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FAILURE IN THE MARKET FOR REVIEWING ECONOMICS PAPERS: GOOD READERS, BAD REFEREES, AND UGLY PAPERS

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Abstract

The paper discusses the problem of incompetent and/or irresponsible refereeing of scientific papers, with emphasis on economics papers. To illustrate, I describe my own confrontation with erroneous published papers, and demonstrate that writing comments on such papers does not always solve the problem. Finally, based on previously suggested as well as on currently used solutions, I propose a change in the review process by abolishing referee anonymity and letting the authors appeal publicly if they think their papers have been evaluated improperly. This change will render the process self-correcting.

Keywords : Review process, referee anonymity, causality, endogeneity, spurious regression.

JEL codes: A11, B41, C51

1. INTRODUCTION

Like any other market for a good or service, the market for reviewing economics papers consists of demanders, i.e., the authors, and suppliers, i.e., the referees and the editors, although the editors usually play the role of middlemen, since they bring the authors and the referees together. Unlike most other markets, however, this market suffers from several failures, mainly because of the secrecy and the subjectivism that characterize it. As a result, individuals who are not responsible for a bad service may end up suffering its consequences, whereas those responsible for it may get away with it. Examples include unnecessary delays in the reviewing process and, more importantly, incompetent and/or irresponsible refereeing. It is often clear from their reports, that the referees miss the main points of the paper because either they do not read it or do not understand it.

First, consider the case of a good paper submitted to a journal for review. Assume that the editor is unfamiliar with the relevant literature, so his/her decision will be based solely on referee reports. If the reports are negative, and thus the editor rejects the paper, the authors and some of the potential readers of the paper will suffer the costs of the unfair rejection. For example, the authors may not get their tenure or promotion, thus incurring a huge cost. More frequently, because of the long delays in the reviewing process, before submitting the paper to another journal, the authors may have to update the data and redo a lot of boring pre-testing (e.g., determination of lag-length, unit-root and cointegration tests, possible structural breaks, etc.) and re-estimation, which is also a significant cost. In this case, the referees are the only individuals responsible for the bad service, but suffer no consequences.

Next, consider the acceptance of a bad paper, solely because of incompetent refereeing. The readers of the paper, whose only intention was to learn something by reading it, will suffer the consequences of the bad service. For some of them, reading the paper will simply be a waste of time, whereas for others it may be more damaging, if they reproduce the errors contained in the paper. The authors may also suffer some consequences, if they get exposed, but it is also possible that they may actually benefit from it, if they operate in a corrupted environment, where the publication of a bad paper may actually count as an achievement! The editor who accepted the bad paper may also get exposed, especially if he/she does it often. But, again, the referees, by hiding behind their anonymity, will suffer no costs, except perhaps the loss of credibility in the eyes of the editor, which is a relatively small cost. In this case, the publication of the bad paper damages the profession; and, as Section 3 shows, writing a comment or a full paper on a published bad paper does not always solve the problem.

This paper discusses the problem of incompetent and/or irresponsible refereeing of scientific papers, with emphasis on economics papers. Unfortunately, this problem remains unsolved in the economics profession, apparently because it is difficult to invent a mechanism that will enable the authors and the readers to hold the referees accountable for their reports and at the same time keep them willing to accept a referee's job. It is worth noting that such a mechanism already exists in Medicine (see Section 4). Until the economics profession adopts it or comes up with its own mechanisms, however, it will be based on the good will of the referees. In this regard, the latter should be reminded of the Golden Rule or ethic of reciprocity: "treat others as you would like to be treated." Examples of incompetent refereeing abound in the economics profession. After reviewing the literature (Section 2), I consider several such examples (Section 3) and propose a solution (Section 4). Section 5 concludes.

2. RELATED LITERATURE

There exists an important and well-documented literature that discusses the pros and cons of the prevailing journal-review process, especially the role of the referees. Although there is some basis for the underlying (implicit) assumption that "referees act in the interest of science as a whole" (Frey 2003, p. 208), most authors from various disciplines express a great deal of dissatisfaction with the prevailing review process and suggest tentative solutions to the various problems. Epstein (1995), for example, offers a dozen suggestions for improving the process. Here, I will focus on the problem of incompetent and/or irresponsible refereeing.

In the discipline of Psychology, some four decades ago, this problem was ranked second (after the publication lag time) in the list of sources of dissatisfaction with the journal-review process; and critics almost always suggested removing the "cloak of anonymity" from the referees, although they expected that this would cause referees to be less willing to honestly report what they think about a manuscript without concern about reprisals, and would make it more difficult for the editors to find referees, thus causing further delays in the reviewing process (see Bowen, et al. 1972, pp. 221 and 224, and Epstein 1995, p. 884).

Between the costs that the elimination of referee anonymity is expected to impose and the cost of irresponsible reviewing because of referee anonymity, Epstein (1995, p. 885) prefers the former, i.e., he wants "to have reviewers assume responsibility for their evaluations by identifying themselves." On several occasions, he says, he received reviews that were "blatantly in error," simply because reviewers suffer no consequences for their actions. Epstein argues that if the reviewers are identified, just like the editors are, then the incidence of such reviews would be considerably diminished.

In addition to eliminating the anonymity of the reviewers, Epstein (1995, p. 884) also proposes the establishment of a mechanism that will enable authors to appeal if they believe that a rejected paper has been improperly evaluated; and if there is a number of similar appeals against the services of a particular reviewer, that reviewer should be provided with feedback. In the event that this feedback does not improve the reviewer's services, the latter should not be demanded any longer.

In the discipline of Management, evidence of incompetent and/or irresponsible refereeing is reported by Bedeian (2003, p. 335). In an e-mail survey of authors who published in two of the discipline's leading journals during 1999-2001, out of the 173 authors who responded, 93 (54.7%) said that they had been asked to review a manuscript they were not competent to evaluate; and, even worse, 34 of these 93 reviewers (i.e., 36.6%), submitted a report! Frey (2003, p. 208) and Tsang and Frey (2007, pp. 129-132) point out that referee anonymity and the absence of author feedback to the referees are two important reasons for incompetent and/or irresponsible refereeing: "Anonymous referees have no property rights to the journal they advise. They may therefore not be concerned about the effect their advice has on the journal ... Absence of author feedback also implies that the referees are seldom held accountable for their comments. This may encourage irresponsible referees to make casual comments because they know very well that their comments will not be challenged."

In Economics, the review process does not seem to be better than that in other disciplines. When Gans and Shepherd (1994) asked the opinion of over 140 leading economists, including all the then living winners of the Nobel Prize and of the John Bates Clark Medal, many responded "with blistering pages" (p. 165). Their survey "demonstrates that many papers that have become classics were rejected initially by at least one journal – and often by more than one” (p. 166). Although many respondents to the survey praised the positive side of the review process, because it often leads to improvement of the papers or prevents bad papers from getting published, many others characterized it as “careless, irresponsible, and narrow-minded” (pp. 176-177).

One of the most disturbing features of the review process that is apparent in the Gans and Shepherd (1994) survey is the great degree of randomness: referees and editors of economics journals rarely agree on the value of a specific paper. This feature is also shared by journals of other disciplines, however. For example, Starbuck (2003, p. 346) reports that, during his first 2-3 months as editor of a Management journal, he received more than 500 pairs of reviews where only a small fraction of pairs of reviewers agreed with each other. Counting an “accept” as 1, a “revise” as 0, and a “reject” as -1, he found that the correlation coefficient for these 500 pairs of recommendations was only 0.12, a number that is statistically significant (because of the large sample size), but practically insignificant.

Frey (2003, pp. 212-213) reports some evidence that the economics profession has been losing its impact on society, and attributes this loss to the existing journal-review process, which “tends to work against originality.” Frey (2003, pp. 206, 208-209) argues that a major reason for this failure is the anonymity of the referees, who have no property rights to the journals they advise and suffer no consequences for their actions.

In every discipline, the consequences of incompetent and/or irresponsible refereeing become extremely serious whenever the authors, under the pressing need to “publish or perish,” adopt changes recommended by referees that they believe are wrong! This phenomenon, a perfect example of what Frey (2003) dubbed “intellectual prostitution,” occurred to 41 of the 173 respondents (23.7%) in Bedeian’s (2003) survey. Note that a 95% confidence interval for the corresponding population proportion is (0.17, 0.30), which includes embarrassingly large values of the true proportion of authors who “prostitute” by sacrificing their intellectual integrity in order to get their papers published.

3. EXAMPLES

As was noted earlier, examples of erroneous papers abound in the economics profession. It was also noted that writing comments on such papers does not seem to be the answer, because, as I demonstrate below, economics journals do not always welcome comments on bad papers, despite the fact that these comments strengthen the “self-correcting mechanism” of the publication process. Therefore, what needs to be done is a change in the current review process by putting an end to referee anonymity and letting the authors appeal publicly if they think that their papers have been treated unfairly.

To illustrate, I describe my own experience, which led me to think that journals that have published wrong papers are not always willing to publish comments on them or rejoinders to authors’ replies, even when the latter contain misleading and untruthful statements. In the latter case, the authors of wrong papers are allowed to get away with treating the critic unfairly. Thus, unless critics are well known in the profession, they often waste their time and get themselves into trouble by writing comments on wrong papers, as it is hard to find other journals to publish their criticisms.

Consider, for example, my paper (Hatzinikolaou, 2000) that criticized the empirical literature on consumption, which has been published in top journals. Although my criticisms concerned important specification and estimation issues with serious policy implications; and although they were well documented both theoretically and empirically; I was unable to publish them in a top journal, thanks to erroneous referee reports.

As a second example, consider my criticisms (Hatzinikolaou, 2010) of the paper by Mavrommati and Papadopoulos (2005), which contains basic econometric errors. Although the journal that published that paper has a companion journal that encourages discussion of articles previously published in these journals, the companion journal rejected my comment. According to the rejection notification, the rejection was based on “referees' decision,” who were not required to write a report, and did not even state the reason for the rejection!

Finally, as a third example, consider my criticisms (Hatzinikolaou, 2007) of the paper by Kollias, Mylonidis, and Paleologou (2007a), henceforth referred to as KMP. Although the Editor was fair enough to publish my comment, he nevertheless rejected my rejoinder to the authors’ reply (KMP, 2007b), despite the fact that the reply cunningly evaded the main issues I raised in my comment, which was not even cited! In what follows, I demonstrate that the referees of the reply should not have allowed it to see the light of publicity.

KMP (2007a) set out to determine empirically the direction of causality (not in the Granger sense, but in the usual one) between GDP growth rate (denoted as *gdp*) and military expenditure as a share of GDP (denoted as *milex*). For this purpose, they use panel data from $N = 15$ countries of the European Union (EU15), 1961-2000 ($T = 40$ annual observations from each country) and estimate two regressions: one of *gdp* on *milex*, and another of *milex* on *gdp*.

A major issue that I raised in my comment was the omitted-variable problem and the false statistical inference on causality. According to standard growth-accounting equations, *gdp* depends on the rates of growth of capital stock, labor force, and total factor productivity, but these variables are absent from the two regressions used by KMP (2007a). I pointed out in my comment that, in the context of a two-variable system, it is impossible to determine causality in the usual sense, since "correlation is no proof of causation."

In their reply, KMP (2007b, p. 581) argue that the reason why they omitted the determinants of *gdp* from their regression of *gdp* on *milex* was that they *were not interested* in the effects of these variables; they wanted "simply to investigate the relationship and causal ordering between the two variables." A competent referee would have prevented such statements from appearing, since they ignore the basic fact that a causal effect of one variable on another could potentially be inferred from the data *only if we hold other relevant factors fixed*.[1]

Thus, for example, in his introductory chapter, Wooldridge (2006, pp. 13-14) writes: "If other factors are not held fixed, then we *cannot* know the causal effect of a price change on quantity demanded. ... The key question in most empirical studies is: Have enough other factors been held fixed to make a case for causality? Rarely is an econometric study evaluated without raising this issue" (my emphasis). In their introductory chapter, Stock and Watson (2003, pp. 8-9) also make clear that in order to infer causality from the data it is necessary that the *ceteris paribus* assumption be (approximately) true. This, of course, is a basic principle in statistics, known for many decades (see, e.g., Simon 1954), and ignoring it leads to false causality inferences.

This is precisely what happened in KMP (2007a, p. 80), where we read: "Equations (3a) and (3b) [the regressions of *gdp* on *milex* and of *milex* on *gdp*] are then estimated using the basic

[1] In fact, we can never be sure that we uncover causality (in the usual sense) by running regressions. Using economic theory, which provides us with some information as to which variable causes which, and good econometrics, we could still attempt to make causality inferences, however. This is what econometricians mean when they emphasize that we must try to satisfy the *ceteris paribus* assumption as closely as possible by including as many relevant explanatory variables as possible.

fixed effects model for the EU15 countries for the time period 1961-2000. The estimation results are presented in Table III. ... The results indicate that there is a clear causal effect running only from *milex* to *gdp*."

Because I pointed out in my comment that it is impossible to make causality inferences in the context of a two-variable system, KMP (2007b, p. 582) deny the obvious fact that they based their causality inference (just quoted) on the results of Table III and write emphatically (but untruthfully) that they based it on the results of Table IV. (The regressions in Table IV include a lagged dependent variable, which could be viewed as a proxy for some of the omitted variables, thus partly escaping my criticism; see Wooldridge 2006, p. 315.) In particular, they write: "The conclusion 'there is a clear [causal] effect running only from *milex* to *gdp*' is drawn from the results reported in Table IV and *not* Table III" (their emphasis). The referee(s) failed, however, to strike this untruthful statement out of the reply.

Another major issue that I raised in my comment was the spurious regression problem in Tables II-IV of KMP (2007a), where the authors (implicitly) *assume* that both *gdp* and *milex* are I(0), but afterwards they report (in their Table V) strong evidence that *gdp* is I(0), whereas *milex* is I(1). In their reply, KMP (2007b, p. 581) try to defend the above strategy by invoking a result on the *consistency* of the slope estimator. According to this result, unlike the time-series case, the panel data estimator consistently estimates the slope coefficient, provided that both $N \rightarrow \infty$ and $T \rightarrow \infty$. On this point, KMP cite Baltagi (2001: 234).

This line of defense is misleading, however. The number of cross sections used by KMP (N = 15) is not large enough to invoke the above consistency result and, more importantly, the issue I raised in my comment was *not* the consistency of the slope estimator, but the bias of its *t*-test, which over-rejects (see Entorf, 1997, Table 1). KMP might have realized their error if they had looked a little further in Baltagi´s book, where he summarizes the evidence produced by Entorf (1997): "Entorf found that for $T \rightarrow \infty$ and N finite, the nonsense regression phenomenon holds for spurious fixed effects models and inference based on *t*-values can be highly misleading" (Baltagi, 2001: 243). Kao (1999: 6) also writes: "the *t*-statistic, t_β, diverges so that inferences about the regression coefficient, β, are wrong with the probability that goes to one asymptotically." If the usual confusion between statistical and economic significance is strongly criticized as bad empirical practice (McCloskey and Ziliak, 1996, and Ziliak and McCloskey, 2004), the error of claiming the existence of an economic relationship where there is none, by misinterpreting the relevant econometric literature, should not be tolerated at all. A competent referee would have struck out of the reply this misleading line of defense.

A third issue that I raised in my comment was the endogeneity problem, which is obvious in the regression of *milex* on *gdp*. Consider, for example, a shock that raises *milex*. To the extent that the additional military expenditure goes to domestic goods, standard Keynesian analysis predicts that *gdp* will rise. Thus, the error term of this regression and the explanatory variable are correlated, and the most crucial assumption of the fixed-effects model is violated, leading to inconsistent estimates. In their reply, KMP (2007b, p. 582) stress that they "do not impose any *a priori* assumptions regarding the endogeneity of *gdp* and *milex*." The truth is, however, that they do (implicitly), since the fixed-effects model assumes exogeneity of the regressors.

It is possible that the strange result of KMP (2007a, Table III), that the regression of *gdp* on *milex* is highly significant, whereas that of *milex* on *gdp* is highly *in*significant (!), might be a consequence of the endogeneity problem just explained as well as of the fact that *milex* is an I(1) process. KMP (2007a, p. 80) wrote in the notes to Table III that their estimation method was fixed-effects GLS (FGLS). Under the present circumstances, however, FGLS is inconsistent. First, as Wooldridge (2006, pp. 428-429) shows, consistency of FGLS requires almost strict exogeneity of the regressors. In particular, in the regression of *milex* on *gdp*, the error term must not be correlated with gdp_{t-1}, gdp_t, and gdp_{t+1}. This is a strong condition and, as I just explained, it can hardly be assumed to hold in this regression. Second, by referring to a regression of y_t on x_{t1}, ..., x_{tk}, Wooldridge (2006, p. 429) warns: "Consistency and asymptotic normality of OLS and FGLS rely heavily on the time series processes y_t and x_{tj} being weakly dependent. Strange things can happen if we apply either OLS or FGLS when some processes have unit roots."

A final issue that I raised in my comment was that the Breusch-Pagan (BP) and Hausman tests are applicable only under the assumptions of spherical disturbances and strict exogeneity of the regressors, which are violated here. In their reply, KMP (2007b, p. 582) argue that these tests are applicable to their Model 2, where there is less evidence against the assumption of spherical disturbances. They ignore, however, the other condition for the applicability of these tests, which is strict exogeneity of the regressors. In Model 2, this condition is obviously violated, since a lagged dependent variable is used as an explanatory variable.

4. A PROPOSED CHANGE IN THE REVIEW PROCESS

Based on previously suggested changes in the review process as well as on the process that is already used by many medical journals (see below), I think that economics journals should abolish referee anonymity and also let the authors appeal publicly if they think their papers have been evaluated improperly. Here is a way to implement this self-correcting process.

The editor could upload the original paper to a specific website, without disclosing the name(s) of the author(s), and invite 2-3 referees to write comments and sign them. Provided that the subject of the paper falls within their expertise, the referees should accept the invitation, acting professionally and strategically. For if other people must referee one's own papers, then one must be willing to referee theirs; and it is not a good idea to turn down an editor by refusing to referee a paper.

Once they accept the editor's invitation, the referees will be obliged to make careful and sensible comments, within a specified period of time, and sign them. (If necessary, they could consult other experts.) The author(s) should also be allowed to respond to these comments in the same website and sign as "author(s)." This dialogue can go on until a given deadline is reached. By reading all these comments, the editor should be able to make a fair decision. If necessary, he/she could consult an expert from the editorial board (see Frey 2003, p. 216). If the decision is positive, the pre-publication history of the paper (its original version and all the comments accompanying it) should remain in the website; whereas if it is negative, the author(s) should be given the option to withdraw the paper and the comments, if they wish. A procedure similar to the one just described has been used for some time by many medical journals, e.g., *BMC International Health and Human Rights*, *Environmental Health*, *Nutrition Journal*, *Implementation Science*, *Trials*, *Reproductive Health*, etc.

The procedure just proposed might make it more difficult for the editors to find referees, thus delaying further the review process. It is not inconceivable, however, that the proposed procedure might in fact *shorten* the average time that elapses from the first submission of a paper to a specific journal to the time of its final acceptance, perhaps by another journal. For, under the current review process, a paper typically suffers several rejections because of the incompetent and/or irresponsible refereeing, thanks to the anonymity of the referees. As a result, the average time of a paper's circulation from journal to journal, which might be called "frictional unpublicity" (an analog of "frictional unemployment"), may be unnecessarily too high. The number of papers that will eventually be accepted by all the relevant journals during a specific time period can be assumed to be fixed, given the constraint of journal space. Therefore, other things equal, by eliminating anonymity and thus inducing more responsible refereeing, the average number of rejections per article may be reduced, thus reducing "frictional unpublicity." The expected gain (in terms of lower "frictional unpublicity") might exceed the expected cost (in terms of the additional delays in the review process because of the additional difficulty to find referees), in which case the proposed procedure would be worth considering.

5. CONCLUDING REMARKS

Authors ought to be grateful to those editors and referees who help to improve their papers significantly, even when they are rejected. Such conscientious editors and referees play their true role as "gate-keepers." But, alas, they are the exception, not the rule (Frey 2003, p. 208). During the last four decades or so, the economics profession has become highly technical and quantitative. Thus, in a competitive academic environment, where tenure and promotions are based on merit, survival requires good understanding of mathematics and econometrics, since papers are often rejected if they are not technical enough (Gans and Shepherd 1994, p. 177). Yet, many economists have insufficient knowledge of mathematics and econometrics. As a consequence, their technical articles often contain serious errors, which invalidate their conclusions, hence the extremely important role of the editors and referees as "gate-keepers." The editors are usually highly qualified individuals, but cannot be experts in every area, and do not have the time to read all the submitted articles. Thus, they "often side with referees and typically act as if referees are more competent than authors" (Tsang and Frey 2007, p. 129). As a result, the role of the referees has become crucial.

But many referees, since they are chosen from the population of authors, also have insufficient knowledge of mathematics and econometrics. This is one reason why their reports often make no sense. Of course, the problem of incompetent refereeing existed even before the economics profession became technical and quantitative, but apparently it has become worse since the 1970s, when "the technical tide rolled in" (Gans and Shepherd 1994, p. 177). In my view, the real cause of incompetent and/or irresponsible refereeing is the anonymity of the referees, who often submit reports although they do not understand the paper, "because they know very well that their comments will not be challenged" (Tsang and Frey 2007, p. 132). As a result, many good papers are turned down and many bad ones are published. Worse still, as Section 3 shows, some authors get away with covering up their errors by making untruthful and misleading statements.

The referee(s) of the bad papers cited in Section 3 obviously lacked knowledge of econometrics and should have declined the referee´s job. Young economists who will read these papers with the good intention to learn something from them might get confused and might end up propagating the errors contained in them. The authors, the referees, and the editors are all responsible. The authors and the editors might be held accountable for their bad service, since their names are known, but the referees will get away with it, thanks to their anonymity.

Unfortunately, examples like these abound in the economics literature. The referees often produce a bad service, but are not taxed for it. In fact, they might even gain by writing in their curriculum vitae that they have served as referees for the journal in question. This is a market failure, which can be fixed by holding the referees accountable for their reports and by letting researchers appeal publicly if they believe that their papers, comments, or rejoinders have not been treated fairly. Section 4 describes how this self-correcting process can be implemented.

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