On Secondhandism and Scientific Appraisal

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There is a quiet but nonetheless significant and persistent disagreement among members of the scholarly community of economists regarding appraisal of scientific merit. On one side are those who believe, implicitly or explicitly, that there is a competitive market for scientific ideas in economics in which major contributions (and the individuals who make them) are identified relatively efficiently. In contrast to what we might refer to as the free-market side of the discipline are those who believe that the prevailing process of scientific appraisal is quite inefficient and therefore unreliable. That is, the process is characterized by market failure.

The ongoing skirmishes between these two camps involve two quite distinct aspects of scholarship: (1) the imperative, as it were, for an individual to publish in mainstream economics journals to be taken seriously by the profession as a whole, and (2) the use of citations as a measure of the impact of a contribution to the scholarly literature of economics. We address these issues in turn.

Publishing in the "Mainstream" Economics Journals

To be relevant in any meaningful sense to a community of scholars, scientific truth cannot simply be self-evident. It is not enough for the individual qua scientist to maintain that he has been enlightened and knows the truth. The community of scholars must be convinced of the veracity of that truth. The truth is not relevant if it is not a shared truth—the smaller the set of true believers and the greater the group's tendency to speak only among themselves, the more likely the group is to be regarded by the rest of the scientific community as merely a cult.[1]

Convincing other scholars that your scientific contribution is substantial may indeed be difficult, especially if that contribution is at odds with prevailing orthodoxy.

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[1]This is a difficulty that has plagued, among others, Austrian economists—they win no converts by ignoring their fellow scholars and non-scholars who happen to not share their world view in favor of preaching to the choir, a point made by Rosen (1997).

The Quarterly Journal of Austrian Economics vol. 3, no. 1 (Spring 2000): 43–48

However, it is axiomatic that you cannot convince others if you do not communicate with them. You may not convince others even when you do communicate with them, but at least you've got a fighting chance. Like it or not, professional communication in economics occurs primarily through our mainstream journals.

One of the arguments made by those who believe that the journal publication process in economics is characterized by market failure is that the agency problem with respect to journal editors and referees is substantial. Yeager (1997, p. 161) argues this point:

> The subscriber to journals has an influence more attenuated and more subject to manipulation by others than the influence of the consumer of ordinary goods and services. Editors and referees have scope for heeding fads and cliquish and personal considerations. They are not risking their own money.

As for tendencies of journal editors and referees to heed fads or to permit personal considerations to influence their professional judgment and behavior, no more or less can be said for retailers in product markets. Both are aptly characterized as middlemen in the production process. A furniture manufacturer produces a new chair. He tries to convince retailers to offer the chair to final customers. The retailers' decision may be affected by fad or personal considerations. In the academic market, new theories are produced by scholars who try to convince journal editors and referees to offer their product to final customers. We concede that middlemen in product markets have their own money at risk, which is not strictly true of journal editors and referees. However, the fact that no cash changes hands or is directly at risk in academic markets does not imply that the middlemen have nothing at stake. Professional reputations are at stake. Most academic scholars agree that money per se is not the only consideration that motivates their efforts. We submit that the desire to develop and maintain, if not enhance, one's professional reputation serves as strong motivation for most scientists. With specific regard to journal editors, the desire to enhance professional wealth (in the form of collegial esteem) motivates them to be diligent retailers of scientific products. Thus, the differential attenuation of consumer influence on producers in the academic market that Professor Yeager ascribes to the personal quirks and passing fancies of the middlemen seems to us to be quite doubtful.[2]

The notion that journal editors are cliquish, in the sense that they exhibit favoritism with respect to the papers they (don't) publish has long been a topic of private conversation among economists and, on occasion, commentary in the professional literature (Gerrity and McKenzie 1978; Laband 1985). The problem with this perception is that it is not supported by the facts. Although journal editors occasionally publish subpar papers authored by colleagues and former graduate students, on balance their use of professional connections enables them to

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[2]We note that Tollison has served for ten years as a joint senior editor of Public Choice.

identify and capture relatively high-impact papers for publication (Laband and Piette 1994; Laband and Smith 1995). This behavior is consistent with a well-functioning competitive model of the journal market and inconsistent with a market-failure model.

It seems to us that the putative shortcomings of journal editors and referees likely reflect the sour grapes of relatively unsuccessful contributors to those journals. Again, the facts are quite well-known: most of the scholarly submissions to the mainstream journals of economics are rejected for publication, including, on occasion, very good ones (Gans and Shepherd 1994). This means nothing more than that the vetting process is subject to type I error. Anyone who has had one or more contributions published by our top mainstream journals almost certainly will confess to having had many more papers rejected for publication by these journals. To make contributions we must convince nonbelievers. This is difficult; 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. 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. The point is, we all understand that rejection is a critical element of the process of advancing economic science. Individuals who are better able to deal with seemingly personal rejection and who can move beyond the disappointment of being informed that their ideas are not as relevant as they would like to believe are likely to be more successful contributors to economic science than individuals who have trouble accepting such rejection. The latter are more likely to react by finding and clinging to others who are willing to reinforce each other's world view, no matter how removed those views may be from the canons held by the scientific community at large. Not surprisingly, the members of such a group likely will believe that their world view is the correct one and that they are being discriminated against by journal editors and referees.

This brings us to the general grumbling, reviewed in Yeager (1997), about why so few so-named Austrian economists publish in the regular journals of economics. What can one say but that their work is flunking an important market test? Criticizing the journals and other icons (the favorite Austrian whipping boy is a version of neoclassical economics that is about 50 years old) strikes us as an attempt to change the subject, become more isolated, dated, and introverted, and forge a basically cult-like existence, in which, like Alice, up is down and down is up, so that failing a market test is really passing it! Who cares about the American Economic Review? We will just chat among ourselves. Or so goes what passes for Austrian economics in the literature that Professor Yeager dotes on.

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.

CITATIONS AS A MEASURE OF SCIENTIFIC WORTH

Among academic economists there seems to be widespread acceptance of citations as a useful measure (with acknowledged imperfections) of the impact of a contribution to our scholarly literature. There are citations-based rankings of individual economists, economics departments, and economics journals. Citations are used, directly or indirectly, in the determination of fitness for raises, promotions, and other forms of academic recognition by a number of economics departments throughout the United States.

It has been argued by several scholars (including ourselves) that citations constitute an academic analog to dollar voting in private goods markets. However, the analogy is not perfect. Dollars spent on a product reflect the consumer's ex ante forecast that the item purchased will produce value in excess of the purchase price. This forecast may or may not be validated ex post. In this sense, dollar votes are an imprecise measure of value. More accurately, they reflect minimum expected value. Citations, by contrast, clearly reveal that the academic consumer received value from the product being cited, irrespective of whether the citation was positive or negative. This is because the citations are issued only after purchase and consumption of the product. Thus, a case can be made that the academic market conveys product information even more accurately through citations than do markets for goods and services using dollar voting.

However, not everyone is comfortable with market evaluations of scholarly contributions. For example, Yeager (1997) argues that relying on markets amounts to riding "piggyback on the appraisals of other people." We are puzzled by this indictment. The problem as we see it is that the alternative to relying on markets to assign value to scientific contributions is that we must rely on the ostensibly firsthand knowledge of some central authority, such as Yeager. We confess to just a bit of trepidation in this regard, on two counts. First, the central authority's knowledge of the true scientific value of every prospective contribution to economic science simply must be frail, at best. Second, personal quirks, fancies, weaknesses for fads, and personal tastes render the judgment of the central authority suspect. Are any of us less subject to these same failings?

To drive the relevance of our concerns home, let's talk about a practical problem that characterizes the scientific community. Department heads are required periodically to evaluate the merit of faculty members' scholarly contributions.[3] How efficiently are they able to reach their conclusions through firsthand review and contemplation? They will be mostly ignorant about relevant theoretical, empirical, and technical aspects of many, if not most, of the contributions they read. Start with the pervasive professional ignorance of even the most knowledgeable department head, mix in the press of other administrative and academic responsibilities he faces, season with a three-week deadline for turning in their

[3]Laband was department head at Auburn University (1994–99); Tollison was department head at Texas A&M University (1973–76).

evaluations of faculty performance, and see how palatable the performance evaluations are to faculty scholars. There simply is little-to-no hope that department heads, through firsthand inspection, can evaluate efficiently the scientific contributions of their faculty.

It is much more likely that firsthand inspection by journal editors and reviewers who are relatively more familiar with the subject matter in question will place an appropriate value on the prospective scholarly contributions. The values will be reflected by the quality of journal that publishes each contribution. The fundamental question is whether the error attributable (for whatever reason) to relatively more knowledgeable appraisers with a reputational stake in the outcomes is greater or less than the error attributable to relatively less knowledgeable appraisers with no stake in the outcome. A simple experiment would clearly indicate empirically where the members of the scientific community expect the greater efficiency in appraisal. Give each member of the community a choice of having the value of his scientific contributions determined by his current department head's personal inspection or by a simple count of articles (or pages) published in scientific publications, adjusted for quality. This is a clear choice between market and nonmarket valuation of scientific contributions.

We also note that firsthandism is far more subject to interest group politics and rent seeking in a departmental setting than is secondhandism. The general idea is that those who spend more time in the department head's office receive raises that bear little relation to their annual productivity. These types of behaviors do not impact academic rewards under a secondhand (market-based) approach.[4]

All this likely will be taken as knocking Austrian economics, but that is not the point. The point is not first- or second-handism; the point is crybabyism about market tests. Austrian economists can and should be held to account by tests that are not of their own devising. This is, of course, pretty much the point Rosen (1997) was making.

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[4]This is a testable hypothesis. Consult the university budget in your library. Regress changes in pay as a function of age, number of committees served on, teaching load, and quality-weighted publications. In a rent-seeking department raises will bear no relation to productivity, especially publications. The omitted variable, time spent in the chair's office, is difficult to quantify, but everyone knows who wears these scarlet letters—"rent seeker."

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