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Correcting for Self-selection Based Endogeneity in Management Research: Review, Recommendations and Simulations

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Correcting for Self-selection Based Endogeneity in Management Research: Review, Recommendations and Simulations

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Abstract

Foundational to management is the idea that organizational decisions are a function of expected outcomes; hence, the customary empirical approach to employ multivariate techniques that regress performance outcome variables on discrete measures of organizational choices (e.g., investments, trainings, strategies and other managerial decision variables) potentially suffer from self-selection based endogeneity bias. Selection-effects represent an internal validity threat as they can lead to biased parameters that render erroneous empirical results and incorrect conclusions with regard to the veracity of theoretical assertions. Our review of the empirical literature suggests that selection-effects have received increasing attention in both micro- and macro-based research in recent years. Yet even when researchers acknowledge the issue, the techniques to correct for selection-effects have not always been employed in the proper manner; thus, estimations often suffer from shortcomings that potentially render flawed empirical findings. We explain the nature of self-selection based endogeneity bias and review the techniques available to researchers in management to correct for selection-effects when organizational decisions are discrete in nature. Furthermore, we engage in Monte Carlo simulations that demonstrate the tradeoffs involved with alternative techniques.

Keywords

Endogeneity, Endogenous Treatment, Heckman, Selection Effects, Switching Regressions Model

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Fundamental to management research is the ambition to make causal claims; yet, our discipline infrequently allows for the randomized experiments that represent the gold standard for making strong causal inferences (Antonakis, Bendahan, Jacquart, & Lalive, 2010). While Bloom et al. (Bloom, Eifert, Mahajan, McKenzie, & Roberts, 2013; Bloom, Liang, Roberts, & Ying, 2014) represent rare examples where such experiments have taken place, Bascle (2008) points out that randomized controlled experiments in the managerial context are often infeasible due to reasons of ethics, expense, and unwillingness of managers and businesses to be randomly placed into treatment and control groups. Accordingly, management researchers customarily rely on observational data sets and regression models where independent variables cannot be exogenously manipulated (Li, 2012). Yet, such empirical contexts yield far greater potential for endogeneity bias to manifest, as pointed out by a number of scholars (e.g., Antonakis et al., 2010; Larcker & Rusticus, 2010; Semadeni, Withers, & Certo, 2014) who have attempted to prod the varied business literatures into being more aware of the endogeneity issue.

Endogeneity bias renders coefficient estimates from standard regressions causally uninterpretable as the estimates will be inconsistent in the sense that they do not converge to the true coefficient values; that is, the estimates derived when the sample size approaches the census of activity. Wooldridge (2002) outlines and clarifies the three sources of endogeneity bias: measurement error, simultaneity, and omitted variables. Measurement error in variable constructs can both attenuate and bias the effect of regression estimators, while simultaneity occurs when one of the predictors is jointly determined along with the dependent variable (Li, 2012). Yet as Bascle (2008) points out, omitted variables have received the greatest amount of attention by management scholars as the principal source of endogeneity. Omitted-variable bias arises when an omitted—or latent—factor exists which both affects the dependent variable and is correlated with one or more explanatory variables. In essence, such a condition ensures that included regressors will correlate with the error term—as variation in the latent variable will manifest in the error term—and this violates the most important of the ordinary least squares (OLS) assumptions (the exogeneity assumption): that is, that the error term has an expected value of zero given any explanatory variable (Wooldridge, 2013).

While the greater issue of endogeneity has received a good deal of attention by scholars attempting to improve the methodological practices in management (e.g., Antonakis et al., 2010; Larcker & Rusticus, 2010; Semadeni et al., 2014) and in related fields and disciplines (e.g., Bollen, 2012; Duncan, Magnusson, & Ludwig, 2004; Foster & McLanahan, 1996; Gennetian, Magnuson, & Morris, 2008), the endogeneity issue is quite extensive and involves a number of different subdimensions that more general studies of endogeneity are not able to cover in extensive detail (see Antonakis et al., 2010 for an exception). One particular subdimension of the greater endogeneity problem that is particularly salient for research in management is the self-selection-based endogeneity issue (Hamilton & Nickerson, 2003; Shaver, 1998). Self-selection-based endogeneity in the management context manifests when the researcher faces discrete explanatory constructs that are endogenous in nature as they represent organizational decisions that are selected into with performance and outcome implications in mind. Accordingly, it is the exact nature of self-selection bias and the methods that are employable to correct for this form of endogeneity that will be the focus of this article.

In analyzing an important subset of the greater endogeneity problem, we aim to provide empirically minded researchers in management clear guidelines with regard to choosing between the different methodological techniques and correctly estimating these procedures for dealing with self-selection-based endogeneity. Our modern contribution attempts to provide value for empirical researchers in management facing the potential issue of self-selection bias as well as the related issue of sample-selection bias. To deliver on these aims, we first review the state of the management literature with respect to its awareness and proficiency in dealing with selection-based biases. Next, we set out and consider the nature of selection-based endogeneity problems in some detail, after which we outline the different methods—with the attendant advantages and disadvantages—available for

researchers to deal with self-selection bias. We provide some practical advice for estimating these procedures so as to ensure that best practices are employed and that the methods are correctly used. Finally, we present an empirical demonstration of the self-selection issue and the trade-offs involved with the different approaches via Monte Carlo simulations before concluding.

Selection-Based Endogeneity in the Management Literature

While Heckman (1976, 1979) first observed that neglecting selection represents a specification error that is akin to the omitted-variable bias problem, the relationship between selection bias and endogeneity bias has not always been well appreciated by management scholars (Echambadi, Campbell, & Agarwal, 2006). Selection bias can be considered a subform of the omitted-variable bias issue as the selection process represents an excluded variable that manifests in the error term and correlates with the endogenous choice construct and the outcome variable (Antonakis et al., 2010). Heckman's (1976, 1979) seminal contribution involves modeling the selection process as a truncation problem so as to introduce a variable—the inverse Mills ratio—into the substantive equation of interest to correct for the selection bias. In essence, this variable—which captures and corrects for the selection process—is missing when standard regression techniques are employed. Moreover, Antonakis et al. (2010) make clear that omitted selection is an important source of endogeneity bias in management research.

Selection-based endogeneity manifests in two main forms: sample-selection and self-selection biases. Heckman's (1976, 1979) foundational work was principally motivated by sample-selection problems, as samples can be nonrepresentative of a true population and thus threaten both internal and external validity—see Berk (1983) for an excellent review of this issue. Yet Heckman (1979) was also conscious of the analogous self-selection problem (our focus here), as he observed that comparing the wages of management trainees with the wages of nontrainees can result in biased estimates of treatment effects. In the self-selection context, bias derives not from sample selection (i.e., no bias exists in the scope of the sample being studied) but instead the studied agents make choices regarding assignment into the mutually exclusive treatment and nontreatment groups based on unobservables that correlate with both outcomes and observable predictors. For example, worker traits that are unobservable to the researcher (e.g., diligence, innate intelligence, etc.) may determine both the selection into management trainee programs and future wages, thus a parameter estimating the relationship between participation in a trainee program and the future wages of a manager can be confounded with the selection process into that trainee program. In fact, Wooldridge (2002) deems self-selection to be a common source of omitted variable bias in empirical work being done in the behavioral and social sciences.

Self-selection-based endogeneity clearly represents an internal validity threat to research throughout management, as the decisions, processes, trainings, investment types, strategies and other discrete business phenomena which we study are not random but instead chosen (i.e., selected into) by managers with outcomes in mind. Thus, our focus here on self-selection biases and the methods that are employable to correct for this form of endogeneity. Yet our contribution does not represent the first work to point out that the management discipline should be mindful of the dangers involved for researchers when organizational decisions are self-selected. Shaver (1998) first decisively observed that firms choose strategies based on their attributes and implications, therefore organizational choices are endogenous and self-selected. Shaver cautions that the customary approach in management to regress performance measures on strategy choice variables could lead to misspecification and incorrect positive and normative conclusions by failing to account for self-selection effects. Shaver prescribed the Heckman (1979) procedure as an effective approach to deal with issues of self-selection in organizational decisions.

Influenced by Shaver's (1998) seminal study, Hamilton and Nickerson (2003) make clear that the management discipline is particularly subject to self-selection-based endogeneity, as the discipline's fundamental premise is that managerial decisions are endogenous to their expected performance

implications. In other words, managers do not make organizational decisions—such as appropriate organizational form, use of markets or hierarchies, and type of investment undertaken—on a random basis. Instead, the outcomes—or performance implications—of these decisions are central to the ultimate organizational decision taken. Omitted variables are then likely to affect both the organizational decision and the performance outcome, thus rendering biased the coefficient estimates from standard regressions of these decisions on performance outcomes. In essence, both Shaver (1998) and Hamilton and Nickerson (2003) point out an inherent contradiction involved with the fact that empirical work in management seldom controlled for self-selection effects; namely, the existence of our discipline is predicated on the idea that managerial decisions are endogenous to expected outcomes (i.e., organizational decisions are not best characterized as randomized processes), hence self-selection effects should actually be both endemic and fundamental to our empirical studies.

Hamilton and Nickerson's (2003) survey of the *Strategic Management Journal* (*SMJ*)—which arguably best represents strategy research in general—for the years 1990 through 2001 delivered some sobering findings that suggested a neglect of selection effects and endogeneity in general. In particular, only 27 out of 196 empirical articles on firm performance actually corrected for endogeneity issues; and only 3 of these articles used some type of instrumental-variable technique (Bascle, 2008). We take off from the Hamilton and Nickerson (2003) review by surveying *SMJ* for the years 2002 through 2014 to gather whether variants of the Heckman (1979) technique have been employed by strategy scholars to deal with the evident selection effects in this field. We find that 47 studies in *SMJ* attempt to correct for potential sample-selection effects (Appendix A), while 55 studies attempt to correct for potential self-selection effects over this period (Appendix B). Accordingly, the issue of selection-based endogeneity has clearly received increasing attention in the period following the Shaver (1998) and Hamilton and Nickerson (2003) studies.

While a number of empirical studies in strategy have adopted methods to correct for selection bias, these studies are often characterized by estimation-strategy defects that potentially render uninformative empirical results. Our review of the literature suggests that the estimation strategies employed by many studies fail to properly identify the selection equation, as constructs that are unique to the selection equation are often not specified; that is, 8 of the 55 self-selection studies clearly do not involve identification variables in the selection equation, while an additional 13 studies potentially do not involve identification variables in the selection equation. Yet even when identification variables are specified, the rationale behind the identification assumption (i.e., why the variables impact selection but not the main equation of interest) is oftentimes not set out by the authors. This inability to confidently conclude accurate identification of selection effects is compounded by the fact that many studies do not report the results of the Heckman procedure or of the selection equation; instead, such procedures tend to be auxiliary tests that are simply reported to editors and referees and are accordingly repressed from final publication.

Despite the efforts to prod strategy scholars to adopt the appropriate methods to deal with selection-based endogeneity, it is fair to say that empirical work in the field still does not consistently employ the correct methodological practices. The correct procedures to deal with selection effects do not appear to have deeply penetrated the strategy literature in that many of the empirical studies addressing selection effects do so in a manner that falls short of best practice. Antonakis et al. (2010, 2014) make clear the dangers involved with such practices as violating essential design and methodological conditions might mean that yielded coefficient estimates cannot be interpreted as even indicating correlation nevertheless causation. Accordingly, the procedures to deal with selection effects are the tools we have to deal with naturally occurring processes that result in truncated or selected samples, and it bares stressing that these tools are not means to compensate for other design issues.

While the above survey indicates the degree to which the procedures to deal with selection effects have been properly undertaken when employed in the empirical strategy literature, an additional question involves the degree to which empirical researchers have neglected the existence of

self-selection effects when they are potentially present. Furthermore, the potential for self-selection bias resides in empirical work throughout the management discipline: that is, both in macro-based (e.g., *SMJ*) and micro-based (e.g., *Administrative Science Quarterly [ASQ]*) scholarship. Accordingly, we reviewed all of the articles published in *SMJ* and *ASQ* in 2014 to establish the degree to which self-selection effects are potentially present in recent empirical scholarship, and whether that scholarship has tended to be conscious of and address any underlying self-selection issues. In particular, we found—reported in Appendix C—that self-selection issues were potentially present in 40 of the 117 articles published in *SMJ* in 2014; yet, self-selection was specifically addressed by the authors in only 18 of these 40 articles (i.e., self-selection was addressed then in only 45% of the cases where it potentially manifests). We also found—reported in Appendix D—that self-selection issues were potentially present in 11 of the 20 articles published in *ASQ* in 2014; yet, self-selection was specifically addressed by the authors in only 5 of these 11 articles (i.e., self-selection was addressed then in only 45.5% of the cases where it potentially manifests).

It appears then that both macro- and micro-based empirical scholarship tend to neglect the presence of self-selection biases in half of the studies where these issues potentially manifest. The frequent neglect of self-selection issues by the macro-scholarship in *SMJ* is particularly troublesome, as the influential articles by Shaver (1998) and Hamilton and Nickerson (2003) specifically targeted the field of strategic management in outlining the nature of selection-based endogeneity and providing prescriptions with respect to correctly addressing this issue by modeling the selection process. Accordingly, if correct procedures to deal with self-selection-based endogeneity are to have penetrated the management discipline, then one might have expected the strategy field to best exhibit the adoption of such methodological approaches in response to these notable urgings. In other words, strategic management should seemingly be at the frontier within management when it comes to correctly addressing self-selection-based endogeneity; yet, our review of empirical scholarship does not find this to be the case.

As to what might partly explain the inconsistent and inadequate attempts by empirical scholarship in the management literature to correct for selection-based endogeneity issues, a few potential causes seemingly arise. First, the prescriptive literature in management has advocated different techniques—for example, Shaver (1998) proffers the Heckman procedure, Hamilton and Nickerson (2003) proffer Lee's (1978, 1982) switching-regression procedure, and Basile (2008) argues that an IV approach can handle many self-selection issues—which potentially leaves researchers unsure as to the appropriate method(s) to employ for a particular empirical context. Second, the prescriptive literature in management arguably has not provided sufficient and clear information on the inevitable upsides and downsides involved with the alternative methods to deal with selection bias; thus, the lack of studies engaging in comparisons of the different methods means that researchers will again be unsure as to which particular method is most appropriate for a particular empirical context. It stands to reason then that the literature within management dealing with selection-bias issues could gain from an integrative study that helps researchers understand the correct approach to take to deal with selection effects in the varied empirical contexts which manifest. This is particularly the case since making the appropriate decision with regard to correcting for selection bias depends on the available data and the precise research question being faced by the researcher.

The Self-Selection-Based Endogeneity Problem

Before delving further into the issue of self-selection bias, it is important to provide further grounding on the related issue of sample-selection bias, as this sets the basic logic for more complex models of selection. Sample-selection bias can be present when the researcher uses nonrandomly selected samples to estimate causal relationships. The issue often arises in practice for two reasons: when the observational units make decisions so that a subset of a particular population is not observed and

when samples of observational data involve some selection by analysts and data processors (Heckman, 1979). Accordingly, empirical situations where the outcome variable is only observed for a portion of the true sample often yield sample-selection issues as a censoring in the data is present. For instance, Heckman (1974) observes—in his motivational application piece—that estimating the return on education for women involves sample-selection issues and biased coefficient estimates because the researcher only observes outcomes (i.e., wages) for working women. Yet the decision to work (i.e., to select into employment and thus select into a sample) is surely endogenous and possibly driven by some of the same omitted drivers of an individual's wage. Accordingly, the estimated return on education in such a selected sample would be biased and unrepresentative of the average return on education in the entire population of women. It should be reiterated that for the selection to generate biased coefficient estimates it is crucial that nonrandomness exists in the data-construction process. Thus by excluding some observations from a population of activity in a systematic manner, the estimates that are actually inferred from the biased sample might actually be a product of random perturbations (Berk, 1983).

Self-selection bias is of a different nature than sample-selection bias, as in these empirical contexts there are no issues with regard to the dependent variable not being observed for relevant subsamples of the population. Instead, the self-selection concern arises when the dependent variables are observed for different subsamples, yet a nonrandomness is involved with the manifestation of these dependent constructs. Staying with the illustrative context of worker wages, Lee (1978) studies the impact of unionism on wages where he has data on the wages of workers who are either unionized or nonunionized; hence, sample-selection bias is seemingly not at play. Yet workers make decisions as to whether they join a union or not; moreover, this decision is endogenous in the sense that unobserved factors (e.g., intelligence, connections, etc.) can affect that decision to join the union, but such unobserved factors can also affect future wages. Lee (1979) summarizes the above when he states that in such cases “decisions are based on the possible outcomes under alternative choices and observed outcomes are final outcomes of the decision process. So decisions and outcomes are interrelated” (p. 977). Yet if standard regression techniques were to be employed in such an empirical context, the error terms would violate the necessary assumption of being uncorrelated with the explanatory variables.

This self-selection problem boils down, in essence, to the problem of treatments not being randomly assigned to the agents being studied in observational data sets (Antonakis et al., 2010). Instead of being randomly assigned into treatment and nontreatment groups, the organizational choices we study in management are a function of managers attempting to enhance outcomes. Since treatments are not randomly assigned to groups of agents, untreated observational agents do not necessarily represent adequate counterfactuals with respect to treated observational agents. The root of the problem is that observational agents (e.g., managers, firms, transactions) experiencing the treatment (e.g., engaging in a particular strategy) might substantially differ from those agents not experiencing the treatment in terms of observable and unobservable factors. It is, of course, the unobserved characteristics which can yield pernicious effects in terms of biases as they are subsumed in the error term. Under such conditions, it is quite possible that the residual in the selection equation into the treatment will correlate with the residual in the main equation of substantive interest. Therefore, the treatment (e.g., a particular strategy choice) will be correlated with the residual term in the main equation. It is in this sense that selection-based endogeneity represents a subset of the omitted-variable problem, as omitted variables in the residual will correlate with the treatment—a violation of the assumption residing behind standard regression techniques.

We can provide a grounded example of how omitting selection can be problematic and lead to biases when self-selection is present. Let us consider, for example, the overall performance implications (e.g., the return on assets [ROA]) for firms when deciding to enter new market niches. King and Tucci (2002) engage in such a study and are fundamentally interested in whether entry into new product areas positively or negatively affects a firm's ROA (i.e., performance). Yet firm entry is a

choice variable by managers (i.e., not randomly assigned by the researcher), thus it is quite possible that unobservable factors (e.g., the intangible “dynamic capabilities” of firms) exist that determine whether a firm chooses to enter a new market segment or not. Thus, the entry decision is explained by other factors that are not observed in the substantive equation of interest. In essence, an additional “selection equation” explains the product entry decision of firms; that is, that binary decision by organizations to obtain treatment. If, however, those latent factors (i.e., those unobserved dynamic capabilities) affect both the outcome variable in the substantive equation as well as the treatment in the selection equation, then the greater self-selection problem is that in such an empirical context the error terms of the two equations will be correlated, thus the entry decision will correlate with the error term in the substantive equation which violates the strict-exogeneity assumption.

Heckman (1979) observes that under selection bias the researcher often finds variable constructs to be significant in the main equation when these constructs actually belong in the selection equation. It stands to reason then that unbiased coefficient estimates can result from over-specifying the substantive equation in the sense that the researcher includes all of the relevant variables which influence both the ultimate dependent variable and the selection into the treatment (Hamilton & Nickerson, 2003). Yet fully capturing all relevant variables represents a challenging task and any remaining latent sources of variation (i.e., omitted variables) which manifest in the error term and are correlated with the treatment represent direct validity threats to causal inferences. In this context, moreover, the omitted factor is not just some simple omitted construct—as is custom with general endogeneity concerns—which happens to correlate with the dependent and independent variables, but instead it is the selection process itself which is omitted from the model. Going back to our use of counterfactuals to provide an intuitive grounding of self-selection bias: the fundamental issue here is that without controlling for the selection process, one cannot use firms that decide not to enter a new market niche as adequate counterfactuals to estimate the performance impact of those firms that do decide to enter a new market niche.

Endogeneity based on self-selection has the potential to undermine a great deal of the empirical literature residing within the management discipline. Since much of what we study involves how organizational decisions impact performance outcomes of one sort or another, the selection into these organizational decisions—and the attendant potential for biases in coefficient estimates—is intrinsic to our research. Furthermore, selection-based endogeneity does not affect coefficient estimates in a consistent manner, as both inflation and deflation of treatment effects can materialize and even the sign of coefficients can be reversed (Berk, 1983). Accordingly, researchers (e.g., Modrek & Cullen, 2013) cannot make the claim that their hypothesized results hold despite the presence of uncorrected selection. It is also important to underscore that not only will the coefficient estimates of variables subject to selection be inconsistent in standard regressions, but there are also potential negative spillover effects to other right-hand-side constructs. In particular, regressors that correlate with variables subject to selection-based endogeneity will also find their coefficient estimates being adjusted to the extent that they correlate with the problematic variables (Antonakis et al., 2010). The greater danger is that researchers neglect self-selection as a fundamental source of endogeneity and that by doing so biased parameters result, which leads to erroneous and inconsistent findings and ultimately incorrect conclusions with regard to the veracity of certain theories (Hamilton & Nickerson, 2003).

Alternative Methods to Deal With Selection-Based Endogeneity

Only by properly modeling the selection process can a researcher correctly deal with selection-based endogeneity issues. The choice of the correct modeling procedure, however, requires a clear understanding of both the source and the specific nature of the endogeneity problem. We thus more formally set out here the different sources of selection-based endogeneity bias and match these sources with appropriate methodological procedures that might be employed to deal with such selection

effects. The different methodological procedures are based on Heckman's (1974, 1976, 1979) seminal contribution—which led to his 2000 Nobel Prize in economics—yet it is important to underscore that these different selection models vary both in terms of motivation and implementation. We first consider the issue of sample-selection bias (and the attendant Heckman procedure to deal with this bias) before turning to the more nuanced issue of self-selection-based endogeneity.

Sample Selection

As previously mentioned, the sample-selection problem reduces to a censoring issue, as the outcome variable is only observed for a subsample of the population. Heckman's (1974) seminal work considered this issue as he pointed out that sample selection can be viewed as a sort of omitted-variable problem. Heckman's basic idea to deal with this issue was to first employ instruments that predict selection into a treated group (i.e., estimate a proper selection equation) and then via that procedure create a hazard-rate variable that captures the likelihood of observing the outcome; that is, the variable captures the difference between the sample for which we observe the outcome and the sample for which we do not observe the outcome. In a second step, he proposes to introduce this variable—the so-called inverse Mill's ratio—into the substantive equation of interest. This allows the researcher to control for selection-based variance—variance that would otherwise manifest in the error term. By engaging in such a two-step procedure, a researcher can obtain unbiased coefficient estimates when sample-selection issues are present. To make correct inferences, however, it is crucial to have unbiased estimates for the standard errors. Yet, the covariance matrix estimated by OLS in the second stage is inconsistent as one of the regressors, the inverse Mill's ratio, is an estimated construct. Hence, crucial to making correct causal inferences with such two-step procedures is the need to consider the appropriate estimation of the variance-covariance matrix—a topic we will discuss extensively. Empirical researchers often neglect, unfortunately, the issue of what is the appropriate estimation of the variance-covariance matrix, thus leading to potentially misleading inferences.

Let us now formalize Heckman's argument which will help us more precisely discuss the appropriate econometric approach to this issue. Researchers are often interested in investigating how the exogenous and observable characteristics (\mathbf{X}_i) of a particular agent i affect an outcome variable Y_i . Thus, they often aim to estimate the following model:

$$Y_i = \mathbf{X}_i' \boldsymbol{\alpha} + \varepsilon_i \quad (1)$$

In cases where the data is censored, the outcome Y_i is only observed in one subsample of the population, $z_i = 1$. Heckman (1974) proposes to model this process via a latent variable approach which assumes that the outcome of interest is observed if and only if an unobserved latent variable (z_i^*) exceeds a particular threshold:

$$z_i = \begin{cases} 1 & \text{iff } z_i^* > 0 \\ 0 & \text{otherwise} \end{cases} \quad (2)$$

Moreover, the latent variable can be expressed as a linear function of the observed (\mathbf{Z}_i) and unobserved (ω_i) characteristics of agent i :

$$z_i^* = \mathbf{Z}_i' \boldsymbol{\beta} + \omega_i$$

To estimate this model, the researcher has to make an (untestable) distributional assumption regarding the error term (ω_i). Generally, it is assumed that the error term is normally distributed with mean 0 and variance σ_{ω}^2 , which is normalized to one for the sake of identification. Thus, Equation (2) becomes a probit model and $\Pr(z_i = 1) = \Phi(\mathbf{Z}_i' \boldsymbol{\beta})$ where $\Phi(\cdot)$ is the cumulative distribution function (CDF) of the standard normal distribution.¹ If the outcome variable is only observed in the subsample $z_i = 1$, then the full model consists of Equations (1) and (2).

In such an empirical context, the sample-selection problem is due to ε_i being correlated with ω_i , thus X_i is correlated with ε_i in violation of the strict exogeneity assumption, that is, $E(\varepsilon_i|X_i) \neq 0$. More specifically, if the variance-covariance matrix of the full model is represented as follows:

$$\Omega = \begin{pmatrix} \sigma_\varepsilon^2 & \rho\sigma_\varepsilon\sigma_\omega \\ \rho\sigma_\varepsilon\sigma_\omega & \sigma_\omega^2 \end{pmatrix},$$

where σ_ε^2 and σ_ω^2 are the variances of the error terms (and the latter is normalized to one) and ρ is the correlation coefficient between the two error terms, then $\rho \neq 0$ implies that OLS estimation of Model (1) is inconsistent. Indeed, latent sources of variation (i.e., omitted variables) which manifest in and are correlated with X_i represent direct validity threats to causal inferences. Yet in this context, the omitted factor is not just some simple omitted construct—as is custom with general endogeneity concerns—but instead the selection process itself is actually omitted from the model.

To formalize this reasoning, consider the OLS regression of Y_i on X_i and take the expectation of Model (1):

$$\begin{aligned} E(Y_i|X_i, z_i = 1) &= E(X_i'\alpha + \varepsilon_i|X_i, z_i^* > 0) = X_i'\alpha + E(\varepsilon_i|\omega_i > -Z_i'\beta) = X_i'\alpha \\ &+ \rho\sigma_\varepsilon \left(\frac{\varphi(Z_i'\beta)}{1 - \Phi(Z_i'\beta)} \right), \end{aligned}$$

where $\varphi(\cdot)$ is the standard normal density function. If the correlation between the error terms, ε_i and ω_i , is not zero (i.e., $\rho \neq 0$), then the selection into subsample $z_i = 1$ is not random and the OLS regression of Y_i on X_i would lead to biased coefficient estimates. In particular, the selection bias is equal to $\rho\sigma_\varepsilon \left(\frac{\varphi(Z_i'\beta)}{1 - \Phi(Z_i'\beta)} \right)$: the product of the covariance between the two error terms ($\rho\sigma_\varepsilon$), and a term measuring the likelihood that the observation is in the subsample $z_i = 1$. The latter term represents the inverse Mill's ratio; thus, consistent and unbiased estimation of the effect of X_i on Y_i requires taking this omitted factor into account. Yet, in the case where sample-selection is random (i.e., $\rho = 0$) then an OLS estimation will deliver consistent estimates of the parameters α .

It is also important to underscore that identification of the selection equation is of fundamental importance. As noted in our review of the literature, a number of empirical researchers employ Heckman-type procedures that do not involve unique identifiers in the selection equation. Such problematic estimations are possible because even when the set of explanatory variables in Equations (1) and (2) are identical (i.e., $X_i = Z_i$), all of the parameters (including ρ and σ_ε^2) can be identified via the functional assumptions regarding the bivariate distribution of the error terms. Yet, Maddala (1983) observes that this type of identification has proven to be quite poor and nontransparent. It is therefore essential that researchers employ additional exclusion restrictions (i.e., additional instruments) to better identify such models and, specifically, the selection mechanism.

In terms of estimation method, the researcher can choose between a limited information maximum likelihood (LIML) estimator and a full information maximum likelihood (FIML) estimator. First, by assuming that ε_i and ω_i follow a bivariate normal distribution, the researcher can estimate all of the parameters of interest via FIML by minimizing the following log-likelihood function:

$$\begin{aligned} \ln L &= \sum_{z_i=0} \ln(1 - \Phi(Z_i'\beta)) + \sum_{z_i=0} \ln\left(\frac{1}{\sqrt{2\pi\sigma_\varepsilon^2}}\right) + \sum_{z_i=1} \frac{1}{2\sigma_\omega^2} (Y_i - X_i'\alpha)^2 \\ &+ \sum_{z_i=1} \ln \Phi\left(\frac{Z_i'\beta + \rho\left(\frac{Y_i - X_i'\alpha}{\sigma_\varepsilon}\right)}{\sqrt{(1 - \rho)^2}}\right). \end{aligned} \quad (3)$$

Under this assumption, the FIML estimator is consistent and asymptotically efficient; though, it might deliver inconsistent estimates if the (untestable) assumption of bivariate normality does not hold.

The LIML estimation procedure is computationally less complex and more robust as compared to the FIML estimator, but LIML is less efficient if the null hypothesis of joint normality is valid. The less severe (but still untestable) assumption that the researcher needs to employ for this estimation procedure is that the random-error term in the selection equation, ω_i , is normally distributed. The LIML estimator from this model is the original two-step estimator proposed by Heckman (1974) and consists of estimating the probit model and computing the inverse Mill's ratio. After doing so, the researcher estimates Equation (1) via OLS with the inverse Mill's ratio added as an additional regressor; and, finally, the researcher should estimate a consistent variance-covariance matrix for Equation (1) to correct for the fact that the inverse Mill's ratio is a predicted explanatory variable.² This last step is critical and is often omitted by researchers—see, for example, the multivariate approach proffered by Hamilton and Nickerson (2003).

While a clear trade-off between efficiency and robustness is present when considering the choice between FIML and LIML, it is difficult to prescribe when the researcher should employ which estimator. If the sample is very small, then it might be advisable to focus on the LIML estimator, though this could lead to imprecise inferences as the results might indicate an insignificant relationship when the estimator is too inefficient. Hence, the most consistent and transparent approach would seemingly be to estimate and report both the FIML and LIML results.

The estimation of the Heckman model is, nowadays, quite easy to implement with common statistical packages. For instance, Stata allows readily estimating the Heckman model by both FIML and LIML procedures via the “heckman” command. This routine allows the researcher to specify several options with regard to the parameter estimates, and with regard to consistent estimations of the variance-covariance matrix in the presence of heteroscedasticity and clustering. In the statistical software R, the package “sampleSelection” contains the necessary tools to estimate sample selection models. We should also note that—to the best of our knowledge—SPSS cannot handle Heckman-type models, thus it does not represent a suitable statistical package to deal with selection-based endogeneity issues of any kind.

Self-Selection: Endogenous Treatment and Endogenous Switching

The self-selection issue—and the related endogenous-treatment and endogenous-switching problems—is akin to the sample-selection issue outlined above, but also involves substantial differences. While the appropriate procedures to deal with self-selection build on Heckman's fundamental contribution and the logic we discussed above concerning sample-selection, the nature of these concerns goes beyond sample-selection as the focal issues are no longer the mere censoring of data. In self-selection cases, the outcome variable is observed for an entire sample which is assumably representative of the population.³ However, a potentially endogenous “treatment” exists which partitions the sample population into two—or more—subsamples. Moreover, one must distinguish between two self-selection variants that we can define as “endogenous treatment” and “endogenous switching.” The main difference between the two is whether the researcher thinks that the treatment merely has an intercept effect on the outcome by shifting the regression line upward or downward (as in endogenous treatment), or whether this effect is also on the coefficient estimates by rotating the regression line (as in endogenous switching). Thus, a fundamental issue with regard to correctly dealing with self-selection bias involves diagnosing the way the treatment impacts the outcome.

Endogenous treatment. Suppose that we are still interested in the aforementioned relationship between agent i 's exogenous and observable characteristics (X_i) and the outcome variable Y_i .

However, a subgroup of the full sample is affected by a treatment which is measured by the dummy variable $z_i = 1$. This model appears to be very similar to the sample-selection model discussed above; though, an important difference is that we now observe Y_i not only when $z_i = 1$, but also when $z_i = 0$. Accordingly, we can pool the entire sample and include the treatment dummy as an explanatory variable in the main equation of interest. This model can be written as follows:

$$Y_i = \alpha_0 + \alpha_1 X_i + \alpha_2 z_i + \varepsilon_i \quad (4)$$

Since the entire sample is employed, sample-selection issues are not necessarily present here. Yet this model restricts the effects of the exogenous factors X_i (the α 's) to be equal for both subgroups. Thus, the effect of the treatment is to simply shift the regression line up or down (an intercept effect). The coefficient estimate for the focal dummy variable accordingly measures the average treatment effect (ATE); that is, the average difference of the potential outcome of the treated group as compared to the potential outcome of the group that has not been treated. In the case of this treatment being randomly assigned, the researcher could consistently and efficiently estimate this equation via OLS. But if the treatment was to be endogenous (i.e., the dummy z_i is correlated with the error term, ε_i), then such an estimation would be inappropriate and lead to inconsistent estimates. In many instances, this type of endogeneity problem can be viewed as a self-selection issue: where the observed agent endogenously chooses to be part of the treated group. As already noted, this process is related to omitted-variable bias as the fundamental source of endogeneity is that there are factors that the researcher cannot observe that affect both the self-selection process and the ultimate outcome variable.

This endogenous-treatment issue can thus be viewed as a classical endogeneity problem that can be dealt with by employing a standard instrumental variable (IV) framework. Specifically, instruments should be employed that explain participation in the treatment, but are also uncorrelated with the error term in the substantive equation. By using the prediction from the selection model instead of the true values of the endogenous treatment, the error term is cleaned of the variability due to self-selection and this allows a consistent estimate of the treatment effect (e.g., Antonakis et al., 2010; Maddala, 1983). The main issue to confront in this case, however, is that the potentially endogenous variable (i.e., the dummy variable z_i above) is not continuous but is instead discrete. If the researcher is willing to make an additional assumption and take a linear approximation to describe the process behind the variable z_i —what Wooldridge (2013) refers to as a linear probability model—then a simple two-stage least squares (2SLS) estimator could be adopted in such situations. The main advantage of using the linear probability model is that the estimated coefficients in the first stage become easily interpretable. In essence, Angrist (2001) and Basile (2008) submit that an IV approach can be employed in several contexts where self-selection-based endogeneity is present.

A number of disadvantages exist, however, with regard to employing a linear approximation 2SLS approach. For one, several observers (e.g., Hanushek & Jackson, 1977; Maddala, 1983) have noted that the linear probability model (i.e., least-squares regressions with a dichotomous or, more generally, a qualitative dependent variable) leads to coefficient estimates that do not have known distributional properties. Hence, standard statistical inferences—such as hypothesis testing and the construction of confidence intervals—are no longer justified in such a context; though, the bootstrapping of standard errors can help in these cases. Moreover, these coefficient estimates are sensitive to the observation range of the employed data and may accordingly understate or overstate the magnitude of the true effects; in addition, they may systematically yield probability predictions that reside outside the unity range (Wooldridge, 2013). Angrist (2001) and Abadie (2003) also warn that using a 2SLS IV procedure as a sort of “best linear approximation” to an underlying nonlinear model might be problematic and provide a

theoretical underpinning for their reasoning. Accordingly, they suggest a weighted least squares estimation procedure to make robust causal inferences on endogenous treatment effects in the presence of nonlinear models. In general, the most-prudent prescriptive advice that we can provide to researchers is that they be cautious when adopting a 2SLS approach to dealing with self-selection issues characterized by discrete outcome variables. It is then comforting to note that our review of the empirical literature in strategic management indicates that these types of procedures have been infrequently employed: only 5 of the 55 self-selection studies in Appendix B employ a 2SLS approach.

A potentially more appropriate means to deal with this self-selection—or endogenous treatment—issue is then to adopt a latent variable approach that is similar to the Heckman (1974, 1978) procedure discussed for treating sample-selection issues. Hence,

$$z_i = \begin{cases} 1 & \text{iff } z_i^* > 0 \\ 0 & \text{otherwise} \end{cases}, \quad (5)$$

and z_i^* is modeled as follows:

$$z_i^* = \mathbf{Z}'\boldsymbol{\beta} + \omega.$$

In such a context, it is again assumed that the variance-covariance matrix for Models (4) and (5) is the following:⁴

$$\boldsymbol{\Omega} = \begin{pmatrix} \sigma_\varepsilon^2 & \rho\sigma_\varepsilon\sigma_\omega \\ \rho\sigma_\varepsilon\sigma_\omega & \sigma_\omega^2 \end{pmatrix}.$$

It is easy to see how closely related the two models (one for sample-selection and the other for endogenous treatment in the context of self-selection) are, which makes it perhaps understandable that many researchers mix-up these two Heckman variants.

Similar to the case of sample-selection, this model can be estimated by means of both FIML and LIML estimators depending on the assumptions the researcher is willing to make with regard to the distribution of the joint error terms ε_i and ω_i . The FIML estimator requires joint normality and, under this hypothesis, is consistent and asymptotically efficient. The LIML estimator is consistent even if the joint-normality hypothesis fails, but LIML is inefficient if the null hypothesis of joint normality is valid. We should also note that estimations of this type can be quite easily performed via Stata by invoking the command “`etregress`,” and performed via R by the function “`selection()`” of the package “`sampleSelection`.”⁵

Endogenous switching. If the researcher thinks that the effect of the treatment is not merely a shift of the intercept but also involves differences in the relevant coefficient estimates (i.e., some or all of the $\boldsymbol{\alpha}$'s) differ according to the treatment), then an endogenous-switching model is necessary to employ. In essence, the endogenous-switching model resembles a “double” sample-selection process and can be represented as follows (e.g., Lee, 1978):

$$\begin{cases} Y_i^1 = \alpha_0^1 + \boldsymbol{\alpha}_1^1 \mathbf{X}_i^1 + \varepsilon_i^1 & \text{if } z_i = 1 \\ Y_i^0 = \alpha_0^0 + \boldsymbol{\alpha}_1^0 \mathbf{X}_i^0 + \varepsilon_i^0 & \text{if } z_i = 0. \end{cases} \quad (6a)$$

$$(6b)$$

The dummy variable (z_i) is again modeled as the dichotomous counterpart of a latent variable z_i^* and it holds that:

$$z_i = \begin{cases} 1 & \text{iff } z_i^* > 0 \\ 0 & \text{otherwise} \end{cases}. \quad (7)$$

The variance-covariance matrix is, in this model, more complex than the previous matrices and can be represented by:

$$\Omega^s = \begin{pmatrix} \sigma_{\varepsilon^0}^2 & \rho^{01} \sigma_{\varepsilon^0} \sigma_{\varepsilon^1} & \rho^0 \sigma_{\varepsilon^0} \sigma_{\omega} \\ & \sigma_{\varepsilon^1}^2 & \rho^1 \sigma_{\varepsilon^1} \sigma_{\omega} \\ & & \sigma_{\omega}^2 \end{pmatrix}.$$

The maximum likelihood (ML) estimates for this model can be shown to be consistent and asymptotically efficient; yet, the estimation may still be cumbersome because the likelihood function to minimize in this case is relatively complex.⁶ Compared to the previous models, this switching-regressions model is certainly more flexible as it allows coefficient estimates to vary across subgroups and thus estimate heterogeneous effects for each of the exogenous factors X_i on the outcome. These results are generally reported as the average treatment effects on the treated (ATET); that is, the average gain from treatment for those agents who were actually treated. It is worth noting that significant correlations between the error terms (ρ^0 and ρ^1) represent the real source of endogenous selection. As an aside, simply subsampling—or considering the interactions between the covariates and the treatment—does not solve the fundamental endogenous-selection problem, as doing so would still mean neglecting the correlation between the two error terms.

As with the previous models, the switching-regressions model can be estimated via FIML and LIML depending on the assumptions regarding the distribution of the error terms. In particular, researchers willing to assume a bivariate normal distribution can employ FIML to simultaneously estimate Equations (7), (6a), and (6b), as such an approach would be both consistent and efficient (Lee, 1979). To estimate a switching-regression model via LIML and FIML, one can do so with Stata by employing the “movestay” command, and with R by using the function “selection()” of the package “sampleSelection.”⁷

The switching-regression model can be alternatively viewed as a sort of “double sample selection” problem, as the researcher could separately estimate the model for the treated and nontreated observations by means of two Heckman sample-selection procedures.⁸ In this case, the researcher would first focus on the observed data from one subsample and estimate Models (6a) and (7); and then focus on the data from the other subsample and estimate Models (6b) and (7). While taking such an approach is feasible, the fact that only variation from within the subsample would be employed in the estimations indicates that it would be a less-efficient procedure as compared to estimating the entire model in a simultaneous manner. Furthermore, undertaking two Heckman sample-selection procedures essentially means setting the parameter ρ^{01} equal to zero. It is, however, worth mentioning the possibility of this type of estimation, as taking such an approach might represent a good approximation when a researcher needs to estimate a more complex structure where the unordered selection process is multivariate and not bivariate (i.e., dichotomous) in nature—the topic of our next subsection.

Extension: Endogenous switching with multivariate selection. Within the management literature that takes into account potential selection effects, the almost universal approach has been to simply consider bivariate contexts. Thus, researchers have rarely corrected for selection effects in a context where managers choose between multiple alternatives. In fact, a number of researchers have excluded additional organizational choices so that they can employ the standard bivariate procedures to deal with selection effects. For example, Brouthers, Brouthers, and Werner (2003) consider the joint venture and wholly owned-subsidiary entry modes but omit licensing agreements in part so they can estimate selection effects in a bivariate setting. Furthermore, the few studies which do entertain multivariate choices—or strategies—on the part of managers do not fully control for selection effects. For instance, Morrow, Sirmon, Hitt, and Holcomb (2007) compare the stock-market reactions to three

different types of strategic investments—new product introductions, strategic alliances, and mergers—but instead of modeling the selection into each particular organizational decision, they take a bivariate shortcut by simply modeling whether the firm selects into any of the three investment types. The above indicate our literature's inability to adequately deal with situations that involve more than two organizational decisions.

The lack of studies which fully embrace an organizational context where managers make choices amongst multiple discrete variables is, nevertheless, understandable. As Berk (1983) points out, things do get quite complicated in a multivariate setting. Furthermore, Hamilton and Nickerson (2003) offer up an approach to deal with multivariate organizational decisions; however, that procedure fails to correct for the heteroscedasticity involved with embedding a predicted value (the inverse Mill's ratio) into the substantive equation of interest. Therefore, standard errors in this equation are not consistently estimated and inferences based on these standard errors are incorrect. As an aside, bootstrapping the standard errors in the second stage may represent a fix to this issue. Nevertheless, omitting multivariate contexts from our sphere of study is problematic in the sense that organizational realities often involve decision contexts that go beyond dichotomous. In short, the real world of managers involves choosing between multiple options when it comes to organizational decisions and the performance outcomes of these decisions are, of course, salient to these decisions.

Accordingly, we go beyond a bivariate setting for organizational decisions to consider a situation where managers choose amongst multiple alternatives that are not ordinal in nature. Consider, for instance, an empirical context where the treatment goes beyond a simple partition of the sample into two subgroups to an empirical context where the treatment partitions the sample into multiple subgroups. For the sake of exposition, imagine that there are three possible outcomes for the treatment that managers self-select into; thus, rendering a more complex version of the model discussed above. We now have three different main equations and three sets of coefficient estimates that we must estimate, one for each subsample:

$$\begin{cases} Y_i^0 = \alpha_0^0 + \alpha_1^0 X_i^0 + \varepsilon_i^0 & \text{if } z_i = 0 \\ Y_i^1 = \alpha_0^1 + \alpha_1^1 X_i^1 + \varepsilon_i^1 & \text{if } z_i = 1 \\ Y_i^2 = \alpha_0^2 + \alpha_1^2 X_i^2 + \varepsilon_i^2 & \text{if } z_i = 2 \end{cases} \quad (8)$$

The most complex part of this empirical estimation revolves around how the researcher would model the selection process into the treatment; that is, the selection equation for z_i . The researcher must first consider whether the outcome can be ordered or not. Second, the researcher must make an assumption with respect to the distribution of the error terms. Finally, the researcher must make some assumptions regarding an even more complex variance-covariance matrix, which can be expressed as follows:⁹

$$\Omega^M = \begin{pmatrix} \sigma_{\varepsilon^0}^2 & \rho^{01} \sigma_{\varepsilon^0} \sigma_{\varepsilon^1} & \rho^{02} \sigma_{\varepsilon^0} \sigma_{\varepsilon^2} & \rho^0 \sigma_{\varepsilon^0} \sigma_{\omega} \\ & \sigma_{\varepsilon^1}^2 & \rho^{12} \sigma_{\varepsilon^1} \sigma_{\varepsilon^2} & \rho^1 \sigma_{\varepsilon^1} \sigma_{\omega} \\ & & \sigma_{\varepsilon^2}^2 & \rho^2 \sigma_{\varepsilon^2} \sigma_{\omega} \\ & & & \sigma_{\omega}^2 \end{pmatrix}$$

Without going into overdue detail, we submit that treating such an empirical context as a “triple sample selection” problem represents one possible means to deal with self-selection effects in a multivariate setting. Thus, the researcher can separately estimate the model for each of the three organizational decisions. For purposes of brevity, we will not exhibit a multivariate organizational choice setting in our simulation-based demonstration, though we will consider a bivariate choice context where an analogous “double sample selection” problem is analyzed.

Empirical Demonstration

In this section we demonstrate the application of the outlined approaches to dealing with self-selection-based endogeneity by means of Monte Carlo simulations. By conducting simulation analysis, we pursue two key objectives. First, we can demonstrate how severely biased the results of an OLS regression are if the self-selection problem is completely neglected since the true parameter values are actually known by the researcher in a Monte Carlo simulation. Thus, it is possible to infer both the direction and the magnitude of the bias by comparing the true parameter values with the estimated parameters. Second, we can empirically demonstrate the strengths and weaknesses of the different techniques to correct for self-selection effects that were previously outlined. For instance by explicitly violating the assumption of joint normality in the error terms for the main and selection equations, we can highlight differences in terms of consistency and precision for the FIML and LIML estimators. Accordingly, our simulations can help guide researchers in deciding which techniques and approaches to deal with selection bias best suits the characteristics of their empirical context.

Monte Carlo simulations employ an explicitly defined data generating process (a model) with a random component (i.e., the error term) to generate fictitious data. In the simulation, the values of the independent variable(s) and the data generating process are fixed whereas in each simulation run a new random error term is drawn from a specified distribution. Furthermore, an econometric estimator is employed to uncover the relationship between the independent variables and the dependent variable in each simulation run as defined by the data generating process. If the estimator is unbiased, then the mean value of the estimated parameters will equal the true parameters specified in the data generating process. Below, we first describe the specification of the simulation model as well as the data generating process before actually presenting the simulation results.

Specification of the Simulation Model

In line with our previous discussion, we model self-selection as an omitted variable issue. More specifically, we define z_i to be a binary treatment variable, that is, $z_i = 1$, if observation i has been treated (has decided to self-select into a specific action) and 0 otherwise. Whether or not observation i receives treatment (pursues a specific action) depends on an unobservable (latent) variable z_i^* . In particular, we assume that:

$$z_i = \begin{cases} 1 & \text{iff } z_i^* \geq 0 \\ 0 & \text{iff } z_i^* < 0 \end{cases}$$

that is, observation i is treated if and only if the value of the unobserved variable z_i^* exceeds a certain threshold which we normalize to zero. Furthermore, we assume that z_i^* is a linear function of five variables (z_{1i} , z_{2i} , z_{3i} , x_{2i} and x_{3i})—which are all independently normally distributed with known mean μ and variance σ^2 —and an error term ω_i which also follows a known distribution:

$$z_i^* = \beta_1 z_{1i} + \beta_2 z_{2i} + \beta_3 z_{3i} + \beta_4 x_{2i} + \beta_5 x_{3i} + \omega_i \quad (9)$$

The dependent variable, denoted by Y_i , is assumed to be a linear function of four variables (x_{1i} , x_{2i} , x_{3i} , and z_i) as well as an error term ε_i . The exact functional relationship between the dependent and the independent variables depends on the type of selection model considered. In the endogenous-treatment model, it holds that:

$$Y_i = \alpha_0 + \alpha_1 x_{1i} + \alpha_2 x_{2i} + \alpha_3 x_{3i} + \alpha_4 z_i + \varepsilon_i \quad (10)$$

whereas in the endogenous-switching model, Y_i is defined as

$$\begin{cases} Y_i^0 = \alpha_0^0 + \alpha_1^0 x_{1i} + \alpha_2^0 x_{2i} + \alpha_3^0 x_{3i} + \varepsilon_i^0 & \text{if } z_i = 0 \\ Y_i^1 = \alpha_0^1 + \alpha_1^1 x_{1i} + \alpha_2^1 x_{2i} + \alpha_3^1 x_{3i} + \varepsilon_i^1 & \text{if } z_i = 1 \end{cases}$$

Next, we assume x_{3i} is unobservable by the researcher; that is, it is only possible to estimate

$$Y_i = \alpha'_0 + \alpha'_1 x_{1i} + \alpha'_2 x_{2i} + \alpha'_4 z_i + \varepsilon'_i \quad (11)$$

and

$$\begin{cases} Y_i^0 = \alpha_0^{0'} + \alpha_1^{0'} x_{1i} + \alpha_2^{0'} x_{2i} + \varepsilon_i^{0'} & \text{if } z_i = 0 \\ Y_i^1 = \alpha_0^{1'} + \alpha_1^{1'} x_{1i} + \alpha_2^{1'} x_{2i} + \varepsilon_i^{1'} & \text{if } z_i = 1 \end{cases} \quad (12a)$$

$$(12b)$$

respectively. In Equation (11), ε'_i is a compound error term since it holds that $\varepsilon'_i = \varepsilon_i + \alpha_3 x_{3i}$. Similarly in Equation (12) it holds that $\varepsilon_i^{0'} = \varepsilon_i^0 + \alpha_3^0 x_{3i}$ and $\varepsilon_i^{1'} = \varepsilon_i^1 + \alpha_3^1 x_{3i}$.

The treatment variable z_i is endogenous in both the endogenous-treatment and endogenous-switching models since x_{3i} is both unobservable (thus manifest in the error term ε'_i) and correlated with z_i (per construction via Equation (9)); that is, $E(z_i \varepsilon'_i) \neq 0$.¹⁰ A few additional points are worth noting: First, the three variables z_{1i} , z_{2i} , and z_{3i} can serve as valid instruments for z_i as these are both relevant (i.e., correlated with the endogenous variable z_i) and exogenous (i.e., uncorrelated with the source of endogeneity x_{3i}). Second, the omission of z_i would lead to biased coefficient estimates for x_{2i} —but not for x_{1i} —since x_{1i} is not part of Equation (9) and is thus uncorrelated with the selection process. Yet, x_{2i} is correlated with the selection process and therefore is also correlated with z_i . Accordingly, omitting z_i from the main equation(s)—(11) or (12)—would lead to an omitted-variable bias when estimating the parameter for x_{2i} .

To provide an illustrative context for our econometric setup, suppose we are interested in explaining the performance effect of a firm's decision to merge. Clearly, the decision to merge is not made randomly but is a choice variable that managers are likely to self-select into. Hence, Y_i represents firm performance and z_i can be viewed as indicating the merger decision (yes or no). Top-management-team characteristics (e.g., the degree of hubris, or the level of group think) might represent unobserved factors—that is, x_{3i} in our econometric setup—which both affect the firm's performance as well as the decision to merge or not. Furthermore, the variables z_{1i} , z_{2i} , and z_{3i} could be factors explaining the extent of the merger wave (e.g., Harford, 2005) or the level of antitrust scrutiny (Clougherty & Seldeslachts, 2013), as conceivably such constructs would solely affect the decision to merge and not affect firm performance. In addition, some firm-specific characteristics (e.g., the ability to turn assets into profits) might simply affect the firm's performance (i.e., x_{1i} in our econometric setup).

In following through on our aims to demonstrate the consequences (i.e., biased coefficient estimates) of neglecting self-selection effects and to illustrate the strengths and weaknesses of the different techniques to deal with self-selection-based endogeneity, we consider a variety of empirical conditions within our simulations. More specifically, we vary the conditions of the simulation along three key dimensions: (a) the sample size (small vs. large), (b) the strength of the instruments (strong vs. weak), and (c) the distributional assumptions regarding the error terms of the selection and structural equations (bivariate normal vs. nonnormal). In addition to these three dimensions, we also consider two general selection-model types (endogenous treatment and endogenous switching) which then yields some 12 different basic conditions for our Monte Carlo simulations. Figure 1 provides an overview of the structure of our simulations.

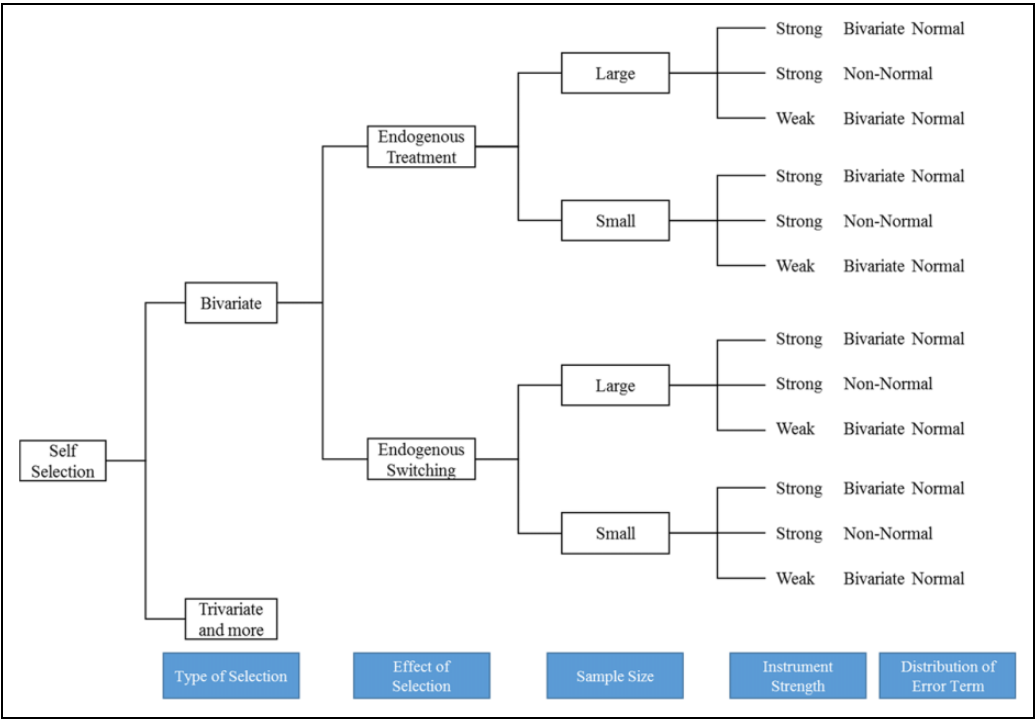


Figure 1. Structure of the Monte Carlo simulations.

Endogenous Treatment

Table 1 provides an overview of the different parameter values chosen for the simulations involving the endogenous-treatment model. In particular, we specify x_{1i} , x_{2i} , and x_{3i} as well as the instruments z_{1i} , z_{2i} , and z_{3i} as independently normally distributed random variables with mean 0 and variance 1. In line with Antonakis et al. (2010), we directly specify the coefficient estimates for the structural and selection equations instead of following an approach that specifies a desired level of correlation between the variables and then calculates the necessary coefficients for these correlations to materialize—an alternative approach that was employed by Semadeni et al. (2014) and Larcker and Rusticus (2010). By directly specifying coefficients instead of correlations, it is easier to analyze potential estimation biases when comparing the estimated parameters with the true parameters.

We set the coefficients for the three IVs equal to 1 when studying the case of strong instruments, whereas we set the coefficients equal to 0.1 when studying the case of weak instruments. We also specify a positive relationship between x_{2i} and z_i , as well as a negative relationship between x_{3i} and z_i . The negative relationship between x_{3i} and z_i leads to a negative bias in z_i 's estimated coefficient; that is, if the self-selection problem is neglected, then α'_4 will be downward biased. In the setting with normally distributed error terms, the error terms for the selection (ϵ_i) and structural (ω_i) equations respectively follow a bivariate-normal distribution with mean 0, variance 1, and correlation 0.3. When studying non-normally distributed error terms, both error terms are assumed to follow a t-distribution with two degrees of freedom. Finally, we set all of the coefficients in the structural equation equal to 1.

We use two different outcome measures to analyze the results of the Monte Carlo simulations. First, we compare the mean values of the coefficient estimates in the structural equation with their

Table 1. Parameterization of the Endogenous Treatment Simulation.

Parameter	Values chosen in simulation
Number of observations	<ul style="list-style-type: none"> • Small sample: $N = 250$ • Large sample: $N = 1,000$
Number of simulations	<ul style="list-style-type: none"> • 500
Distribution of independent variables	<ul style="list-style-type: none"> • $x_{1i} \sim N(0, 1)$ • $x_{2i} \sim N(0, 1)$ • $x_{3i} \sim N(0, 1)$ • $z_{1i} \sim N(0, 1)$ • $z_{2i} \sim N(0, 1)$ • $z_{3i} \sim N(0, 1)$
Distribution of error terms	Bivariate-normal distribution of error terms: <ul style="list-style-type: none"> • $\omega_i, \varepsilon_i \sim \mathcal{N}(0, \Sigma)$ • $\Sigma = \begin{pmatrix} 1 & \\ 0.3 & 1 \end{pmatrix}$ • Nonnormal distribution of error terms: • $\omega_i \sim t(2)$ • $\varepsilon_i \sim t(2)$
Coefficients in selection equation	Strong instruments: <ul style="list-style-type: none"> • $\beta_1 = 1$ • $\beta_2 = 1$ • $\beta_3 = 1$ • $\beta_4 = 2$ • $\beta_5 = -2$ Weak instruments: <ul style="list-style-type: none"> • $\beta_1 = 0.1$ • $\beta_2 = 0.1$ • $\beta_3 = 0.1$ • $\beta_4 = 2$ • $\beta_5 = -2$
Coefficients in structural equation	<ul style="list-style-type: none"> • $\alpha_0 = 1$ • $\alpha_1 = 1$ • $\alpha_2 = 1$ • $\alpha_4 = 1$

true counterparts as specified in the data generating process. Second, we analyze the mean of the estimated standard errors of the coefficient estimates across all simulation runs to compare the precision of the different estimators.

Endogenous Treatment Results. Table 2 reports the simulation results for the four different approaches—OLS, IV-OLS, nonlinear LIML, and nonlinear FIML—to estimate the endogenous-treatment model.¹¹ Panel A reports the results for the small-sample (250 observations) simulations, while panel B reports the results for the large-sample (1,000 observations) simulations. Within each panel, we report the results of three different simulation setups: first, a setup with strong instruments and bivariate-normally distributed error terms; second, a setup which involves strong instruments but non-normally distributed error terms; and third a setup with weak instruments and bivariate-normally distributed error terms. The first column in Table 2 reports a naïve approach where the treatment is (wrongly) assumed to be exogenous; hence, the model is estimated via OLS. The subsequent six columns involve modeling the selection process; hence, results for both the main equation and the selection equation are respectively reported for an IV estimation that adopts a linear-probability model estimated by OLS, a LIML estimation using a nonlinear probit model, and an FIML estimation

Table 2. Endogenous Treatment Estimations.

	OLS	IV-OLS		LIML		FIML	
	Main	Main	First stage	Main	Selection	Main	Selection
Panel A: Small sample size: $N = 250$							
Strong instruments, bivariate-normally distributed errors							
x_1	1.02 (0.09)	1.01 (0.09)	0.02 (0.00)	1.01 (0.09)	0.02 (0.01)	1.01 (0.09)	0.02 (0.01)
x_2	1.22 (0.10)	0.97 (0.14)	0.23 (0.00)	0.99 (0.13)	0.99 (0.02)	0.99 (0.12)	0.98 (0.02)
z	-0.01 (0.20)	1.00 (0.43)		0.93 (0.38)		0.92 (0.33)	
z_1			0.12 (0.00)		0.49 (0.01)		0.49 (0.01)
z_2			0.12 (0.00)		0.50 (0.01)		0.49 (0.01)
z_3			0.13 (0.00)		0.58 (0.01)		0.55 (0.01)
F		24.85					
J		0.67					
λ				-0.76 (0.06)			
$\rho(\ln)$						-0.62 (0.04)	
$\sigma(\ln)$						0.34 (0.00)	
Strong instruments, non-normally distributed errors							
x_1	1.01 (0.20)	1.00 (0.20)	0.01 (0.00)	1.00 (0.20)	0.02 (0.01)	1.01 (0.20)	0.02 (0.01)
x_2	1.22 (0.22)	0.99 (0.31)	0.22 (0.00)	1.01 (0.29)	0.83 (0.01)	0.92 (0.27)	0.80 (0.01)
z	-0.06 (0.45)	0.94 (1.04)		0.86 (0.94)		1.25 (0.74)	
z_1			0.11 (0.00)		0.41 (0.01)		0.38 (0.01)
z_2			0.11 (0.00)		0.42 (0.01)		0.39 (0.01)
z_3			0.12 (0.00)		0.47 (0.01)		0.43 (0.01)
F		19.86					
J		0.53					
λ				-0.69 (0.55)			
$\rho(\ln)$						-0.41 (0.03)	
$\sigma(\ln)$						1.08 (0.00)	

(continued)

Table 2. (continued)

	OLS	IV-OLS		LIML		FIML	
	Main	Main	First stage	Main	Selection	Main	Selection
Weak instruments, bivariate-normally distributed errors							
x_1	1.02 (0.08)	1.00 (0.12)	0.01 (0.00)	1.00 (0.09)	0.02 (0.01)	1.00 (0.09)	0.01 (0.01)
x_2	1.30 (0.10)	0.92 (0.85)	0.27 (0.00)	0.99 (0.29)	0.97 (0.01)	0.95 (0.19)	0.96 (0.01)
z	-0.20 (0.20)	1.18 (3.11)		0.93 (1.02)		1.10 (0.62)	
z_1			0.02 (0.00)		0.05 (0.01)		0.05 (0.01)
z_2			0.01 (0.00)		0.04 (0.01)		0.04 (0.01)
z_3			0.02 (0.00)		0.10 (0.01)		0.08 (0.01)
F		0.92					
J		0.69					
λ				-0.69 (0.38)			
$\rho(\ln)$						-0.67 (0.11)	
$\sigma(\ln)$						0.38 0.01	
Panel B: Large sample size: $N = 1,000$							
Strong instruments, bivariate-normally distributed errors							
x_1	1.04 (0.04)	1.05 (0.04)	-0.01 (0.00)	1.05 (0.04)	-0.05 (0.00)	1.05 (0.04)	-0.05 (0.00)
x_2	1.26 (0.05)	1.06 (0.07)	0.23 (0.00)	1.04 (0.06)	0.86 (0.00)	1.04 (0.06)	0.87 (0.00)
z	-0.02 (0.09)	0.91 (0.22)		0.95 (0.20)		0.96 (0.18)	
z_1			0.12 (0.00)		0.44 (0.00)		0.44 (0.00)
z_2			0.12 (0.00)		0.45 (0.00)		0.46 (0.00)
z_3			0.12 (0.00)		0.46 (0.00)		0.45 (0.00)
F		84.95					
J		0.38					
λ				-0.74 (0.02)			
$\rho(\ln)$						-0.60 (0.01)	
$\sigma(\ln)$						0.34 (0.00)	

(continued)

Table 2. (continued)

	OLS	IV-OLS		LIML		FIML	
	Main	Main	First stage	Main	Selection	Main	Selection
Strong instruments, non-normally distributed errors							
x_1	1.03 (0.11)	1.04 (0.11)	-0.01 (0.00)	1.04 (0.11)	-0.04 (0.00)	1.05 (0.11)	-0.04 (0.00)
x_2	1.25 (0.12)	1.05 (0.16)	0.21 (0.00)	1.05 (0.15)	0.74 (0.00)	0.91 (0.14)	0.71 (0.00)
z	-0.05 (0.23)	0.90 (0.57)		0.90 (0.53)		1.60 (0.42)	
z_1			0.11 (0.00)		0.38 (0.00)		0.34 (0.00)
z_2			0.11 (0.00)		0.37 (0.00)		0.35 (0.00)
z_3			0.11 (0.00)		0.40 (0.00)		0.36 (0.00)
F		68.45					
J		0.48					
λ				-0.69 (0.15)			
$\rho(\ln)$						-0.42 (0.01)	
$\sigma(\ln)$						1.18 (0.00)	
Weak instruments, bivariate-normally distributed errors							
x_1	1.03 (0.04)	1.04 (0.05)	-0.02 (0.00)	1.06 (0.05)	-0.07 (0.00)	1.06 (0.05)	-0.07 (0.00)
x_2	1.34 (0.05)	1.17 (0.33)	0.26 (0.00)	0.95 (0.18)	0.85 (0.00)	0.99 (0.10)	0.86 (0.00)
z	-0.21 (0.10)	0.46 (1.26)		1.31 (0.68)		1.17 (0.33)	
z_1			0.02 (0.00)		0.06 (0.00)		0.05 (0.00)
z_2			0.02 (0.00)		0.06 (0.00)		0.06 (0.00)
z_3			0.02 (0.00)		0.08 (0.00)		0.06 (0.00)
F		2.75					
J		0.39					
λ				-0.92 (0.16)			
$\rho(\ln)$						-0.67 (0.03)	
$\sigma(\ln)$						0.37 (0.00)	

Note: FIML = full information maximum likelihood; IV = instrumental variable; LIML = limited information maximum likelihood; OLS = ordinary least squares.

using a nonlinear probit model. For each parameter in the main and selection equations, we report the mean coefficient estimate as well as—in parentheses—the mean value for the coefficients' standard errors. Furthermore, the IV-OLS estimation reports the mean first-stage F value from the test of excluded instruments (F), as well as the mean p value from Hansen's J test for instrument exogeneity (J). For the LIML estimations, we report Heckman's λ which is the product of the correlation between the error terms in both the main and selection equations (ρ) and the standard deviation of the error term in the selection equation (σ). For the FIML estimations, we report the log of σ together with the hyperbolic tangent of ρ —that is, $\text{atanh } \rho = \frac{1}{2} \ln \left(\frac{1+\rho}{1-\rho} \right)$ —which Stata estimates instead of ρ for numerical stability during optimization.

We start by considering the naïve approach where the treatment variable z_i is (wrongly) assumed to be exogenous; hence, the model is estimated via OLS. The results for the setup with strong instruments and bivariate-normally distributed error terms show that the OLS estimates for x_{2i} and z_i are heavily biased both in the small and large samples. In terms of the coefficient estimates for x_{2i} , they respectively average 1.22 and 1.26 in the small and large samples which diverges quite a bit from the true coefficient values of 1. In terms of coefficient estimates for z_i , they suffer from a negative bias (due to the negative relationship between the unobservable variable x_{3i} and the treatment variable z_i) and thus respectively average -0.01 and -0.02 in the small and large samples instead of the true coefficient value of 1. These results are particularly worrisome as the naïve OLS approach indicates that the treatment has no—or even worse, a negative—effect on the dependent variable when in fact the true effect is positive. When we consider the two additional simulation setups (strong instruments with non-normally distributed errors, and weak instruments with bivariate-normally distributed errors), we find virtually identical empirical results. Accordingly, these simulations highlight how neglecting the problem of self-selection can generate drastically misleading conclusions with respect to the effect of an explanatory construct that is subject to endogenous treatment. As an aside, these simulations also indicate that the estimated coefficients for the other explanatory variable (x_{2i}) which correlates with the treatment decision also manifests bias. Yet as expected, the bias is larger for the most problematic variable (i.e., the endogenous variable z_i) than for x_{2i} . The lack of correlation between x_{1i} and z_i in our context yields unbiased coefficient estimates for x_{1i} that are quite close to the true value of 1.

We now turn to a simple 2SLS estimator where we adopt a linear probability model to control for the factors that affect our treatment variable z_i .¹² We first consider the simulation setup involving strong instruments and bivariate-normally distributed error terms. It appears that z_{1i} , z_{2i} , and z_{3i} represent good instruments for z_i , as the mean Kleibergen–Paap Wald F statistic is 24.85 (84.95) in the small (large) sample—well above conventional threshold of 10. Furthermore, the mean p value of Hansen's J test for exogeneity of instruments is 0.67 (0.38) in the small (large) sample—well above the 10% confidence level. Moreover, the coefficient estimates in both the small and large samples confirm that employing the three instruments leads to a significant reduction in estimation biases for both x_{2i} and z_i . As an aside, the average estimated standard errors from 2SLS estimations are larger than the standard errors from the naïve OLS estimation, since the 2SLS estimator only employs the exogenous variation in z_i (due to the variation in z_{1i} , z_{2i} , and z_{3i}) to identify the treatment effect. Similar to the naïve OLS results, the estimated coefficient estimate for x_{1i} is close to its true value of 1—that is, this estimate is not biased.

Turning to the simulation setup with strong instruments and non-normally distributed error terms (but still considering the IV-OLS approach), the three variables z_{1i} , z_{2i} , and z_{3i} again serve as good instruments for z_i with a mean first-stage F value of 19.86 (68.45) in the small (large) sample, and mean p values for Hansen's J test of 0.53 (0.48) in the small (large) sample. Furthermore, these simulations again indicate that employing an IV-2SLS estimator ameliorates the self-selection-based endogeneity issue to a certain extent, as the mean coefficient estimates for x_{2i} and z_i are respectively

0.99 (1.05) and 0.94 (0.90) in the small (large) sample; thus, they are generally quite close to the true value of 1.

Yet, the ability of the IV-2SLS estimator to eliminate the endogeneity issue critically hinges on the availability of strong instruments. The simulation setup involving weak instruments and bivariate-normally distributed errors highlights this observation, as we only obtain first-stage F values of 0.92 (2.75) in the small (large) samples, which rightly indicates that instruments are weak; whereas the mean p value of Hansen's J test averages 0.69 (0.39) in the small (large) samples which rightly indicates that instruments are exogenous. Due to weak instruments, the coefficient estimates for x_{2i} and z_i are respectively 0.92 (1.17) and 1.18 (0.46) in the small (large) sample. This finding clearly indicates that merely employing an IV-OLS approach to estimate models of endogenous self-selection is not sufficient. Indeed, it is essential to test the strength of the instruments to ensure that there is a sufficiently large correlation between the endogenous-treatment variable and the instruments.

We now turn to the maximum likelihood estimators which should be more appropriate in this empirical context, as they correctly treat the self-selection process within a nonlinear probability (probit) model. Recall that the LIML estimator is characterized by being more robust as it delivers consistent coefficient estimates even if the null hypothesis of bivariate-normally distributed error terms for the substantive and selection equations does not hold. But if this untestable hypothesis holds, then the FIML estimator is both consistent and more efficient as compared to the LIML estimator. Thus, the FIML estimator potentially allows for more precise causal inferences.

When considering the simulation setup of strong instruments and bivariate-normally distributed error terms, we observe that the LIML and FIML coefficient estimates for the endogenous treatment variable (z_i) are almost identical in both the small and large samples—an unsurprising result since both estimators are consistent. Furthermore, the average estimated standard errors for z_i are somewhat smaller for the FIML coefficients as compared to the LIML coefficients in both the small and large samples—indicative of FIML's superior efficiency. But when the assumption of bivariate-normally distributed error terms is violated, the consistency in results is quite different. Since the LIML estimator is more robust to the violation of the normality assumption, it still delivers estimates for x_{2i} and z_i which are quite close to their true values with respective mean coefficients of 1.01 and 0.86 (1.05 and 0.90) in the small (large) sample. On the other hand, the FIML estimates indicate significant bias, as x_{2i} and z_i yield respective coefficient-estimate averages of 0.92 and 1.25 (0.91 and 1.60) in the small (large) sample. These results underline the importance of the assumption concerning bivariate-normally distributed error terms, as this assumption is critical for the FIML procedure to yield consistent coefficients.

When we consider the third setup (weak instruments and bivariate-normally distributed errors), both the LIML and the FIML estimators perform better than the IV-2SLS estimator in this weak instrument context. Furthermore, the FIML estimator performs slightly better than the LIML estimator due to its greater precision. In the small-sample case, the coefficient estimates for x_{2i} and z_i are relatively close when comparing the LIML and FIML estimations; however, the FIML estimates are a bit closer to the true values and their mean standard error is significantly smaller. In the large sample, moreover, the greater precision of the FIML estimator leads to coefficients for x_{2i} and z_i which are significantly closer to the true value than the coefficients yielded via LIML estimation. A further comparison of the LIML and FIML coefficients with the results from the IV-2SLS procedure reveals that the 2SLS estimator performs slightly (substantially) better than the LIML (FIML) estimator under non-normally distributed error terms and a small sample size. However, the performance of both LIML and FIML is quite superior to that of IV-2SLS under the condition of weak instruments. This finding is quite important as many empirical applications are characterized by the difficulty of finding strong IVs. In such empirical contexts, it is then potentially more appropriate to apply LIML and FIML procedures as opposed to a 2SLS estimator.

Endogenous Switching Regressions

We turn now to the richer switching-regressions approach where treatment effects go beyond a simple intercept effect. While the selection equation remains the same as that employed in the endogenous-treatment model (i.e., the process determining z_i which is represented in Equation (9)), we now have two separate main equations (one for each subsample) as the effect of x_{1i} and x_{2i} on the dependent variable Y_i will depend on the treatment variable z_i . Hence, the model we estimate now consists of Equations (9), (12a), and (12b).

Following our earlier discussion of this method, we employ two approaches to estimate a switching-regressions model that factors self-selection bias. First, we use an endogenous switching-regressions model and simultaneously estimate (9), (12a), and (12b) together with the complete variance-covariance matrix via FIML. Second, we partition the estimation of this model into two separate sample-selection models and apply the basic Heckman procedure. We take the latter approach for the sake of completeness and to indicate what may be feasible in a multivariate context where a full FIML estimation might become too cumbersome for empirical researchers who are unwilling to program the likelihood function by themselves.¹³ The later sample-selection-like approach involves first estimating the model for the case of $z_i = 0$; that is, Equation (9) and Equation (12a). Thus, we act as if the sample was censored and we lack observations for the subsample where $z_i = 1$; by doing so, we can identify all of the relevant α^0 parameters. Second, we estimate the model for $z_i = 1$ —that is, Equation (9) and Equation (12b)—in a similar fashion and thus identify all of the relevant α^1 parameters. As noted in our discussion of alternative procedures, the inefficiencies involved with such a “split” regression procedure (as well as the nonordered nature of the specified selection process) may limit the applicability of this sample-selection-like approach to instances where organizational decisions are self-selected and multivariate in nature.

Table 3 provides an overview of the different parameter values chosen in the simulation of the endogenous-switching model. Only two differences exist in the parameterization of the endogenous-switching model as compared to our simulations involving the endogenous-treatment model. First, we have doubled the sample size in the case of endogenous switching due to our splitting the sample roughly in half by respectively estimating Heckman-type models on the samples where $z_i = 0$ and $z_i = 1$. The second difference concerns the specification of the α 's: if $z_i = 0$, then we set the vector of coefficients α^0 equal to 1; and, if $z_i = 1$, then we have set the vector of coefficients α^1 equal to 2.

Endogenous switching results. Table 4 reports the simulation results for the four different approaches—OLS, sample-selection-like via LIML estimation, sample-selection-like FIML, and endogenous switching-regressions via FIML—to estimating switching-regressions models. Panels A and B again respectively report the estimation results for the small-sample (500 observations) simulations and large-sample (2,000 observations) simulations. Within each panel, we again report the results for three different simulation setups: first, a setup with strong instruments and bivariate-normally distributed error terms; second, a setup which involves strong instruments but non-normally distributed error terms; and third, a setup with weak instruments and bivariate-normally distributed error terms. For each parameter in the structural and selection equations, we again report the mean coefficient estimate as well as—in parentheses—the mean value for the coefficient's standard errors. In addition, we report Heckman's λ for the LIML estimations, as well as the log of σ together with the hyperbolic tangent of ρ for the FIML estimations.

To create a good benchmark for comparison purposes, the first two columns of Table 4 respectively report the results for a naïve approach (where selection bias is not taken into account) where we simply split the sample between $z_i = 0$ and $z_i = 1$ and run separate OLS regressions on each of the subsamples. The next four columns involve modeling the selection process as a

Table 3. Parameterization of the Endogenous Switching Simulation.

Parameter	Values chosen in simulation
Number of observations	<ul style="list-style-type: none">• Small sample: $N = 500$• Large sample: $N = 2,000$
Number of simulations	<ul style="list-style-type: none">• 500
Distribution of independent variables	<ul style="list-style-type: none">• $x_{1i} \sim N(0, 1)$• $x_{2i} \sim N(0, 1)$• $x_{3i} \sim N(0, 1)$• $z_{1i} \sim N(0, 1)$• $z_{2i} \sim N(0, 1)$• $z_{3i} \sim N(0, 1)$
Distribution of error terms	<p>Bivariate-normal distribution of error terms:</p> <ul style="list-style-type: none">• $\omega_i, \varepsilon_i \sim \mathcal{N}(0, \Sigma)$• $\Sigma = \begin{pmatrix} 1 & \\ 0.3 & 1 \end{pmatrix}$ <p>Nonnormal distribution of error terms:</p> <ul style="list-style-type: none">• $\omega_i \sim t(2)$• $\varepsilon_i \sim t(2)$
Coefficients in the selection equation	<p>Strong instruments:</p> <ul style="list-style-type: none">• $\beta_1 = 1$• $\beta_2 = 1$• $\beta_3 = 1$• $\beta_4 = 2$• $\beta_5 = -2$ <p>Weak instruments:</p> <ul style="list-style-type: none">• $\beta_1 = 0.1$• $\beta_2 = 0.1$• $\beta_3 = 0.1$• $\beta_4 = 2$• $\beta_5 = -2$
Coefficients in the structural equation	<ul style="list-style-type: none">• $\alpha_0^0 = 1 \alpha_0^1 = 2$• $\alpha_1^0 = 1 \alpha_1^1 = 2$• $\alpha_2^0 = 1 \alpha_2^1 = 2$

sample-selection-like procedure via LIML estimation; here, the approach is to consider the self-selection issue as two separate sample-selection models and apply a standard Heckman selection approach to each of the two submodels. The subsequent four columns also involve modeling the selection process as a sample-selection-like procedure, though this approach employs FIML estimation. Finally, the last three columns of Table 4 present the results involving the FIML estimator for an endogenous switching regression model—the approach which should deliver the most efficient estimates as it employs all of the available information and imposes the restriction that the coefficient for the selection into the two equations is the same.

We start by considering the naïve approach where self-selection bias is not taken into account. Unsurprisingly, the OLS coefficients for x_{2i} indicate significant bias in both equations—that is, when $z_i = 0$ and $z_i = 1$ —across the different simulation setups and sample sizes. For instance, if we consider the case of strong instruments and bivariate-normally distributed error terms, then the OLS estimates for x_{2i} in the two equations indicate mean values of $\alpha_2^{1'} = 2.37$ (2.55) and $\alpha_2^{0'} = 1.24$ (1.23) in the small (large) sample, instead of the respective true values of 2 and 1. As alluded to above, very similar results that are indicative of significant bias manifest when one

Table 4. Endogenous Switching Estimations.

Exogenous		Sample-selection-like						Endogenous switching regressions			
OLS		LIML			FIML			FIML			
z = 1	z = 0	z = 1		z = 0		z = 1		z = 0		z = 1	z = 0
		Main	Selection	Main	Selection	Main	Selection	Main	Selection	Main	Selection
Panel A: Small Sample											
Strong instruments, bivariate-normally distributed errors											
x ₁	2.18 (0.12)	0.92 (0.09)	2.20 (0.13)	-0.03 (0.00)	0.94 (0.09)	2.20 (0.13)	-0.02 (0.00)	0.94 (0.09)	0.02 (0.00)	2.20 (0.02)	0.94 (0.01)
x ₂	2.37 (0.14)	1.24 (0.10)	1.85 (0.19)	0.83 (0.01)	0.92 (0.13)	1.84 (0.16)	0.85 (0.01)	0.97 (0.12)	-0.84 (0.01)	1.84 (0.03)	0.96 (0.01)
z ₁				0.40 (0.36)	-0.40 (0.30)	0.39 (0.36)	0.39 (0.36)	-0.42 (0.30)	-0.42 (0.30)		0.41 (0.00)
z ₂				0.38 (0.44)	-0.38 (0.36)	0.39 (0.40)	0.39 (0.40)	-0.40 (0.34)	-0.40 (0.34)		0.40 (0.00)
z ₃				0.40 (0.57)	-0.40 (0.47)	0.41 (0.46)	0.41 (0.46)	-0.39 (0.41)	-0.39 (0.41)		0.41 (0.00)
λ	-1.80 (0.17)				1.08 (0.08)						
ρ(ln)						-1.19 (0.04)		0.78 (0.03)		-1.18 (0.04)	-0.77 (0.03)
σ(ln)						0.84 (0.00)		0.38 (0.00)		0.83 (0.00)	0.38 (0.00)
Strong instruments, non-normally distributed errors											
x ₁	2.17 (0.22)	0.94 (0.20)	2.19 (0.22)	-0.02 (0.00)	0.96 (0.21)	2.20 (0.22)	-0.02 (0.00)	0.96 (0.21)	0.02 (0.00)	2.20 (0.06)	0.96 (0.06)
x ₂	2.31 (0.24)	1.24 (0.22)	1.85 (0.33)	0.72 (0.01)	0.93 (0.30)	1.78 (0.27)	0.71 (0.01)	0.83 (0.26)	-0.70 (0.01)	1.77 (0.09)	0.81 (0.09)
z ₁				0.35 (0.35)	-0.35 (0.35)		0.33 (0.33)		-0.33 (0.33)		0.31 (0.00)

(continued)

Table 4. (continued)

Exogenous		Sample-selection-like						Endogenous switching regressions					
OLS		LIML			FIML			FIML					
z = 1	z = 0	z = 1		z = 0		z = 1		z = 0		Main	Selection		
		Main	Selection	Main	Selection	Main	Selection	Main	Selection				
z ₂		(0.46) 0.33 (0.56)	(0.44) -0.33 (0.53)	(0.44) -0.33 (0.53)	(0.46) 0.32 (0.52)	(0.46) 0.32 (0.52)	(0.44) -0.32 (0.49)	(0.44) -0.32 (0.49)	(0.00) 0.30 (0.00)	(0.00) 0.30 (0.00)			
z ₃		(0.77) 0.35 (0.77)	(0.72) -0.35 (0.72)	(0.72) -0.35 (0.72)	(0.62) 0.34 (0.62)	(0.62) 0.34 (0.62)	(0.59) -0.33 (0.59)	(0.59) -0.33 (0.59)	(0.00) 0.32 (0.00)	(0.00) 0.32 (0.00)			
λ		-1.72 (0.75)	1.09 (0.65)	1.09 (0.65)									
ρ(ln)					-0.69 (0.03) 1.27 (0.00)	0.60 (30.75) 1.10 (0.00)			-0.70 (0.03) 1.27 (0.00)	-0.59 (0.03) 1.11 (0.00)			
σ(ln)													
Weak instruments, bivariate-normally distributed errors													
x ₁	2.21 (0.12) 2.64 (0.14)	0.93 (0.08) 1.39 (0.10)	2.23 (0.14) 1.80 (0.53)	-0.01 (0.00) 0.90 (0.01) -0.05 (0.37)	0.93 (0.09) 1.19 (0.36)	0.01 (0.00) -0.90 (0.01) 0.05 (0.30)	2.22 (0.13) 1.94 (0.22)	-0.01 (0.00) 0.91 (0.01) -0.03 (0.36)	0.93 (0.09) 1.24 (0.23)	0.01 (0.00) -0.90 (0.01) 0.04 (0.29)	2.23 (0.02) 1.93 (0.05)	0.93 (0.01) 1.22 (0.06)	0.91 (0.01) 0.00 (0.00) -0.02 (0.00) 0.03 (0.00) 0.03 (0.00)
x ₂													
z ₁													
z ₂													
z ₃													
λ													
ρ(ln)													
σ(ln)													

(continued)

Table 4. (continued)

Exogenous		Sample-selection-like						Endogenous switching regressions					
OLS		LIML			FIML			FIML					
z = 1	z = 0	z = 1		z = 0		z = 1		z = 0		Main	Main	Selection	
		Main	Selection	Main	Selection	Main	Selection	Main	Selection				
Panel B: Large sample													
Strong instruments, bivariate-normally distributed errors													
x ₁	2.05	1.01	2.05	-0.01	1.00	2.05	-0.01	1.00	0.01	2.05	1.01	0.89	
	(0.06)	(0.04)	(0.06)	(0.00)	(0.05)	(0.06)	(0.00)	(0.05)	(0.00)	(0.00)	(0.00)	(0.00)	
	2.55	1.23	2.13	0.89	0.98	2.05	0.89	0.99	-0.89	2.06	0.98	0.01	
	(0.07)	(0.05)	(0.10)	(0.00)	(0.06)	(0.09)	(0.00)	(0.06)	(0.00)	(0.01)	(0.00)	(0.00)	
z ₁			0.46		-0.46		0.44		-0.46			0.45	
			(0.25)		(0.21)		(0.25)		(0.21)			(0.00)	
z ₂			0.41		-0.41		0.41		-0.42			0.41	
			(0.31)		(0.25)		(0.29)		(0.24)			(0.00)	
z ₃			0.45		-0.45		0.46		-0.45			0.45	
			(0.39)		(0.32)		(0.34)		(0.30)			(0.00)	
λ	-1.39		0.80										
	(0.04)		(0.02)										
ρ(ln)						-0.96		0.63		-0.96	-0.62		
						(0.01)		(0.01)		(0.01)	(0.01)		
σ(ln)						0.80		0.36		0.80	0.36		
						(0.00)		(0.00)		(0.00)	(0.00)		
Strong instruments, non-normally distributed errors													
x ₁	2.04	1.00	2.04	-0.01	1.00	2.04	-0.01	1.00	0.01	2.04	1.00	0.72	
	(0.11)	(0.11)	(0.11)	(0.00)	(0.11)	(0.11)	(0.00)	(0.11)	(0.00)	(0.01)	(0.02)	(0.00)	
x ₂	2.50	1.22	2.16	0.75	0.99	2.01	0.74	0.87	-0.74	2.00	0.84	0.00	
	(0.13)	(0.12)	(0.17)	(0.00)	(0.16)	(0.15)	(0.00)	(0.14)	(0.00)	(0.03)	(0.02)	(0.00)	
z ₁				0.39		-0.39		0.37		-0.37		0.35	
				(0.33)		(0.33)		(0.33)		(0.33)		(0.00)	
z ₂				0.35		-0.35		0.34		-0.34		0.32	
				(0.41)		(0.39)		(0.38)		(0.37)		(0.00)	

(continued)

Table 4. (continued)

Exogenous		Sample-selection-like						Endogenous switching regressions			
OLS		LIML			FIML			FIML			
z = 1	z = 0	z = 1		z = 0		z = 1		z = 0		z = 1	z = 0
		Main	Selection	Main	Selection	Main	Selection	Main	Selection		
z ₃			0.38 (0.54)		-0.38 (0.51)		0.37 (0.46)		-0.36 (0.44)		0.35 (0.00)
λ		-1.17 (0.17)		0.78 (0.16)							
ρ(ln)					-0.52 (0.01)					-0.54 (0.01)	-0.44 (0.01)
σ(ln)					1.28 (0.00)					1.29 (0.00)	1.18 (0.00)
Weak instruments, bivariate-normally distributed errors											
x ₁	2.02 (0.06)	1.00 (0.04)	-0.03 (0.00)	1.01 (0.04)	0.03 (0.00)	2.03 (0.06)	-0.03 (0.00)	1.01 (0.04)	0.03 (0.00)	2.03 (0.00)	1.01 (0.00)
x ₂	2.78 (0.07)	1.38 (0.05)	0.92 (0.00)	1.16 (0.18)	-0.92 (0.00)	2.08 (0.12)	0.92 (0.00)	1.11 (0.13)	-0.92 (0.00)	2.09 (0.01)	1.11 (0.02)
z ₁			0.06 (0.25)		-0.06 (0.21)		0.05 (0.25)		-0.06 (0.21)		0.05 (0.00)
z ₂			0.03 (0.51)		-0.03 (0.42)		0.02 (0.35)		-0.03 (0.36)		0.03 (0.00)
z ₃			0.05 (0.68)		-0.05 (0.57)		0.05 (0.42)		-0.05 (0.46)		0.05 (0.00)
λ		-1.20 (0.30)		0.49 (0.15)							
ρ(ln)					-0.89 (0.02)					-0.89 (0.02)	-0.46 (0.05)
σ(ln)					0.77 (0.00)					0.77 (0.00)	0.33 (0.00)

considers the small-sample and large-sample simulations for the two additional setups: strong instruments and non-normally distributed errors, and weak instruments and bivariate-normally distributed errors.

We now turn to the approach where we estimate our model by treating it as two separate sample-selection problems (applying a standard Heckman selection approach to each of the two submodels). Thus, for instance, when analyzing firms that undertake mergers ($z_i = 1$), we act as if we do not observe the presence of firms that do not undertake mergers ($z_i = 0$). Furthermore, in splitting the samples to implement the Heckman procedure, we separately run both LIML and FIML estimations. When we consider the results in Table 4, it indicates that this sample-selection-like procedure can successfully reduce the endogeneity bias manifested in the estimates for x_{2i} . For instance, if we consider the case of strong instruments and bivariate-normally distributed error terms, then we obtain respective coefficient estimates for x_{2i} of 1.85 (1.84) when $z_i = 1$ and 0.92 (0.97) when $z_i = 0$ with the LIML (FIML) estimations in the small sample, and respective coefficient estimates for x_{2i} of 2.13 (2.05) and 0.98 (0.99) with the LIML (FIML) estimations in the large sample. These averaged coefficients represent a significant improvement in terms of bias as compared to the naïve approach as the corresponding values are much closer to the respective true values of 2 and 1.

Notice also that the mean estimated standard errors are lower for the FIML estimations as compared to the LIML estimations when one considers the above simulation setup with bivariate-normally distributed error terms. In light of the fact that FIML estimation is relatively more efficient when the assumption of bivariate-normally distributed error terms holds, this result does not surprise. However, in the second simulation setup we run simulations in a context of strong instruments and non-normally distributed error terms. There, we obtain respective mean LIML coefficient estimates for x_{2i} of 1.85 (2.16) and 0.93 (0.99) in the small (large) sample, whereas we obtain respective mean FIML coefficient estimates for x_{2i} of 1.78 (2.01) and 0.83 (0.87) in the small (large) sample. With the exception of the coefficient estimate for x_{2i} when $z_i = 1$ and the sample is large, the coefficients yielded by the LIML estimations manifest less bias (i.e., they are closer to the true values) as compared to the coefficient estimates yielded by the FIML estimations. Thus, LIML is preferable to FIML estimation when the assumption of bivariate-normally distributed error terms is violated, as LIML performs significantly better in terms of yielding less-biased coefficient estimates.

When considering the simulation setup of weak instruments and bivariate-normally distributed error terms, we observe that FIML estimations are generally preferable as compared to LIML estimations. First off, the results are somewhat mixed in the small-sample context: the respective mean coefficient estimates for x_{2i} under LIML estimation are 1.80 and 1.19, and under FIML estimation they are 1.94 and 1.24. Accordingly, the estimates from LIML are closer to the true coefficient values (i.e., less biased) when $z_i = 0$, however the estimates from FIML are closer to the true coefficient values when $z_i = 1$. The above said, FIML unambiguously outperforms LIML in the large-sample context. Namely, the respective mean coefficient estimates for x_{2i} under LIML estimation are 2.23 and 1.16, while they are 2.08 and 1.11 under FIML estimation. In addition, the mean estimated standard errors for the FIML estimates are significantly smaller than those obtained via LIML for both the small and large samples. It is also worth noting that even in the weak instrument context, both the LIML and FIML estimators indicate a significant reduction in bias for the x_{2i} coefficient when compared with the naïve OLS estimations. It behooves us to note then that employing weak instruments to model endogenous self-selection represents a preferable course for empirical researchers as compared to outright neglecting the selection process in such a weak instrument context. In part, this is due to the fact that some identification comes about from the functional restrictions regarding the distribution of error terms as these are rightly assumed to be normal.

Finally, we present the results for the full endogenous switching regression model via FIML estimation in the last three columns of Table 4. The idea here is that the researcher can simultaneously estimate Equations (9), (12a), and (12b). When reviewing the estimation results for the endogenous-

switching regressions as a whole, we observe that the coefficient estimates across all three simulation setups for both small and large samples yield means that are very close—and in several cases numerically identical—to the mean coefficients obtained by engaging in a sample-selection-like Heckman procedure that uses FIML estimation. This convergence is—in part—driven by the FIML estimator being employed for both procedures. Moreover, this is reassuring in that it indicates that the simplified procedure using Heckman's approach on the subsamples can be viewed as a useful approximation of the more appropriate full-blown FIML estimator. That said, the mean estimated standard errors from the simultaneous endogenous-switching procedure are always significantly smaller as compared to the mean standard errors obtained from the sample-selection-like procedure; thus, our simulation results clearly indicate that employing such a procedure leads to a substantial increase in the precision of the estimations. Since many empirical applications place great importance on gathering whether a particular effect is evident or not (and less importance on the exact magnitude of the effect), such an observation is quite important. Gaining precision by employing a simultaneous approach might then be pivotal in identifying whether or not a particular explanatory construct subject to self-selection actually has an effect on the outcome variable. It is, nevertheless, important to keep in mind that the FIML estimator relies on the assumption that the error terms in the selection and main equations follow a bivariate normal distribution. If this assumption is violated, the LIML estimator delivers superior results in terms of reduced bias.

An additional issue requiring discussion involves how effective are the different methodological approaches when it comes to predicting the existence of self-selection bias. This is often a crucial question for empirical researchers since the presence of selection bias motivates whether they go beyond a simple OLS approach and employ more sophisticated procedures that model the selection process. In our parameterization, the selection problem is driven by the correlation between the error terms in the selection and main equations, as well as by the existence of an omitted variable that affects both the selection process and the main outcome variable. When considering the Heckman sample-selection-like procedure that employs LIML estimation, the coefficient estimates capturing the selection term (the λ) always have the correct sign (positive for $z_i = 0$ and negative for $z_i = 1$).¹⁴ While it is not straightforward in our context to establish whether the estimated selection terms depart from their true value, such bias is often not a major issue for researchers as they are primarily interested in gathering whether a selection effect exists and whether it goes in a particular direction; that is, the sign and significance (rather than the magnitude) of the selection term is often critical. Most relevant then for the empirical researcher is whether the coefficient estimates for the selection term are, on average, not significantly different from zero when instruments are weak and the sample is relatively small. In particular, researchers often face situations where instruments are weak and samples are relatively small; hence, they might fail to identify selection effects in such situations due to biased selection-term estimates, or imprecisely estimated selection terms due to large standard errors.

When considering the procedures (Heckman sample-selection-like, and the endogenous switching regression) that employ FIML estimation, the empirical results concerning the significance of selection vary across a few dimensions.¹⁵ First, the correlation coefficient (ρ)—the relevant parameter when considering selection—is biased on average, though this bias tends to be small and definitely smaller than the bias observed for the estimated variance (σ). Second as expected, the FIML procedures perform particularly well when instruments are strong and errors are bivariate-normally distributed. Third, the mean coefficient estimates for the correlation coefficient are now—unlike the case of LIML estimation—significant when instruments are weak and the errors bivariate-normally distributed. Thus in this context, researchers might be able to detect “significant” selection effects even when the point estimate for the correlation is biased. Accordingly, the weak instrument problem is less severe when researchers employ FIML estimation due to the identification being partially driven by restrictions on the error-term distribution. The above said, the FIML estimator might fail to identify selection when the normality assumption is violated. In sum, employing the FIML estimator

to diagnose selection effects can be very useful in cases where instruments are not particularly strong so long as the researcher is willing to accept the assumption of bivariate-normally distributed error terms.

Conclusions and Prescriptions

Our study first provides a clear caution to empirical researchers throughout management that neglecting the endogenous nature of the selection into a particular organizational decision can lead to biased and inconsistent empirical results. Moreover, such biased results can lead to spurious causal inferences that prevent the generation of well-informed theoretical and managerial conclusions. Taking both an informal and formal approach, we outline the exact nature of self-selection-based endogeneity bias. Furthermore, our empirical simulations illustrate how employing a naïve OLS approach in a context where endogenous treatment and endogenous switching are present can lead to biased and erroneous findings. While the management literature has begun to appreciate over the last decade the importance of factoring self-selection effects due to the prevalence of this issue in much of our empirical scholarship, it is nevertheless important to continue to remind, outline and demonstrate to researchers that they need to be conscious of selection-based biases in their empirical estimations. Beyond the need for researchers to correctly recognize the existence of selection-based endogeneity, it is also important that researchers clearly comprehend how this endogeneity manifests; that is, what is the exact source of the endogeneity bias. It is, of course, the self-selection by managers into different organizational decisions which represents the principal channel via which omitted factors raise the issue of self-selection-based endogeneity bias in the management discipline. Moreover in many management applications, organizational decisions tend to be discrete rather than continuous; hence, the researcher must correctly model this discrete process to deal with the selection effects.

In light of selection bias ultimately being a subset of endogeneity bias, a fundamental issue of concern for empirical researchers in management is the identification of appropriate exclusion restrictions (i.e., instruments) to tackle the endogeneity problem. Unfortunately, this issue still seems to be a major problem in empirical research in management. In particular, many empirical studies appear to employ instruments that are not truly exogenous as they correlate with the error term in the substantive equation. Furthermore, many studies suffer from weak instrumentation in the sense that the instruments do not significantly (or only weakly) correlate with the potentially endogenous variable. Such practices can lead to coefficient estimates that involve greater biases as compared to results employing naïve OLS estimates. Even more troubling is the common practice by researchers to be quite un-transparent regarding this issue, as the selection-equation estimations are often not reported. In addition to not spending adequate time to outline the implicit identification strategy, diagnostics which test the quality of the instruments are often not mentioned or discussed. It is also worth mentioning that the choice of instruments and the choice of methodological approach can interact and play a crucial role in empirical applications. We strongly urge then that empirical scholars spend considerably more time, care and attention with regard to properly identifying their selection equations, as far too often the approach to this crucial issue seems to be cavalier and superficial.

Basile (2008), Semadeni et al. (2014), and others have advocated the use of simple linear-probability models to estimate selection equations (i.e., the process which explains the manifestation of the treatment) in the context of endogenous-treatment models. Our empirical simulations suggest that an IV-OLS approach can yield relatively unbiased coefficients in the context of strong instruments. However, our simulations also suggest that employing such an approach might lead to misleading results when instruments are weak. It should be noted then that empirical researchers in management often face situations where it is difficult to establish a strong set of instruments; thus, employing a linear estimator via 2SLS can generate incorrect inferences and conclusions in the all-

too-frequent context of weak instruments. The methodological literature also cautions that relying on a linear approximation to model a discrete treatment variable can involve estimation issues. It is comforting then that our review of the literature suggests that this approach has not been commonly taken up by empirical researchers in management. Yet it is worth mentioning the limitations of this approach so that future researchers continue to be cautious with respect to employing a linear approximation to model the selection into a treatment.

The empirical researcher must then be quite careful in setting up the correct modeling approach to deal with the exact nature of the endogenous self-selection and how the treatment variable affects the outcome of ultimate interest. In particular, it is important in the context of self-selection-based endogeneity to differentiate between endogenous treatment (which only involves an intercept effect) and endogenous switching (which also involves different slope coefficients for the other explanatory variables). Under endogenous treatment, a researcher may be convinced (or more interested) that the treatment effect exclusively manifests as an intercept effect. In this case, one assumes that all other covariates similarly affect the outcome variable independent of the treatment; hence, the treatment simply shifts the regression line up or down. Under endogenous switching, however, a researcher holds that the treatment effect is mediated by the other explanatory variables and thus also involves a slope effect. Moreover, these two variants (endogenous treatment and endogenous switching) of self-selection bias are distinct from the foundational sample-selection bias. Yet the differences between endogenous treatment, endogenous switching and sample-selection are quite subtle; thus, these differences have not been fully appreciated in the empirical literature and researchers far too often mix-up these different types of selection effects. Since the alternative methodological approaches line up with the different ways via which selection can manifest, the choice of modeling approach should be principally guided by the specific research question being faced by the researcher.

While a few different modeling approaches exist to deal with the different forms of selection-bias, the empirical researcher mindful of selection-based endogeneity can also choose between different estimation methods. Characteristic of the different selection models is that the selection process is generally modeled as a probit, the error term in the selection equation is commonly assumed to be normally distributed, and the alternative estimators are based on maximum likelihood procedures. In particular, we extensively discussed the choice between the LIML and FIML estimators in the context of full selection models that entail a selection equation into the treatment as well as one or more main substantive equations. The LIML estimator is certainly more robust but it is also less efficient as compared to the FIML estimator. FIML is asymptotically efficient, but it hinges on the additional assumption of joint normality between the error terms of the selection and substantive equations. While there is no clear-cut rule with respect to choosing between these two estimators, it is fair to state that the additional assumption residing behind the FIML estimator becomes less restrictive when the sample gets larger. Thus, the LIML estimator is potentially advisable when the sample is relatively small. Moreover, our main suggestion regarding this issue is that researchers be cautious and transparent regarding the choice between LIML and FIML; that is, discuss the trade-offs involved with the two estimators, and show the empirical results while employing both estimators.

Selection into organizational decisions is not a randomized process but is instead an endogenous process undertaken by managers with the intent to enhance organizational outcomes. Since empirical researchers in management often study the effect of these discrete organizational choices on outcomes by employing observational data sets, the issue of unobservable factors which correlate with both the organizational decision and the outcome variable of interest indicates that self-selection-based endogeneity is both fundamental and characteristic of much of what we study in management. Our contribution here attempts to clarify the nature of selection-based endogeneity bias and provide researchers in management with a transparent understanding of the different methodologies

available to correctly model and control for selection effects. Our hope is that this effort might spur researchers in management to be more comprehensive and precise in dealing with selection effects in their empirical applications. Enhanced efforts in our discipline to think carefully and be transparent about the sources of endogeneity in our empirical models will lead to sounder causal inferences that are both consistent and unbiased, which in turn ultimately leads to stronger theoretical and managerial conclusions: the ultimate test of our discipline.

Appendix A. Sample-Selection Studies in *Strategic Management Journal*.

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Self-selection present
Baum, J. A., Calabrese, T., & Silverman, B. S. (2000). Don't go it alone: Alliance network composition and startups' performance in Canadian biotechnology. <i>Strategic Management Journal</i> , 21(3), 267-294.	Auxiliary, Lee's generalization of Heckman				
Chang, S. J., & Rosenzweig, P. M. (2001). The choice of entry mode in sequential foreign direct investment. <i>Strategic Management Journal</i> , 22(8), 747-776.	Omits observations	None			
Canyon, M. J., Peck, S. I., & Sadler, G. V. (2001). Corporate tournaments and executive compensation: Evidence from the UK. <i>Strategic Management Journal</i> , 22(8), 805-815.	Tests for selection	No	Yes	No	
Westphal, J. D., & Fredrickson, J. W. (2001). Who directs strategic change? Director experience, the selection of new CEOs, and change in corporate strategy. <i>Strategic Management Journal</i> , 22(12), 1113-1137.	Heckman 2-stage	No	Not reported	Not reported	Evident but not discussed
Gulati, R., & Higgins, M. C. (2003). Which ties matter when? The contingent effects of interorganizational partnerships on IPO success. <i>Strategic Management Journal</i> , 24(2), 127-144.	Heckman 2-stage	No	Yes	No	
Delmar, F., & Shane, S. (2003). Does business planning facilitate the development of	Lee's generalization of Heckman	Yes	Yes	Yes	

(continued)

Appendix A. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Self-selection present
new ventures? <i>Strategic Management Journal</i> , 24(12), 1165-1185.					
Shamsie, J., Phelps, C., & Kuperman, J. (2004). Better late than never: A study of late entrants in household electrical equipment. <i>Strategic Management Journal</i> , 25(1), 69-84.	Heckman 2-stage	Yes	No	No	
Sanders, W. M., & Boivie, S. (2004). Sorting things out: Valuation of new firms in uncertain markets. <i>Strategic Management Journal</i> , 25(2), 167-186.	Heckman 2-stage	No	No	No	
Huyghebaert, N., & Van de Gucht, L. M. (2004). Incumbent strategic behavior in financial markets and the exit of entrepreneurial start-ups. <i>Strategic Management Journal</i> , 25(7), 669-688.	Heckman 2-stage	No	Not clear	No	
Mishina, Y., Pollock, T. G., & Porac, J. F. (2004). Are more resources always better for growth? Resource stickiness in market and product expansion. <i>Strategic Management Journal</i> , 25(12), 1179-1197.	Heckman 2-stage	No	No	No	Evident but not discussed
Haas, M. R., & Hansen, M. T. (2005). When using knowledge can hurt performance: The value of organizational capabilities in a management consulting company. <i>Strategic Management Journal</i> , 26(1), 1-24.	Alternative 2-part logit model	Yes	Yes	No	
Nachum, L., & Zaheer, S. (2005). The persistence of distance? The impact of technology on MNE motivations for foreign investment. <i>Strategic Management Journal</i> , 26(8), 747-767.	Fixed industry effects	There is none	N/A	N/A	
Park, N. K., & Mezias, J. M. (2005). Before and after the technology sector crash: The	Omits observations	There is none	N/A	N/A	Evident but not discussed

(continued)

Appendix A. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Self-selection present
effect of environmental munificence on stock market response to alliances of e-commerce firms. <i>Strategic Management Journal</i> , 26(11), 987-1007.					
Higgins, M. C., & Gulati, R. (2006). Stacking the deck: The effects of top management backgrounds on investor decisions. <i>Strategic Management Journal</i> , 27(1), 1-25.	Heckman 2-stage	No	No	Not reported	Evident but not discussed
Westphal, J. D., Boivie, S., Chng, M., & Han, D. (2006). The strategic impetus for social network ties: Reconstituting broken CEO friendship ties. <i>Strategic Management Journal</i> , 27(5), 425-445.	Heckman 2-stage	No	Not clear	Not reported	
Henderson, A. D., Miller, D., & Hambrick, D. C. (2006). How quickly do CEOs become obsolete? Industry dynamism, CEO tenure, and company performance. <i>Strategic Management Journal</i> , 27(5), 447-460.	Lee's generalization of Heckman	No	Not clear	Yes	
Hayward, M. L., & Shimizu, K. (2006). De-commitment to losing strategic action: Evidence from the divestiture of poorly performing acquisitions. <i>Strategic Management Journal</i> , 27(6), 541-557.	Auxiliary Heckman 2-stage	No	Not clear	Not reported	Evident but not discussed
Wright, P., Kroll, M., Krug, J. A., & Pettus, M. (2007). Influences of top management team incentives on firm risk taking. <i>Strategic Management Journal</i> , 28(1), 81-89.	Auxiliary logit estimation	No	No	No	
Karaevli, A. (2007). Performance consequences of new CEO "outsiderness": Moderating effects of pre-and post-succession contexts. <i>Strategic Management Journal</i> , 28(7), 681-706.	Heckman 2-stage	No	Yes	No	Evident but not discussed

(continued)

Appendix A. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Self-selection present
Nerkar, A., & Shane, S. (2007). Determinants of invention commercialization: An empirical examination of academically sourced inventions. <i>Strategic Management Journal</i> , 28(11), 1155-1166.	Lee's generalization of Heckman	No	Yes	No	
Danneels, E. (2008). Organizational antecedents of second-order competences. <i>Strategic Management Journal</i> , 29(5), 519-543.	Auxiliary diagnostics	No	No	No	
Semadeni, M., Cannella, A. A., Jr., Fraser, D. R., & Lee, D. S. (2008). Fight or flight: Managing stigma in executive careers. <i>Strategic Management Journal</i> , 29(5), 557-567.	Heckman 2-stage	Yes	Yes	Not reported	Evident but not discussed
Boyd, J. L., & Bresser, R. K. (2008). Performance implications of delayed competitive responses: Evidence from the US retail industry. <i>Strategic Management Journal</i> , 29(10), 1077-1096.	Diagnostics comparing sample with population	N/A	N/A	N/A	
Mitsuhashi, H., Shane, S., & Sine, W. D. (2008). Organization governance form in franchising: Efficient contracting or organizational momentum? <i>Strategic Management Journal</i> , 29(10), 1127-1136.	Lee's generalization of Heckman	No	Yes	Yes	
Mackey, A. (2008). The effect of CEOs on firm performance. <i>Strategic Management Journal</i> , 29(12), 1357-1367.	Auxiliary Heckman 2-stage	No	Not clear	Not clear	
Meyer, K. E., Estrin, S., Bhaumik, S. K., & Peng, M. W. (2009). Institutions, resources, and entry strategies in emerging economies. <i>Strategic Management Journal</i> , 30(1), 61-80.	Auxiliary Heckman 2-stage	No	Not clear	No	
Yang, H., Lin, Z. J., & Lin, Y. L. (2010). A multilevel framework of firm boundaries: Firm characteristics, dyadic differences, and network attributes. <i>Strategic Management Journal</i> , 31(3), 237-261.	Heckman 2-stage	Yes	Yes	Yes	

(continued)

Appendix A. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Self-selection present
Surroca, J., Tribó, J. A., & Waddock, S. (2010). Corporate responsibility and financial performance: The role of intangible resources. <i>Strategic Management Journal</i> , 31(5), 463-490.	Robust over subsamples	N/A	N/A	N/A	
Nadkarni, S., Herrmann, P., & Perez, P. D. (2011). Domestic mindsets and early international performance: The moderating effect of global industry conditions. <i>Strategic Management Journal</i> , 32(5), 510-531.	Auxiliary Heckman 2-stage	No	Not clear	No	
Kotha, R., Zheng, Y., & George, G. (2011). Entry into new niches: The effects of firm age and the expansion of technological capabilities on innovative output and impact. <i>Strategic Management Journal</i> , 32(9), 1011-1024.	Auxiliary Heckman 2-stage	No	Not clear	Not clear as not reported	
Ethiraj, S. K., Ramasubbu, N., & Krishnan, M. S. (2012). Does complexity deter customer-focus? <i>Strategic Management Journal</i> , 33(2), 137-161.	Heckman 2-stage	No	Yes	Yes	
Diestre, L., & Rajagopalan, N. (2012). Are all "sharks" dangerous? New biotechnology ventures and partner selection in R&D alliances. <i>Strategic Management Journal</i> , 33(10), 1115-1134.	Auxiliary Heckman 2-stage	No	Not clear	Not clear as not reported	
Lavie, D., Haunschild, P. R., & Khanna, P. (2012). Organizational differences, relational mechanisms, and alliance performance. <i>Strategic Management Journal</i> , 33(13), 1453-1479.	Inclusion as controls for variables that predict sample selection	No	No	N/A	Also evident but not discussed
Kang, J. (2013). The relationship between corporate diversification and corporate social performance. <i>Strategic Management Journal</i> , 34(1), 94-109.	Heckman 2-stage	Yes	Yes	No	

(continued)

Appendix A. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Self-selection present
Wiersema, M. F., & Zhang, Y. A. (2013). Executive turnover in the stock option backdating wave: The impact of social context. <i>Strategic Management Journal</i> , 34(5), 590-609.	Heckman 2-stage	No	Yes	No	
Zhu, D. H. (2013). Group polarization on corporate boards: Theory and evidence on board decisions about acquisition premiums. <i>Strategic Management Journal</i> , 34(7), 800-822.	Heckman 2-stage	No	Yes	Yes	
Ndofor, H. A., Vanevenhoven, J., & Barker, V. L. (2013). Software firm turnarounds in the 1990s: An analysis of reversing decline in a growing, dynamic industry. <i>Strategic Management Journal</i> , 34(9), 1123-1133.	Auxiliary Heckman 2-stage	No	Not clear	Not clear	Also evident but not discussed
Mudambi, R., & Swift, T. (2014). Knowing when to leap: Transitioning between exploitative and explorative R&D. <i>Strategic Management Journal</i> , 35(1), 126-145.	Auxiliary Heckman 2-stage	No	Yes	Not clear	
Eggers, J. P. (2014). Competing technologies and industry evolution: The benefits of making mistakes in the flat panel display industry. <i>Strategic Management Journal</i> , 35(2), 159-178.	Heckman 2-stage	Yes	Yes	No	
Xie, X., & O'Neill, H. M. (2014). Learning and product entry: How diversification patterns differ over firm age and knowledge domains in US generic drug industry. <i>Strategic Management Journal</i> , 35(3), 440-449.	Heckman 2-stage	No	Not clear	Yes	
Stern, I., Dukerich, J. M., & Zajac, E. (2014). Unmixed signals: How reputation and status affect alliance formation. <i>Strategic Management Journal</i> , 35(4), 512-531.	Heckman 2-stage	Yes	No	No	

(continued)

Appendix A. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Self-selection present
Krause, R., & Semadeni, M. (2014). Last dance or second chance? Firm performance, CEO career horizon, and the separation of board leadership roles. <i>Strategic Management Journal</i> , 35(6), 808-825.	Heckman 2-stage	No	Yes	No	
Berry, H. (2014). Global integration and innovation: Multicountry knowledge generation within MNCs. <i>Strategic Management Journal</i> , 35(6), 869-890.	2-stage bivariate probit	No	Yes	Yes	
Marcel, J. J., & Cowen, A. P. (2014). Cleaning house or jumping ship? Understanding board upheaval following financial fraud. <i>Strategic Management Journal</i> , 35(6), 926-937.	Auxiliary Heckman 2-stage	No	Yes	Not clear as not reported	
Feldman, E. R., Gilson, S. C., & Villalonga, B. (2014). Do analysts add value when they most can? Evidence from corporate spin-offs. <i>Strategic Management Journal</i> , 35(10), 1446-1463.	Heckman 2-stage	Yes	Yes	Yes	
Patel, P. C., & Cooper, D. (2014). The harder they fall, the faster they rise: Approach and avoidance focus in narcissistic CEOs. <i>Strategic Management Journal</i> , 35(10), 1528-1540.	Heckman 2-stage	No	Yes	Yes	
Stettner, U., & Lavie, D. (2014). Ambidexterity under scrutiny: Exploration and exploitation via internal organization, alliances, and acquisitions. <i>Strategic Management Journal</i> , 35(13), 1903-1929.	Heckman 2-stage	No	Yes	Yes	

Appendix B. Self-Selection Studies in *Strategic Management Journal*.

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Sample selection present
Nickerson, J. A., Hamilton, B. H., & Wada, T. (2001). Market position, resource profile, and governance: Linking Porter and Williamson in the context of international courier and small package services in Japan. <i>Strategic Management Journal</i> , 22(3), 251-273.	Instruments in a recursive system	N/A	Yes	N/A	
Kale, P., Dyer, J. H., & Singh, H. (2002). Alliance capability, stock market response, and long-term alliance success: The role of the alliance function. <i>Strategic Management Journal</i> , 23(8), 747-767.	Heckman 2-stage	No	Not clear as not reported	Yes	
Leiblein, M. J., Reuer, J. J., & Dalsace, F. (2002). Do make or buy decisions matter? The influence of organizational governance on technological performance. <i>Strategic Management Journal</i> , 23(9), 817-833.	Heckman 2-stage & Lee procedures	Yes	Yes	Yes	
Leiblein, M. J., & Miller, D. J. (2003). An empirical examination of transaction- and firm-level influences on the vertical boundaries of the firm. <i>Strategic Management Journal</i> , 24(9), 839-859.	Tests for selection via Heckman procedure	No	Not clear	No	
Brouthers, K. D., Brouthers, L. E., & Werner, S. (2003). Transaction cost-enhanced entry mode	Heckman 2-stage	Yes	Yes	Yes	

(continued)

Appendix B. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Sample selection present
choices and firm performance. <i>Strategic Management Journal</i> , 24(12), 1239-1248.					
Krishnan, R. A., Joshi, S., & Krishnan, H. (2004). The influence of mergers on firms' product-mix strategies. <i>Strategic Management Journal</i> , 25(6), 587-611.	Heckman 2-stage	No	No	No	
Singh, K., & Mitchell, W. (2005). Growth dynamics: The bidirectional relationship between interfirm collaboration and business sales in entrant and incumbent alliances. <i>Strategic Management Journal</i> , 26(6), 497-521.	Heckman 2-stage	Yes	Yes	Yes	
Cantwell, J., & Mudambi, R. (2005). MNE competence-creating subsidiary mandates. <i>Strategic Management Journal</i> , 26(12), 1109-1128.	Heckman 2-stage & Lee procedures	Yes	No	Yes	
Miller, D. J. (2006). Technological diversity, related diversification, and firm performance. <i>Strategic Management Journal</i> , 27(7), 601-619.	Heckman's 2-stage	Yes	Yes	Yes	
Arend, R. J. (2006). SME-supplier alliance activity in manufacturing: Contingent benefits and perceptions. <i>Strategic Management Journal</i> , 27(8), 741-763.	Heckman 2-stage	Yes	Yes	Yes	
Morrow, J. L., Sirmon, D. G., Hitt, M. A., & Holcomb, T. R. (2007). Creating value in the face of declining performance: Firm	Heckman 2-stage	Yes	Yes	Yes	

(continued)

Appendix B. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Sample selection present
strategies and organizational recovery. <i>Strategic Management Journal</i> , 28(3), 271-283.					
Lazzarini, S. G. (2007). The impact of membership in competing alliance constellations: Evidence on the operational performance of global airlines. <i>Strategic Management Journal</i> , 28(4), 345-367.	Auxiliary Heckman 2-stage	No	Yes	No	
Ethiraj, S. K. (2007). Allocation of inventive effort in complex product systems. <i>Strategic Management Journal</i> , 28(6), 563-584.	Heckman 2-stage	Yes	Yes	Yes	
Williams, C. (2007). Transfer in context: Replication and adaptation in knowledge transfer relationships. <i>Strategic Management Journal</i> , 28(9), 867-889.	Structural equation modeling	N/A	N/A	N/A	
Capron, L., & Shen, J. C. (2007). Acquisitions of private vs. public firms: Private information, target selection, and acquirer returns. <i>Strategic Management Journal</i> , 28(9), 891-911.	Heckman 2-stage	Yes	No	No	
Lavie, D. (2007). Alliance portfolios and firm performance: A study of value creation and appropriation in the US software industry. <i>Strategic Management Journal</i> , 28(12), 1187-1212.	Auxiliary Heckman 2-stage	No	Not clear as not reported	Not reported	
Sinha, R. K., & Noble, C. H. (2008). The adoption of radical manufacturing	Heckman 2-stage	No	Not clear	No	Also evident and discussed

(continued)

Appendix B. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Sample selection present
technologies and firm survival. <i>Strategic Management Journal</i> , 29(9), 943-962.					
Marcel, J. J. (2009). Why top management team characteristics matter when employing a chief operating officer: A strategic contingency perspective. <i>Strategic Management Journal</i> , 30(6), 647-658.	Auxiliary Heckman 2-stage	No	Not clear	Not clear	
Iyengar, R. J., & Zampelli, E. M. (2009). Self-selection, endogeneity, and the relationship between CEO duality and firm performance. <i>Strategic Management Journal</i> , 30(10), 1092-1112.	Switching regressions model	Yes	Yes	Yes	
Shamsie, J., Martin, X., & Miller, D. (2009). In with the old, in with the new: Capabilities, strategies, and performance among the Hollywood studios. <i>Strategic Management Journal</i> , 30(13), 1440-1452.	Auxiliary Heckman 2-stage and IV approaches	No	Not clear	Not reported	
Zhang, Y., & Li, H. (2010). Innovation search of new ventures in a technology cluster: The role of ties with service intermediaries. <i>Strategic Management Journal</i> , 31(1), 88-109.	Heckman 2-stage	No	3 industry dummy variables	No	
Corredoira, R. A., & Rosenkopf, L. (2010). Should auld acquaintance be forgot? The reverse transfer of knowledge through mobility ties. <i>Strategic Management Journal</i> , 31(2), 159-181.	Auxiliary IV approach	No	Not clear	N/A	

(continued)

Appendix B. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Sample selection present
Chen, S. F. S. (2010). Transaction cost implication of private branding and empirical evidence. <i>Strategic Management Journal</i> , 31(4), 371-389.	Heckman 2-stage & Lee procedures	Yes	No	Yes	
Hoang, H. A., & Rothaermel, F. T. (2010). Leveraging internal and external experience: Exploration, exploitation, and R&D project performance. <i>Strategic Management Journal</i> , 31(7), 734-758.	Heckman's 2-stage with multinomial logit first stage	No	Yes	No	Also evident
Arikan, A. M., & Capron, L. (2010). Do newly public acquirers benefit or suffer from their pre-IPO affiliations with underwriters and VCs? <i>Strategic Management Journal</i> , 31(12), 1257-1289.	Heckman 2-stage	Yes	Yes	No	Also evident and tested
Parmigiani, A., & Holloway, S. S. (2011). Actions speak louder than modes: Antecedents and implications of parent implementation capabilities on business unit performance. <i>Strategic Management Journal</i> , 32(5), 457-485.	Heckman 2-stage	Yes	Yes	Yes	
Ho, J. L., Wu, A., & Xu, S. X. (2011). Corporate governance and returns on information technology investment: Evidence from an emerging market. <i>Strategic Management Journal</i> , 32(6), 595-623.	Auxiliary Heckman 2-stage & IV approaches	No	No	No	

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Appendix B. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Sample selection present
Oh, C. H., & Oetzel, J. (2011). Multinationals' response to major disasters: How does subsidiary investment vary in response to the type of disaster and the quality of country governance? <i>Strategic Management Journal</i> , 32(6), 658-681.	Auxiliary Heckman 2-stage	No	Not clear	Not reported	
Gore, A. K., Matsunaga, S., & Eric Yeung, P. (2011). The role of technical expertise in firm governance structure: Evidence from chief financial officer contractual incentives. <i>Strategic Management Journal</i> , 32(7), 771-786.	Heckman 2-stage	Yes	Yes	Yes	
Hasan, I., Kobeissi, N., & Wang, H. (2011). Global equity offerings, corporate valuation, and subsequent international diversification. <i>Strategic Management Journal</i> , 32(7), 787-796.	Lee's generalization of Heckman	No	Yes	Yes	
Jensen, P. H., Thomson, R., & Yong, J. (2011). Estimating the patent premium: Evidence from the Australian Inventor Survey. <i>Strategic Management Journal</i> , 32(10), 1128-1138.	Auxiliary Heckman 2-stage	No	Not clear	Not clear as not reported	
Park, H. D., & Steensma, H. K. (2012). When does corporate venture capital add value for new ventures? <i>Strategic Management Journal</i> , 33(1), 1-22.	IV approach via bivariate probit model	Yes	Yes	N/A	

(continued)

Appendix B. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Sample selection present
Quigley, T. J., & Hambrick, D. C. (2012). When the former CEO stays on as board chair: Effects on successor discretion, strategic change, and performance. <i>Strategic Management Journal</i> , 33(7), 834-859.	Heckman 2-stage	No	Yes	No	
Macher, J. T., & Boerner, C. (2012). Technological development at the boundaries of the firm: A knowledge-based examination in drug development. <i>Strategic Management Journal</i> , 33(9), 1016-1036.	Heckman 2-stage	Yes	Yes	Yes	
Jansen, J. J., Simsek, Z., & Cao, Q. (2012). Ambidexterity and performance in multiunit contexts: Cross-level moderating effects of structural and resource attributes. <i>Strategic Management Journal</i> , 33(11), 1286-1303.	Auxiliary regressions fail to explain selection, and results robust over subsamples	No	Not clear	N/A	
Weigelt, C. (2013). Leveraging supplier capabilities: The role of locus of capability deployment. <i>Strategic Management Journal</i> , 34(1), 1-21.	Lee's generalization of Heckman	Yes	Yes	Yes	Also evident and auxiliary Heckman employed to rule out
Lim, D. S., Celly, N., Morse, E. A., & Rowe, W. G. (2013). Rethinking the effectiveness of asset and cost retrenchment: The contingency effects of a firm's rent creation mechanism. <i>Strategic Management Journal</i> , 34(1), 42-61.	Heckman 2-stage	Yes	Yes	No	

(continued)

Appendix B. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Sample selection present
Chung, C. N., & Luo, X. R. (2013). Leadership succession and firm performance in an emerging economy: Successor origin, relational embeddedness, and legitimacy. <i>Strategic Management Journal</i> , 34(3), 338-357.	Heckman 2-stage	No	Yes	Yes	
Mulotte, L., Dussauge, P., & Mitchell, W. (2013). Does pre-entry licensing undermine the performance of subsequent independent activities? Evidence from the global aerospace industry, 1944-2000. <i>Strategic Management Journal</i> , 34(3), 358-372.	Heckman 2-stage with ordered probit	Yes	Yes	Yes	
Miller, D., Minichilli, A., & Corbetta, G. (2013). Is family leadership always beneficial? <i>Strategic Management Journal</i> , 34(5), 553-571.	Heckman 2-stage	No	Yes	Yes	
Zahavi, T., & Lavie, D. (2013). Intra-industry diversification and firm performance. <i>Strategic Management Journal</i> , 34(8), 978-998.	Heckman 2-stage	Yes	No	No	
Lahiri, N., & Narayanan, S. (2013). Vertical integration, innovation, and alliance portfolio size: Implications for firm performance. <i>Strategic Management Journal</i> , 34(9), 1042-1064.	Heckman 2-stage	Yes	Yes	Yes	
Carson, S. J., & John, G. (2013). A theoretical and empirical investigation of property rights sharing in outsourced research,	Procedure where residuals from selection equation are entered as regressors in the substantive equation.	Yes	Yes	Yes	

(continued)

Appendix B. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Sample selection present
development, and engineering relationships. <i>Strategic Management Journal</i> , 34(9), 1065-1085.					
Tong, T. W., & Li, S. (2013). The assignment of call option rights between partners in international joint ventures. <i>Strategic Management Journal</i> , 34(10), 1232-1243.	Heckman 2-stage	No	Not clear	No	
Wu, B. (2013). Opportunity costs, industry dynamics, and corporate diversification: Evidence from the cardiovascular medical device industry, 1976-2004. <i>Strategic Management Journal</i> , 34(11), 1265-1287.	2-step procedure	Yes	Yes	Yes	
Yang, H., Zheng, Y., & Zhao, X. (2014). Exploration or exploitation? Small firms' alliance strategies with large firms. <i>Strategic Management Journal</i> , 35(1), 146-157.	Heckman 2-stage	No	No	No	
Jia, N. (2014). Are collective political actions and private political actions substitutes or complements? Empirical evidence from China's private sector. <i>Strategic Management Journal</i> , 35(2), 292-315.	Auxiliary 2-stage probit model	Yes	Yes	N/A	
Patel, P. C., Fernhaber, S. A., McDougall-Covin, P. P., & van der Have, R. P. (2014). Beating competitors to international markets: The value of	Heckman 2-stage	No	Yes	Yes	

(continued)

Appendix B. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Sample selection present
geographically balanced networks for innovation. <i>Strategic Management Journal</i> , 35(5), 691-711.					
Laamanen, T., Brauer, M., & Junna, O. (2014). Performance of acquirers of divested assets: Evidence from the US software industry. <i>Strategic Management Journal</i> , 35(6), 914-925.	Heckman 2-stage with multivariate choices	No	Not clear	No	
Pathak, S., Hoskisson, R. E., & Johnson, R. A. (2014). Settling up in CEO compensation: The impact of divestiture intensity and contextual factors in refocusing firms. <i>Strategic Management Journal</i> , 35(8), 1124-1143.	Auxiliary Heckman 2-stage	No	Yes	No	
Heide, J. B., Kumar, A., & Wathne, K. H. (2014). Concurrent sourcing, governance mechanisms, and performance outcomes in industrial value chains. <i>Strategic Management Journal</i> , 35(8), 1164-1185.	Auxiliary Heckman 2-stage	No	Yes	No	Also evident but not discussed
Herrmann, P., & Nadkarni, S. (2014). Managing strategic change: The duality of CEO personality. <i>Strategic Management Journal</i> , 35(9), 1318-1342.	Auxiliary Heckman 2-stage	Yes	Yes	No	
Jain, A., & Thiétart, R. A. (2014). Capabilities as shift parameters for the outsourcing decision. <i>Strategic Management Journal</i> , 35(12), 1881-1890.	Auxiliary Heckman 2-stage and switching regressions approaches	No	No	No	

(continued)

Appendix B. (continued)

Article	Correction method	Reported selection equation	Identification variable(s)	Significant selection	Sample selection present
Shen, R., Tang, Y., & Chen, G. (2014). When the role fits: How firm status differentials affect corporate takeovers. <i>Strategic Management Journal</i> , 35(13), 2012-2030.	Auxiliary Heckman 2-stage	No	Not clear	Not clear as not reported	
Rubera, G., & Tellis, G. J. (2014). Spinoffs versus buyouts: Profitability of alternate routes for commercializing innovations. <i>Strategic Management Journal</i> , 35(13), 2043-2052.	Auxiliary Heckman 2-stage	No	Yes	No	

Appendix C. Presence and Neglect of Self-Selection Effects in *Strategic Management Journal* Studies (2014).

Article	Self-selection present	Self-selection addressed
Cheng, B., Ioannou, I., & Serafeim, G. (2014). Corporate social responsibility and access to finance. <i>Strategic Management Journal</i> , 35(1), 1-23.	No	N/A
Kleinbaum, A. M., & Stuart, T. E. (2014). Inside the black box of the corporate staff: Social networks and the implementation of corporate strategy. <i>Strategic Management Journal</i> , 35(1), 24-47.	Yes	Yes
Sears, J., & Hoetker, G. (2014). Technological overlap, technological capabilities, and resource recombination in technological acquisitions. <i>Strategic Management Journal</i> , 35(1), 48-67.	Yes	No
Ahearne, M., Lam, S. K., & Krause, R. (2014). Performance impact of middle managers' adaptive strategy implementation: The role of social capital. <i>Strategic Management Journal</i> , 35(1), 68-87.	Yes	No
Belderbos, R., Tong, T. W., & Wu, S. (2014). Multinationality and downside risk: The roles of option portfolio and organization. <i>Strategic Management Journal</i> , 35(1), 88-106.	No	N/A
Bridoux, F., & Stoelhorst, J. W. (2014). Microfoundations for stakeholder theory: Managing stakeholders with heterogeneous motives. <i>Strategic Management Journal</i> , 35(1), 107-125.	N/A	N/A
Mudambi, R., & Swift, T. (2014). Knowing when to leap: Transitioning between exploitative and explorative R&D. <i>Strategic Management Journal</i> , 35(1), 126-145.	No	N/A

(continued)

Appendix C. (continued)

Article	Self-selection present	Self-selection addressed
Yang, H., Zheng, Y., & Zhao, X. (2014). Exploration or exploitation? Small firms' alliance strategies with large firms. <i>Strategic Management Journal</i> , 35(1), 146-157.	Yes	Yes
Eggers, J. P. (2014). Competing technologies and industry evolution: The benefits of making mistakes in the flat panel display industry. <i>Strategic Management Journal</i> , 35(2), 159-178.	No	N/A
Schilke, O. (2014). On the contingent value of dynamic capabilities for competitive advantage: The nonlinear moderating effect of environmental dynamism. <i>Strategic Management Journal</i> , 35(2), 179-203.	Yes	No
Alcacer, J., & Oxley, J. (2014). Learning by supplying. <i>Strategic Management Journal</i> , 35(2), 204-223.	Yes	Yes
Brahm, F., & Tarzjan, J. (2014). Transactional hazards, institutional change, and capabilities: Integrating the theories of the firm. <i>Strategic Management Journal</i> , 35(2), 224-245.	No	N/A
Klingebiel, R., & Rammer, C. (2014). Resource allocation strategy for innovation portfolio management. <i>Strategic Management Journal</i> , 35(2), 246-268.	Yes	No
Bauer, F., & Matzler, K. (2014). Antecedents of M&A success: The role of strategic complementarity, cultural fit, and degree and speed of integration. <i>Strategic Management Journal</i> , 35(2), 269-291.	Yes	No
Jia, N. (2014). Are collective political actions and private political actions substitutes or complements? Empirical evidence from China's private sector. <i>Strategic Management Journal</i> , 35(2), 292-315.	Yes	Yes
Arora, A., Belenzon, S., & Rios, L. A. (2014). Make, buy, organize: The interplay between research, external knowledge, and firm structure. <i>Strategic Management Journal</i> , 35(3), 317-337.	Yes	No
Bromiley, P., & Harris, J. D. (2014). A comparison of alternative measures of organizational aspirations. <i>Strategic Management Journal</i> , 35(3), 338-357.	N/A	N/A
Baumann, O., & Stieglitz, N. (2014). Rewarding value-creating ideas in organizations: The power of low-powered incentives. <i>Strategic Management Journal</i> , 35(3), 358-375.	N/A	N/A
Arend, R. J., Patel, P. C., & Park, H. D. (2014). Explaining post-IPO venture performance through a knowledge-based view typology. <i>Strategic Management Journal</i> , 35(3), 376-397.	No	N/A
Tuschke, A., Sanders, W. M. G., & Hernandez, E. (2014). Whose experience matters in the boardroom? The effects of experiential and vicarious learning on emerging market entry. <i>Strategic Management Journal</i> , 35(3), 398-418.	Yes	Yes
Mas-Ruiz, F. J., Ruiz-Moreno, F., & Ladron de Guevara Martinez, A. (2014). Asymmetric rivalry within and between strategic groups. <i>Strategic Management Journal</i> , 35(3), 419-439.	N/A	N/A
Xie, X., & O'Neill, H. M. (2014). Learning and product entry: How diversification patterns differ over firm age and knowledge domains in U.S. generic drug industry. <i>Strategic Management Journal</i> , 35(3), 440-449.	No	N/A
Holburn, G. L. F., & Vanden Bergh, R. G. (2014). Integrated market and nonmarket strategies: Political campaign contributions around merger and acquisition events in the energy sector. <i>Strategic Management Journal</i> , 35(3), 450-460.	Yes	No
Menz, M., & Scheef, C. (2014). Chief strategy officers: Contingency analysis of their presence in top management teams. <i>Strategic Management Journal</i> , 35(3), 461-471.	Yes	No

(continued)

Appendix C. (continued)

Article	Self-selection present	Self-selection addressed
Hambrick, D. C., & Quigley, T. J. (2014). Toward more accurate contextualization of the CEO effect on firm performance. <i>Strategic Management Journal</i> , 35(4), 473-491.	N/A	N/A
Luo, X., Kanuri, V. K., & Andrews, M. (2014). How does CEO tenure matter? The mediating role of firm-employee and firm-customer relationships. <i>Strategic Management Journal</i> , 35(4), 492-511.	N/A	N/A
Stern, I., Dukerich, J. M., & Zajac, E. (2014). Unmixed signals: How reputation and status affect alliance formation. <i>Strategic Management Journal</i> , 35(4), 512-531.	No	N/A
Fauchart, E., & Cowan, R. (2014). Weak links and the management of reputational interdependencies. <i>Strategic Management Journal</i> , 35(4), 532-549.	N/A	N/A
O'Brien, J. P., & David, P. (2014). Reciprocity and R&D search: Applying the behavioral theory of the firm to a communitarian context. <i>Strategic Management Journal</i> , 35(4), 550-565.	Yes	No
Vandaie, R., & Zaheer, A. (2014). Surviving bear hugs: Firm capability, large partner alliances, and growth. <i>Strategic Management Journal</i> , 35(4), 566-577.	No	N/A
Lipparini, A., Lorenzoni, G., & Ferriani, S. (2014). From core to periphery and back: A study on the deliberate shaping of knowledge flows in interfirm dyads and networks. <i>Strategic Management Journal</i> , 35(4), 578-595.	N/A	N/A
Kulchina, E. (2014). Media coverage and location choice. <i>Strategic Management Journal</i> , 35(4), 596-605.	No	N/A
Singla, C., Veliyath, R., & George, R. (2014). Family firms and internationalization-governance relationships: Evidence of secondary agency issues. <i>Strategic Management Journal</i> , 35(4), 606-616.	Yes	No
Patel, P. C., & Chrisman, J. J. (2014). Risk abatement as a strategy for R&D investments in family firms. <i>Strategic Management Journal</i> , 35(4), 617-627.	Yes	No
Fremeth, A. R., & Shaver, J. M. (2014). Strategic rationale for responding to extra-jurisdictional regulation: Evidence from firm adoption of renewable power in the US. <i>Strategic Management Journal</i> , 35(5), 629-651.	No	N/A
Baum, J. A., Cowan, R., & Jonard, N. (2014). Does evidence of network effects on firm performance in pooled cross-section support prescriptions for network strategy? <i>Strategic Management Journal</i> , 35(5), 652-667.	N/A	N/A
Humphery-Jennar, M. (2014). Takeover defenses, innovation, and value creation: Evidence from acquisition decisions. <i>Strategic Management Journal</i> , 35(5), 668-690.	Yes	Yes
Patel, P. C., Fernhaber, S. A., McDougall-Covin, P. P., & van der Have, R. P. (2014). Beating competitors to international markets: The value of geographically balanced networks for innovation. <i>Strategic Management Journal</i> , 35(5), 691-711.	Yes	Yes
Lewis, B. W., Walls, J. L., & Dowell, G. W. S. (2014). Differences in degrees: CEO characteristics and firm environmental disclosure. <i>Strategic Management Journal</i> , 35(5), 712-722.	Yes	No
Toh, P. K. (2014). Chicken, or the egg, or both? The interrelationship between a firm's inventor specialization and scope. <i>Strategic Management Journal</i> , 35(5), 723-738.	No	N/A

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Appendix C. (continued)

Article	Self-selection present	Self-selection addressed
Gu, Q., & Lu, X. (2014). Unraveling the mechanisms of reputation and alliance formation: A study of venture capital syndication in China. <i>Strategic Management Journal</i> , 35(5), 739-750.	No	N/A
Leitterstorf, M. P., & Rau, S. B. (2014). Socioemotional wealth and IPO underpricing of family firms. <i>Strategic Management Journal</i> , 35(5), 751-760.	Yes	No
Larraneta, B., Zahra, S. A., & Galan, J. L. (2014). Strategic repertoire variety and new venture growth: The moderating effects of origin and industry dynamism. <i>Strategic Management Journal</i> , 35(5), 761-772.	No	N/A
Hiatt, S. R., & Sine, W. D. (2014). Clear and present danger: Planning and new venture survival amid political and civil violence. <i>Strategic Management Journal</i> , 35(5), 773-785.	No	N/A
Furlan, A., Cabigiosu, A., & Camuffo, A. (2014). When the mirror gets misted up: Modularity and technological change. <i>Strategic Management Journal</i> , 35(6), 789-807.	No	N/A
Krause, R., & Semadeni, M. (2014). Last dance or second chance? Firm performance, CEO career horizon, and the separation of board leadership roles. <i>Strategic Management Journal</i> , 35(6), 808-825.	No	N/A
O'Connell, V., & O'Sullivan, D. (2014). The influence of lead indicator strength on the use of nonfinancial measures in performance management: Evidence from CEO compensation schemes. <i>Strategic Management Journal</i> , 35(6), 826-844.	No	N/A
Sundaramurthy, C., Pukthuanthong, K., & Kor, Y. (2014). Positive and negative synergies between the CEO's and the corporate board's human and social capital: A study of biotechnology firms. <i>Strategic Management Journal</i> , 35(6), 845-868.	No	N/A
Berry, H. (2014). Global integration and innovation: Multicountry knowledge generation within MNCs. <i>Strategic Management Journal</i> , 35(6), 869-890.	No	N/A
Kogut, B., Colomer, J., & Belinky, M. (2014). Structural equality at the top of the corporation: Mandated quotas for women directors. <i>Strategic Management Journal</i> , 35(6), 891-902.	N/A	N/A
Foss, N. J., & Hallberg, N. K. (2014). How symmetrical assumptions advance strategic management research. <i>Strategic Management Journal</i> , 35(6), 903-913.	N/A	N/A
Laamanen, T., Brauer, M., & Junna, O. (2014). Performance of acquirers of divested assets: Evidence from the US software industry. <i>Strategic Management Journal</i> , 35(6), 914-925.	Yes	Yes
Marcel, J. J., & Cowen, A. P. (2014). Cleaning house or jumping ship? Understanding board upheaval following financial fraud. <i>Strategic Management Journal</i> , 35(6), 926-937.	No	N/A
Plourde, Y., Parker, S. C., & Schaan, J.-L. (2014). Expatriation and its effect on headquarters' attention in the multinational enterprise. <i>Strategic Management Journal</i> , 35(6), 938-947.	No	N/A
Lecuona, J. R., & Reitzig, M. (2014). Knowledge worth having in "excess": The value of tacit and firm-specific human resource slack. <i>Strategic Management Journal</i> , 35(7), 954-973.	No	N/A
Schilling, M. A., & Fang, C. (2014). When hubs forget, lie, and play favorites: Interpersonal network structure, information distortion, and organizational learning. <i>Strategic Management Journal</i> , 35(7), 974-994.	N/A	N/A

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Appendix C. (continued)

Article	Self-selection present	Self-selection addressed
Wang, L., Madhok, A., & Li, S. X. (2014). Expatriation and its effect on headquarters' attention in the multinational enterprise. <i>Strategic Management Journal</i> , 35(7), 995-1012.	N/A	N/A
O'Brien, J. P., David, P., Yoshikawa, T., & Delios, A. (2014). How capital structure influences diversification performance: A transaction cost perspective. <i>Strategic Management Journal</i> , 35(7), 1013-1031.	Yes	Yes
Soule, S. A., Swaminathan, A., & Tihanyi, L. (2014). The diffusion of foreign divestment from Burma. <i>Strategic Management Journal</i> , 35(7), 1032-1052.	Yes	No
Taracki, M., Ates, N. Y., Porck, J. P., van Knippenberg, D., Groenen, P. J. F., & De Haas, M. (2014). Strategic consensus mapping: A new method for testing and visualizing strategic consensus within and between teams. <i>Strategic Management Journal</i> , 35(7), 1053-1069.	N/A	N/A
Semadeni, M., Withers, M. C., & Certo, T. (2014). The perils of endogeneity and instrumental variables in strategy research: Understanding through simulations. <i>Strategic Management Journal</i> , 35(7), 1070-1079.	N/A	N/A
Cook, A., & Glass, C. (2014). Above the glass ceiling: When are women and racial/ethnic minorities promoted to CEO? <i>Strategic Management Journal</i> , 35(7), 1080-1089.	No	N/A
Shinkle, G. A., & McCann, B. T. (2014). New product deployment: the moderating influence of economic institutional context. <i>Strategic Management Journal</i> , 35(7), 1090-1101.	Yes	No
Chang, S.-J., & Wu, B. (2014). Institutional barriers and industry dynamics. <i>Strategic Management Journal</i> , 35(8), 1103-1123.	No	N/A
Pathak, S., Hoskisson, R. E., & Johnson, R. A. (2014). Settling up in CEO compensation: The impact of divestiture intensity and contextual factors in refocusing firms. <i>Strategic Management Journal</i> , 35(8), 1124-1143.	Yes	Yes
Giarratana, M. S., & Mariani, M. (2014). The relationship between knowledge sourcing and fear of imitation. <i>Strategic Management Journal</i> , 35(8), 1144-1163.	No	N/A
Heide, J. B., Kumar, A., & Wathne, K. H. (2014). Concurrent sourcing, governance mechanisms, and performance outcomes in industrial value chains. <i>Strategic Management Journal</i> , 35(8), 1164-1185.	Yes	Yes
Fang, C., Kim, J.-H., & Milliken, F. J. (2014). When bad news is sugarcoated: Information distortion, organizational search and the behavioral theory of the firm. <i>Strategic Management Journal</i> , 35(8), 1186-1201.	N/A	N/A
Mirabeau, L., & Maguire, S. (2014). From autonomous strategic behavior to emergent strategy. <i>Strategic Management Journal</i> , 35(8), 1202-1229.	N/A	N/A
Conti, R. (2014). Do non-competition agreements lead firms to pursue risky R&D projects. <i>Strategic Management Journal</i> , 35(8), 1230-1248.	No	N/A
Bromiley, P., & Rau, D. (2014). Towards a practice-based view of strategy. <i>Strategic Management Journal</i> , 35(8), 1249-1256.	N/A	N/A
Wu, B., Wan, Z., & Levinthal, D. A. (2014). Complementary assets as pipes and prisms: Innovation incentives and trajectory choices. <i>Strategic Management Journal</i> , 35(9), 1257-1278.	N/A	N/A
Frank, D. H., & Obloj, T. (2014). Firm-specific human capital, organizational incentives, and agency costs: Evidence from retail banking. <i>Strategic Management Journal</i> , 35(9), 1279-1301.	No	N/A
Vaara, E., Junni, P., Saralla, R. M., Ehrnrooth, M., & Kovesnikov, A. (2014). Attributional tendencies in cultural explanations of M&A performance. <i>Strategic Management Journal</i> , 35(9), 1302-1317.	N/A	N/A

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Appendix C. (continued)

Article	Self-selection present	Self-selection addressed
Herrmann, P., & Nadkarni, S. (2014). Managing strategic change: The duality of CEO personality. <i>Strategic Management Journal</i> , 35(9), 1318-1342.	Yes	Yes
Xia, J., Ma, X., Lu, J. W., & Yiu, D. W. (2014). Outward foreign direct investment by emerging market firms: A resource dependence logic. <i>Strategic Management Journal</i> , 35(9), 1343-1363.	No	N/A
Hoehn-Weiss, M. A., & Karim, S. (2014). Unpacking functional alliance portfolios: How signals of viability affect young firms' outcomes. <i>Strategic Management Journal</i> , 35(9), 1364-1385.	Yes	No
Castaner, X., Mulotte, L., Garrette, B., & Dussauge, P. (2014). Governance mode vs. governance fit: Performance implications of make-or-ally choices for product innovation in the worldwide aircraft industry, 1942-2000. <i>Strategic Management Journal</i> , 35(9), 1386-1397.	Yes	Yes
Giachetti, C., & Dagnino, G. B. (2014). Detecting the relationship between competitive intensity and firm product line length: Evidence from the worldwide mobile phone industry. <i>Strategic Management Journal</i> , 35(9), 1398-1409.	No	N/A
Shapira, Z., & Shaver, J. M. (2014). Confounding changes in averages with marginal effects: How anchoring can destroy economic value in strategic investment assessments. <i>Strategic Management Journal</i> , 35(10), 1414-1426.	N/A	N/A
Chatterji, A. K., & Fabrizio, K. R. (2014). Using users: When does external knowledge enhance corporate product innovation? <i>Strategic Management Journal</i> , 35(10), 1427-1445.	No	N/A
Feldman, E. R., Gilson, S. C., & Villalonga, B. (2014). Do analysts add value when they most can? Evidence from corporate spin-offs. <i>Strategic Management Journal</i> , 35(10), 1446-1463.	No	N/A
Koh, P.-S., Qian, C., & Wang, H. (2014). Firm litigation risk and the insurance value of corporate social performance. <i>Strategic Management Journal</i> , 35(10), 1464-1482.	Yes	No
Nadolska, A., & Barkema, H. G. (2014). Good learners: How top management teams affect the success and frequency of acquisitions. <i>Strategic Management Journal</i> , 35(10), 1483-1507.	No	N/A
Poppo, L., & Zhou, K. Z. (2014). Managing contracts for fairness in buyer-supplier exchanges. <i>Strategic Management Journal</i> , 35(10), 1508-1527.	No	N/A
Patel, P. C., & Cooper, D. (2014). The harder they fall, the faster they rise: Approach and avoidance focus in narcissistic CEOs. <i>Strategic Management Journal</i> , 35(10), 1528-1540.	No	N/A
Elisaesser, F., Tsang, E. W. K., & Runde, J. (2014). Models of causal inference: Imperfect but applicable is better than perfect but inapplicable. <i>Strategic Management Journal</i> , 35(10), 1541-1551.	N/A	N/A
Schnatterly, K., & Johnson, S. G. (2014). Independent boards and the institutional investors that prefer them: Drivers of institutional investor heterogeneity in governance preferences. <i>Strategic Management Journal</i> , 35(10), 1541-1551.	Yes	No
Knudsen, T., Levinthal, D. A., & Winter, S. G. (2014). Hidden but in plain sight: The role of scale adjustment in industry dynamics. <i>Strategic Management Journal</i> , 35(11), 1569-1584.	N/A	N/A
Wang, R. D., & Shaver, J. M. (2014). Competition-driven repositioning. <i>Strategic Management Journal</i> , 35(11), 1585-1604.	N/A	N/A

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Appendix C. (continued)

Article	Self-selection present	Self-selection addressed
Etzion, D., & Pe'er, A. (2014). Mixed signals: A dynamic analysis of warranty provision in the automotive industry, 1960-2008. <i>Strategic Management Journal</i> , 35(11), 1605-1625.	N/A	N/A
Reuer, J. J., Klijn, E., & Lioukas, C. S. (2014). Board involvement in international joint ventures. <i>Strategic Management Journal</i> , 35(11), 1626-1644.	No	N/A
Di Stefano, G., King, A. A., & Verona, G. (2014). Kitchen confidential? Norms for the use of transferred knowledge in gourmet cuisine. <i>Strategic Management Journal</i> , 35(11), 1645-1670.	No	N/A
Wagner, S., Hoisl, K., & Thoma, G. (2014). Overcoming localization of knowledge—The role of professional service firms. <i>Strategic Management Journal</i> , 35(11), 1647-1688.	No	N/A
Love, J. H., Roper, S., & Vahter, P. (2014). Learning from openness: The dynamics of breadth in external innovation linkages. <i>Strategic Management Journal</i> , 35(11), 1703-1716.	Yes	No
Liu, K., Arthurs, J. D., Nam, D., & Mousa, F.-T. (2014). Information diffusion and value redistribution among transaction partners of the IPO firm. <i>Strategic Management Journal</i> , 35(11), 1717-1726.	Yes	No
Henisz, W. J., Dorobantu, S., & Nartey, L. J. (2014). Spinning gold: The Financial returns to stakeholder engagement. <i>Strategic Management Journal</i> , 35(12), 1727-1748.	No	N/A
Alcacer, J., & Chung, W. (2014). Location strategies for agglomeration economies. <i>Strategic Management Journal</i> , 35(12), 1749-1761.	No	N/A
Grimpe, C., & Hussinger, K. (2014). Resource complementarity and value capture in firm acquisitions: The role of intellectual property rights. <i>Strategic Management Journal</i> , 35(12), 1762-1780.	No	N/A
Sakhartov, A. V., & Folta, T. B. (2014). Resource relatedness, redeployability, and firm value. <i>Strategic Management Journal</i> , 35(12), 1781-1797.	N/A	N/A
Eesley, C. E., Hsu, D. H., & Roberts, E. B. (2014). The contingent effects of top management teams on venture performance: Aligning founding team composition with innovation strategy and commercialization environment. <i>Strategic Management Journal</i> , 35(12), 1798-1817.	Yes	Yes
Ault, J. K., & Spicer, A. (2014). The institutional context of poverty: State fragility as a predictor of cross-national variation in commercial microfinance lending. <i>Strategic Management Journal</i> , 35(12), 1818-1838.	No	N/A
Fitza, M. A. (2014). The use of variance decomposition in the investigation of CEO effects: How large must the CEO effect be to rule out chance? <i>Strategic Management Journal</i> , 35(12), 1839-1852.	Yes	No
Datta, S., & Iskandar-Datta, M. (2014). Upper-Echelon executive human capital and compensation: Generalist vs. specialist skills. <i>Strategic Management Journal</i> , 35(12), 1853-1866.	Yes	No
Zhao, Z. J., & Chadwick, C. (2014). What we will do versus what we can do: The relative effects of unit-level NPD motivation and capability. <i>Strategic Management Journal</i> , 35(12), 1867-1880.	No	N/A
Jain, A., & Thiétart, R. A. (2014). Capabilities as shift parameters for the outsourcing decision. <i>Strategic Management Journal</i> , 35(12), 1881-1890.	Yes	Yes
Vanneste, B. S., Puranam, P., & Kretschmer, T. (2014). Trust over time in exchange relationships: Meta-analysis and theory. <i>Strategic Management Journal</i> , 35(12), 1891-1902.	N/A	N/A

(continued)

Appendix C. (continued)

Article	Self-selection present	Self-selection addressed
Stettner, U., & Lavie, D. (2014). Ambidexterity under scrutiny: Exploration and exploitation via internal organization, alliances, and acquisitions. <i>Strategic Management Journal</i> , 35(13), 1903-1929.	No	N/A
York, J. G., & Lenox, M. J. (2014). Exploring the sociocultural determinants of de novo versus de alio entry in emerging industries. <i>Strategic Management Journal</i> , 35(13), 1930-1951.	No	N/A
Chatain, O. (2014). How do strategic factor markets respond to rivalry in the product market? <i>Strategic Management Journal</i> , 35(13), 1952-1971.	N/A	N/A
Garcia-Sanchez, J., Mesquita, L. F., & Vassolo, R. S. (2014). What doesn't kill you makes you stronger: The evolution of competition and entry-order advantages in economically turbulent contexts. <i>Strategic Management Journal</i> , 35(13), 1972-1992.	N/A	N/A
Yang, Y., Narayanan, V. K., & De Carolis, D. M. (2014). The relationship between portfolio diversification and firm value: The evidence from corporate venture capital activity. <i>Strategic Management Journal</i> , 35(13), 1993-2011.	No	N/A
Shen, R., Tang, Y., & Chen, G. (2014). When the role fits: How firm status differentials affect corporate takeovers. <i>Strategic Management Journal</i> , 35(13), 2012-2030.	Yes	Yes
Walter, S. G., Heinrichs, S., & Walter, A. (2014). Parent hostility and spin-out performance. <i>Strategic Management Journal</i> , 35(13), 2012-2030.	Yes	Yes
Rubera, G., & Tellis, G. J. (2014). Spinoffs versus buyouts: Profitability of alternate routes for commercializing innovations. <i>Strategic Management Journal</i> , 35(13), 2043-2052.	Yes	Yes
Kim, C., & Bettis, R. A. (2014). Cash is surprisingly valuable as a strategic asset. <i>Strategic Management Journal</i> , 35(13), 2053-2063.	No	N/A
Alessandri, T. M., & Seth, A. (2014). The effects of managerial ownership on international and business diversification: Balancing incentives and risks. <i>Strategic Management Journal</i> , 35(13), 2064-2075.	No	N/A

Appendix D. Presence and Neglect of Self-Selection Effects in Administrative Science Quarterly Studies (2014).

Article	Self-selection present	Self-selection addressed
Kovacs, B., & Sharkey, A. J. (2014). The paradox of publicity: How awards can negatively affect the evaluation of quality? <i>Administrative Science Quarterly</i> , 59(1), 1-33.	No	N/A
Ou, A. Y., Tsui, A. S., Kinicki, A. J., Waldman, D. A., Zhixing, X., & Jiwan Song, L. (2014). Humble chief executive officers' connections to top management team integration and middle managers' responses. <i>Administrative Science Quarterly</i> , 59(1), 34-72.	Yes	No
Hernandez, E. (2014). Finding a home away from home: Effects of immigrants on firms' foreign location choice and performance. <i>Administrative Science Quarterly</i> , 59(1), 73-108.	Yes	Yes

(continued)

Appendix D. (continued)

Article	Self-selection present	Self-selection addressed
Ody-Brasier, A., & Vermeulen, F. (2014). The price you pay: Price-setting as a response to norm violations in the market for champagne grapes. <i>Administrative Science Quarterly</i> , 59(1), 109-144.	Yes	Yes
Perkins, S. (2014). When does prior experience pay? Institutional experience and the multinational corporation. <i>Administrative Science Quarterly</i> , 59(1), 145-181.	Yes	Yes
Joshi, A. (2014). By whom and when is women's expertise recognized? The interactive effects of gender and education in science and engineering teams. <i>Administrative Science Quarterly</i> , 59(2), 202-239.	Yes	No
Zhu, D. H., Shen, W., & Hillman, A. J. (2014). Recategorization into the in-group: The appointment of demographically different new directors and their subsequent positions on corporate boards. <i>Administrative Science Quarterly</i> , 59(2), 240-270.	No	N/A
Malter, D. (2014). On the causality and cause of returns to organizational status: Evidence from the grands crus classes of the medoc. <i>Administrative Science Quarterly</i> , 59(2), 271-300.	Yes	No
Rogan, M., & Sorenson, O. (2014). Picking a (poor) partner: A relational perspective on acquisitions. <i>Administrative Science Quarterly</i> , 59(2), 301-329.	Yes	No
Desai, S. D., Chugh, D., & Brief, A. P. (2014). The implications of marriage structure for men's workplace attitudes, beliefs, and behaviors toward women. <i>Administrative Science Quarterly</i> , 59(2), 330-365.	Yes	Yes
DiBenigno, J., & Kellogg, K. C. (2014). Beyond occupational differences: The importance of cross-cutting demographics and dyadic toolkits for collaboration in a U.S. hospital. <i>Administrative Science Quarterly</i> , 59(3), 375-408.	N/A	N/A
Knudsen, T., & Srikanth, K. C. (2014). Coordinated exploration: Organizing joint search by multiple specialists to overcome mutual confusion and joint myopia. <i>Administrative Science Quarterly</i> , 59(3), 409-441.	N/A	N/A
Almandoz, J. (2014). Founding teams as carriers of competing logics: When institutional forces predict banks' risk exposure. <i>Administrative Science Quarterly</i> , 59(3), 442-473.	No	N/A
Ashforth, B. E., & Reingen, P. H. (2014). Functions of dysfunction: Managing the dynamics of an organizational duality in a natural food cooperative. <i>Administrative Science Quarterly</i> , 59(3), 474-516.	N/A	N/A
Xu, D., Lu, J. W., & Gu, Q. (2014). Organizational forms and multi-population dynamics: Economic transition in China. <i>Administrative Science Quarterly</i> , 59(3), 517-547.	No	N/A
Barsade, S. G., & O'Neill, O. A. (2014). What's love got to do with it? A longitudinal study of the culture of companionate love and employee and client outcomes in a long-term care setting. <i>Administrative Science Quarterly</i> , 59(4), 551-598.	Yes	No
Zhu, H., & Chung, C.-N. (2014). Portfolios of political ties and business group strategy in emerging economies: Evidence from Taiwan. <i>Administrative Science Quarterly</i> , 59(4), 599-638.	Yes	Yes
Tilcsik, A. (2014). Imprint-environment fit and performance: How organizational munificence at the time of hire affects subsequent job performance. <i>Administrative Science Quarterly</i> , 59(4), 639-668.	No	N/A
Funk, R. J., & Hirschman, D. (2014). Derivatives and deregulation: Financial innovation and the demise of Glass-Steagall. <i>Administrative Science Quarterly</i> , 59(4), 669-704.	N/A	N/A
Tiziana, C., Gino, F., & Kouchaki, M. (2014). The contaminating effects of building instrumental ties: How networking can make us feel dirty. <i>Administrative Science Quarterly</i> , 59(4), 705-735.	Yes	No

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Notes

1. If one assumes that the error term follows a logistic distribution, then you generate a logit model.
2. A consistent variance-covariance matrix that allows correctly measuring standard errors and other relevant statistics (t statistics and the creation of confidence intervals) and allows making correct inferences can be estimated by using an asymptotic approximation (as in the ML case) or by bootstrapping.
3. Clearly, there might contemporaneously be problems of sample selection and self-selection based endogeneity; however, we will not discuss such issues here.
4. To yield identification, it is again assumed that $\sigma_{\omega}^2 = 1$.
5. The Stata page <http://www.stata.com/manuals13/teetregress.pdf> contains a comprehensive description of the command “`etregress`.” Details concerning the R package `sampleSelection` can be found under <https://cran.r-project.org/web/packages/sampleSelection/sampleSelection.pdf>.
6. For the sake of identification, it is again normally assumed that $\sigma_{\omega}^2 = 1$.
7. Alternatively, the Stata researcher could use the “`etregress`” command where, instead of only using the treatment dummy as a regressor, she also adds as additional regressors the interactions among each of the exogenous variables included in \mathbf{X}_i and the treatment. This model is similar, though not identical, to (6). For a discussion, see the section “Average Treatment Effect on the Treated (ATET)” in the following document: <http://www.stata.com/manuals13/teetregress.pdf>.
8. As discussed above in the subsection on sample selection, the Stata command “`heckman`” can be used to perform this estimation.
9. Notice that, as above, σ_{ω}^2 is assumed to be equal to 1 for the sake of identification. Moreover, ρ^{01} , ρ^{02} , and ρ^{12} are not defined, as Y_i^0 , Y_i^1 , and Y_i^2 are never simultaneously observed.
10. This parameterization of the model makes the source of endogeneity explicit. Thus, endogeneity is determined by an unobservable common factor (x_{3i}), which affects both the substantive and the selection equations. We also assume an additional source of endogeneity by imposing a nonzero correlation between the error terms ω_i and ε_i in their bivariate distribution.
11. We estimate our simulation models with the statistical program Stata. The programs describing the entire Monte Carlo simulations—as well as the estimation routines—can be obtained by the authors upon request.
12. We use the “`ivreg2`” command in Stata, which is extremely transparent—and thus highly recommended—as it reports many diagnostics regarding the quality of instruments.
13. The endogenous-switching procedure with multivariate outcomes is not implemented in standard statistical packages. By making some specific assumptions on the process governing the multivariate selection (e.g., ordered or not), it would however be possible to write down the likelihood function and program the ML estimator in Mata (Stata’s full-blown programming language) or other programming languages such as R or Matlab.
14. Recall that if the only source of selection was the correlation between the error terms (as assumed in our theoretical discussion), then the coefficient estimate for the selection term in the LIML case when $z_i = 0$ should be equal to 0.3 since it represents the product between $\sigma = 1$ (the variance of ε_i) and $\rho = 0.3$. Furthermore, this coefficient should have the opposite sign in the case where $z_i = 1$. In our empirical

context, however, we assumed an additional source of endogeneity: the omitted variable x_{3i} . Hence, the “true” value of λ is more difficult to recover as $E(\varepsilon_i | \omega_i > -\mathbf{Z}_i'\boldsymbol{\beta})$ is not simply $\rho\sigma_\varepsilon \left(\frac{\phi(\mathbf{Z}_i'\boldsymbol{\beta})}{1-\Phi(\mathbf{Z}_i'\boldsymbol{\beta})} \right)$.

15. In the FIML estimation, we report the logarithms of the correlation coefficient and the variance, respectively. In our parameterization, we assume a correlation among the error terms in the selection and main equations of 0.3 and a variance of 1, which implies that the logarithms are equal to -0.53 and 0 , respectively.

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