

THE TACTICS OF SECONDHANDISM

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MARKET TESTS

In this *Journal* of Spring 2000, Robert Tollison joins David Laband in reiterating a stretched conception of market test. Laband and Tollison recommend grading academic performance by the sorts of statistics that Laband compiles, which involve article and page counts, impressions of journal quality, and citations. As I said in the 1997 article that Laband and Tollison attack, not even the actual commercial market is a test of truth and beauty or excellence. Granted, if the quantity produced of some good or service finds willing buyers at a price at least covering all costs, that fact implies that resources have not been diverted from alternative outputs that consumers would have valued more highly. Losses are a retrospective sign of waste (apart from a quasi-exception for business owners who derive satisfaction from using their own wealth even in money-losing ways). Such a market test exerts healthy discipline.

Furthermore, social cooperation through the market and in other ways itself has moral value. The market method of organizing economic activity is indeed better, by the standard of human happiness, than alternative methods. But financial success in some broad or narrow market niche has no deeper philosophical significance; in itself, it is no further sign of excellence or virtue. (The lesson of Hayek 1960, chapter 6, is well worth taking to heart.) Market success does not prove that the tastes catered to, the goods and services provided, or the providers themselves are admirable. Facts alone do not yield appraisals (you can't get an "ought" from an "is").

"The market" is a metaphor. It makes no appraisals. "Choices are made only by humans rather than by personified abstractions such as 'the market'" (James M. Buchanan 1995, quoted by Lee 1996, p. 787; here and in his article of 2000, Lee makes the point eloquently). Overreaching claims for the market,

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especially as a transpersonal arbiter of truth, decency, and excellence, tend to discredit the valid and quite different case for a free society. (On backlash from exaggerated claims of market perfection, compare Heyne 2000, first full paragraph of p. 138.) He is a poor champion of the market system who cannot defend it as it really is, “warts and all.” It is sad to see public understanding of the case for the market undercut by the market’s would-be friends.

Laband and Tollison invite such backlash, unintentionally. They identify their own position on “secondhandism and scientific appraisal” with “the free-market side of the discipline”; they impute belief in “market failure” to their critics (2000, p. 43). The market system, far from being a substitute for good judgment and morality, presupposes morality. Yet secondhandism has morally questionable aspects in some of its applications (cf. McCloskey 1992, commenting on Laband and Taylor 1992).

If the fairly literal test of the commercial market has limited (though great) significance, even less significant is its supposed analogue in the metaphorical market for science and scholarship. There, truth, not marketability, is the goal—and truth as such, not different, incompatible brands of truth for different consumers. (In the commercial market, by contrast, businesses do cater to widely divergent tastes and do, appropriately, satisfy even demands for inexpensive goods of relatively low quality.) To speak of truth is not to traffic in metaphysics about Truth with a capital T. I mean merely that scientific endeavor is the pursuit of propositions of generality and depth corresponding to the way things actually are (to borrow words from Peter Bauer and George Will separately).

TRUTH AND GAMES

Scientific or scholarly or academic life has at least two strands. First is trying to find and communicate truth or knowledge. Second is the academic game itself—the pursuit of prestige, admiration, and money. Self-promotion and gamesmanship enter in. Of course, the two aspects of academic life overlap. Even someone overridingly concerned with truth desires admiration, wants to deserve it, and cares about its sources and reasons. Furthermore, hope for fame and fortune can indeed be a strong and respectable incentive to the pursuit of scientific truth (as well as a temptation to politicking and the like).

Still, how closely the two aspects of scientific activity correspond is affected by the tone and policies prevailing in the academic world. The preachings of Laband and Tollison, if heeded, would impair the correspondence and increase tension between the two aspects.

“The truth is not relevant if it is not a shared truth,” they write (Laband and Tollison 2000, p. 43). I am not sure just what they mean by “relevant” or in how

broad or how narrow a context they apply their remark. Perhaps they mean relevance to scoring well in the academic game. Their remark does sound like relativism. Yet reality is what it is, regardless of how many or how few people share a correct perception of it. Being influential or enjoying prestige may sometimes carry a presumption that one's ideas are right, but it is not the same as their actually being right.

Sharing truth—communicating ideas—is important, of course. Curiously, Laband and Tollison seem to value communication only in a narrow “market” associated with a notion of prestigious journals. But how narrow or how broad a market properly “counts”—how small or large a set of persons addressed? No market answers that question by itself. Does the appropriate market include all employers and potential employers of economists or all actual and potential consumers of economic information? Or is it a much narrower set of appraisers, people inclined to receive and transmit bandwagon effects relating to the supposed frontiers of the discipline? More comes later about questions like these.

STANDARDS AND ERSATZ STANDARDS

I want to forestall misinterpretation. If we had to rank economists of the past, if all copies of their writings and of others' discussions of their writings had been irrecoverably lost, if no information about them survived other than statistics of the kinds that Laband compiles, and if we waive the question of what purpose a ranking under such circumstances might serve, then I do suppose that consulting those statistics would be more plausible than any alternative that comes to mind. Saying so is not much of a concession. Acting on a hunch with some slight basis is more plausible than acting at random. Actually, we do *not* have to rank economists under such circumstances and by such a method.

Two other points, which are not even *semiconcessions*, already appeared in my 1997 article.¹ First, some standards must apply in science and scholarship; not all scribblings can command equal respect and attention. Partly in unavoidable consequence, unfashionable ideas face an uphill battle. Second, life requires much reliance on secondhand knowledge. Wholly firsthand appraisals (as of academic candidates' qualifications, accomplishments, and characters) are scarcely possible; often one's own direct knowledge must be supplemented by the judgments of other people.

¹On these and other matters, I am struck by how little attention Laband and Tollison pay to what I actually wrote in the article under attack and to the writings I cited. My and other observers' reasons for concern about secondhandism and groupthink may perhaps be mistaken, but they are relevant to the issues under discussion.

Laband and Tollison's ersatz standards partly crowd out sounder ones. Far from being apologetic, Laband and Tollison make an actual virtue of exaggerated secondhandism and their stretched conception of market test. Such thinking and attitudes, to the extent that they have influence, worsen the defects of the already weak analogy between the academic quasi-market and the real market. Academe is not immune from fads and bandwagon effects such as occur in many areas of life, for example, the adulation of celebrities. Analogues of the Keynesian beauty contest appear in economic research and styles of exposition under pressures to do not so much what the individual economist thinks best as what is thought to win acclaim.² Such tendencies tilt the playing field more steeply against unfashionable ideas.

Laband and Tollison make much of citations as indicators of influence or fame.³ Admiring influence so registered, even independently of substance, is analogous to admiring the political "realism" justly attacked by Clarence Philbrook (1953)—admiring perceived influence on policy even when obtained by the advisor's compromising with his own honest judgments. Citations properly serve any of several purposes: They steer the reader to facts and arguments supporting the author's points or to supplementary discussions. They give credit to other authors for ideas or findings or particularly apt formulations. When one researcher is criticizing another's ideas or results, citations give the reader a chance to check the attacked work and see whether it is being dealt with fairly. Less admirably, citations may be used as moves in the academic game—to borrow the prestige of the other writers cited, to signal that one's own work is à la mode, on the supposed frontier of the discipline, or to signal familiarity with recondite sources or areas of knowledge. If an author expects citations to be put to the uses of Laband and Tollison, he might even give or withhold them for that reason. The parasitic use of citations can corrupt their primary use.

Laband and Tollison even claim more for citations as they use them than for the actual market (2000, p. 46):

²Brown (1948; 1950, pp. 51–53, 63–64, provides an early and eloquent diagnosis of fadism in economics. More recently, macroeconomics provides glaring examples. The impact of "big players" has been analyzed chiefly in actual markets, but the analysis can be extended to fads and bandwagon effects in other activities. "A Big Player is anyone who habitually exercises discretionary power to influence the market while himself remaining wholly or largely immune from the discipline of profit and loss" (Koppl and Yeager 1996, p. 368; also see works cited there, as well as Butos and Koppl 1995).

³Or even of notoriety, as noted in the text below. Ronald Coase remarked many years ago (in conversation, so I quote only from memory): "In the academic game, the important thing is to have been heard of. People may forget in just what connection they heard of you, but they're likely to remember whether they heard of you. It is better to have been heard of for murdering your wife than not to have been heard of at all."

[D]ollar 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 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.

The terminology and the whole analogy are strained; the claim is bizarre. I know someone whose idea of economic research—as I have told him—is to ransack the literature for passages that express or can be interpreted as expressing fallacies, then triumphantly to pounce on and demolish those fallacies. According to the Laband and Tollison test, even perpetrators of crude fallacies, far from deserving scorn for cluttering up the literature, deserve the positive points that their citations bring them; for the fallacy-hunters have “received value” from the works cited.

Laband and Tollison make excuses for assessing people by Labandian numbers. Some such method is a practical necessity. It possesses objectivity (or so Laband and Tollison seem to suggest on p. 47). Yet personal judgment necessarily enters into constructing the numerical indexes. Conformably with their secondhand nature, the indexes fail to show who judged whose work up or down and for what reasons. Furthermore, publication indexes cannot realistically be the sole measure of academic performance. Professors have other duties, even teaching; and subjective judgment unavoidably enters into assessing and weighting various kinds of performance.

In Laband and Tollison’s view, “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” (p. 46). Never mind the insinuation that I aspire to the role of central authority. Notice again the notion that “markets,” impersonal markets, make judgments and that some mechanism or statistical process should “assign value to scientific contributions.” Yet such value does not exist in the abstract. A researcher learns from the actual substance of his colleagues’ work, not from mere summary numbers pertaining to journal quality and citations. Writings have value for the persons who use the reported facts and ideas, such as other scientists, engineers and technicians, consumers who ultimately benefit from technological progress, and citizens in general who benefit from progress in economic knowledge (to the extent that such knowledge is actually heeded in policymaking).

In grading academics for appointments, promotions, and so forth, the alternative to Laband’s approach is not reliance on some supposed central

authority, such as me. No, the alternative is that the decisionmakers and their informed consultants frankly lay out their own judgments, for which they take responsibility, and the reasons for them. Let them not hide behind some sort of statistical precipitate of the anonymous judgments of other people. Let these appraisers discuss their tentative judgments with one another and possibly revise them. An academic department might name a committee to actually read candidates' writings, perhaps seeking supplementary information from outside, and to report its members' assessments and reasons to the broader decisionmaking group.

FURTHER WORRIES

Economists should understand that people, including academics, respond to incentives and that inappropriate incentives can bring unintended consequences. Responses to "success indicators" in the Soviet planned economy provided examples. As caricatured in the humor magazine *Krokodil*, if a nail factory's output was measured in number of units, the factory would produce very many tiny nails; but if total weight counted instead, it would produce few but huge nails. (Compare Tullock 1965, chapter 23, on how bureaucrats react to attempts to measure their performances numerically.) Standardized tests of school children reportedly elicit "teaching to test"; certain measures of performance steer the attention of the police to violators easy to catch.

The individually sensible response of young professors under Laband-and-Tollison-type pressures may well be to toil away in some prevalent fad on one of the supposed frontiers of the discipline, which may well involve work addressed to some small in-group, resistant to informed evaluation by outsiders and enjoying scant wider relevance—all in hopes of being prestigiously published. (There is no necessary contradiction in identifying both faddism and narrow specialization. Numerous small modish topics may exist, as well as methodological, rhetorical, and stylistic fads infecting many specialties at once.)

It is perverse to push academics lacking a comparative advantage in modish work to waste their energies on it anyway. Why prod them to write articles in which few people are really interested (except perhaps as a basis for Brownie points or as inputs to more such work by other similarly motivated academics)?⁴ They might make more solid contributions in other ways; the principle of opportunity cost applies even in academe. Even within an academic department, diversity of talents and specializations has value.

⁴However the points are calculated, only x percent of candidates can wind up in the top x percent; yet much effort and talent may be misdirected into trying to wind up there anyway. Economists should understand the differences between a race for positional values and the creation of values of other kinds.

Let's face it: few economists are capable of frequently finding important new knowledge. At the same time, widespread ignorance prevails about the core of economics, the very logic of a market economy. Long-exploded errors persist among policymakers and the general public. The quasi-market works less well for knowledge in economics than for knowledge in the natural sciences (especially than for industrially applicable, as distinct from politically applicable, scientific knowledge). Even economists far apart on the ideological spectrum do agree on important issues about which the general public and even noneconomist intellectuals are ignorant.

If economics has much of value to teach, the persistence of ignorance and error over the decades and centuries suggests a lapse of communication worth trying to remedy. Room remains for devising improved ways to make economic principles clear and to communicate them widely. Why should such efforts be disdained? Proficiency in some advanced technique or work on the frontier of some narrow specialty may indeed be valuable, but it is no proof in itself of understanding the very basics of economics. (I have encountered a few economists who constitute examples of this point.)

These thoughts make me wonder how consistent Laband and Tollison are in their faith in the market test. Are they prepared to rank publications by their circulation numbers? Or, as in some circles, do skillfully written contents and wide readership affect appraisals negatively?

As for appraising persons, it is one thing to make appraisals responsibly when choices must be made, as among candidates for employment, promotion, and professional honors. It is another thing to make appraisals in a vacuum, out of the context of necessity—secondhand appraisals that may even be abused for thirdhand appraisals. Turning academic economics into its own subject matter is narcissism. I wonder whether the apparent popularity of articles ranking departments and journals and persons traces to their appeal as material for gossip, like the appeal of tabloids sold at supermarket checkouts. (Similar thoughts come to mind about some strands of economic imperialism, as in writings that strain to attribute rent-seeking motives to ever more institutions and officials.)

IS LABAND-AND-TOLLISON-TYPE-THINKING IN FACT INFLUENTIAL?

So far I have been worrying about the Laband and Tollison market test to the extent that it is applied. But do Laband and Tollison in fact have influence? I do not know. Conceivably, almost everybody ignores them; on the other hand, they are not unique in thinking as they do. What I do know is that they *try* to

have an impact. Their numerical systems of rating persons and departments and journals are intended to affect what people do. Even in their article, they preach at economists. They preach about publishing in prestigious journals, which implies (perhaps unintentionally) preaching about research on topics considered most acceptable to prestigious journals, preaching about what to emphasize and what not, preaching about research methods, preaching about styles of exposition (sometimes even involving strategic obscurity), and preaching about trying to associate oneself (as by judicious citations) to current fashions in the profession. The message, in short, is: compromise your standards. Put less effort into the kinds of work that you consider important, in which you have a comparative advantage, and that you enjoy. (Joy is a legitimate incentive, but one eroded by Laband-and-Tollison-type attitudes and practices.) Switch toward catering to editors and referees (who themselves operate under similar pressures to the extent that Laband and Tollison prevail).

Such preaching distresses me not merely because it is or might become influential but because it is officious and repellent in itself.

STYLE OF ARGUMENT

Laband and Tollison beg the question they purportedly discuss: as unabashed secondhanders, they simply assert or assume that their supposed market test is indeed the correct measure of excellence. But what justification is it of certain criteria that the players try to satisfy them when they are applied? Laband and Tollison, citing earlier papers coauthored by Laband, argue, for example, that journal editors try to publish “high-impact papers.” Well, it is no surprise that people respond to incentives, but to offer this fact as justifying a particular structure of incentives is circular reasoning.

A further sign of the weakness of Laband and Tollison’s case is their resort to emotive words like “sour grapes” and “crybabyism.” Such name-calling hardly applies to the eminent economists, including Nobel laureates, who have expressed concern about the state of much of the academic literature, including what might be called its narcissistic or incestuous aspects.⁵ What opinion of the literature may we infer, by the way, precisely among economists who excuse neglecting actually to read the work even of candidates being appraised?

⁵Examples of concern are cited in my articles of 1995 and 1997. Another example is McCloskey (1995). I cannot resist quoting from McCloskey (p. 414) an exchange between George Borts and Harry Johnson when both were journal editors. Borts: “[W]e get more good articles than we know what to do with!” Johnson: “Then why don’t you publish a few?”

The tragedy of misdirected efforts is even worse than one might think from what actually gets into the journals. Economists with experience in refereeing manuscripts could testify to this point.

The personal nature of Laband and Tollison's attack further appears in how they characterize an alternative to secondhand appraisals, namely, reliance "on the ostensibly firsthand knowledge of some central authority such as Yeager."⁶ Yet this is *not* the alternative, as I have explained above. Further, Laband and Tollison make a snide remark about "the literature that Professor Yeager dotes on"—the literature of a cult for which "up is down and down is up," for which "failing a market test is really passing it," and whose members are content just to chat among themselves, forgoing Wimbledon in hopes of winning the Austrian Open (p. 45). How do they know that I "dote" on such literature or that I "dote" on any literature? As I made clear in 1997, I am no spokesman for any particular school or sect. I have a low opinion of much Austrian literature, as well as a high opinion of some of it, opinions that I have formed myself and have not taken over secondhand. Of course much crummy work, along with excuses for it, is knocking around in economics, as in other fields. What does that fact have to do with the issue under discussion—appraisal by a supposed market test?

Are Laband and Tollison willing to let their remark about what I "dote" on remain as an example of their standards of accuracy and relevance? Their analogy with tennis tournaments reflects, by the way, their obsession with the game aspect of academic life.

CONCLUSION

Let me be clear about what I am *not* saying. I never questioned the need for standards, nor the uphill battle that unpopular ideas necessarily and even appropriately face, nor the necessity of secondhandism of some kinds and degrees and for some purposes. I am not sweepingly condemning the literature of academic economics. Economists continue making solid contributions despite everything.

I regret the perversion of standards through glorification of secondhandism. When appraisals are necessary, they should be kept as close as practicable to persons who have the most direct knowledge and who bear responsibility for their judgments. I regret the strengthening of incentives to jump onto bandwagons. I implore readers to learn lessons from the characters in Ayn Rand's novels who either are secondhanders themselves or exploit other people's

⁶I do not know whether the following remark is also directed against me personally, but it is a sneer at someone, if only a straw man: "It is not enough for the individual *qua* scientist to maintain that he has been enlightened and *knows* the *truth*" (2000, p. 43).

While unhappy about the likely consequences of Laband and Tollison's attitudes and practices, I am not angry about being personally attacked. Their attack helps reveal the nature and weakness of their case.

secondhandism and their susceptibility to intellectual intimidation.⁷ (Rand provides insights into the craving for prestige that I could scarcely hope to reproduce here.)

We should beware of relativism about truth. Beyond the gamelike or fame-and-fortune side of academic careers, the truth-and-beauty side deserves cultivation. Communication is important, even including skillful communication of knowledge to students and to the broad public.

Above all, I warn against discrediting the valid case for the free society by misconstruing the market as an entity in its own right that transcends the mere men and women who trade on it, an entity that makes superior judgments even about good and bad. Obstacles to understanding the logic of a market economy are great enough already. A spurious linkage in people's minds between a twisted version of free-marketry and the serious, valid, and quite different case for the free market can only harm the cause of freedom. Over-reaching boomerangs.

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⁷Consider the way Laband and Tollison draw a scarcely veiled contrast between work at the supposed frontier of the discipline and inferior work, as well as the general tone of their writing. A whiff appears of "the argument from intimidation," so identified by Ayn Rand (Binswanger 1986, pp. 32–34).

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