priori theory, thus suggesting that theories are somehow epistemically privileged and safely dichotomized from history. This view makes economics seem too different from the natural sciences" (p. 196). Again, there is a confusion between economics and psychology. There are numerous phenomena that cause people to go back and reconsider. It could be a sunset, a Mozart opera, or an intellectual unease that just comes upon a person; it all depends upon the individual in question. In some cases, of course, there is nothing that can lead a person to reconsider a pet theory. But this issue must be sharply distinguished from a subtly different one. If the theory seems to be out of step with the facts, must we necessarily renounce the theory? Suppose, for example, that an empirical finding determines that an increase in the minimum wage level led to a *decrease* in the teen unemployment rate, or that the imposition of a strict rent-control ordinance

called forth a vast building spree of rental housing. Would economists necessarily be forced to reject their typical views of such legislation?¹¹ To ask this question is to answer it. The point is, there is a vast distinction to be drawn between the natural sciences and the human sciences. It is only in the latter case that motives and purposes are comprehensible to us.

The George Mason University professor then goes on to designate as “hermeneutic” the insight that economic explanation must be grounded in individual purposes (p. 196). If we allow him to get away with this bit of definitional legerdemain, we must indeed conclude with him that Mises was a hermeneutician, because if there was anyone who insisted that praxeology be based on the purposes of the individual, it was Mises.

Perhaps the most interesting part of the Lavoie contribution is his analysis of the textual evidence. He starts out well, citing Mises in several of his more “Euclidean” utterances. His first criticism is that Mises is a “bit free” (p. 200) in attributing certainty and incontestability to his pronouncements. Two responses are in order here. First, there is a failure to distinguish the claim that economics is an apodictically certain enterprise from the one that any particular economist, such as Mises, is apodictically certain. In the enumerations of Mises’ words in his text, our author indicates only the former declaration, not the latter. Second, if Lavoie wishes to attack Mises for expressions of excessive certainty, it is incumbent upon him not merely to specify instances where he does indeed express certainty, but where these expressions are in fact excessive. Namely, he has to show him to be *wrong*, in at least one instance. For example, let us suppose that Mises, as mathematician, had claimed not only that “ $2 + 2 = 4$ ” was apodictically certain in mathematics, but that he, Mises, was himself indubitably certain about the truth of this equation. Were Lavoie to criticize Mises for this, on the grounds that he was *excessively* certain about this, it would be insufficient to merely charge the use of certainty; he would have to show it to be *false*. Otherwise, although there would indeed be certainty, it would not be shown to be excessive.

Next, Lavoie objects to the exclusivity that Mises claims for the focus of praxeology on human purposiveness (p. 201). But this does not “suggest an insulation from criticism.” Were this true, then Mises, or his followers, when presented with a criticism, would merely have dismissed it, without giving reasons. Interestingly enough, Lavoie is unable to substantiate his charge of “insularity,” although he is fully familiar with the Austrian literature. Nor does he deign to discuss or even refer us to a valid economics that does *not* ground itself on individual purpose. Neoclassical macroeconomics, for instance, has virtually entirely banned consideration of individual purpose from its realms. Is Lavoie claiming that this mode of thought can make a contribution to our knowledge of economic reality?

At last we arrive at the nub of the Lavoie thesis: Mises as hermeneutician. Our author starts off with a quote from Mises where he states, quite reasonably,

that “Reasoning and scientific inquiry can never bring full ease of mind, apodictic certainty, and perfect cognition of all things” (p. 203). After that, the analysis goes sharply downhill. Lavoie first sees in this modest statement a contradiction with previously cited “Euclidean” avowals of Mises. But he again fails to contend with the distinction between psychology and economics. One can, as Mises does, adopt a modest attitude toward one’s own abilities, even in behalf of all mankind, and still noncontradictorily demonstrate the apodictic nature of an intellectual discipline such as geometry or economics.

Second, Lavoie’s treatment is marred by his continued and unabated gratuitous attacks on poor old Euclid, without benefit of citation. From whence comes “Euclidean uncriticizability” or “close it off in Euclidean fashion from rational criticism” (p. 203)? When and where did Euclid (or Mises, for that matter) claim that all criticism of their views was per se illegitimate?

Third, the “hermeneutic a priori” would appear to differ from its Euclidean counterpart only in that the latter, but not the former, is “immune to criticism” (p. 204). Certainly, part of the Misesian defense for the a priori nature of the praxeological axioms was that those who denied this would “contradict themselves in practice by arguing their behavioralistic case to other ‘minds’ in the scientific community” (p. 204).

Fourth, his case is marred by a needless bow to pragmatism. Contrary to Lavoie, the assumption of intersubjectivity *is* based on firm philosophical foundations. Were it not, what would it mean to assert that “this procedure works in everyday life” (p. 206)? Without the philosophical grounding, we would have no reason to reject the hypothesis that all other people are just cleverly constructed robots.

As for the remainder of the chapter, it consists of Lavoie citing numerous modest expressions by Mises and, by Lavoie’s torturing them, teasing out a hermeneutical implication, solely on the grounds of the constraint of these statements.¹² He only succeeds in showing, contrary to his previous assertions, that Mises was a careful, moderate, and cautious economist.¹³

Boehm

“Time and Equilibrium: Hayek’s Notion of Intertemporal Equilibrium Reconsidered” by Stephan Boehm is a welcome addition to the Austrian literature in general and to our knowledge of capital theory, interest rates, time, and their complex relationships. It also makes a significant contribution to history of economic thought, serving as a sort of secondary source material for, and clarifier of, Hayek’s very turgid *The Pure Theory of Capital* (1941, 1975).

The author distinguishes authoritatively between Hayek’s notion of equilibrium (intertemporal mutual plan compatibility) and the more mechanistic, static, and a temporal version still popular in the profession (pp. 20–21).

He properly credits Hayek for strenuously resisting the widely accepted notion that capital is merely a “homogeneous substance” (p. 22) or an “amorphous mass” (p. 23) and instead insisting upon its heterogeneity. He criticizes the Arrow-Debreu equilibrium model from a Hayekian perspective, showing that the concept of “movement toward” an equilibrium makes no sense in the former case (p. 24). Most important, he places great emphasis on the importance of subjectivism in capital theory, on Hayek’s success, and on Arrow-Debreu’s failure to distinguish between the perspective of the human actor and that of the omniscient economist-observer (p. 25).

Further, Boehm is correct in focusing on something of an inconsistency that crops up in *Prices and Production* (1932). On the one hand, this book posits what is in effect an equilibrium model of the structure of production; on the other hand, Hayek attempts to show the destabilizing effects of government monetary policy. The difficulty is that, strictly speaking, the existence of money is incompatible with the certainty that underlies equilibrium. However, it is easy to make too much of this criticism. There is somewhat of a lack of appreciation that the evenly rotating economy is used by Hayek merely as a heuristic device. One searches in vain in *Prices and Production* for the Rational Expectations type of confusion that would ascribe to the real world the existence of actual equilibrium.

As well, there is one other minor criticism that can be leveled against this chapter. Boehm mentions without demur that there might possibly be “no such thing as *the* rate of return or interest since there are as many rates as there are commodities” (p. 19). But why is there no profit-and-loss-inspired tendency for all these different rates to come together?

But these are minor quibbles with a superior product, which will repay, with great interest, the effort of reading it.

Garrison

Roger Garrison’s contribution to the Festschrift, “From Lachmann to Lucas: On Institutions, Expectations, and Equilibrating Tendencies,” is nothing short of magnificent. By creating a “tendency toward equilibrium spectrum” and then placing on it Lachmann (“never”), Mises-Hayek (“sometimes”), and Lucas (“always”), Professor Garrison does more to elucidate the views of the Austrian, the Rational Expectations, and the “kaleidic” schools of thought on the equilibrating tendencies in an economy than an essay of ten thousand words or more could have done. Through this astute and innovative feat, moreover, the author of this chapter is able to once again establish praxeology as the moderate and reasonable view that takes on an intermediate position between two extremist beliefs that might otherwise have appeared more attractive than they are. (See Garrison [1982].)

Garrison uses Lachmann's concern with future expectations to cast doubt on Lucas's assertion that the economy must always and ever be in equilibrium. He mobilizes the Mises-Hayek insight that on the free market, those who are better able to anticipate consumer demands will tend to have command over more and more resources and thus will be able to cast a disproportionate impetus toward equilibrium. This undercuts the extreme Lachmannian skepticism that there can be even a tendency toward equilibration.

Further, with this spectrum device, the Auburn University professor can focus attention on the crucially important role played by institutions. The Hayekian criticism of Keynes is that there is not enough disaggregation in this system to allow for the equilibrating role of entrepreneurial success. But this can only occur, shows Garrison, in a marketplace where businessmen can reap the reward of their superior insight. Paradoxically, or perhaps not so paradoxically, Keynesian-inspired government "stabilization" measures can actually retard movements toward equilibrium. Says Garrison: "They nullify the market forces that give rise to equilibrating tendencies, thus causing the economy to perform in the very way that Keynes envisioned it" (p. 99).

Notwithstanding the numerous brilliant insights gracing this essay, there are two minor points upon which criticism can be offered. First, in the course of his quite proper formulation of the Hayekian theory of capital, Garrison asserts that "Successful investment over time requires that the investor's decisions be consistent *both with the subsequent decisions of other investors and with the ultimate demands of the consumers*" (p. 96, emphasis added).

If we interpret this unsympathetically, counterexamples are easy to come by. For instance, if one entrepreneur and he alone correctly forecasts consumer interest in, say, a hula hoop, then he will make more profit, not less, than if he is in accord with the decisions of other investors. To be sure, there is a sense in which it is true that not only must the thriving entrepreneur be attuned to the consumer, he must also act in concert with his fellow businessmen. For example, even if he correctly anticipates future demand, but no one else does, he may be so far out of step, even though correct in some ultimate sense, that the complementary factors of production may not be available. Sometimes this is called "being too far ahead of one's time." But even in this case it may not be necessary to be "consistent with the subsequent decisions of other investors." For it may be possible, if he is rich enough, for him to finance the production of not only the item in question, but also the complementary factors as well. In such a case, his earnings would be even higher. It is only if this is beyond the capability of one single entrepreneur that Garrison's stricture will be correct.

Second, Professor Garrison maintains the position that "Investors who are able successfully to complete their projects gain command over greater quantities of resources. In turn, the subsequent decisions of these successful investors have increased weight in *determining the market rate of interest*" (p. 98, emphasis added). The difficulty here is that this would appear to be contrary to the

Austrian pure time preference theory of interest-rate determination. Since our author is himself on record as supporting this view (see Garrison, 1979; also Rothbard, 1962, chapter 6; Mises, 1966, pp. 479–523), we need not take time here to defend it. Of course, one might read the passage as claiming that since these investors will now have more money *qua consumer*, their own time preference will now be given a greater weight in interest-rate determination. If this were true, it would be unexceptionable. However, in the second sentence after the one just cited, Garrison quite clearly avers that it is “subsequent *investment* decisions by these investors” (p. 98, emphasis added) that constitute the operational factor involved.

But apart from these two minor problems, I must conclude that the Garrison chapter makes an important contribution to Austrian theory.

Hutchison

In “Philosophical Issues That Divide Liberals: Omniscience or Omni-nescience about the Future?” Terence Hutchison provides an insightful, detailed, and useful survey of the economic literature regarding assumptions of knowledge. He covers such schools of thought as the physiocrats, the French Enlightenment, the utopians, the classical economists, the neoclassicals, the Rational Expectationists, the institutionalists, the Austrians, and the neo-Austrians. In this brief (seventeen-page) essay, mention is also made of the following economists: Popper, Mises, Smith, Mandeville, Tucker, Galiani, Viner, Quesnay, Malthus, James Mill, Ricardo, McCulloch, J.S. Mill, Senior, Walras, Cairnes, Wieser, Robbins, Leslie, Nasse, Menger, Knight, Keynes, Hayek, Shackle, Lachmann, and Coase.

Not only is Professor Hutchison exhaustive, he is also largely correct in his analysis. Briefly, his thesis is that most economists and schools of thought have highly overestimated the knowledge possessed by the economic actor and have been altogether too ambitious in their claims in behalf of the economics profession. These “optimistic assumptions, or pretensions” were not made so much by Smith, Mandeville, Tucker, and Galiani, as they were by Quesnay, the physiocrats, both Mills, Ricardo, and McCulloch. The “perfectly competitive economy,” beloved of the neoclassical school, and Walras, along with the Rational Expectationists, “mathematical and quantitative methods,” and the assumption of full or perfect knowledge all come in for some particularly heavy criticism in terms of claiming more than is justified or being particularly unrealistic.

Our author is especially and quite properly critical of the notion that “even if ignorance might be conceded as an important characteristic of individuals, knowledgeable or even omniscient governments could correct or compensate for their errors and maintain an equilibrium” (p. 128).

If Hutchison is derogatory about the overestimation of knowledge in economics, this is also true for his views regarding its underestimation. In this context he castigates the neo-Austrian views of Shackle and Lachmann: “They seem . . . to depend on assumptions, or arguments, which constitute too extreme a reaction from conventional, utopian superationalism towards a kind of subrationalism, or even irrationalism” (p. 130). This, I submit, hits the nail exactly on the head.

The essay also features a fascinating discussion of the relationship between knowledge (or ignorance) and the possibility of human freedom. Hutchison concludes, quite wisely, that “significant freedom can hardly exist at the poles of omniscience and omni-nescience about the future, but can only emerge in a mixed world . . . which . . . combines elements of predictability and unpredictability” (p. 131).

By now, the reader will have deduced that I liked this contribution quite a bit. However true, there are still a few nits to pick with its author. First of all, Hutchison carries his war against certainty to an extreme on one occasion by denying that there can ever be incontrovertible economic knowledge. Siding with Hayek vis-à-vis Mises, Hutchison states: “In other words, apriorism cannot tell us anything about real world causation” (p. 128). But consider the statement: “On the free market, there is a tendency for profits to equalize in different industries, given that account is taken of risk.” This (or any correct tendency claim, for that matter) would appear to be certain, at least in the sense that no state of the world could falsify it; as well, not only does it tell us something about real-world causation, it explains an essential element of real-world economic causation.

Second, an equation is made between “a libertarian school of economic thought” and the neo-Austrianism of Shackle and Lachmann (p. 129). On an aesthetic level, perhaps a better synonym would have been *libertine* rather than *libertarian*. In a more serious vein, libertarianism is a political philosophy, while Austrianism is an economic school of thought. If the normative-positive distinction is to be maintained, as it should (my value judgment), equations of this sort are problematic.

Third, there is a difficulty with Professor Hutchison’s view that it is question begging “simply to deny the ‘possibility’ of the predictions on which some kinds of governmental policies might be based, while claiming the possibility of the kind of predictive capacity required . . . for the effective operation of smoothly self-adjusting markets.” But Austrian economists do not “simply . . . assert that one kind is, in principle, impossible, while the other kind is intellectually respectable and well worth attempting” (p. 132). On the contrary, they prove this with a wealth of analysis, showing how the different institutions, government and the market, operate in very different ways. (See Rothbard, 1977.)

But these points occupy a minuscule part of the essay, which is well worth reading, and makes an important contribution to our store of knowledge.

Kirzner

This is yet another brilliant contribution to the Austrian edifice, one to which Israel Kirzner has already given so much. In this paper on subjective costs, our author draws heavily on the insightful work of the Austrian James Buchanan¹⁴—the better to criticize that of the neoclassicals Armen Alchian, George Stigler, and William Baumol.

Like a magician pulling rabbits out of a hat, Kirzner begins by distinguishing no less than four different interpretations of cost. Consider the case of a swimming pool. First, there are the disadvantages of building it, such as the reduction of one's bank balance and the attendant noise created by unruly neighborhood children. Second is the objective opportunity forgone, such as a car, which can no longer be acquired. Third is the objective alternative that could have been produced with the raw materials that instead went into the construction of the pool (for example, a summer cottage). Fourth, and most important, is the subjective "assessment at the moment of his decision regarding the pool of what he would be giving up" in order to obtain it, not in terms of a definite item, such as an auto or a cottage, but in terms of the "utility prospects deliberately sacrificed" (pp. 142–43).

In Alchian's view, the opportunity cost of the pool is either the objectively known car or the cottage; it is definitely not the essentially subjective loss of utility imposed by the noisy neighborhood children. However, in Kirzner's subjectivist approach, this by no means follows. On the contrary, it is possible that the noise is one of the perceived prospects that are knowingly and willingly given up at the moment of choice. It all depends upon the mindset of the *chooser*; it is not and cannot be given to outside observers such as Alchian.

In order to clarify this distinction further, Kirzner next takes up the case of the "expensive wife." In the view of George Stigler and others (p. 145), the cost of marrying a woman (who stays at home to raise children) is the opportunity cost of the salary she forgoes by not working. Therefore, it is cheaper to marry an uneducated female, with poor employment prospects, than her highly market-skilled sister. States Kirzner in reply:

Before this marriage decision the prospective groom had no alternative prospect whatever of enjoying the woman's high professional income; his decision to marry her involved *no sacrifice* by him of her income at all. . . . To be sure, once the two have married, a *subsequent* decision that she stay at home carries with it the cost of her foregone [SIC] income. But this is irrelevant to the injunction to the would-be groom to marry the uneducated girl in order to avoid high costs. (p. 145)

Continuing his critique of the notion that the outside observer can determine costs, our author considers two homeowners in similar circumstances

who are contemplating constructing identical swimming pools. He gives four reasons for supposing that they may have *different* costs (1) One homeowner may forget to incorporate the noisy-kid factor into his calculations. (2) They form different expectations as to the likelihood of this inconvenience. (3) They may attach different weights to its importance. (4) The outside observer faces not just practical impediments in his determination. “To rank the costs faced by different decision makers is as conceptually impossible a task as is that of comparing utilities interpersonally. (In fact, of course, these two tasks are merely variants of a single impossible undertaking.)” (p. 147)

Next, it is Baumol’s turn under the gun. In his view, money outlays can measure subjective costs. If it costs twice as much to kill a beaver as a deer, then one beaver should naturally exchange for two deer. Thus, the subjective costs of one beaver, for everyone in a flexible marketplace in equilibrium, would be two deer, no more and no less. This line of reasoning is rejected by Kirzner on the grounds that it is not enough to talk about the sacrifice of “*objects*, as opposed to *perceptions* of these objects” (p. 150). In the two-commodity world of deer and beaver, it is perhaps tempting to do just that. But in the real complex world, we cannot assume that two people will look upon objects in the exact same way, even if the objects are physically identical. “If the cost notion is to serve as an *explanation* of why a person made the decision he did, it will not do . . . [to make this equation] . . . unless we can rely upon the assured, complete awareness of the objective facts” (pp. 150–51). Baumol, in other words, is assuming away the possibility of error in the learning process, and it is only through this process that we have any hope of achieving full awareness.

Our author concludes his analysis with a consideration of the doctrine of social costs. Since cost has no meaning outside of and apart from an actual decision, and the act of choice can only be an individual one, all subjective costs must of necessity be private ones. To the extent that the concept of social cost has any meaning at all, it must refer to hypothetical (and thus metaphorical) decisions, not real ones. And this goes for the so-called objective opportunity costs as well. The core of any meaning they may have can only be made intelligible in terms of subjectivity.

Although Kirzner does not mention the political implications of his work, they are not too difficult to discern. Since outside observers are never in a position to determine the costs of actual market participants, all public policy that implicitly relies on objective costs (e.g., tax, antitrust, and utility legislation) is without intellectual foundation.

No analysis of any article would be fully complete without a mention of its shortcomings, and in this case the faults were obvious and dramatic. First of all, I felt compelled to read through this chapter on four separate occasions (and, of course, profited immensely from each perusal). Second, I felt compelled to underline virtually every single sentence—many several times over,

in different colors of ink—and to write congratulatory notations in the margins throughout. After a while, my copy of this chapter was unreadable and I had to obtain another book. If you wish to see a brilliant mind in action, to gain a never-to-be-forgotten insight into praxeological reasoning at its best, and have alternative costs of time that preclude extensive study, then read this chapter. It is truly exquisite.

Mittermaier

Anthropomorphism is the assignment of human mental processes to a non-human entity. When this is done in behalf of objects, such as stones, trees, storms and molecules, it is illegitimate. In more superstitious times, this was common practice. Nowadays, of course, the physical sciences have exposed anthropomorphism as a fallacy and have banished it. However, the rejection of this doctrine in that area has become so well entrenched, and those who have done so have taken on such a great aura of prestige that as a result, a horde of imitators has arisen in the social sciences. Because of their efforts, this precept has been not only expunged from the physical sciences, where it does not belong, but from the human sciences as well, where it does.

Karl Mittermaier has designated such later development as a “mechanomorphism” and defined it as improperly “ascrib[ing] mechanical properties to what is otherwise recognized as an aspect of human affairs” (p. 237).

In criticizing the mechanomorphisms of the positivist economists, Professor Mittermaier makes an important contribution to Austrianism. He is particularly sharp and critical with regard to macroeconomic aggregates, which he characterizes as “treat[ing] an economic system as though it were a mechanical system” (p. 237). He captures precisely the orthodox view on saving and consumption as “a macro-economic fluid whose flow sometimes has to be augmented and sometimes diminished” (p. 238).

In his analysis, to slavishly and uncritically copy classical mechanics from the physical to the social sciences is to commit the logical fallacy of violating the coherence rule (p. 247). This rule “stipulates that a question should be posed in terms which all belong to the same domain of thought and that the corresponding answer should be composed in terms which all belong to the same domain of thought as the terms of the question” (p. 247).¹⁵ That is, I take it, if we are discussing actual people with beliefs, fears, and expectations, people who make purposeful decisions, then by all means do not conflate this category of discourse with talk of indifferent curves, “utils,” and other such tools of the trade of the modern mechanomorphic neoclassical economist.

Important as are these positive elements of his paper, it is marred by a tendency to go to the other extreme and dismiss as mechanomorphism several concepts that can be strongly grounded in human purposiveness. Consider

“equilibrium,” for example. The author of this chapter asserts that “Equilibrium clearly comes from this domain of thought, and talk of equilibrating or market forces must be regarded as mechanomorphic” (p. 237). There is no doubt that in the hands of *some* economists, equilibration is treated as no more than a balancing act, akin to that attained by a weight scale. But as others use the concept, equilibrium (or the ERE) is interpreted in terms of purposiveness or human action.

The concept of time would appear to be another instance. Mittermaier objects to its use in economics because it may be “easily interpreted as a prediction” (p. 238). Yes, to be sure, there is always the danger that time may be used mechanomorphistically, completely apart from human motivations, but our author comes perilously close to asserting that this *need* be the case.

Another difficulty with the chapter is that our author complains that Mises devoted only “about 300 words” (p. 246) to the explication of the concept of human action. He realizes that the elaboration can be found in *Human Action*, but is unhappy with the fact that one would need a “fine-tooth comb” (p. 247) to find it. He seems unaware of the contribution of Murray N. Rothbard (1962) in this regard.

Further, Mittermaier holds that private property implies means and ends, but denies the obverse (p. 247). Yet, how is it possible to have means and ends, surely the result of human purpose, without private property?

White

“A Subjectivist Perspective on the Definition and Identification of Money” by Lawrence H. White is a gem of an article. It carefully sets up praxeological considerations and utilizes them to address a perplexing problem that would otherwise be far more intractable.

In the first section, the University of Georgia professor grounds his analysis in the subjectivist perspective; he will seek a monetary definition comprehensible in terms of individual human action and purpose, not based on correlations with any statistical aggregates. He arrives at GAMOE, the “generally accepted media of exchange” in an economy. White shows how the other textbook definitions of money (such as, store of value, unit of account, and standard of deferred payments) are all subsidiary to money’s essential nature of facilitating exchange.

In terms of actual identification of money candidates, he concludes in section 2 that only hand currency, coins, traveler’s checks, and checkable claims on banks pass muster; that is, he arrives at that old warhorse, M1. He specifically rejects claims made in behalf of the moneyness of noncheckable bank liabilities and money-market mutual fund shares.

White is particularly eloquent and forceful in rejecting Yeager’s objections to the inclusion of traveler’s checks in the money supply. He does so on the

grounds that the criterion of repeated circulation (which this medium lacks) would rule out of court, under a gold standard, those parts of gold bullion intended to be used not for exchange, but for industrial purposes. As well, the narrower Yeager criteria of routine circulation would reject corn as money, even though it were the most popular exchange medium in a barter system, since some of it could be eaten. This would also repudiate “checkable demand deposits in a multibank system . . . [since] demand deposit claims on Bank A are not accepted by customers of other banks with the intention of passing them along without redemption” (p. 307).

Unfortunately, however, White does not spend nearly as much time criticizing the views of Rothbard, a more serious protagonist. Instead, in the space of an exceedingly short footnote, White cursorily dismisses the Rothbardian claim that passbook or savings accounts should be considered money on the ground that they can be turned in for cash on demand. The author of this chapter maintains that “this feature is irrelevant when they fail to satisfy the medium of exchange criterion for money which Rothbard (1978, p. 144) himself enunciates” (p. 314, note 26). One question that remains unresolved, based on this reply, is whether or not passbook accounts redeemable on demand constitute money under the medium of exchange criterion. It would appear that such funds could be used to transact business and that at least some people actually do so. Another inquiry that remains open, unhappily, is how White would respond to what can only be considered Rothbard’s attempt to anticipate this very objection:

Suppose that, through some cultural quirk, everyone in the country decided not to use five-dollar bills in actual exchange. They would only use ten-dollar and one-dollar bills, and keep their longer-term cash balances in five-dollar bills. . . . If a man wanted to spend some of his cash balance, he could not spend a five-dollar bill directly; instead, he would go to a bank and exchange it for five one-dollar bills for use in trade. In this hypothetical situation, the status of the five-dollar bill would be the same as that of the savings deposit today. But while the holder of the five-dollar bill would have to go to a bank and exchange it for dollar bills before spending it, surely no one would say that his five-dollar bills were not part of his cash balance or of the money supply. (Rothbard, 1976, p. 181)

While this paper otherwise makes a sterling contribution to Austrian thought and to monetary theory in general, there are, in addition, two very minor difficulties. First, on pp. 301–2, its author positions himself strongly in the subjectivist camp, criticizing econometric analysis and the statistical and aggregative perspectives as not in keeping with the individuals’ plans. But on page 304 we find him announcing that his work will be of “vital importance” for statistical work, for price indexes, and for other aggregates. And so it will be. Paradoxically, even the use of macroeconomic concepts can benefit from

methodologically individualistic insights. But one must consider it a lacunae that no discussion of this seeming contrariety is provided, nor is there any critique of this mode of analysis, as would appear consistent with White's own basic outlook.

Second, the author of this paper maintains that his study is "consistent with the methodological subjectivism espoused by Ludwig M. Lachmann" (p. 311). This, however, is somewhat misleading. On the one hand, there is nothing in this chapter that is uniquely connected with Professor Lachmann's version of subjectivism. Any other Austrian version of subjectivism, individual purposiveness, or human action would suffice at least as well. On the other, a case could easily be made out that the Lachmann "kaleidic" vision is *not* compatible with White's work. For suppose that expectations are not convergent. That is, assume with the honoree of this Festschrift that people diverge in their expectations about everything under the sun. How, then, would it be possible to construct a comprehensible theory of money, as White has so admirably done, given that implicit in its identification is not merely individual subjectivism, but rather intersubjectivity? If, moreover, inherent in the definition of money is an intention to use it as a medium of exchange in the future, but no one can ever know anyone else's future intentions or expectations, how can we even coherently define money, much less identify any?

Conclusion

Due to my focusing the detailed analysis on by far the best chapters in this Festschrift, the reader may be left with a better impression of the book than it deserves. In point of fact, this volume is replete with errors and is most disappointing for a book touted as the most modern, up-to-date version of Austrianism and as a challenge to the economics of Mises. Indeed, the majority of its chapters simply ignore the basic tenets of this school of thought. And several of those that do not ignore praxeology misconstrue it.

But it is important that an overall assessment of the work of the followers of Professor Lachmann not be lost sight of amid the welter of minutiae about their specific errors. And, unfortunately it is not just that they are wrong about equilibrium, methodology, hermeneutics, time preference, and so on; even worse, if possible, is the fact that there has been a virtual cessation of focus on real economic problems such as money and banking, business cycles, utility and welfare economics, and monopoly theory. Virtually all that is heard from this quarter is an endless repetition that the market is a process, that equilibrium is a red herring or worse, that no one can ever know anything, and that all is subject to interpretation.

It is possible to construct a continuum in this regard. On the one extreme would be the mainstream empiricists, who believe that their regression equations

can test economic axioms. This might well be called an overreliance on economic research. On the other extreme would be the followers of Lachmann, who have all but eschewed economic research; the impossibility of knowing the future, the divergence of expectations, radical subjectivism, it would appear, make this an extremely dubious path for them to follow. And as moderates on the continuum are the Misesians, who maintain that history can illustrate but not test theory, and who are nevertheless vitally interested in doing just this type of work.

Notes

1. All page references, unless otherwise identified, are to this Lachmann Festschrift.

2. It is a personal pleasure to be able to review the Lachmann Festschrift and thereby be able to honor this great man, however indirectly, because as it happens, I, along with Walter Grinder, was able to play a small role in support of the initial invitation that first brought him to NYU as a visiting professor.

3. S.C. Littlechild, *Economic Affairs*, June 1987, p. 43. Similarly, David L. Prychitko, "Ludwig Lachmann and the Farther Reaches of Austrian Economics," *Critical Review*, vol. I, no. 3, p. 66, makes the point that this book is "in the Austrian tradition" and claims that "For anyone interested in the farther reaches of Austrian economics, this is a book worth reading."

4. The magnificent contribution of Garrison on this subject serves as an antidote to these musings; unfortunately it is confined only to his chapter.

5. To be sure, this Baird criticism was published one year after the publication of the Rizzo-O'Driscoll chapter in the Lachmann Festschrift, in 1986. But an earlier draft of the Baird paper was presented to O'Driscoll and Rizzo at a Liberty Fund Conference on Time and Ignorance in November 1984.

6. In his fourth footnote, High claims that nothing in George Selgin's "Praxeology and Understanding" (unpublished manuscript later published as Selgin [1987]) "contradicts the defense of Lachmann presented here." But this is hard to reconcile with the criticism that Selgin actually leveled at Lachmann (see especially pp. 32–33). Professor High also claims in this footnote that "Selgin's views are much more compatible with those of Lachmann than his paper would suggest." In this, he is contradicted by his colleague at George Mason University, Don Lavoie, who correctly asserts in his first footnote: "George Selgin's critique of Lachmann and Shackle, which interprets them both as historicists, points out that since the Austrian school began with Menger's devastating criticism of the historicists of his day, it would be unfortunate if contemporary Austrians were to revert to this antitheoretical stance." While Lavoie mistakenly disagrees with Selgin's contention that Lachmann is an example of historicism, he at least sees, as High does not, that in Selgin's view this is so.

7. Notice how truly great a departure is this view from those we have come to associate with Professor Lachmann. Usually, and typically, we may rely on that eminent economist to express a skeptical view, not to say a nihilistic one. But in this case, in stark contrast, he is amazingly exuberant and optimistic about the knowledge and abilities attained by our profession.

8. This is a tautology, not an empirical claim. For if we did not know anything about *x*, we would have to be completely unaware of it. And if that were true, the question of our knowledge about it could not even arise. And if we were even but aware of *x*, we would know something about it, namely its existence.

9. See Selgin (1987, pp. 34–36).

10. For Lavoie, “formalism” is equated with the neoclassical fixation on equilibrium states, which he now dubs “Euclideanism” (p. 193). In contrast, there is “antiformalism,” which is now equated with hermeneutics. Thus, with a sleight-of-hand redefinition, there would appear to be no room for a Mises who rejects both neoclassical formalism and hermeneutical antiformalism. One must, by definition, be either a neoclassical Euclidean formalist or an “Austrian” hermeneutical antiformalist. To paraphrase the old adage, “When faced with needless and extraneous definitions, reach for your wallet—or count your fingers.” There is a complication, however. Our author also characterizes Euclideanism as a “fully axiomatized, linearly constructed system of strict deduction” (p. 198), which is a far more apt description of Mises.

11. According to the survey conducted by Frey et al. (1984), 10 percent of U.S. economists disagreed with the proposition that “A minimum wage increases unemployment among young and unskilled workers,” and 1.9 percent denied that “A ceiling on rents reduces the quantity and quality of housing available.”

12. For example, Lavoie (p. 207) cites Mises as follows: “In asserting the a priori character of praxeology we are not drafting a plan for a future new science different from the traditional sciences of human action.” Lavoie concludes from this that Mises could not have meant to refer to “strictly linear deduction” (p. 207). But in the very next sentence following the one quoted, Mises maintains not “that the theoretical science of human action should be aprioristic, but that it is and always has been so” (*Human Action*, p. 40). Lavoie seeks to support his interpretation of Mises as a sort of empiricist, not a praxeologist, when he quotes him as stating: “All that man can do is to submit all his theories again and again to the most critical re-examination” (p. 207). Very reasonable. But again Mises’ very next sentence supplies the crucial context neglected by Lavoie: “This means for the economist to trace back all theorems to their *unquestionable and certain* ultimate basis, the category of human action . . .” (*Human Action*, p. 68, emphasis added). Contrary to Lavoie, Mises is not looking for greater certainty about the world from more and more empirical induction; rather, he is urging us to rigorously check our basic premises and the logic of our deductions. Further, Lavoie disingenuously interprets Mises’ statement “Economics does not follow the procedure of logic and mathematics” (p. 205) as support for his contention that Mises did not mean “for economic theory to be forced into a fixed, hierarchically deductive structure, like the theorems of geometry” (p. 205). But anyone who reads the relevant passage from *Human Action* (p. 66) will discover that this is not at all what Mises stated. Yes, to be sure, there are important differences between geometry and praxeology. But this has nothing whatever to do with hierarchy and deduction, as misleadingly adduced by Lavoie. On the contrary, the differences have to do with the assumptions, the applications, and the “solutions of concrete historical and political problems” (*Human Action*, p. 66). Geometry, in other words, is concerned with spaces and lines; praxeology, with acting human beings. But apart from that, their methodologies are very similar.

13. I have interpreted Lavoie as claiming Mises as a hermeneutic anticipator. According to an alternative perspective, Lavoie argues, in effect, that Ludwig von Mises had a bipolar personality disorder, a species of philosophical schizophrenia. As a result, Mises could not decide between formalistic certainty and radical skepticism. In this view, when Mises speaks of “apodictic certainty,” Lavoie puts Mises on the couch and sees only self-congratulatory flourish. When Mises addresses the need for careful thought, psychiatrist Lavoie diagnoses this as questioning the fundamentals of economic law. This interpretation is buttressed by Lavoie’s habit of dismissing Mises for using empirically based arguments when he is merely utilizing the methodology of his opponents in order to employ *reductio ad absurdum*. For example, Lavoie (p. 206) interprets Mises as a pragmatist (“we accept this pragmatic point of view”) but, as a perusal of the actual page of *Human Action* (p. 24) makes clear, Mises was only employing this view as part of a *reductio ad absurdum*. Mises adopted pragmatism, but only temporarily, for the sake of argument, in order to overturn this philosophy.

14. James Buchanan may not be an Austrian on most other questions, but he certainly is with regard to subjective costs. See his *Cost and Choice* (1969).

15. His critique of the failure of the mainstream orthodoxy to be bound by the coherence rule is dramatic and incisive. He states:

The student is introduced to a topic reeking with the richness of social life. He is then taken by a little legerdemain through a blur and suddenly finds himself in an eerie world of continuous functions. He watches the functions shift about and, when they have stopped, notes down the coordinates of their points of intersection. He is then taken again through the blur and, behold, he finds himself once more among familiar human faces. The recommendation of this paper is that the subjectivist case against mechanomorphism be based on the ideal that such blurs be removed. (p. 249)

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