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Endogeneity in management accounting research: A comment

Laurence van Lent*

Tilburg University

Abstract

Chenhall and Moers (2006) provide an excellent overview of the econometrics of endogeneity. In response to their discussion I argue that researchers should be courageous enough to set aside endogeneity concerns when their research question is important. Theory does not admit a definite answer to the question whether endogeneity is present in a particular model and econometrics has few technical solutions to offer. Since we cannot be sure endogeneity exists, and if we were to be sure of its existence, there is little we can do about it, researchers are well advised to move on to more serious problems.

1. Introduction

I am sure many Soviet *apparatchiks* wholly agreed with *Pravda*'s October 1986 editorial comment on pop music that "Rock and Roll has a right to exist but only if it is melodious, meaningful, and well performed." Much to the chagrin of these Party fuddy-duddies, however, often the more interesting music is pounding, rhythmic, devoid of meaning, and edgy. As in music, so in empirical accounting research: While some might take the position that accounting research should be based on fully developed theoretical arguments, sophisticated econometrics, and complete data sets, perhaps the most revealing studies in (management) accounting explore areas in which theory offers contradictory predictions, measurement instruments are underdeveloped, and available data do not admit advanced statistical procedures. Indeed, empirical research in accounting should be driven *not* by the degree to which smooth theory, elegant techniques, or computer-friendly data are convenient or available, but rather by important

* Address for correspondence: Tilburg University, PO Box 90153, 5000 LE Tilburg, the Netherlands. Telephone: +31 13 466 8288. E-mail: vanlent@uvt.nl

questions about human behavior in organizations and markets, as asking important questions is perhaps the most crucial step in any research project. Then, having identified their questions, conscientious empirical researchers should turn to the effort of answering their questions by using theory to develop hypotheses and collecting data to test their hypotheses.

Perhaps more so in management accounting than in other areas of accounting, these well-known stages of the research cycle are fraught with potential problems. For instance, theory is often non-existent or its assumptions are too strong to admit application, samples are often small and non-random, constructs are not always measured in a reliable and valid fashion, and finding data with which one can confront theory is frequently a formidable obstacle. And of course, endogeneity may also be an issue. Conducting empirical work involves weighing each of these considerations against the others, evaluating what can be done to address the problems associated with theory, data, instruments, and endogeneity and then deciding whether the required investments to solve these problems pay off sufficiently. Given the plentitude of issues competing for the researcher's attention, endogeneity might just well be a low-ranking priority. Why?

First, in practice there is little that can be done about endogeneity. The textbook solutions typically recommend using instrumental variable (IV) estimation, employing a proxy that is not "endogenous" but is highly correlated with the structural variable of interest, or collecting additional data that allow for the use of panel data techniques. However, valid instruments are the closest thing to the "holy grail" in accounting research, as they are highly desirable but illusive (Larcker and Rusticus 2005), proxies are likely to be imperfect (i.e., correlated with one or more of the explanatory variables in

the structural equation), and additional data that meet the requirements of panel data techniques are often hard to obtain in management accounting settings.

Second, while invoking theory is often the remedy advocated as a panacea for endogeneity, to date the field of management accounting does not boast one unifying theory, that is, a dominant theoretical perspective or way of analyzing management accounting problems, that is generally accepted by researchers, and in many sub-fields the extant theory is only in its nascent stages, inherently incomplete and deeply contested. One only has to read the discussion between Zimmerman (2001) and Hopwood (2002) to understand how problematic it is to talk about theory in management accounting. As such, a researcher can probably find one “theory” to argue that a model suffers from endogeneity and another “theory” to argue the opposite. Given these circumstances, there is a real danger that using theory to justify the designation of certain variables as endogenous or exogenous amounts to little more than post-hoc rationalization of research design choices.

Nevertheless, theoretical developments in the economics of organization have made endogeneity a more prominent issue in managerial accounting. In the remainder of this comment I briefly discuss these advances. I describe endogeneity as a specification problem in a regression framework and I offer a way of thinking about endogeneity that may be helpful to empirical researchers. I then argue that even in light of these advances, endogeneity is not likely to be resolved, and thus empirical research should benefit most from putting endogeneity issues to the side and moving on to more serious problems.

2. Theory and the heightened awareness about endogeneity

Endogeneity is a concern because its presence affects the way in which a model should be estimated. Focusing on the estimation consequences of endogeneity implies an econometric definition of the problem. Accordingly, many econometrics textbooks offer a definition of endogeneity that concentrates on the (non-zero) correlation between the error term in the structural equation and any explanatory variables. While it is mathematically trivial to demonstrate a bias in estimators when the orthogonality condition between structural disturbances and explanatory variables is not fulfilled, identifying such a bias does not further our understanding of endogeneity. Indeed, crucial questions remain unanswered. For instance, what is the source of the bias, what is the substantive meaning of the correlation between the error term and the independent variables, and when is it innocuous to assume orthogonality? Without clarifying the economic or theoretical rationale implied by the orthogonality condition, endogeneity is likely to be considered an arcane, technical concern of little significance to empirical researchers.

A more intuitive definition of endogeneity is suggested by considering the actual practice of empirical research. Empiricists try to identify causal effects, which is done by measuring the effect of certain causes while holding all other causes in the model constant. Elsewhere I propose that one can follow Heckman (2000) and think of endogeneity as the case in which the *ceteris paribus* condition is not fulfilled whenever the relation of interest is examined (Nikolaev and van Lent 2005). Establishing whether the *ceteris paribus* condition holds is not clear-cut, however, especially when the causes cannot be independently varied. For instance, *ceteris paribus* is too strong an assumption when *simultaneity* or *omitted variables* are present. Consider the following two examples.

Suppose first that you want to investigate the relation between a division manager's leadership style and the investment opportunity set of the division. The hypothesis is that more open, democratic, and empathizing division managers will be more effective in encouraging their employees to find new, profitable investments. Yet, it may be the case that corporate headquarters put managers with an open, democratic style in charge of divisions with growth opportunities. If so, then examining the effect of leadership style on a division's investment opportunity set does not fulfill the *ceteris paribus* condition because variations in investment opportunities will immediately feed back to the leadership style of the division managers.

Consider next the case in which you are interested in the effect of establishing inter-firm networks or alliances on financial performance. The idea is that firms reap efficiency gains and other benefits from becoming a network partner and such firms are significantly more profitable than non-network firms. Suppose the researcher omits from his model the variable "market competition." Because firms in a very competitive market tend to observe lower economic performance and thus, in an effort to cope with competitive pressures, they are likely to form alliances, we cannot examine the effect of varying "networks" on "financial performance" as it is unreasonable to assume that "market competition" is unrelated with "networks." Therefore, when causes are interrelated, establishing the causal effect of one, holding everything else equal, is a difficult feat.

Econometricians advise the researcher to apply introspection (Wooldridge 2002) to assess whether endogeneity affects a model's estimation or to convince peer reviewers that the model is "reasonable" (Greene 2000; Kennedy 2003). Introspection renders

endogeneity a subjective matter. It also may explain at least in part the growing awareness of endogeneity in management accounting research, as peers routinely rely on prior research to evaluate whether they think a model is reasonable.

Since the early 1990s, theoretical work on the economics of organizations has been increasingly insistent about the need to think of firms as systems of interrelated attributes (Milgrom and Roberts 1992; Smith and Watts 1992; Holmstrom and Milgrom 1994; Milgrom and Roberts 1995; Brickley, Smith, and Zimmerman 2001). The major innovation in this line of inquiry is that the economic analysis of organizations does no longer require that a model's problem have "well-behaved" features such as smoothly concave objective functions and convex choice sets to conduct comparative statics. As Roberts (2004) points out, there is no reason to believe that these traditional conditions hold when analyzing organizational design problems. For example, increasing returns to scale, learning effects, and indivisibilities are all incompatible with concave objective functions. New methods of comparative statics allow economists to deal with large numbers of choice variables without assuming concave objective functions. Instead, the key assumption is that the choice variables interact. In the context of organizational design, choices, or *attributes*, such as a firm's strategy and structure are assumed to be linked to one another and in turn to change together in response to an environmental change. Because some organization design choices work best in concert with other choices (i.e., are *complementary*), "coherent clusters" emerge. For example, a make-to-order strategy complements both extensive communication between the Sales and Manufacturing departments and management accounting systems that support lateral flows of information (Bouwens and Abernethy 2000). Hence, we should observe these

organizational design choices together, and, as a consequence, it makes little sense to model one choice as the causal determinant of the other.¹

From the above discussion, it is quite easy to understand why endogeneity has increasingly become an issue in applied work. Recall that endogeneity problems are said to exist if the *ceteris paribus* condition does not hold. To the extent that complementarities underlie organizational design choices, it is unreasonable to consider the relation between two choices within a coherent cluster in isolation, holding all other choices constant. Similarly, one cannot conclude causality if we observe covariance across clusters of choices (since clusters are likely to respond to a third, exogenous factor). Ideally, therefore, empirical researchers should explore the interactions among all the choice variables that make up a coherent cluster and document how different clusters of choices influence outcomes. However, given the econometric problems with implementing such a research design, it is unreasonable to expect that this can actually be done.

Partial relief may come from the growing evidence that some elements of organizational design are “slow moving,” such as the assignment of decision rights within firms (Abernethy and Lillis 2001; Nagar 2002; Abernethy, Bouwens, and van Lent 2004), whereas others can be more easily adapted to environmental and strategic changes. Firms use strategy and the more malleable elements of formal design, e.g., performance measurement and the information system, to respond to changes in markets or technology (Roberts 2004; Bouwens and van Lent 2006). Thus, while the elements of an organization’s design will interact, it is likely that the impact of the malleable elements

¹ On the other hand, exploring how “clusters of choices” respond to exogenous variables (e.g., environmental changes) is a line of inquiry that deserves closer attention.

on the inert elements is of second-order importance. This should allay endogeneity concerns researchers have when using slow-moving elements of an organization's design as explanatory variables in regressions.

However important these theoretical developments may be, they are of limited use to management accounting researchers in practice—at least when it comes to “solving” the endogeneity problem. In essence, exhortations to incorporate relevant theory are somewhat disingenuous because we “have a lot of theory, by social science standards, but [we] still run a substantial deficit in degrees of freedom” (Bartels 1990). Simply put, theory is just not specific enough to ensure whether endogeneity is present or absent in a given context and I doubt whether theory will ever achieve a level of development in which it will admit unequivocal judgment on this question. No wonder some econometricians have near-existentialist doubts about what can be learned from the data we collect:

“There is little question that the absence of completely defined models impinges seriously on the usefulness of data in economics. On pessimistic days I doubt that economists have learned anything from the mountains of computer print-outs that fill their offices. On especially pessimistic days, I doubt that they ever will. But there are optimistic days as well.” (Leamer 1983, p. 325)

In sum, theory will not solve the endogeneity problem. At best, it will cast suspicion on certain specifications and will question whether some variables can be analyzed without modeling their effects on other variables as well. Yet, any such suspicion is rather unhelpful since theory is unlikely to provide convincing evidence that endogeneity does indeed affect the analysis.

3. Endogeneity as a regression specification problem.

Suppose you accept that theory in the field of management accounting is too incomplete, contested, and underdeveloped to offer certainty about the correct specification of the structural model of interest, which, after all, is supposed to be the formal reflection of theory. The next mental step will then be easy to take: If we are unsure about what the true structural model is, it is a bit silly to expend much effort worrying about the (endogeneity bias in) *particular* parameter values of whatever empirical translation of the structural model we happen to be estimating. That is, theoretical uncertainty must lead to uncertainty about the structural model. And if researchers are uncertain about the structural model (in the case of management accounting, they should be), then they need to investigate plausible alternative specifications and report how inferences change with these specifications. Sensitivity analyses are currently mostly presented as checks on empirical, operational choices. For instance, researchers compare the behavior of one proxy variable with another, or they show how the results of maximum likelihood estimation differ from those of generalized method of moments. Far less often, however, do researchers reports sensitivity analyses that explore different plausible specifications of the structural model.² Much can be learned by showing that several plausible structural models produce similar results or by discussing how seemingly small changes in the specification of a structural model lead to substantively different conclusions (Bartels 1990).³

² Clearly, robustness of the results against different operational choices (e.g., when using two different proxies for the same variable in the structural model) is something to aim for. Here I mean to suggest that the nature of sensitivity analysis across structural models is distinctly unlike that of the more common robustness checks, as in this case the aim is to find plausible models that produce significantly different results.

³ The literature suggests statistical procedures to take specification uncertainty into account (Bartels 1997). Intuitively, the results from a variety of different model specifications are averaged while discounting the data-mining effect of estimating many different specifications, especially if the results tend to change significantly with the specification.

Routinely adding even more sensitivity analyses to papers may make for unwieldy papers and unhappy editors. Perhaps even more importantly, it will bore readers to death. With respect to the former, the best way to keep the length of papers in check while acknowledging theoretical uncertainty is to distinguish between two sets of variables, namely, those we care about and those we include in the structural model as controls (Nikolaev and van Lent 2005). Then, because the goal of most researchers is usually not to pin down the exact, true specification of the structural model, but rather to learn about the relations among a limited set of key variables, we can relay concerns about the potential endogeneity among the other variables in the “Limitations and future research” section of the paper, reserving specification-robustness tests to the key variables.⁴

With respect to not distracting readers to the point of boredom, my perception is that many readers currently skip the “sensitivity section” in papers because it usually does not provide any new information. Typically, authors report that they have done many things, but whatever they did, “results remained qualitatively unchanged.” That the results do not change in the robustness checks in most papers has less to do with the soundness of the research as with self selection. If authors cannot show that results remain “qualitatively unchanged,” reviewers and editors tend to take that as their cue to reject the paper. I feel it is time to change this practice and ensure that the sensitivity

⁴ If there are no valid instruments available, sensitivity tests can also be helpful. For illustrative purposes, consider the following simple case. Suppose we have two variables, x and y , which are inter-related: $y = \alpha x + \varepsilon$ and $x = \beta y + \nu$. In the absence of a valid instrument variable, we cannot estimate α , but if the researcher has a good idea about the range of reasonable values of β (and if $\text{cov}(\varepsilon, \nu) = 0$), it is possible to explore how α behaves at different values within this range. By using $\nu = x - \beta y$ as an instrument for x , α can be identified. If α does not change much or retains its sign at different “reasonable” values of β , then we can be much more comfortable about the results.

section becomes one of the most interesting parts of empirical papers. Authors should be pressed to try something different *until results change significantly*. By reporting what plausible alternative specification of the structural model yields different results, we start to appreciate much better what exactly the limits of our knowledge are. Suppose a researcher believes “decentralization” is positively associated with the “use of non-financial measures.” She finds support for this relation in several plausible alternative structural models, but when she adds “information asymmetry” as an additional explanatory factor among the non-financial measures, its association with decentralization disappears. Should this be a reason to reject the paper? Scientific progress is probably better served by reporting the result and leaving the matter to future research, as the finding suggests the need for further exploration of the role of information asymmetry and its relation with both decentralization and the use of non-financial measures. What’s more, surprising outcomes from these sensitivity tests may be sufficiently interesting to warrant publication. It will take courageous authors, reviewers, and editors to implement this suggestion. But given the aims and scope of European Accounting Review, I think this journal can be a good place to start.

4. Concluding remarks

Whereas many in the Communist Party *publicly* supported the party stance on rock music, a good number of them also enjoyed listening to it in private, especially if it was not “melodious, meaningful, or well performed.” Many empirical researchers will publicly agree that endogeneity is an important issue. The most confident among them, however, will ignore it—without worries—in their own research.

Theory in (management) accounting is neither strong nor unified enough to solve the endogeneity problem. And most likely it never will be. Further, econometrics does not offer any practical solutions to endogeneity bias. Given that we can never be (theoretically) sure endogeneity exists, and if we were to be sure of its existence, there is little we can do about it from a practical standpoint, does it make sense to worry about endogeneity? My answer is that we shouldn't expend great effort worrying about endogeneity, but we shouldn't fully ignore it either. Researchers need to consider endogeneity upfront and evaluate whether their research question is interesting or provoking even in the presence of potential endogeneity issues. Because endogeneity is just one dimension on which to judge the value of research, it may often be appropriate to disregard it to stimulate academic discussion or to initiate more research in a particular area. At the same time, researchers need to be careful about how they construe their results and should discuss alternative interpretations. Such a discussion should indicate how endogeneity is expected to affect parameter estimates (direction, size of the bias) as well as acknowledge the fact that in the absence of a true structural model (or a true theory), in all likelihood the model under consideration is misspecified. Pretending to know everything about the structural model's specification, while being ignorant about parameter values, is an untenable position (Bartels 1990). If we are always dealing with misspecified models, then it is the duty of conscientious empirical researchers to defend how they choose between alternative misspecified models (Leamer 1978; Bartels 1985). Importantly, authors should also show how results change when plausible alternative specifications of the structural model are used and such changes should be interpreted as an indication of the soundness, not the weakness, of the research design.

There will certainly be value to increasing management accountants' understanding about (the econometrics of) endogeneity. A potentially sobering note comes, however, from an experiment carried out by Donald Green, who asked a group of master students to each develop a regression model (choosing between just three potential independent variables) and estimate parameters using purpose-simulated data. He finds that the observed variability of the parameter estimates greatly exceeds what one would expect based on sampling error alone. What is particularly worrying though is that students who were told that one should specify models in a theoretically sensible way, but whose training also emphasized statistical testing theory and econometrics, were more likely to misspecify their models by including *endogenous* regressors and excluding causally relevant but statistically insignificant variables (Green 1990). Let's hope that learning more about the econometrics of endogeneity does not cause management accounting researchers to abandon common sense.

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