

AUSTRIAN JOURNALS: A CRITIQUE OF ROSEN, YEAGER, LABAND AND TOLLISON, AND VEDDER AND GALLAWAY

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There is no secret concerning the reasons Murray N. Rothbard inaugurated the *Review of Austrian Economics* (RAE). It was to promote the Austrian research agenda, and to enhance two-way communication between economists guided by praxeological considerations and their counterparts in the positivist mainstream tradition. Crucial for communication, of course, is publication. But praxeologists were all but frozen out of neoclassical publications. Stated in the introduction to the inaugural issue of the RAE (Rothbard and Block 1987, p. xi):

Articles that simply assume a familiarity on the part of the profession with methodological norms and theoretical developments within the Austrian tradition are unlikely to be published; the profession, by and large, has no such familiarity. Articles that devote substantial space to stating and defending the methodological norms and retracing the theoretical development are also unlikely to be published; they are seen, and correctly so, as unoriginal. Articles whose backgrounds are extensive in absolute terms but brief in relation to the remainder of the article do not constitute a workable compromise; they are rejected on the basis of length. The constraints do not totally preclude the publication of Austrian-oriented articles in mainline journals, but they make such events much more difficult.

A similar vision was adopted by the editors of the *Quarterly Journal of Austrian Economics* (QJAE) (Block, Hoppe, and Salerno 1998, p. iii): “to promote the development and extension of Austrian economics and to promote the analysis of

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¹I speak of the *Review of Austrian Economics* only during the time I was associated with it, 1987–1997.

contemporary issues in the mainstream of economics from an Austrian perspective.” Both the *QJAE* and the *RAE*¹ are now publishing regularly and have been doing so ever since their respective inceptions. These periodicals have featured pathbreaking research from dozens of Austrians, and have managed to attract the contributions of several notable mainline economists such as Leland Yeager, Richard Timberlake, Harold Demsetz, Gordon Tullock, and Bruce Caldwell. To be sure, the goal “to completely displace the crisis-ridden but entrenched positivism (of the mainstream) by a vital and full-blown praxeological paradigm” (p. iii) has not yet been attained, but it is undeniable that the neoclassicals have been put on notice that their previous unchallenged preeminence is no more.

ROSEN

Judging from several published reactions to this state of affairs, the positivists are not at all pleased with the new Austrian kid on the block. First out of the starting blocks in dismay at this Austrian renaissance was Rosen (1997). Ostensibly an inquiry as to whether there are any gains to be made from intellectual trade between Austrian and neoclassical economics, this is instead a frontal attack on the former school of thought, accusing it of academic superficiality on the ground it has failed the “market test.”

Seemingly, there are several areas in which Rosen thinks mainstream practitioners of the dismal science can benefit from praxeological insights; e.g., competition and entrepreneurship (Rosen 1997, p. 149) and information and economic change (p. 150). However, no sooner does he make these overtures but that he takes it all back: “Yet, it is hard to assess exactly what is lost without it [e.g., these supposed Austrian contributions]. The neoclassical approach does not ignore change. Rather, it analyzes change in a more empirically manageable way” (p. 150). The essence of his article can be found in its last section, where he applies “natural selection” and “competition . . . to the market for ideas” (p. 150) and asks: “What is the fact that neoclassical economics has scored higher than Austrian economics on the evolutionary/survival test telling us?” (p. 151).

The answer to this query, it would appear, is that Austrians should cease and desist from research and publication. We have lost the market test, have we not? Surely, we must admit, mainline economists are more numerous than praxeologists. Therefore, why don’t we play fair, abandon Austrianism, fold-up our tents, and attempt to pass muster under neoclassical auspices. He even comes close to asserting that refereed periodicals such as *QJAE* should discontinue publication: “From the Austrian point of view, there is no dearth of intellectual entrepreneurs looking for heretofore unrecognized opportunities

to peddle their wares” (Rosen 1997, p. 151). We should do this because we have long ago worn out our welcome:

The great Austrian economists were recognized as such in their day. The Austrian approach dominated American economics at the turn of the century. They are not forgotten and many of their ideas have been incorporated into the canon. However, others have been forgotten. (p. 151)

In other words, mainline economics has already long since taken unto itself what was true of praxeology, and consigned the rest to the dust bin of history, where it richly belongs. Only spoilsports such as the editors of *QJAE* would continue to promote this school of thought at this late date.

But if anything indicates just how tightly closed-off is the mainline scholarly press in economics to the Austrian School, and thus the continued need for independent journals emanating from this quarter, it is the fact that the *Journal of Economic Perspectives*, the publisher of Rosen (1997), did not call upon a praxeologist to take the other side of this debate, and defend Austrianism against his attack. Instead, they chose Yeager, one of their own,² and an avowed opponent of Austrianism on business cycle theory, subjectivism, methodological individualism, the hundred percent backed dollar, the gold standard, time preference, interest rates, and other public policies and methodologies associated with this school of thought (Yeager 1997).

YEAGER

As a presumed defender of Austrianism against the onslaught against this perspective by Rosen, Yeager can hardly be said to have gotten off on the right foot. He implies that all Austrians follow a “party line,”³ he mistakenly conflates Austrianism and libertarianism, and he sees this school of thought as a “sect” (Yeager, 1997, p. 155). For a person who claims to be a “fellow traveler” (p. 153) of Austrianism, this is all but incomprehensible. How, moreover, is it possible to defend against such a critique when Yeager offers no citations?

²To his credit, Yeager (1997, p. 153) explicitly disavows that he is “a spokesman for the Austrian school.” As well, all throughout his paper, he refers to Austrian economists in the third person.

³Because Yeager takes no marching orders, presumably from Austrian central control, he is “free to reject” Austrian “fondness for methodological brooding, pointless profundities and verbal gymnastics” (Yeager 1997, p. 155). Since, in sharp contrast, I am tightly controlled by the praxeological bosses, this permits me to object to this schoolyard type of name calling. Had Yeager attained the usual scholarly requirements, he would have supplied some examples, or citations, documenting this “brooding,” etc.; certainly, the disinterested reader might have benefitted from some evidence in this regard. What makes matters worse is that Yeager is presumably defending the Austrian side of this debate vis-à-vis Rosen.

In addition, Yeager (1997, p. 155) maintains, again without benefit of citation, that some Austrians have wanted to ban mathematics from economics. There are problems here. First of all, Austrians do not reject mathematics out of hand. Rather, while denying its relevance for economic theory, we accept it for purposes of doing economic history. Second, in the ranks of Austrian economists are included distinguished econometricians and mathematicians (Hoppe 1988). So, it is not a case of “the Austrians distaste for mathematics” (Yeager 1997, p. 156), but rather opposition to *misplaced* mathematics. Third, Austrianism, as a praxeological science, is not an inductive but rather a deductive enterprise. As such, it is akin to mathematics, not the physical sciences (Hoppe 1988, 1992; Selgin 1988; Rothbard 1993; Mises 1966; Herbener 1995). How can we oppose mathematics *per se* if Austrianism is but a branch of this endeavor?

Another gratuitous and undocumented attack by Yeager on Austrianism is the claim that “some Austrians go so far as to suspect an affinity between statism and a passion for statistics” (Yeager 1997, p. 156), as if an interest in the intricacies of statistical theory would incline a scholar toward dirigisme.⁴

Nor can we allow to pass without criticism Yeager’s attempt to conflate the view of Rosen and Mises (p. 157). The latter does not at all use “*a priori*” in an unusual way, in analyzing the essence of economics. For Mises, economics is not at all a positivist science which apes physics and chemistry in applying empirical methodology to economic issues and facts. Rather, it is akin to the discipline of geometry, or pure mathematics, which rely upon the deductive, not the inductive method (Hoppe 1988, 1992; Selgin 1988; Rothbard 1993; Mises 1966; Herbener 1995).

Yeager’s (1997, p. 157) position on the future places him at the extreme Lachmannian–Schakellian (Lachmann 1976) wing of the Austrian distribution: “As for predictions, Austrians take another fact seriously: the economic world is an open rather than closed system and as such has an unknowable future.” Nothing in the future is knowable? Hardly. I know that I shall never become the Queen of England, and that Murray N. Rothbard will not be posthumously awarded the Nobel Prize in economics.

Despite the fact that Yeager is no Austrian, and seems to bear a great enmity to this school of thought and its practitioners with his incessant name calling, his (Yeager, 1997) reply to Rosen (1997) still functions as a good antidote to the latter. Of great value is Yeager’s criticism of Rosen’s analogy between ideas on the one

⁴ Advocacy of government-forced collection of statistics, as is done by the Bureau of Labor Statistics, or the Commerce Department, or coerced census reporting of information not called for in the Constitution, is, of course, entirely a different matter. See on this Rothbard (1997a), Van Creveld (1999).

hand and goods and services on the other. States Yeager (1997, p. 159) of Rosen: “He evidently holds it against the Austrians that they do not pass his market test in the intellectual atmosphere created by members of his own camp, an atmosphere pervaded by narrow yet tacit methodological preaching.” Yeager (p. 161), moreover, is perfectly justified in plaintively asking “Since when, anyway, was the market, even the actual business market, the arbiter of excellence in consumer goods, literature, art, music, science, or scholarship? Since when does the market decide truth and beauty?” Surely rap music sales vastly outstrip those of baroque music, and Puff Daddy is far more popular than Mozart. According to the “market” criterion adumbrated by Rosen, we would be forced to conclude that mere popularity, mere purchase levels, are an unambiguous indicator of beauty. *The National Enquirer* outsells the *Wall Street Journal*; presumably, according to Rosen, there is more truth in the former than in the latter.

But the fact of the matter is that Rosen is committing a category mistake. He is conflating scientific fact, and truth, on the one hand, with profits and sales on the other. He is attributing to markets that which properly belongs to the realm of truth or falsity.

Yeager states:

Scholars should approach each school (and each sect) for what they can learn from it, not as a target of polemics for polemics’ sake and not as a foil for self-congratulation. The neoclassical and Austrian Schools, each stripped of excrescences, are complementary. Aspiring Austrian economists should indeed take the standard Ph.D. courses. (1997, p. 164)

But where are the “excrescences” in praxeology? Where are the “polemics”? Further, if there is as much “complementarity” as Yeager claims, why does he content himself with only the claim that “aspiring Austrian economists should indeed take the standard Ph.D. courses”? Why is this not coupled with the obverse, namely, that aspiring neoclassical economists should take the standard *Austrian* Ph.D. courses? The two-way street advocated by Yeager, curiously, runs in only one direction.

LABAND AND TOLLISON

The major thesis of Laband and Tollison is that there is no warrant for the existence of any academic, scholarly refereed journals devoted to the Austrian perspective. The reason that “so-named Austrian economists” (Laband and Tollison 2000, p. 45)⁵ fail to “publish in the regular journals of economics . . . is that

⁵Presumably, we are to infer from this that there really is not a core Austrian perspective worthy of the appellation “Economics” without the “so named,” and worthy of

their work is flunking an important market test.” In other words, whether real or only “so named,” Austrians are purveying a particularly inferior brand of economics. The mainstream journal editors and referees see through the fallacies⁶ of praxeology, and, not taken in by them, reject submissions from this dubious quarter.

And how do the Austrians react to these eminently justified rebuffs? Do they go back to the drawing board, rethink their misbegotten positions, and return with a higher quality (e.g., more mainstream) product? They do not. Do they realize that “even the best and brightest among us usually fail. To succeed as scientists we must come to grips with rejection and failure to convince others” (Laband and Tollison 2000, p. 45)? Again, unhappily, no. Do these Austrians incorporate the fact that “sometimes the failure is temporary, overcome by redoubled effort to prove one’s point. Sometimes the failure is permanent and the putative contribution must be discarded as either incorrect or insubstantial” (p. 45). Not a bit of it. Very much to the contrary, they engage in “general grumbling”; they indulge in “sour grapes” (p. 45) and “crybabyism” (p. 47). If they had any sense, the Austrians would regroup, and realize that the negative judgment made of them by their intellectual betters is a valid one.⁷ They would keep trying to “convince nonbelievers” on the editorial boards of mainstream economic journals even though “this is difficult.” And why should Austrians do this? Because this is the only way “to make contributions.”

Here, in a nutshell, is Laband and Tollison’s advice to Austrians: knuckle down, shape up, get real, avoid the evils of crybabyism, grumbling, and sour grapes; but above all, never never, never start your own journals. For to do this is to leave the intellectual battlefield. They state (p. 45):

Imagine a young tennis player who complains that he cannot win his matches because the linesmen call too many of his shots out. Sure, the linesmen make mistakes, but does anyone think that the best players are not playing in the major tournaments? It is one thing to win the Austrian Open and quite another to win Wimbledon.

distinction from that of any other school of thought. But were this true, why are Laband and Tollison castigating Austrians for inaugurating journals devoted to this topic? If there are no real Austrians, but only “so-named” ones, why the fuss on the part of Laband and Tollison over a “so-named” Austrian journal? As a nonreal journal, it should hardly give them offense.

⁶But Austrianism is more than merely incorrect. For Laband and Tollison, too, this school of thought is composed of “true believers” (Laband and Tollison 2000, p. 44) and leads a “basically cult like existence” (p. 43).

⁷But not a perfect one. Laband and Tollison (p. 45) concede that “on occasion . . . (the neoclassical) vetting process is subject to type I error.”

In the event, however, the Austrians have eschewed this counsel. While still continuing to devote a part of their time and effort to publishing in mainstream journals (following Rothbard's advice about "two way streets") they have also had the temerity to start up and contribute to refereed periodicals open to their own perspective.

This is neither the time nor the place to take on the major challenge of Laband and Tollison: that all of value in Austrian economics has long since been incorporated by the neoclassical school; that what remains the sole provenance of praxeology is all wrong; that there is nothing both true and original in praxeological economics as currently practiced. Much of the research and publication in this tradition has been devoted to blazing new and important paths (e.g., appraisal, information, fractional-reserve banking, subjectivism, socialism, property rights) and to a panoply of criticism of the mainstream showing that its analysis in great part is either misguided or pernicious.⁸ Instead, I shall critically examine Laband and Tollison's case against the very existence of Austrian scholarly journals.

Let us begin with their tennis analogy. My contention is the Austrians are not poor players of this game, called down for their errors by very accurate albeit imperfect linesmen. Rather, Austrians are engaged in baseball, and don't even make it to first base since tennis has no umpires capable of distinguishing a ball from a strike. While there are indeed two schools of economics, competing with each other in seeking truth in terms of explaining and understanding economic issues, the two are very different enterprises. They have different rules, criteria of success, methodologies, literatures, histories, etc. To ask one to abide by the rules of the other would be to put the former at a great disadvantage.

But this is a two-way street. Yes, Austrians have great difficulty publishing in mainstream journals. However, the same undoubtedly applies when neoclassicals submit articles to praxeological journals. In point of fact, however, it is undoubtedly the case that non-Austrians are represented in praxeological journals to a far greater extent than is the reverse.⁹

⁸If Laband and Tollison wish to enter the lists and debate praxeologists on any of these specifics, they will be following in the path of several eminent neoclassical economists who have already done so. For recent criticisms of Austrians by neoclassicals see Tullock (1988), Demsetz (1979, 1997), Yeager (1987, 1988, 1989, 1994, 1997), Timberlake (1987, 1988, 1989), Caplan (1999), Nozick (1977). For Austrian rejoinders to Tullock, see Salerno (1989); to Demsetz, see Block (1977a, 1995, 2000); to Yeager, see Block (1988), Barnett (1989b) and Salerno (1994b, 1995); to Timberlake, see Rothbard (1988, 1992) and Barnett (1989a); to Caplan, see Block (1999) and Hülsmann (1999); to Nozick (1977), see Block (1980). Unhappily, Laband and Tollison write as if this literature did not exist.

⁹See the literature cited in the previous note.

Another difficulty with the Laband and Tollison position becomes clear when we attempt to generalize. That is, let us search around and see if we can uncover *other* schools of thought beside the Austrian which have set up journals of their own, in order to escape from the judgment of tennis linesmen to whose tender mercies they would otherwise have been consigned. Exhibit A in this regard is surely the Public Choice School. These scholars have had the audaciousness not only to create the journal *Public Choice*, but several others as well, including *Constitutional Political Economy*, *The European Journal of Political Economy*, *The Journal of Public Finance and Public Choice*, and *The Journal of Public Economics*. Public Choicers approach questions of politics from an entirely new and different methodology (economics) compared to what has gone on with this field before their advent. That is, previous to the arrival of Buchanan and Tullock and their numerous colleagues, such issues had long been addressed using the method of political scientists. In order to be logically consistent, Laband and Tollison would have to upbraid the Public Choice School for starting their own journals. Instead of sour graping and “chatting” (Laband and Tollison 2000, p. 6) among themselves,¹⁰ these scholars should have subjected themselves to the righteous judgment of the editors and referees of political science journals.

I mean this as no criticism of Buchanan and Tullock.¹¹ But had they allowed political science journal editors to determine the fate of their school of thought it is entirely likely that it would be far less developed than it is now. For the “tennis linesmen” of political science are no more fair to public choicers than are those of mainstream economics to Austrians.

But this is the mere beginning of the troubles for Laband and Tollison on this ground. It is only the veritable tip of the iceberg. Exhibit B might be the work of Gary Becker. Much of his intellectual contribution was to explore questions hitherto in the domain of sociology.¹² He did so, of course, with the tools of economic analysis, not sociological analysis. Had he submitted his unpublished works to the editors of sociological journals, is there anyone rash enough to believe, as Laband and Tollison imply, that they would have been judged by fair and competent “linesmen”? It is of course impossible to say for

¹⁰In addition to setting up journals, both Austrians and Public Choicers have also conducted annual meetings of their own in competition with the ASSA.

¹¹For criticism of the Public Choice School from an Austrian point of view see Rothbard (1997b) and Block and DiLorenzo (forthcoming).

¹²See in this regard Becker (1957, 1964, 1974a, 1974b, 1975, 1976, 1978a, 1978b, 1980, 1981b, 1984, 1990a, 1990b, 1990c, 1991, 1993, 1996). To modern economic sensibilities, this very large literature (I confine myself only to a sampling of his books, for brevity) sounds like economics, not sociology; but especially when Becker began his career, and even now-days, his work was widely seen as “poaching” on the preserves of sociology.

sure, but it is extremely likely that had Becker followed the path advocated by Laband and Tollison, he would have enjoyed far less publishing success than he did.

But, again, this phenomenon is ubiquitous; it transcends any one individual, such as Becker, or even any one school of thought, such as Public Choice or Austrianism. To begin with, it applies to every single solitary subdiscipline of economics. Consider labor economics, as but one case in point, by consulting the *Journal of Economic Literature* listings for subfields in economics, or by perusing only the titles of the hundreds of actual economics journals (Cabell 1998). Why create the *Journal of Labor Economics* (JOLE), or the *Journal of Labor Research* (JLR) or any of the other prominent mainstream scholarly periodicals specializing in labor economics? According to Laband and Tollison (2000), if we generalize from these authors, the editors, referees and contributors to JOLE, JLR, and their many counterparts should have, instead of inaugurating their own periodicals, tamely submitted to the judgment of the “tennis linesmen” of the *American Economic Review* (AER), the most prominent economics journal of them all. From whence sprang the sheer audacity of JOLE, JLR, etc., in rebelling against the just (albeit imperfect) rule of the even more prestigious AER? In other words, if QJAE is illicit because it is an attempt to evade the just rule of the economics profession as a whole, then this goes, too, for JOLE, JLR, and all the rest.

And it goes in spades. For the journals devoted to labor economics (ditto for international trade, economic development, history of thought, economic history, money, macro, etc.) at least share a common logical positivist methodology with AER. Far less reason, then, for *them* to break away from the just authority of the “tennis linesmen” of the *American Economic Review* than for Gary Becker, Public Choice or Austrianism to seek to escape the rule of those with whom they do not share a methodology.

Again, we have only begun to scratch the surface of the logical implications of Laband and Tollison (2000). For if their message applies to schools of thought within economics, if *none* of these other journals were justified in opening their doors, the implication is that there should only be *one* economics journal, namely the AER. But why should there be *any* economics journals at all? Economics, after all, is but one discipline out of many in the social sciences. Surely, using the “logic” of Laband and Tollison, there were others before economics. The dismal science broke away from these others, presumably, due to the same need for specialization and division of labor which now accounts for the separation between neoclassical and Austrian economics. But if the latter is not justified, then neither is the former. Within social science, journals from the

various disciplines of sociology, political science, history, anthropology, psychology, etc., should all coalesce into one big social science journal. And if the case for a journal articulating the vision of these traditional social science disciplines is thrown into question by Laband and Tollison, how much weaker, even, is the case for specialized journals for the newer social science subdisciplines such as “feminist” studies, “queer” studies, “black” studies, “transgendered” studies, “multicultural” studies, etc.

As a matter of fact, the logical implication of Laband and Tollison is not only that there should be but one economics journal, not only that there should be but one social science journal, but that there should be only one scholarly publication, period. For, why should the chemists be able to escape the proper authority of the physics “tennis line-men”? Why should the biologists be allowed to rebel against their proper lords and masters, the chemists? And why should any of these disciplines, let alone economics, be allowed to kick over the traces initially forged by the *first* discipline, the “Queen of the Sciences,” that is, philosophy. Chemists, too, are just a bunch of whiners, who “chat” amongst themselves, indulge in “sour grapes” and “cult-like” behavior.

A word on Laband and Tollison’s claim that their view represents the “free market” and that the opposite perspective, the one which supports the existence of such journals as the *QJAE* is characterized by “market failure.” This contention must be rejected on several grounds. It comes with particular ill grace from authors who argue for, in effect, only one economics journal (and in the extreme, the publication of only one periodical in total). This is incompatible with the neoclassical economics interpretation of single sellers of items being “monopolists.” On the one hand, the logical implication of Laband and Tollison, as we have seen, is that there should be only one publication; on the other, it is a well-established neoclassical claim that one of practically anything is equivalent to “monopoly,” which, in their lexicon, constitutes a “market failure.” The point is that it is their *own* perspective, not that of the Austrians, which is compatible with market failure. And, in turn, it is the Austrians, not the mainstream economists, who have done more than anyone else to undermine the intellectual credibility of the concept of market failure (Rothbard 1962).

Since when, moreover, is it an example of antimarket activity to set up a new business venture? Surely this is an example of entrepreneurship, something fully recognized at least by Austrians (Kirzner 1973), if not neoclassical economists, as an integral aspect of business enterprise. Presumably, if Laband and Tollison were analyzing not academic publishing but rather ordinary

commerce, they would have stood with the butter interests which tried to prevent the colorization of margarine, with the horse and buggy industry which tried to stifle the early horseless carriage firms with a welter of restrictions. Laband and Tollison are in the tradition of the medieval guilds which long succeeded in preventing competition against themselves.

As a young Austrian economist, I wondered for many years how the academic establishment would respond to Austrian criticisms, when and if they ever condescended to recognize this literature. Although there have indeed been substantive criticisms¹³ penned, a substantial part of the response, exemplified by Laband and Tollison (2000) and Rosen (1997) is the *ad hominem* dismissal of praxeology on the ground that there are far more adherents of the logical positivist paradigm than there are of praxeology.

VEDDER AND GALLAWAY

Vedder and Gallaway (2000) have written a very interesting and informative paper on the issue of whether journals which specialize in Austrian economics are justified. They answer this in the affirmative (p. 13): "Since historically the discrimination against Austrian scholarship has been high, a new Austrian academic infrastructure has emerged. . . . Institutions evolve to meet needs prompted by human action."

Their analysis is problematic, however. One difficulty is that Vedder and Gallaway unnecessarily concede to the very editors they accuse of bias against Austrians a modicum of legitimacy. They state (p. 1): "presumably the rise in the last generation of Austrian economics' market share" in the academic milieu reflects mainly the empirical fact that Austrian concepts have proven useful and relevant in explaining contemporary human action." But if *increases* in Austrian market share stem from the explanatory use and relevance of the contributions of this school, then so does the *level* of its market share arise from this quarter. However, according to Vedder and Gallaway's Figure 1, this market share is pitifully low, barely above 3 percent. I agree with Vedder and Gallaway that this is a roughly accurate estimate of the proportion of economists now practicing who could be characterized as Austrian. But this means, then, that the praxeological share of correct analysis of contemporary human action is also at about this level. And that is a claim I find very much incorrect.

Another problem arises with their statement that the "leading economic journals . . . are mainstream forums" (p. 34). In my view, the leading economic journals are the Austrian ones. To be sure, I have no objection to the main subject of their query, to wit, what is the case for and against praxeologists

¹³See note 8 above.

publishing in the leading *mainstream* journals, only to deprecating the Austrians journals vis-à-vis the *American Economic Review* and its ilk.

A third difficulty arises with regard to their claim that the Austrian is complementary to, rather than rivalrous with, the other free-market schools of thought such as the Chicago, the Public Choice, and the New Institutionalists (p. 35). This impacts, also, their problematic Figure 2.

Imagine a Venn Diagram, with two overlapping circles indicating, respectively, the Austrian views (A) and those of the Chicago, Public Choice and New Institutionalists Schools (B).¹⁴ We further label as X the views which are unique to praxeologists (e.g., which are not shared by the other free-market schools of thought), as Z those perspectives which are unique to the Chicago, Public Choice, and New Institutionalists Schools (e.g., which are rejected by the praxeologists) and as Y those positions which are shared by both camps. What are some examples for each of these subcategories? X contains the praxeological finding that economics is a deductive system, methodological individualism, the understanding of competition as rivalry, not based on the number of participants in a market but rather on whether entry is legal, and alternative or opportunity costs on a subjective basis; on a public policy level, adherence to a gold standard, a 100 percent backed dollar (no fractional-reserve banking), opposition to the concept of market failure, fixed exchange rates, rejection of all antitrust legislation and advocacy of laissez-faire capitalism. In very sharp contrast, Z would include interpersonal comparisons of utility, objective costs, logical positivism, the idea of competition as based on the number of competitors in an industry, acceptance of the logical coherence of indifference curves, and the notion that economic laws must be tested by empirical evidence; on a public policy level, there would be acceptance of at least some antitrust (e.g., antimerger, price fixing provisions), advocacy of fiat currency and fractional-reserve banking, flexible exchange rates, some sort of limited welfare state (e.g., negative income tax, school vouchers, tradeable emissions rights). What are the areas of agreement in Y? That an increase in the money supply (not the “greed rate”) accounts for inflation, the median voter analysis, the importance of markets, freely fluctuating prices, institutions, and equilibrium tendencies; on a public policy level, that rent control, minimum wages, unions, tariffs, central planning, government ownership or regulation of airline prices, nationalization of newspapers, steel mills, and oil companies are disruptive to an economy.

With this introduction, we are ready to tackle the claims of Vedder and Gallaway (2000, p. 36). Can these authors be fellow travelers of “both the

¹⁴Although there are of course differences amongst these three, for present purposes of distinguishing them all from the Austrian perspective, I in effect assume these away.

Austrian and Chicago Schools simultaneously”? Yes, if they completely eschew both X and Z, and embrace Y and Y alone. Then and only then can they be fellow travelers of both. But to the extent they accept X they must reject Z, and of course *vice versa*, since these elements are contrary to one another. Logically Vedder and Gallaway can reject both X and Z, but cannot accept both of them.¹⁵ What if Vedder and Gallaway agree with Y, but also with some parts of X and others of Z. Then they are partially Austrians, partially neoclassicals, and somewhat internally self-contradictory; i.e., they are eclectic.

The next mistake of these authors (p. 37) is their claim that Gordon Tullock when he edited *Public Choice* and James Bennett, the present editor of the *Journal of Labor Research* exhibit no discrimination against publishing in their journals “Austrian ideas.” While I have no doubt this is true of Austrian ideas in category Y (which *also* include neoclassical ideas), I defy anyone to show me the publication of Austrian ideas in category X (which are decidedly *not* compatible with those of the mainstream) in either of these journals.

I entirely agree that Hayek and Kirzner “have been more accommodating of non-Austrian beliefs” than either Mises or Rothbard (p. 37). Hayek has made numerous compromises with doctrine which can be categorized as violative of X, and even Y (Block 1996), while Kirzner (and Mises, too, in this one case) has opposed the total elimination of antitrust (Block 1977b). But none of these economists ever published anything he knew to be untrue, much less with the goal of enhancing his career. Nor can it be denied that the award to Hayek of the Nobel prize in 1974 heightened interest in Austrian economics. However, I date the modern rise of praxeology a decade earlier with the publication of several of Rothbard’s books and articles, particularly Rothbard (1962). In any case, the version of Austrian economics most directly furthered by Hayek’s prize was a watered down interpretation of the praxeology of Mises and Rothbard.¹⁶

I especially appreciate Vedder and Gallaway’s (2000, p. 37) application of the median voter phenomenon to referees of mainstream journals to explain why Austrians have an even more serious obstacle placed in their path for obtaining an acceptance than otherwise would have been supposed.

Consider now the case posed by Vedder and Gallaway (p. 38) of the author of an Austrian paper on the Mises–Hayek trade cycle theory, whose editor

¹⁵If Vedder and Gallaway sign on to both X and Y, they are Austrians, pure and simple; if Y and Z, then they are members of the Chicago, Public Choice, or New Institutional Schools.

¹⁶The “dehomogenization literature” blazed by Salerno is of relevance here. On this see Salerno (1990, 1993, 1994a) and Block and Garschina (1996).

demands econometric evidence. Would it be a violation of Austrian principle to comply? In one sense, of course it is, since empirical work is considered by praxeologists to illustrate, not test, apodictic theory. That is, regression analysis is a legitimate part of Austrian economic history, but this does not hold true for theory.¹⁷ However, it is by no means necessary to advise the editor about this complication. All the author need do, if he judges it worthwhile, is to provide what he considers an empirical illustration of the principle. If the editor wants to interpret this as evidence of relevance to the Austrian business cycle theory, that is his business. However, if this editor demands that the Austrian author characterize this illustration as evidence, then all bets are off. We have now moved into the thickets of area Z. If the praxeologist publishes on this basis, he is indeed selling out in an unprincipled manner.

I also have problems with Vedder and Gallaway's (p. 39) version of the publication process of their project "Out of Work." They state "Rothbard (1983) confided in us that he did not like the double-blind refereeing system, but was being coerced into following it to demonstrate the presence of scholarly standards." This is entirely incompatible with my own recollections of this issue. As coeditor with Murray Rothbard of the *Review of Austrian Economics*, I can assure the reader that Murray took on the task of inaugurating this journal on a double blind refereeing basis completely voluntarily. Those who know him know full well that, apart from the state of course, Murray was never "coerced" into doing *anything* in his entire adult life. Last, they imply (p. 41) that regression analysis is "mainstream" methodology, i.e., a part of Z not, Y. But this is not necessarily so. It is true if and only if the econometrics is used to *test* praxeological discoveries. But regressions can also be used to *illustrate* theory, in which case they fall into category Y, not Z, which is claimed by both schools of thought, not by the neoclassicals alone.

REFERENCES

- Barnett II, William. 1989a. "Comment on Professor Timberlake's Squared Rule for the Equilibrium Value for the Marginal Utility of Money." *Review of Austrian Economics* 3:151-57.
- . 1989b. "Subjective Cost Revisited." *Review of Austrian Economics* 3:137-38.
- Becker, Gary. 1957. *The Economics of Discrimination*. Chicago: University of Chicago Press.
- . 1964. *Human Capital*. New York: National Bureau of Economic Research.
- . 1974a. *A Theory of Social Interactions*. New York: National Bureau of Economic Research.
- . 1974b. *Essays in the Economics of Crime and Punishment*. New York: National Bureau of Economic Research.
- . 1975. *A Theory of Marriage*. Chicago: Center for the Study of the Economy and the State, University of Chicago.

- . 1976. *The Economic Approach to Human Behavior*. Chicago: University of Chicago Press.
- . 1978a. *The Effect of the State on the Family*. Chicago: Center for the Study of the Economy and the State, University of Chicago.
- . 1978b. *A Positive Theory of the Redistribution of Income*. Chicago: Center for the Study of the Economy and the State, University of Chicago.
- . 1980. *Altruism in the Family and Selfishness in the Market*. London: London School of Economics.
- . 1981a. *A Theory of Political Behavior*. Toronto: University of Toronto.
- . 1981b. *A Treatise on the Family*. Cambridge, Mass.: Harvard University Press.
- . 1984. *Human Capital and the Rise and Fall of Families*. Chicago: Economics Research Center.
- . 1990a. *Human Capital, Fertility, and Economic Growth*. Cambridge, Mass.: National Bureau of Economic Research.
- . 1990b. *An Empirical Analysis of Cigarette Addiction*. Chicago: Center for the Study of the Economy and the State, University of Chicago.
- . 1990c. *A Simple Theory of Advertising as a Good*. Chicago: Center for the Study of the Economy and the State, University of Chicago.
- . 1993. *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education*. Chicago: University of Chicago Press.
- . 1996. *Accounting for Tastes*. Cambridge, Mass.: Harvard University Press.
- Block, Walter. 1977a. "Coase and Demsetz on Private Property Rights." *Journal of Libertarian Studies* 1(2): 111–15.
- . 1977b. "Austrian Monopoly Theory: A Critique." *Journal of Libertarian Studies* 1(4): 271–79.
- . 1980. "On Robert Nozick's 'On Austrian Methodology'." *Inquiry* 23(4): 397–444.
- Block, Walter. 1988. "On Yeager's 'Why Subjectivism?'" *Review of Austrian Economics* 2:199–208.
- . 1995. "Ethics, Efficiency, Coasean Property Rights and Psychic Income: A Reply to Harold Demsetz." *Review of Austrian Economics* 8(2): 61–126.
- . 1996. "Hayek's Road to Serfdom." *Journal of Libertarian Studies* 12(2): 327–50.
- . 1999. "Austrian Theorizing: Recalling the Foundations." *Quarterly Journal of Austrian Economics* 2(4): 21–40.
- . 2000. "Private Property Rights, Erroneous Interpretations, Morality and Economics: Reply to Demsetz." *Quarterly Journal of Austrian Economics* 3(1): 63–78.
- Block, Walter, Hans-Hermann Hoppe, and Joseph T. Salerno 1998. "Editorial." *Quarterly Journal of Austrian Economics* 1(1): iii–iv.
- Block, Walter, and Kenneth M. Garschina. 1996. "Hayek, Business Cycles and Fractional Reserve Banking: Continuing the De-Homogenization Process." *Review of Austrian Economics* 9(1): 77–94.

- Block, Walter, and Thomas DiLorenzo. Forthcoming. "Constitutional Economics and the Calculus of Consent." *Journal of Libertarian Studies*.
- Cabell, David W.E., ed. 1999. *Cabell's Directory of Publishing Opportunities in Accounting, Economics and Finance*. 7th ed. Beaumont, Texas: Cabell Publishing.
- Caplan, Bryan. 1999. "The Austrian Search for Realistic Foundations." *Southern Economic Journal* 65(4): 823–38.
- Demsetz, Harold. 1979. "Ethics and Efficiency in Property Rights Systems." In *Time, Uncertainty and Disequilibrium: Explorations of Austrian Themes*. Mario Rizzo, ed. Lexington Mass.: D.C.Heath.
- Demsetz, Harold. 1997. "Block's Erroneous Interpretations." *Review of Austrian Economics* 10(2): 101–09.
- Herbener, Jeffrey M. 1995. "Calculation and the Question of Arithmetic." *Review of Austrian Economics* 9(1): 151–64.
- Hoppe, Hans-Hermann. 1988. *Praxeology and Economic Science*. Auburn, Ala.: Mises Institute.
- Hoppe, Hans-Hermann. 1992. "On Praxeology and the Praxeological Foundation of Epistemology and Ethics." In *The Meaning of Ludwig von Mises*. Jeffrey M. Herbener, ed. Boston: Dordrecht.
- Hülsmann, Jörg Guido. 1999. "Economic Science and Neoclassicism." *Quarterly Journal of Austrian Economics* 2(4): 3–20.
- Kirzner, Israel M. 1973. *Competition and Entrepreneurship*. Chicago: University of Chicago Press.
- Laband, David N., and Robert D. Tollison. 2000. "On Scientific Appraisal." *Quarterly Journal of Austrian Economics* 3(1): 43–48.
- Lachmann, Ludwig. 1976. "From Mises to Shackle: An Essay on Austrian Economics and the Kaleidic Society." *Journal of Economic Literature* 14(10): 54–62.
- Mises, Ludwig von. [1944] 1966. *Human Action*. 3rd. Rev. Chicago: Regnery.
- Nozick, Robert. 1977. "On Austrian Methodology." *Synthese* 36:353–92.
- Rosen, Sherwin. 1997. "Austrian and Neoclassical Economics: Any Gains from Trade?" *Journal of Economic Perspectives* 11(4):139–52.
- Rothbard, Murray N., and Walter Block. 1987. "Introductory Editorial." *Review of Austrian Economics* 1:xi–xiii.
- Rothbard, Murray N. 1988. "Timberlake on the Austrian Theory of Money: A Comment." *Review of Austrian Economics* 2:179–87.
- . 1992. "Aurophobia: or, Free Banking on What Standard? A Review of *Gold, Greenbacks and the Constitution*, by Richard M. Timberlake." *Review of Austrian Economics* 6(1): 97–108.
- . [1962] 1993. *Man, Economy, and State*. Auburn, Ala.: Ludwig von Mises Institute.
- . 1997a. "Statistics: Achilles' Heel of Government." *The Logic of Action Two: Applications and Criticism from the Austrian School*. Cheltenham, U.K.: Edward Elgar. Pp. 180–84.
- . 1997b. "Buchanan and Tullock's *The Calculus of Consent*." *The Logic of Action One: Applications and Criticism from the Austrian School*. Cheltenham, U.K.: Edward Elgar. Pp. 269–74.

- Salerno, Joseph T. 1989. "Comment on Tullock's 'Why Austrians Are Wrong About Depressions.'" *Review of Austrian Economics* 3:141–45.
- . 1990. "Ludwig von Mises as Social Rationalist." *Review of Austrian Economics* 4:26–54.
- . 1993. "Mises and Hayek Dehomogenized." *Review of Austrian Economics* 6(2): 113–46.
- . 1994a. "Ludwig von Mises's Monetary Theory in Light of Modern Monetary Thought." *Review of Austrian Economics* 8(1): 71–115.
- . 1994b. "Reply to Leland Yeager." *Review of Austrian Economics* 7(2): 111–25.
- . 1995. "A Final Word: Calculation, Knowledge and Appraisalment." *Review of Austrian Economics* 9(1): 141–42.
- Selgin, George A. 1988. "Praxeology and Understanding: An Analysis of the Controversy in Austrian Economics." *Review of Austrian Economics* 2:19–58.
- Timberlake, Jr., Richard H. 1987. "A Critique of Monetarist and Austrian Doctrines on the Utility and Value of Money." *Review of Austrian Economics* 1:81–96.
- . 1988. "Reply to Comment by Murray N. Rothbard." *Review of Austrian Economics* 2:189–97.
- . 1989. "Marginal Utility Equilibrium between Money and Goods: A Reply to Professor Barnett's Criticism." *Review of Austrian Economics* 3:159–61.
- Tullock, Gordon. 1988. "Why the Austrians are Wrong About Depressions." *Review of Austrian Economics* 2:73–78.
- van Creveld, Martin. 1999. *The Rise and Decline of the State*. Cambridge: Cambridge University Press.
- Vedder, Richard, and Lowell Gallaway. 2000. "The Austrian Market Share in the Marketplace for Ideas, 1871–2025." *Quarterly Journal of Austrian Economics* 3(1): 33–42.
- Yeager, Leland B. 1987. "Why Subjectivism?" *Review of Austrian Economics* 1:5–31.
- . 1988. "Reply to Comment by Walter Block." *Review of Austrian Economics* 2:209–10.
- . 1989. "Reply to Comment by William Barnett II." *Review of Austrian Economics* 3:139–40.
- . 1994. "Mises and Hayek on Calculation and Knowledge." *Review of Austrian Economics* 7(2): 93–109.
- . 1995. "Rejoinder: Salerno on Calculation, Knowledge, and Appraisalment." *Review of Austrian Economics* 9(1): 137–41.
- . 1997. "Calculation and Knowledge: Let's Write *Finis*." *Review of Austrian Economics* 10(1): 133–36.
- . 1997. "Austrian Economics, Neoclassicism, and the Market Test." *Journal of Economic Perspectives* 11(4): 153–63.