Construct validity and other issues pertaining to 'The impact of research designs on R_ in linear regression models: An exploratory meta-analysis.'

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INTRODUCTION

I initially commented on Heribert Reisinger's (1997) paper, "The impact of research designs on R^2 in linear regression models: An exploratory meta-analysis," in a double-blind review context. Some of my comments are incorporated in his present paper, while others are not. Unfortunately, my major original criticism of Reisinger's work, namely, that it suffers from serious construct validity problems, has not been addressed in the revised version of the manuscript. This is because it is all but impossible to do so. Nevertheless, the construct validity problems in Reisinger's paper impede any meaningful interpretation of his meta-analytic results. This issue is discussed below.

CONSTRUCT VALIDITY PROBLEMS

I am curious as to why the author chose to undertake this study. I have difficulty visualizing what he hoped to accomplish as a result. In any given meta-analysis there are construct validity—or so-called "apples and oranges"—problems. That is, many studies in a meta-analysis are only roughly or approximately measuring the *same thing* (Bangert-Drowns 1986). And this is a real problem even when a meta-analysis is attempting to quantitatively summarize the empirical literature in an, ostensibly, *well defined subject area* (say, the impact of personality on consumption behavior).

But the present paper has no such focus. It merely examines the size of R^2 in a (small) sample of articles dealing with topics that are *extremely diverse, and in no way related to one another*. So why, for example, should the reader be interested in the average R2 values of a study on, say, "The Impact of Personality on Consumption Behavior" versus "Socio-economic Status and TV Viewing Habits" versus "The Effects of Strategic Marketing on Company Performance"? Variations in R^2 are likely to be much more a function of the *substantive nature* of the studies being undertaken than they are of research design. And as mentioned above, these studies are for the most part totally unrelated, so why should the reader be concerned with a quantitative summary of such studies? Even though this study is exploratory, why should I be interested in comparing goodness-of-fit measures for "apples and oranges"? More pointedly, most econometrics texts emphasize that R^2s from equations with different dependent variables (and this usually means, e.g., raw values for y versus the log of y, etc.—not the kind of apples-and-oranges problems we are dealing with here) should not be compared.

The above comments were offered to Reisinger in my initial review of his paper. He alludes to them on page 2 of his revised manuscript, noting that: "When we presented the results of our meta-analysis before, we sometimes had to defend our work against the argument that variations in R^2 are likely to be much more a function of the substantive nature of the study being undertaken than of research design. *This argument is of course true*, but the main focus of the present study is to find some regularities in R^2 that originate in the research designs of marketing studies, *apart from the clear impact of the substantive nature of a study on R^2*" (my emphasis). But because it is impossible to disentangle the "clear impact of the substantive nature of a study on R^2," one must be open to the distinct possibility that the obtained results for the influence of research design are spurious. Only when we have a large number of studies addressing the *same topic* can we have any confidence that variations in R^2 are likely attributable to differences in research designs.

SOME ADDITIONAL REMARKS

On page 2 of the revised manuscript, Reisinger observes: "The meta-analysis has an exploratory nature because no *a priori* theory concerning the influences of the various potential impacts on R^2 exists (besides the difference between cross-sectional and time-series data)." This is only partly true. For example, as Reisinger subsequently acknowledges (page 6), it is well known that as additional explanatory variables are added to a regression equation, the value of R^2 will increase. Consequently, it is to be expected that his H_{07} is rejected.

Similarly, he notes (page 5) that there is no obvious reason why "correlation between the regressors," or multicollinearity, should have an influence on R^2. But as I pointed out in my original review, multicollinearity does not affect R^2 (see Kennedy 1992), and the OLS estimates are still BLUE. So it is not surprising that his H_{06} (COR) results did not reject the null hypothesis. For whatever reasons, Reisinger chose not to include this information in his revised manuscript.

While Reisinger examined the impact of some violations of the assumptions of the classical linear regression model on R^2, e.g., homoskedasticity and multicollinearity, he did not include an assessment of autocorrelation among the error terms. This is certainly an issue that arises with time series data, and often cross-sectional, too. For example, positively autocorrelated errors usually inflate R^2. Similarly with spatial autocorrelation (Haggett, Cliff, and Frey 1977). Reisinger informed this (blind) reviewer that: "One of our rules in this specification stage said that we would incorporate no qualitative variable in our final meta-analysis with less than 10% of the observations per level." Perhaps this restriction should have been spelled out in his manuscript. It is also worth noting that heteroskedasticity confuses the traditional interpretation of R^2 because the intercept term is often absent after correcting for the problem (Hubbard 1983).

I also pointed out that R^2s are sensitive to the range of variations in the dependent variable. This is usually illustrated in econometrics texts by comparing R^2s from estimation of the consumption versus savings functions. Yet the range of the dependent variable is not one of the research design variables incorporated in this study. In fairness, I also added that this would not be an easy thing to code.

The "meat" of Reisinger's study—the results shown in Tables 2 and 3—unfortunately are not especially informative. In my original review I asked the author to provide more *descriptive statistics* for every hypothesis (category) in Table 2. For example, what *is* the average R^2 in *JMR* versus *IJRM* versus *ML*, and for cross-sectional versus time series data, etc.? And why should the researcher be interested in these, and other hypothesized results? Reisinger's response, which struck me as rather disingenuous, was that these values can be derived from the parameter estimates in the tables. An author should not presume upon the reader. One should provide user-friendly, descriptive statistics about the magnitude of effect sizes directly, the ultimate goal of any meta-analysis, and downplay the *p*-values.

A couple of final points. Reisinger (page 3) states that: "The exploratory nature of this study can be compared to some extent to Ehrenberg's seminal book (1988) on repeat buying. Ehrenberg's main interest lies in finding empirical generalization in buying behavior." But a comparison of Reisinger's paper with Ehrenberg's book is misleading. Ehrenberg's work is quite different from what Reisinger is attempting. Ehrenberg focused on *deliberate replications* of buyer behavior for *similar* products, and gradually increased the scope of his work by *systematically* searching for empirical generalizations over *highly differentiated* buying (and other) situations. Reisinger's paper does no such thing. It simply compares R^2s from a small sample of studies whose only commonality is that they are published in marketing journals. Unlike Ehrenberg's work, Reisinger's studies are in no way related.

Lastly, Reisinger notes that: "Recently, Peterson (1994) published a comparable (but more extensive) meta-analysis concerning Cronbach's coefficient alpha." It might have been instructive if Reisinger had reported Peterson's (1994, p. 381) conclusion that: "With few exceptions, there were no substantive relationships between the magnitude of coefficient alpha and the research design characteristics investigated." Perhaps this same conclusion applies to Reisinger's R^2 paper.

CONCLUSIONS

Let us summarize some of the findings in Reisinger's paper. These are that the larger the number of regressors, the higher the value of R^2, and that time-series data yield higher R_2s than cross-sectional data. Both of these results, which Reisinger admits, are already well known. Contrary to Reisinger's assertion, the fact that multicollinearity has no effect on R^2 is known *a priori*, as is the influence of positive autocorrelation among the errors (which Reisinger did not address because of sample size problems). Similarly, certain problems associated with heteroskedasticity are also known *a priori*. Other findings were that larger sample sizes, and primary (versus secondary) data, produce smaller R^2s, and that *JMR* has lower R^2s than *IJRM* and *ML*.

Interpreting the results in his paper, however, is difficult because of construct validity and other problems. The value of Reisinger's contribution would have been enhanced, and the reader better served, had he presented his findings in the form of readily understood descriptive statistics about effect sizes for each of the hypotheses examined.

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