Econometric Errors in an *Applied Economics* Article

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ABSTRACT

In a recent paper, Mavrommati and Papadopoulos (2005) estimated a relationship between a firm's advertising intensity—the ratio of the firm's advertising expenditure to its sales, A/S, the dependent variable—and a number of explanatory variables, including firm profitability (PRF). My comment does not contribute to the subject (the determinants of advertising intensity), but only to the econometric methods used in the paper.

Mavrommati and Papadopoulos used panel data from $N = 172$ firms in the Greek food industry over the time period 1990-1997 ($T = 8$ annual observations per firm). The paper is interesting, but is econometrically incorrect, which renders its results questionable. The purpose of this comment is to prevent novice researchers from repeating the errors, omissions, and confusions described below, and to police standards at the journals.[2]

First, Mavrommati and Papadopoulos (2005, 1784) make an incorrect statement about the standard F-test. Although they state correctly (but unnecessarily) that a regression is significant when the observed F-ratio exceeds the appropriate critical value, they immediately add: "For the same regression equation to be a significant predictor, the observed F-ratio should be at least four times as large as the tabulated F-value." This statement is obviously incorrect.

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2. Originally, I submitted this comment to *Applied Economics Letters*, but it was promptly rejected without their stating the reason. The rejection letters and my response are given in the Appendix at the end of this comment.

Second, Mavrommati and Papadopoulos (2005, 1783) write: "in the panel data we do not pay attention to the Durbin-Watson value because it exploits the existence of autocorrelation only in time series data." This statement is also incorrect. Bhargava, Franzini, and Narendranathan (1982) have proposed a generalization of the Durbin-Watson test to the fixed-effects model of panel data, the preferred model of Mavrommati and Papadopoulos (2005, 1785). Bhargava et al denote their test statistic as d_p. Other tests for serial correlation have also been proposed for both fixed- and random-effects models (see Baltagi 2001, 90-101, Greene 2008, 652-655, and Wooldridge 2002, 274-276). Mavrommati and Papadopoulos failed to apply any of these tests, however, which makes their significance tests suspect.

Third, as the practice of modern applied econometrics suggests, not only is it necessary to test for serial correlation in panel data, but it is also necessary to test the hypothesis of a random-walk error process. As Bhargava et al (1982, 541) point out, this hypothesis is "of considerable interest," since its acceptance would imply that the most efficient parameter estimates of the fixed-effects model can be obtained from its differenced version. More crucially, failure to test this hypothesis amounts to evading the spurious-regression problem, which exists even when N is much larger than T, as is the case in this paper (see Entorf 1997, 292). Bhargava et al (1982, 545) recommend the use of their d_p statistic to test for random-walk disturbances, but other tests are also available (see Greene 2008, 767-768). Mavrommati and Papadopoulos failed to use any of these tests, however, thus leaving their paper open to criticism with respect to the spurious-regression problem.

Fourth, Mavrommati and Papadopoulos (2005, 1785-1786) confuse the Hausman test for correlation between the firm-specific effects (a_i), when treated as random, and the explanatory variables (X_k, $k = 1, ..., K$) with the Durbin-Wu-Hausman (DWH) test for correlation between the equation's disturbance (ε_{it}) and the X_k's, when the a_i's are treated as fixed parameters (i.e., when the fixed-effects model is used). In particular, immediately after describing the Hausman test for correlation between the a_i's (when treated as random) and the X_k's, Mavrommati and Papadopoulos (2005, 1786) add: "The test indicates whether or not an instrumental variable technique needs to be used." This statement is incorrect, however. The role of the Hausman test described by the authors is to choose between fixed and random effects (see Greene 2008, 208-209), and in this paper the test strongly rejects the random-effects model, since the value of the test statistic is $X_9^2 = 41.25$ (not X_{10}^2, as the authors write) for regression 1a, and $X_{10}^2 = 38.58$ for regression 1b (see their Table 3). Given these values of the test statistic, which are significant even at the 1% level, if the Hausman test described by the authors was a test for choosing between the standard fixed-effects and the instrumental variable

estimator, as the authors erroneously claim in the previously quoted statement, then they should have chosen the instrumental variable estimator. Instead, however, Mavrommati and Papadopoulos conclude that there is no endogeneity and consequently choose the standard fixed-effects estimator. To decide on this issue, one can use the DWH test (which the authors simply call "Hausman-Wu test"), which compares the instrumental variable estimator with the least squares estimator (see Greene 2008, 321-325). It is not clear, however, whether or not the authors actually applied the DWH test, since they failed to mention which explanatory variables they suspected as being correlated with ε_{it} and which instrumental variables they used to implement the DWH test. My criticism on this issue is concerned more with the considerable confusion contained in the last paragraph of Section VI and less with the correct implementation of the tests.

Further, Mavrommati and Papadopoulos (2005, 1786) report the following: "We estimate the relevant F-statistic (Gujarati, 1992) for the fixed effects models ($F_{(1a)}^{*} = 17.27$, $F_{(1b)}^{*} = 17.27$) against the theoretical value of F ($F_{(1a)(0.1)}$, $F_{(1b)(0.1)}$). Thus, the Hausman specification test ($F^{*} < F_{(0.1)}$) shows that an endogeneity does not exist in both models." If this is in fact the DWH test, then this conclusion is incorrect, because the reported observed value of the test statistic ($F^{*} = 17.27$) is highly significant, given that the degrees of freedom of the numerator is a small number (equal to the number of explanatory variables which are suspected of being correlated with ε_{it}), whereas the degrees of freedom of the denominator exceed 1000 (see their Table 3). This evidence leads to the conclusion that the standard fixed-effects estimator (used by Mavrommati and Papadopoulos) is inconsistent, and therefore an instrumental variable estimator was called for (see Wooldridge 2002, Chapter 11). Theoretically, the explanatory variable PRF (firm profitability) is likely to be correlated with ε_{it}, since positive (negative) shocks in advertising intensity, i.e., large positive (negative) values of ε_{it}, are likely to increase (decrease) firm profitability.

In sum, the Mavrommati and Papadopoulos paper would have been a nice micro-econometric application had the econometrics been done properly. Unfortunately, several econometric errors, omissions, and confusions render its results questionable. This comment aims to prevent novice researchers from propagating these mistakes. I believe that the journals can reduce significantly errors and confusions of the type described here by adopting a strict mandatory data and code archive (see McCullough et al 2008),[3] because such a replication policy is likely to force researchers to be more careful in carrying out an empirical paper as well as in reporting and interpreting the results of their research. McCullough (2007) makes specific recommendations on how this goal can be

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3. I thank an anonymous referee of this Journal for bringing this point to my attention.

achieved and suggests that we all learn from the success of the *Journal of Applied Econometrics* toward this end.

Appendix: My Correspondence with *Applied Economics Letters*

The present comment is on Mavrommati and Papadopoulos (2005), which appeared in *Applied Economics*. That journal has a companion journal called *Applied Economics Letters*, which publishes short articles of original research and encourages discussion of articles previously published in *Applied Economics* and in *Applied Financial Economics*. Here I reproduce the correspondence between *Applied Economics Letters* and me.

1. The rejection letter, dated 23 Feb 2010.
[The following rejection letter was received by email four days after I had submitted the comment to *AEL*.]

> Dear Professor Hatzinikolaou
> AEL-2010-0082
> Measuring advertising intensity and intangible capital in the Greek food industry: a comment
>
> The editor has now received the referees' decision on your paper, in the light of which Applied Economics Letters is unable to publish your article.
>
> Applied Economics Letters asks the opinion of two referees who are experts in the relevant field of research. The paper is also read by one of the editors. If both of the referees and an editor concur in their view, their decision is final. We consult a third referee if there is a difference of opinion. In order to expedite the proceedings, which is one of the objectives of a letters journal, we do not require a full report on the paper from the referees. The choice of referees and the fact that we require a consensus view between the referees and an editor ensures, however, that the process is as fair as possible.
>
> Thank you for giving Applied Economics Letters the chance to consider your work and please consider us again in the future as an outlet for your research.
>
> Kind Regards
> Editorial Office

2. My response, dated 23 Feb 2010.

> Dear Sir/Madam:
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> I would appreciate your sending me the referee reports regarding the comment I submitted on 19 Febr. 2010 (AEL-2010-0082).
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> My comment points out basic errors, and I would like to know on what grounds it has been rejected. I just checked Manuscript Central, but, to my surprise, I did not find any referee reports. I thought that the purpose of the fee I paid ($50) was to handle the costs of obtaining and sending out referee reports to the authors. Are there any reports for my comment? I have served several times as a referee for your Applied Economics journals. Every time, I provide a detailed report to help the Editot [sic] make a fair decision. As an author, I expect to be treated just as fairly.
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> Yours sincerely
> D. Hatzinikolaou

3. The response of the AEL Editorial Office, dated 23 Feb 2010.

> Dear Prof Hatzinikolaou
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> We do not ask for reports for letters submisssions [*sic*]. The referees say a yes or no - it is all in the instructions for authors and your email below. It has always been the same format sinec [*sic*] the journal started. There is speed dedicated to letters that means no reports are sought.
>
> Sorry for your disappointment.
>
> kind regards
> Editorial office

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About the Author

Dimitris Hatzinikolaou is an associate professor of Economics at the University of Ioannina, Greece. He received his PhD in Economics from the University of Connecticut in 1993 and during 1994-1998 he served as Lecturer B at Flinders University of South Australia, where he earned tenure. In 2000, he was hired as an assistant professor by the University of Ioannina, and in 2007 he was promoted to associate professor. He has published a number of articles on macroeconomics and econometrics in refereed journals, e.g., *Journal of Development Economics*, *Southern Economic Journal*, *American Statistician*, *Economic Modeling*, *Empirical Economics*, etc. His email is dhatzini@cc.uoi.gr.

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