Rejoinder to "Construct validity and other issues pertaining to 'The impact of research designs on R^2 in linear regression models: an exploratory meta-analysis'."

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INTRODUCTION

Raymond Hubbard was one blind referee of Reisinger's (1997) paper, "The impact of research designs on R^2 in linear regression models: an exploratory meta-analysis". In his commentary, Hubbard (1997) reports about several issues in connection with the review process of this paper. Therefore, I think it is necessary to give several insights in the review process from my point of view as the author.

STAGES IN THE REVIEW PROCESS

After submitting the first version of the paper under consideration to the *Journal of Empirical Generalisations in Marketing Science*, the result of the first stage of the review process was one positive and one – in the words of the editor – "less positive" review (the one of Raymond Hubbard). The editor asked me to address the comments made by the

second reviewer, especially with regard to the major criticism (see Hubbard 1997, section "Construct validity problems"). My defense was as follows:

"The second referee is in doubt whether results of the present study are of principle interest and importance. It is true that the articles comprising the sample for the meta-analysis are dealing with topics that are '... extremely diverse, and in no way related to one another' (at least, all studies focus on a marketing spectrum). It is also true that '... variations in R^2 are likely to be more a function of the substantive nature of the studies being undertaken than they are of research design'.

The main focus of the present paper is to find – apart from the clear impact of the substantive nature of a study on R^2 – some regularities in R^2 that originate in the research designs of marketing studies. As I see it, if the results show some regularity e.g. between R^2 and the sample size, this finding is very important in assessing the overall fit of the model for the application at hand. As the proportion of variance explained is a basic benchmark in evaluating the results of an empirical regression model (in empirical work, almost all reported results of regression analyses include R^2 in their findings), it is important to point out that the research design may have some influence on R^2 (and apparently it does), independently from the substantive nature of a study.

The exploratory nature of the present meta-analysis can be compared to some extent to the seminal book of Ehrenberg (1988) on repeat-buying. In Table 2.1 (p. 19) Ehrenberg deduces some regularities on consumer buying behavior on the basis of panel data of different products (soap flakes, clothing, flour, detergents, and others; the only thing all these products have in common is that they are frequently bought goods). In this case you could also argue that the substantive nature of the decision process leading to the buying behavior

in the various product fields cannot be compared directly, but Ehrenberg's main interest lies in finding some empirical generalizations in buying behavior. He does not study substantive reasons for buying behavior.

The second key criticism of the referee says that '... More pointedly, most econometrics texts emphasize that r-squares from equations with different dependent variables should not be compared', but this rule holds only for model selection purposes where you try to find the 'best' model out of various competing models. Model selection is not a key concern in my paper, so I don't think that this special criticism is justified in the context of the present meta-analysis. In the regression analyses of my meta-analysis, R^2 is always the dependent variable. However, my meta-analysis does not deal with model selection in terms of maximizing the proportion of variance explained.

My last comments focus on a recent Special Issue on Empirical Generalizations in Marketing (*Marketing Science*, 1995, Vol. 14,3,2). Leading Marketing Scientists participated at a Wharton conference in 1994. Completion of the papers presented there resulted in this Special Issue. Bass (1995) proposed as a definition of an empirical generalization that it is '... a pattern or regularity that repeats over different circumstances and that can be described simply by mathematical, graphic, or symbolic methods' (p. G7). As I see it, my work lies within this definition. Bass and Wind (1995) further note that during the conference there remained some unresolved criteria for research. '... At the one extreme are those who feel that empirical generalizations do not have to be based on theory, derived from theory or leading to the development of a theory. Yet, others require that empirical generalizations be theoretically sound' (p. G2). In fact, in this respect there is no general agreement among leading marketing researchers. I think, one would classifiy the position of Bangert-Drowns (1986) rather at the theory guided end of the above mentioned continuum, whereas the present paper can be classified as an empirically driven research."

In the next step of the review process, I was invited by the editor to submit a revised version of my manuscript. Most of the above defending arguments have been incorporated in this version, as the editor suggested to do so. The revised version has been finally accepted and published. There was no additional communication between the two (blind) referees and me.

ADDITIONAL COMMENTS TO TWO MAJOR ISSUES

Two major criticisms, respectively suggestions for improvements raised by the reviewers, have not been addressed in the revised version.

The first and more important one is the construct validity problem (see Hubbard 1997, and the discussion in the previous section). Resolving this problem within the framework of my meta-analysis was virtually impossible, as Hubbard properly points out. Nevertheless, I do not agree with the conclusion that these problems "... impede <u>any</u> meaningful interpretation of the findings". For instance, I am especially convinced of the existance of a negative effect of the sample size on R^2. Future work will show, whether my results do really suffer from serious construct validity problems. Simulation studies could help us possibly to study the relationship between R^2 and the sample size in more detail, avoiding any of these problems.

The second major issue I did not incorporate in the revised version, was Hubbard's suggestion to provide more descriptive statistics for every hypothesis (category) in Table 2 of the original manuscript. Actually, I did prepare a new table in line with Hubbard's

argument, but after finishing it, I realized that I still preferred the old table. Having in mind that the first reviewer as well as all the people in my academic surrounding did not criticise the presentation of the findings, I finally decided to keep Table 2 as it was. Obviously, this decision was problematic, and I would like to clarify this matter (besides the rather technical argument that the average R^2s in the various categories of the explanatory variables can be derived from the parameter estimates in Table 2). Hubbard (1997) points out that "... 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." Because of the exploratory nature of the paper under consideration, my main focus was <u>not</u> the absolute magnitude of effect sizes, but rather the existance or non-existance of systematic effects on R^2. For that reason, I did not want to downplay the *p*-values. Paying more attention to the absolute magnitude of effect sizes would probably require a considerable larger sample size than the one available within the present study.

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