On the Nuisance of Control Variables in Regression Analysis

Paul Hünermund  
Copenhagen Business School, Kilevej 14A, Frederiksberg, 2000, DK.  
phu.si@cbs.dk

Beyers Louw  
Maastricht University, Tongersestraat 53, 6211 LM Maastricht, NL.  
jb.louw@maastrichtuniversity.nl

September 28, 2022

Abstract

Control variables are included in regression analyses to estimate the causal effect of a treatment on an outcome. In this article, we argue that the estimated effect sizes of control variables are unlikely to have a causal interpretation themselves though. This is because even valid controls are possibly endogenous and therefore represent a combination of several different causal mechanisms operating jointly on the outcome, which is hard to interpret theoretically. We recommend to refrain from reporting marginal effects of controls in regression tables and to focus exclusively on the variables of interest in the results sections of quantitative research papers. Moreover, we advise against using control variable estimates for subsequent theory building and meta-analyses.
Introduction

Multivariate regression analysis is an important tool for empirical research in management, organization studies, and economics. These methods account for confounding influence factors between a treatment and an outcome by including a set of control variables in order to obtain consistent effect estimates. Notwithstanding their importance for causal inference, in practice scholars often overstate the role of control variables in regressions. In this paper we argue that, while essential for the identification of causal effects, control variables generally have no structural interpretation themselves. This is because even valid controls are often correlated with other unobserved factors, which render their marginal effects uninterpretable from a causal inference perspective (Westreich and Greenland, 2013; Keele et al., 2020). Consequently, researchers need to be careful with attaching too much meaning to control variables and should consider to ignore them when interpreting the results of their analyses.

Drawing substantive conclusions from control variable estimates is common, however. Authors frequently make use of formulations such as: “control variables have expected signs” or “it is worth noting the coefficients of our control variables”. Below, we present the results of a literature review of papers published in the last five years in Organization Science and Strategic Management Journal, in which we found that 47 percent of papers that made use of regression methods also explicitly discussed the estimated effect sizes of controls. This is in line with Carlson and Wu (2012), who identified 48 percent of papers published in the Academy of Management Journal, Journal of Applied Psychology and Strategic Management Journal in 2007 that interpreted and discussed the effects of controls. Moreover, in our own experience as authors of quantitative research papers, we frequently encountered instances in which reviewers asked us to provide an interpretation of control variable coefficients. The justification that was often given was that, although they were not the main focus of the analysis, controls could still provide valuable information for other researchers in the field who are investigating related research questions.

The methodological literature in organizational research usually highlights that control variables should carry the same importance in an empirical analysis as the main independent variables of interest (Becker, 2005; Spector and Brannick, 2011; Carlson and Wu, 2012; Atinc et al., 2012). To increase rigor and improve transparency of published research articles, Becker (2005) recommends to report all regression coefficients of control variables as well as their significance levels. Similarly, Spector and Brannick (2011) advocate that controls should be given equal status to the main treatment variable in the analysis. Atinc et al. (2012) consider it to be best practice to provide an ex-ante prediction of the sign of the relationship between the controls and outcome variable based on theory, which should subsequently be checked against the empirical evidence. In a
recent paper, Becker et al. (2016) provides a more cautionary recommendation regarding generalizing from control variable estimates if it involves out-of-sample extrapolation, but otherwise considers it to be appropriate.

Overall, the general consensus, both in the methodological and applied literature, thus seems to be that reporting and interpreting control variable estimates is safe. In the following, we will explain why this is not necessarily the case. We will explicate the view that control variables, while certainly an important ingredient in many causal research designs, do not have the same status as the main variables of interest in an empirical analysis. In particular, we will argue that in many situations valid controls, which allow for causal effect identification, can nonetheless be endogenous. Therefore, interpreting their estimated effect sizes in light of prior theory could lead to potentially misleading conclusions. A valid causal interpretation of control variables rests on strong assumptions and usually requires to account for all influence factors of the outcome variable under study. Since this is unlikely to be fulfilled in many research contexts, we recommend to omit estimated coefficients of control variables from regression tables, or relegate them to an appendix. Finally, we discuss what our recommendations imply for the practice of meta-analysis, which has recently gained traction in many fields including organizational research (Aguinis et al., 2011).

Do researchers attach substantive meaning to control variables?

To assess the degree to which researchers interpret control variable estimates in their studies, we conducted a review of all articles published in Organization Science and Strategic Management Journal between January 2015 and December 2020. We chose these two journals because of their high prestige in the management and organization field as well as their reputation for high-quality empirical research. Our sample includes all quantitative articles that employed parametric regression models such as OLS, logit, probit, Poisson, etc. This choice was made because effect sizes of control variables can usually not be summarized by a single coefficient (or marginal effect) in non- and semiparametric models. The use of these methods is anyway not very common in our sample, however.

We manually coded papers according to whether they interpret or draw substantive insights from the coefficients or marginal effect estimates of control variables. Examples of such an interpretation range from “the control variable CEO tenure is positively related to performance” to “the effect sizes of control variables are in line with previous studies”. The latter interpretation is thereby of relevance because authors of future research papers might be tempted to develop theory based on this seemingly accumulating empirical evidence. The result of our review shows that interpreting control variables was common practice in the analyzed journals during our period of observation. For the Strategic Management Journal, we identified a total of 497 quantitative research articles,
of which 233 (47 percent) proceeded to interpret the effects of controls variables. For Organization Science, out of a total of 274 quantitative articles, 130 (47 percent) provided an interpretation of control variable estimates. Detailed results of the literature review are reported in Appendix A.

The structural interpretation of control variables

The relationship between the main explanatory variables and controls in a regression can be complex, therefore it is useful to explicitly depict them in a causal diagram (Pearl, 2000). Durand and Vaara (2009) were the first to introduce causal graphs to the management literature and demonstrate their usefulness as a tool for empirical research. Figure 1a presents a simple model with a treatment variable \( X \) and an outcome variable \( Y \). Both variables are connected by an arrow, denoting the direction of causal influence between them. In addition, there are two confounding variables, \( Z_1 \) and \( Z_2 \), that are affecting the treatment and the outcome. \( Z_1 \) and \( Z_2 \) are correlated, as a result of a common influence factor they share, which is denoted by the dashed bidirected arc in the graph. The fact that \( Z_1 \) and \( Z_2 \) are correlated creates what is known as a backdoor path between the treatment and the outcome (Pearl, 2000). \( X \) and \( Y \) are not only connected by the direct causal path \( X \rightarrow Y \), but also by a second path, \( X \leftarrow Z_1 \leftrightarrow Z_2 \rightarrow Y \), which creates a spurious, non-causal correlation between them.

Backdoor paths are defined as any sequence of arrows connecting the treatment and outcome variable (irrespective of their orientation) that remains if arrows emitted by the
treatment are deleted from the graph (Pearl, 2000). Because of the latter requirement they are easy to spot in the causal diagram. Since all the arrows emitted by X are deleted, backdoor paths have to point into X instead; i.e., they enter “through the backdoor”, which is where the name comes from.

Control variables in a multivariate regression model are invoked to block such backdoor paths and obtain a consistent estimate of the causal effect of X on Y, in which case one speaks of an effect to be causally identified. For this purpose, it is sufficient to control for any variable that lies on the open path. Thus, in the example of Figure 1a, the researcher has the choice between either controlling for Z₁ or Z₂, since both would allow to identify the causal effect of interest. The choice between different admissible sets of control variables is thereby of high practical relevance. Researchers often have fairly detailed knowledge about the treatment assignment mechanism Z₁ → X; e.g., because there are organizational or administrative rules that determine individual treatment status, which can be exploited for identification purposes (Angrist, 1990; Flammer and Bansal, 2017). At the same time, the set of variables Z₂ that are direct influence factors of Y will likely be quite large. Thus, in practical applications it might be much easier to control the treatment assignment mechanism instead of trying to include all variables that have an effect on the outcome in a regression.

Nevertheless, although controlling for Z₁ is sufficient to obtain a consistent estimate for X, its marginal effect will itself not correspond to any causal effect of Z₁ on Y. That is because Z₁ is correlated with Z₂ and will thus partially pick up an effect of Z₂ on Y too (Cinelli and Hazlett, 2020). To illustrate this phenomenon quantitatively, we parameterize the causal graph in Figure 1a in the following way:

\[
\begin{align*}
    z_1 & \leftarrow u + \varepsilon_1, \\
    z_2 & \leftarrow u + \varepsilon_2, \\
    x & \leftarrow z_1 + \varepsilon_3, \\
    y & \leftarrow x + z_2 + \varepsilon_4, \\
\end{align*}
\]

(1)

with \(N = 10,000\), and \(U, \varepsilon_i\) being standard normal. True effect sizes are set to one. Note that \(U\) is assumed to be unobserved and appears in the functions assigning values to \(Z_1\) and \(Z_2\). This creates an error correlation between the two variables. We then run a regression of \(Y\) on \(X\) and \(Z_1\), which gives a consistent coefficient estimate for \(X\) (\(\beta_X = 1.017\), std. err. = 0.015; bootstrapped with 1,000 replications), while the effect of \(Z_1\) (\(\beta_{Z_1} = 0.499\), std. err. = 0.018) turns out to be biased. By contrast, if we also include \(Z_2\) in the regression, the coefficient of \(Z_1\) drops to zero (\(\beta_{Z_1} = -0.019\), std. err. = 0.013), which

\[\text{1}\text{Technical note: Requiring the path to be previously unblocked rules out that the variable which is adjusted for is a collider (Huërmond and Bareinboim, 2021). A discussion of collider variables in causal graphs goes beyond the scope of this note.}\]
corresponds to its actual causal effect on $Y$ in this example (since $Z_1$ does not appear in line 4 of eq. 2).

Figures 1b and 1c highlight under which conditions effect estimates of control variables can be interpreted causally. In Figure 1b there are two backdoor paths: $X \leftarrow Z_1 \rightarrow Y$ and $X \leftarrow Z_1 \leftarrow Z$. Both paths can be intercepted by $Z$, which is thus a valid control variable. Data simulated according to the following system:

$$
\begin{align*}
    z_1 &\leftarrow u + \varepsilon_1, \\
    x &\leftarrow z_1 + \varepsilon_2, \\
    y &\leftarrow x + z_1 + u + \varepsilon_3,
\end{align*}
$$

with parameters chosen as before, confirm that the causal effect of $X$ can be consistently estimated in a regression of $Y$ on $X$ and $Z_1$ ($\beta_X = 0.993$, std. err. = 0.012). However, once again the coefficient estimate for $Z_1$ is biased ($\beta_{Z_1} = 1.503$, std. err. = 0.014). Although $Z_1$ is a valid control variable in eq. 2, it is nonetheless endogenous (Frölich, 2008). Note that the unobserved variable $U$ enters the combined error term $\nu = u + \varepsilon_3$ in line 3 of eq. 2. At the same time, $U$ is an argument of the function assigning values to $Z_1$ (line 1 of eq. 2), which lets $Z_1$ become correlated with the error term.

This is different in Figure 1c. Here the two backdoor paths are $X \leftarrow Z_1 \rightarrow Y$ and $X \leftarrow Z_1 \rightarrow Z_1 \rightarrow Y$. When we simulate data according to:

$$
\begin{align*}
    z_1 &\leftarrow u + \varepsilon_1, \\
    x &\leftarrow z_1 + u + \varepsilon_2, \\
    y &\leftarrow x + z_1 + \varepsilon_3,
\end{align*}
$$

we now find that a regression of $Y$ on $X$ and $Z_1$ provides a consistent estimate for the effect of $X$ ($\beta_X = 1.001$, std. err. = 0.008) as well as for $Z_1$ ($\beta_{Z_1} = 1.004$, std. err. = 0.014). In this situation, the regression coefficient for the control variable $Z_1$ has a causal interpretation. This is because, unlike in the previous situations, we are able to account for all influence factors of $Y$, apart from the exogenous error term $\varepsilon_3$. In particular, there is no unobserved variable $U$ jointly affecting the outcome $Y$ and (at least one of) the regressors ($X, Z_1$) anymore.

Finally, Figure 1d depicts a more complex setting, with several admissible sets of controls, each sufficient to identify the causal effect of $X$ on $Y$ (Textor and Liśkiewicz, 2011). One possibility in this situation is to simply control for $Z_1$, which is the only direct influence factor of $X$, and thus blocks all paths entering $X$ through the backdoor. To

---

2Simulation results discussed in the text are also reported next to each other in Table 3 of the Appendix.
witness, we simulate data from the system:

\[
\begin{align*}
    z_1 & \leftarrow u_1 + \varepsilon_1, \\
    z_2 & \leftarrow z_1 + u_1 + u_2 + \varepsilon_2, \\
    z_3 & \leftarrow z_2 + \varepsilon_3, \\
    z_4 & \leftarrow z_2 + \varepsilon_4, \\
    z_5 & \leftarrow z_2 + u_2 + \varepsilon_5, \\
    x & \leftarrow z_1 + \varepsilon_6, \\
    y & \leftarrow x + z_3 + z_4 + z_5 + \varepsilon_7,
\end{align*}
\] (4)

and regress \( Y \) on \( X \) and \( Z_1 \), which gives a consistent estimate for the effect of \( X \) (\( \beta_X = 0.991, \) std. err. = 0.057). Similarly, controlling for the direct influence factors of \( Y \) (\( Z_3, Z_4, \) and \( Z_5 \)) also blocks all backdoor paths and leads to a consistent effect estimate (\( \beta_X = 1.006, \) std. err. = 0.007). A third alternative is to control for the entire set of confounders (\( Z_1, Z_2, Z_3, Z_4, \) and \( Z_5 \); = 1.003, std. err. = 0.010), although this would be the most data-intensive identification strategy leading to slightly less precise estimates compared to the previous specification, due to lower degrees of freedom. This example illustrates that the minimally sufficient set of controls (here: \( Z_1 \)) for identifying the causal effect of \( X \) is often much smaller than the total number of confounding variables in a model. At the same time, the estimated marginal effects for the control variables only have a causal interpretation if all the direct influence factors of \( Y \) (here: \( Z_3, Z_4, \) and \( Z_5 \)) are accounted for in the regression (see columns 8 and 9 in Table 3 in the Appendix). As we argued above, this is unlikely to be the case, since in many real-world settings the number of causal factors determining \( Y \) might be prohibitively large.\(^3\)

**Example**

To illustrate the previous points with a practical example, we turn to a recently published paper by Azoulay et al. (2021). They investigate the effect of early-career exposure to frontier research on the career trajectory of potential innovators. Their specific empirical setting is the Associate Training Program (ATP) of the National Institutes of Health (NIH) in the United States. The ATP was started in 1953 as a training program for recent MD graduates. Participants were sent to the NIH intramural campus in Bethesda, Maryland, to receive a two to three years research training under the supervision of NIH investigators. Since the NIH was originally established within the Marine Hospital Service, participation in the program fulfilled a draftee’s military service obligation. Therefore, applying to the ATP became particularly popular among young physicians during the Vietnam War period (1965–1975).

After a first screening round, applicants were invited to an interview on NIH campus to determine who would eventually be selected to participate in the program. Selection

\(^3\)Alternatively, if omitted factors are unrelated to all other regressors, a causal interpretation of control variable coefficients would be possible too. This seems likewise implausible in many applications though.
criteria were related to applicants’ prior research activities (which Azoulay et al. measure by their number of pre-ATP publications), their academic achievements (proxied by whether they were elected to the ΑΩΑ Honor Medical Society), experience (i.e., whether they held a Ph.D. at the time of application and their number of internships), and the reputation of the institutions where applicants received their training (measured by NIH grants for applicants’ medical school and internship hospital). Importantly, Azoulay et al. argue that although the pool of applicants to the ATP was indeed a highly selected group, selection at the (second) interview stage was based entirely on these observable characteristics. Applicants were early in their career and rather homogeneous in their characteristics. It was therefore hard to select them based on their future research potential beyond a few observable markers. This feature of the particular institutional setting allows Azoulay et al. to employ a selection-on-observables design. Based on that they estimate that ATP participants were twice as likely to pursue a research-focused career later on compared to unsuccessful applicants. As a result, trainees accumulated more publications, citations and grant funding over their life-cycle. Furthermore, they were significantly more likely to receive prestigious career awards, including the Nobel Prize, and become elected members of the National Academy of Sciences.

Figure 2 synthesizes the assumptions leading to the empirical strategy in Azoulay et al. in form of a causal diagram. Controlling for applicants’ prior research activities, academic achievements, experience and school reputation (the authors incorporate several covariates for each of these dimensions, including medical school and internship hospital fixed effects) is a valid backdoor adjustment set for estimating the causal effect of ATP participation on the choice of pursuing a research career in this graph. The analysis depends crucially on the assumption that the unobserved (latent) variable \( \text{research potential} \) does not directly affect program participation (ATP Participation \( \leftrightarrow \) Research

\footnote{For simplicity, we focus on career choice as the primary outcome and disregard several other dimensions of scientific career success that Azoulay et al. investigate.}
Potential); i.e., interviewers are not able to select applicants based on private information.

Azoulay et al. employ an inverse probability weighting estimator (Austin and Stuart, 2015). As such, covariates are only used to estimate the propensity score of receiving treatment and do not appear in an outcome regression. However, in this setting it would also not be advisable to interpret the effect of control variables such as prior research activities on career choice. The latent node research potential jointly affects an applicant’s prior research activities as well as future career choices. Thus, while prior research activity is a valid control for the effect of ATP participation, it is also endogenous, similar to the situation in Figure 1b. Consequently, even if we were to find a positive correlation between prior research activities and pursuing a research career (which is not reported in Azoulay et al.), it would be premature to conclude that, e.g., early publication success during medical school is a significant driver of subsequent career choices, since both of these variables are likely confounded by an applicant’s overall ability. The research design only allows to draw policy conclusions for the treatment variable ATP participation. Researchers should therefore be careful not to overinterpret their empirical results, even if that promises to offer interesting additional perspectives on a given research topic.

Discussion and Recommendations

Attaching substantive meaning to the marginal effects of biased control variables is problematic, as researchers could develop false intuitions or draw erroneous managerial and policy conclusions based on them. Therefore it is advisable to not discuss the results obtained for control variables in quantitative papers, unless the researcher can be sure that they have accounted for all relevant influence factors of the outcome in a regression (all-causes regression). Since in many practical settings this is unlikely to be the case, we recommend to treat controls as nuisance parameters, which are included in the analysis for identification purposes (and discussed as such) but their effects are not reported in the output tables (Liang and Zeger, 1995; Meehl, 1971). This corresponds to the way control variables are treated by non-parametric matching estimators (Heckman et al., 1998) and modern machine learning techniques for high-dimensional settings (Chernozhukov et al., 2018). These methods similarly do not report estimation results related to controls, either because there would be simply too many covariates in the analysis (which is the primary use-case for machine learning) or marginal effects of control variables are not even returned by the estimation protocol (as in the matching case).

Our recommendations thereby depart from prior literature insofar as control variable should not be promoted to have equal status with the other variables in the study (Spector and Bramnick, 2011, p. 297). Research designs based on control variables are employed to estimate the causal effect of a treatment variable on an outcome. As such, the treatment variable cannot be endogenous, otherwise estimates would be bi-
ased and other, more suitable research designs (such as instrumental variables, regression discontinuity designs, etc.) should be applied. By contrast, control variables can be endogenous (Frölich, 2008) and, as we argued in the preceding theoretical discussion, will likely be so in practice. Controls should be chosen to close all backdoor paths between a treatment and outcome, based on a theoretical model of the context under study (Bono and McNamara, 2011). As we have demonstrated previously, it is thereby not necessary to include all causal influence factors of the outcome variable in a regression. Our example (Azoulay et al., 2021) illustrates that in many cases it might actually be easier to control the treatment assignment mechanism instead, if institutional knowledge is richer about what determines treatment take-up compared to the potentially long list of variables that affect the outcome. Moreover, in many situations, researchers have the choice between different valid adjustment sets (see Figure 1d), which highlights their auxiliary nature for the analysis.

Since accounting for all influence factors of the outcome might be unrealistic in many contexts and control variables are therefore likely to be endogenous, interpreting their effect sizes in light of theory is potentially dangerous. Authors could infer wrong conclusions for managerial advice and subsequent studies might be inclined to build theory based on biased empirical results. To avoid this, we therefore recommend to refrain from interpreting control variables in published papers. Moreover, predicting the sign of control variable estimates ex-ante (Atinc et al., 2012) is difficult if endogenous control variables can pick up the effect of a multitude of other influence factors. Therefore, formulations such as “estimates of control variables have expected signs” should be avoided. As a nudge to stir the research community away from overinterpreting control variable estimates, not reporting them in regression tables, or at least relegating them to an appendix, seems appropriate. We emphasize that we agree with Becker (2005) in that control variables should be carefully discussed and authors need to justify their validity based on prior theory. However, their estimated coefficients are less relevant. It suffices, in our view, if the rationale for selecting specific control variables is discussed in the empirical section and regression tables contain a comment about their inclusion in the table notes.

Our recommendations are thereby in line with Westreich and Greenland (2013) who discuss a similar problem with respect to the interpretation of potentially endogenous controls in epidemiology. Because epidemiological studies usually present the results of multivariate regression analyses right after a table with descriptive statistics of the data, they coined the term table 2 fallacy. Keele et al. (2020) discuss related examples from the field of political science. They emphasize that for estimates of control variables to be given a causal interpretation, their effects need to be themselves causally identified. Since this is only plausible if there are no omitted variables (or the controls are unrelated to the omitted variables), we recommend researchers to focus attention on one causal factor (or a small set) at a time, for which backdoor paths can realistically be enumerated, and
treat control variables as nuisance parameters instead.

Finally, we caution against including estimates of potentially biased controls in meta-analyses (Aguinis et al., 2011). Such analyses pool the effects of a focal variable on an outcome across several studies. According to Becker (2005), systematic reporting of control variables facilitates cumulative science and knowledge aggregation by significantly increasing the pool of studies from which effect sizes for meta-analyses can be drawn from. This perspective overlooks, however, that control variables are unlikely to have a causal interpretation themselves. By contrast, their coefficients represent a combination of several different causal mechanisms and therefore do not provide accurate information about a theoretically meaningful quantity. Moreover, coefficients might vary substantially depending on which admissible adjustment set is used (see the example in Figure 1d).

Consequently, meta-analyses should restrict themselves to the main treatment variable(s), for which a plausible identification argument can be established, which highlights once again the unequal status of treatment and control variables in a regression analysis.

To conclude, there is no reason to be worried if the estimated coefficients of control variables do not have expected signs, since they are likely to be biased anyway in practical applications. Instead, researchers should rather focus on interpreting the marginal effects of the main variables of interest in their manuscripts. The estimation results obtained for controls, by contrast, have little substantive meaning and can therefore safely be omitted—or relegated to an appendix. This approach will not only prevent researchers from drawing wrong causal conclusions based on endogenous controls, but will furthermore allow to streamline the discussion sections of quantitative research papers and save on valuable manuscript space.

References

Aguinis, H., Pierce, C. A., Bosco, F. A., Dalton, D. R., and Dalton, C. M. (2011). Debunking myths and urban legends about meta-analysis. *Organizational Research Methods, 14*(2):306–331.

Angrist, J. D. (1990). Lifetime Earnings and the Vietnam Era Draft Lottery: Evidence from Social Security Administrative Records. *The American Economic Review, 80*(3):313–336.

Atinc, G., Simmering, M. J., and Kroll, M. J. (2012). Control variable use and reporting in macro and micro management research. *Organizational Research Methods, 15*(1):57–74.

Austin, P. C. and Stuart, E. (2015). Moving towards best practice when using inverse probability of treatment weighting (iptw) using the propensity score to estimate causal treatment effects in observational studies. *Statistics in Medicine, 34*(28):3661–3679.
Azoulay, P., Greenblatt, W. H., and Heggeness, M. L. (2021). Long-term effects from early exposure to research: Evidence from the NIH “Yellow Berets”. *Research Policy*, 50(9):1–19.

Becker, T. E. (2005). Potential problems in the statistical control of variables in organizational research: A qualitative analysis with recommendations. *Organizational Research Methods*, 8(3):274–289.

Becker, T. E., Atinc, G., Breaugh, J. A., Carlson, K. D., Edwards, J. R., and Spector, P. E. (2016). Statistical control in correlational studies: 10 essential recommendations for organizational researchers. *Journal of Organizational Behavior*, 37(2):157–167.

Bono, J. E. and McNamara, G. (2011). From the editors, publishing in AMJ—Part2: Research design. *Academy of Management Journal*, 54:657–660.

Carlson, K. D. and Wu, J. (2012). The illusion of statistical control: Control variable practice in management research. *Organizational Research Methods*, 15(3):413–435.

Chernozhukov, V., Chetverikov, D., Demirer, M., Duflo, E., Hansen, C., Newey, W., and Robins, J. (2018). Double/debiased machine learning for treatment and structural parameters. *The Econometrics Journal*, 21(1):C1–C68.

Cinelli, C. and Hazlett, C. (2020). Making sense of sensitivity: Extending omitted variable bias. *Journal of the Royal Statistical Society: Series B (Statistical Methodology)*, 82(1):39–67.

Durand, R. and Vaara, E. (2009). Causation, counterfactuals, and competitive advantage. *Strategic Management Journal*, 30(12):1245–1264.

Flammer, C. and Bansal, P. (2017). Does a long-term orientation create value? Evidence from a regression discontinuity. *Strategic Management Journal*, 38(9):1827–1847.

Frölich, M. (2008). Parametric and nonparametric regression in the presence of endogenous control variables. *International Statistical Review*, 76(2):214–227.

Heckman, J. J., Ichimura, H., and Todd, P. (1998). Matching as an econometric evaluation estimator. *The Review of Economic Studies*, 65(2):261–294.

Hünermund, P. and Bareinboim, E. (2021). Causal inference and data fusion in econometrics. Technical Report R-51.

Keele, L., Stevenson, R. T., and Elwert, F. (2020). The causal interpretation of estimated associations in regression models. *Political Science Research and Methods*, 8(1):1–13.
Liang, K.-Y. and Zeger, S. L. (1995). Inference based on estimating functions in the presence of nuisance parameters. *Statistical Science*, 10(2):158–173.

Meehl, P. E. (1971). High school yearbooks: A reply to schwarz. *Journal of Abnormal Psychology*, 77(2):143–148.

Pearl, J. (2000). *Causality: Models, Reasoning, and Inference*. Cambridge University Press, New York, United States, NY, 1st edition.

Spector, P. E. and Brannick, M. T. (2011). Methodological urban legends: The misuse of statistical control variables. *Organizational Research Methods*, 14(2):287–305.

Textor, J. and Liškiewicz, M. (2011). Adjustment criteria in causal diagrams: An algorithmic perspective. In *Proceedings of the 27th Conference on Uncertainty in Artificial Intelligence*, pages 681–688. AUAI press.

Westreich, D. and Greenland, S. (2013). The table 2 fallacy: presenting and interpreting confounder and modifier coefficients. *American Journal of Epidemiology*, 177(4):292–298.
## Online Appendix

### A Literature Reviews

Table 1: Strategic Management Journal

| Volume | Issue | Total | Count | Articles |
|--------|-------|-------|-------|----------|
| 41     | 13    | 6     | 2     | Polidoro, 2020; Wiersema, Ahn & Zhang, 2020; |
| 41     | 12    | 4     | 2     | Kiss, Libaers, Barr, Wang & zachary, 2020; Zheng & Wang, 2020; |
| 41     | 11    | 5     | 2     | Argyres, Rios & Silverman, 2020; Zhou & Park, 2020; |
| 41     | 10    | 4     | 3     | Rawley & Seamans, 2020; Uribe, 2020; Zhu, hu & Shen, 2020; |
| 41     | 9     | 6     | 3     | Lee, 2020; Chahine & Zhang, 2020; Blagoeva, Kuvashan & Jansen, 2020; |
| 41     | 8     | 4     | 2     | Dutt & Mitchel, 2020; Moeen & Mitchel, 2020; |
| 41     | 7     | 7     | 3     | Belderbos, Tong & Wu, 2020; Hoon Oh, Shapiro, Ho & Shin, 2020; Skiti, 2020; |
| 41     | 7     | 7     | 2     | Belderbos, Tong & Wu, 2020; Oh, Shapiro, Ho & Shin, 2020; |
| 41     | 6     | 6     | 2     | Chattopadhyay & Bercovitz, 2020; Smulowitz, Rousseau & Bromiley, 2020; |
| 41     | 5     | 4     | 2     | Sakakibara & Balasubramanian, 2020; Rocha & van Praag, 2020; |
| 41     | 4     | 7     | 2     | Aggarwal, 2020; Bonet, Capelli & Homari, 2020; |
| 41     | 3     | 4     | 2     | Arikain, Arikain & Shenkar, 2020; Agarwal, Braguinsky & Ohyama, 2020; |
| 41     | 2     | 5     | 2     | Ryu, Reuer & Brush, 2020; Jia, Gao & Julian, 2020; |
| 41     | 1     | 5     | 0     | - |
| 40     | 13    | 7     | 1     | Hsu, Kovács & Koçak, 2019; |
| 40     | 12    | 6     | 4     | Kim, 2019; Petrenko, Aime, Recendes & Chandler, 2019; Guldiken, Mallon, Fainsmidt, Judge & Clark, 2019; Shi, Conelly, Mackey & Gupta, 2019; |
| 40     | 11    | 5     | 2     | Woo, Canella & Mesquita, 2019; Zweiger, Stettler, Baldauf & Zamudio, 2019; |
Table 1: Strategic Management Journal

| Volume | Issue | Total | Count | Articles                                                                                                                                 |
|--------|-------|-------|-------|------------------------------------------------------------------------------------------------------------------------------------------|
| 40     | 10    | 6     | 3     | Ridge, Imgram, Abdurakhmonov & Hasija, 2019; Gómez–Solórzano, Tortoriello & Soda, 2019; Kavusan & Frankort, 2019;                           |
| 40     | 9     | 5     | 0     | -                                                                                                                                          |
| 40     | 8     | 6     | 1     | Barlow, Verhaal & Angus, 2019;                                                                                                          |
| 40     | 7     | 5     | 2     | Corsino, Mariani & Torrisi, 2019; Andrus, Withers, Courtright & Boivie, 2019;                                                            |
| 40     | 6     | 5     | 2     | Hiatt & Carlos, 2019; Piazzai & Wijnberg, 2019;                                                                                          |
| 40     | 5     | 4     | 2     | Hill, Recendes & Ridge, 2019; Yu, Minniti & Na-son, 2019;                                                                                |
| 40     | 4     | 5     | 3     | Paik, Kang & Seamans, 2019; Bruce, de Figueiredo & Silverman, 2019; Zheng, Ni & Crilly, 2019;                                           |
| 40     | 3     | 3     | 2     | Chatterji, Delecourt, Hasan & Koning, 2019; Bigelow, Nickerson & Park, 2019;                                                             |
| 40     | 2     | 5     | 3     | Criscuolo, Alexy, Sharapov & Salter, 2019; Ren, Hu & Cui, 2019; Boone, Lokshin, Guenter & Belderbos, 2019;                               |
| 40     | 1     | 7     | 4     | Haans, 2019 (Appendix); Chatterji, Cunningham & Joseph, 2019; Westphal & Zhu, 2019; Belderbos, Tong & Wu, 2019;                        |
| 39     | 13    | 5     | 1     | Garg & Zhao, 2018;                                                                                                                      |
| 39     | 12    | 5     | 4     | Cui, Yang & Vertinsky, 2018 (Appendix); Ranganathan, Ghosh & Rosenkopf, 2018; Arslan, 2018; Asgari, Tandon, Singh & Mitchell, 2018;  |
| 39     | 11    | 8     | 5     | Feldman, Gartenberg & Wulf, 2018; Claussen, Essling & Peukert, 2018; Burbano, Mamer & Snyder, 2018; Koch-Bayram & Wernicke, 2018; Mata & Alves, 2018; |
| 39     | 10    | 8     | 4     | Eberhardt & Eesley, 2018; Hornstein & Zhao, 2018; Kang & Zaheer, 2018; Albino-Pimentel, Dus-sauge & Shaver, 2018;                   |
| 39     | 9     | 6     | 3     | Khanna, Guler & Nerkar, 2018; Hawk & Pachecode-Almeida, 2018; Scheper & Barker, 2018;                                                     |
| 39     | 8     | 5     | 4     | Yayavaram, Srivastava & Sarkar, 2018; Gandal, Markovich & Riordan, 2018; Manning, Massini, Peeters & Lewin, 2018; Shi & Connelly, 2018; |
| Volume | Issue | Total | Count | Articles |
|--------|-------|-------|-------|----------|
| 39     | 7     | 8     | 4     | Byun, Frake & Agarwal, 2018; Mawdsley & Somaya, 2018; Alvarez-Garrido & Guler, 2018; Gupta, Mortal & Guo, 2018; |
| 39     | 6     | 0     | 0     | - |
| 39     | 5     | 9     | 5     | Chen & Garg, 2018; Kaul, Nary & Singh, 2018; Flammer, 2018; Ramírez & Tarziján, 2018; Wiersema, Hishimure & Suzuki, 2018; |
| 39     | 4     | 8     | 5     | Hawn, Chatterji & Mitchell, 2018; Choudhury & Haas, 2018; Bode & Singh, 2018; Tarakci, Ateş, Floyd, Ahn & Wooldridge, 2018; Rhee & Leonardi, 2018; |
| 39     | 3     | 0     | 0     | - |
| 39     | 2     | 6     | 4     | Chen, Kale & Hoskisson, 2018; Choi & McNamara, 2018; Deichmann & Jensen, 2018; Pek, Oh & Rivera, 2018; |
| 39     | 1     | 8     | 3     | Furr & Kapoor, 2018; Vidal & Mitchell, 2018; Jiang, Xia, Canella & Xiao, 2018; |
| 38     | 13    | 8     | 4     | Chem, Qian & Narayanan, 2017; Rabier, 2017; Dorobantu & Odziemkowska, 2017; Li, Yi & Cui, 2017; |
| 38     | 12    | 5     | 3     | Lee & Puranam, 2017; Werner, 2017; Theeke & Lee, 2017; |
| 38     | 11    | 8     | 4     | Carnahan, 2017; Kölbel, Busch & Jancso, 2017; Bos, Faems & Noseleit, 2017; Li & Zhou, 2017; |
| 38     | 10    | 8     | 4     | Moeen, 2017; Raffiee, 2017; Jiang, Canella, Xia & Semadeni, 2017; Wei, Ouyang & Chen, 2017; |
| 38     | 9     | 8     | 5     | Souder, Zaheer, Sapienza & Ranucci, 2017; Caner, Cohen & Pil, 2017; Shan, Fu & Zheng, 2017; Wang, Zhao & Chen, 2017; Li, Xia & Lin, 2017; |
| 38     | 8     | 9     | 5     | Zhou & Wan, 2017; Kulchina, 2017; Kim & Steensma, 2017; Steinbach, Holcomb, Holmes, Devers & Canella, 2017; Makino & Chan, 2017; |
| 38     | 7     | 10    | 3     | Armanios, Eesley, Li & Eisenhardt, 2017; Ref & Shapira, 2017; McCann & Bahl, 2017; |
| 38     | 6     | 7     | 3     | Roy & Cohen, 2017; Dowell & Muthulingam, 2017; Vanacker, Collewaert & Zahra, 2017; |
| Volume | Issue | Total | Count | Articles |
|--------|-------|-------|-------|----------|
| 38     | 5     | 9     | 5     | Stan & Puranam, 2017; Asgari, Singh & Mitchell, 2017; Kuusela, Keil & Maula, 2017; Girod & Whittington, 2017; Connelly, Tihanyi, Ketchen, Carnes & Ferrier, 2017; |
| 38     | 4     | 8     | 1     | Silverman & Ingram, 2017; |
| 38     | 3     | 10    | 4     | Bermiss, Hallen, McDonald & Pahnke, 2017; Chatterjee, 2017; Oh & Oetzel, 2017; Blake & Moschieri, 2017; |
| 38     | 2     | 11    | 5     | Flammer & Luo, 2017; Madsen & Walker, 2017; Mackey, Barney & Dotson, 2017; Fonti, Maoret & Whitbred, 2017; Deb, David & O’Brien, 2017; |
| 38     | 1     | 0     | 0     | - |
| 37     | 13    | 6     | 2     | Hawn & Ioannou, 2016; Stuart & Wang, 2016; |
| 37     | 12    | 7     | 3     | Wang, Zhao & He, 2016; Easley, Decelles & Lenox, 2016; Wu & Salomon, 2016; |
| 37     | 11    | 10    | 7     | Ghosh, Ranganathan & Rosenkopf, 2016; Kalnins, 2016; Chang, Kogut & Yang, 2016; Tsang & Yamanoi, 2016; Massimo, Colombo & Shafi, 2016; Chadwick, Guthrie & Xing, 2016; Park, Borah & Kotha, 2016; |
| 37     | 10    | 8     | 2     | Husted, Jamali & Saffar, 2016; Van Reenen & Penning, 2016; |
| 37     | 9     | 6     | 1     | Gomulya & Boeker, 2016; |
| 37     | 8     | 10    | 5     | Fonti & Maoret, 2016; Rodríguez & Nietro, 2016; Zhu & Yoshikawa, 2016; Yu, Umashankar & Rao, 2016; Jain, 2016; |
| 37     | 7     | 12    | 3     | Bennet & Pierce, 2016; Anand, Mulotte & Ren, 2016; Geng, Yoshikawa & Colpan, 2016; |
| 37     | 6     | 8     | 3     | Smith & Chae, 2016; Klingebiel & Joseph, 2016; Karna, Richter & Riesenkampf, 2016; |
| 37     | 5     | 5     | 4     | Roy & Sarkar, 2016; Lungeanu, Stern & Zajac, 2016; Tyler & Caner, 2016; Brandes, Dharwadkar & Suh, 2016; |
| 37     | 4     | 6     | 4     | Adner & Kapoor, 2016; Maslach, 2016; Poppo, Zhou & Li, 2016; Eckhardt, 2016; |
| Volume | Issue | Total | Count | Articles |
|--------|-------|-------|-------|----------|
| 37     | 3     | 9     | 3     | Feldman, Amit & Villalonga, 2016; Pe’er, Vertinsky & Keil, 2016; Barroso, Giarratana, Reis & Sorenson, 2016; |
| 37     | 2     | 8     | 3     | Chen, Crossland & Huang, 2016; Desender, Aguilera, Lópezpuertas-Lamy & Crespi, 2016; Kang, 2016 |
| 37     | 1     | 6     | 2     | Dezsö, Ross & Uribe, 2016; Ge, Huang & Png, 2016; |
| 36     | 13    | 8     | 3     | Joseph & Gaba, 2015; Macher & Mayo, 2015; Zhu & Chen, 2015; |
| 36     | 12    | 7     | 3     | Fuentelsaz, Garrido & Maicas, 2015; Malhotra, Zhu & Reus, 2015; Chen, 2015; |
| 36     | 11    | 8     | 5     | Zheng, Singh & Mitchell, 2015; Speckbacher, Neumann & Hoffmann, 2015; Skilton & Bernardes, 2015; Bermiss & Murmann, 2015; Fosfuri, Giarratana & Roca, 2015; |
| 36     | 10    | 4     | 3     | Kaplan & Vakili, 2015; Chen, Crossland & Luo, 2015; Ang, Benischke & Doh, 2015; |
| 36     | 9     | 7     | 4     | Chittoor, Kale & Puranam, 2015; Chang & Shim, 2015; Banalieva, Eddleston & Zellweger, 2015; Hashai, 2015; |
| 36     | 8     | 8     | 4     | Bidwell, Won, Barbulescu & Mollick, 2015; Steensma, Chari & Heidl, 2015; Durand & Vergne, 2015; Lange, Boivie & Westphal, 2015; |
| 36     | 7     | 9     | 6     | Elfenbein & Knott, 2015; Blettner, He, Hu & Bettis, 2015; Arrfelt, Wiseman, McNamara & Hult, 2015; Ioannou & Serafeim, 2015; Wowak, Mannor & Wowak, 2015; Pacheco & Dean, 2015; |
| 36     | 6     | 7     | 3     | Kim, 2015 (Appendix); Chizema, Liu, Lu & Gao, 2015; Miller, Xu & Mehrotra, 2015; |
| 36     | 5     | 7     | 5     | Bertrand & Capron, 2015; Ganco, Ziedonis & Agarwal, 2015; Younge, Tong & Fleming, 2015; Damraju, Barney & Makhija, 2015; Madsen & Rodgers, 2015; |
| 36     | 4     | 6     | 3     | Greve & Seidel, 2015; Harmon, Kim & Mayer, 2015; Tortoriello, 2015; |
Table 1: Strategic Management Journal

| Volume | Issue | Total | Count | Articles                                                                                       |
|--------|-------|-------|-------|------------------------------------------------------------------------------------------------|
| 36     | 3     | 4     | 3     | Diestre, Rajagopalan & Dutta, 2015; Chadwick, Super & Kwon, 2015; Kapoor & Furr, 2015;            |
| 36     | 2     | 6     | 4     | Pacheco-de-Almeida, Hawk & Yeung, 2015; Chown & Lui, 2015; Argyres, Bigelow & Nickerson, 2015;   |
| 36     | 1     | 2     | 1     | Cain, Moore & Haran, 2015;                                                                      |

Table 2: Organization Science

| Volume | Issue | Total | Count | Articles                                                                                      |
|--------|-------|-------|-------|------------------------------------------------------------------------------------------------|
| 31     | 6     | 7     | 3     | Park & Zhang, 2020; Maoret, Tortoriello & Iubbatti, 2020; Assenova, 2020;                      |
| 31     | 5     | 6     | 2     | Giarratana & Santaló, 2020; Tasseli, Zappa & Lomi, 2020;                                       |
| 31     | 4     | 5     | 3     | Chown, 2020; Boone & Özcan, 2020; Moreira, Klüeter & Tasseli, 2020;                           |
| 31     | 3     | 9     | 5     | Claes & Vissa, 2020; Younkin & Kaskooli, 2020; Tilleman, Russo & Nelson, 2020; Withers, Howard & Tihanyi, 2020; Aharonson, Bort & Woywode, 2020; |
| 31     | 2     | 6     | 2     | Chambers & Baker, 2020; Hallen, Cohen & Bingham, 2020;                                         |
| 31     | 1     | 5     | 2     | Diestre & Santaló, 2020; Jacqueminet, 2020;                                                   |
| 30     | 6     | 7     | 4     | Rahmandad & Vakili, 2019; Bowers & Prato, 2019; Negro & Olzak, 2019; Rietveld, Schilling & Bellavitis, 2019; |
| 30     | 5     | 5     | 3     | Moore, Payne, Filatotchev & Zajac, 2019; Berchicci, Dutt & Mitchell, 2019; Furr, 2019;        |
| 30     | 4     | 8     | 2     | Gaba & Greve, 2019; Bird, Short & Toffel, 2019;                                                |
| 30     | 3     | 6     | 2     | Lee, 2019; Blevins, Sauerwald, Hoobler & Robinson, 2019;                                       |
| 30     | 2     | 8     | 3     | Chatman, Greer, Sherman & Doerr, 2019; Furlan, Galeazzo, 2019; Eklund & Kapoor, 2019;         |
| 30     | 1     | 9     | 4     | Knott & Turner, 2019; Zhang, 2019; Rockart & Wilson, 2019; Godart & Galunic, 2019;           |
Table 2: Organization Science

| Volume | Issue | Total | Count | Articles |
|--------|-------|-------|-------|----------|
| 29     | 6     | 8     | 1     | Keum & Eggers, 2018; |
| 29     | 5     | 9     | 4     | Furlotti & Soda, 2018; Berry, 2018; Albert, 2018; Ertug, Gargiulo, Galunic & Zou, 2018; |
| 29     | 4     | 5     | 4     | Zhao, Ishihara, Jennings & Lounsbury, 2018; Hiatt, Carlos & Sine, 2018; James & Vaaler, 2018; Madsen & Desai, 2018; |
| 29     | 3     | 5     | 2     | Lee, 2018; Blevins, Sauerwald, Hoobler & Robertson, 2018; |
| 29     | 2     | 5     | 2     | Maslach, Branzei, Rerup & Zbaracki, 2018; Retveld & Eggers, 2018; |
| 29     | 1     | 7     | 3     | Conti, 2018; Durand & Georgallis, 2018; Hsu, Koçak & Kovács, 2018; |
| 28     | 6     | 6     | 1     | Greve & Yue, 2017; |
| 28     | 5     | 6     | 4     | Ditriaidis, Lee, Ramarajan & Battilana, 2017; Tzabbar & Margolis, 2017; Lee & Kapoor, 2017; Ozmel, Yavuz, Reuer & Zenger, 2017; |
| 28     | 4     | 5     | 3     | Merluzzi, 2017; Keum & Kelly, 2017; Mannucci, 2017; |
| 28     | 3     | 8     | 3     | Laursen, Moreire, Reichstein & Leone, 2017; Kapoor & Agarwal, 2017; Reuer & Devarakonda, 2017; |
| 28     | 2     | 7     | 2     | Chattopadhyay & Choudhury, 2017; Bonet & Salvador, 2017; |
| 28     | 1     | 8     | 7     | Obloj & Zenger, 2017; Lee & Meyer-Doyle, 2017; Gartenberg & Wulf, 2017; Hoehn-Weiss, Karim & Lee, 2017; McEvily, Zaheer & Kamal, 2017; Ferguson & Carnabuci, 2017; Eberhart, Eesley & Eisenhardt, 2017; |
| 27     | 6     | 8     | 4     | Quintane & Carnabuci, 2016; Boone & Özcan, 2016; Mollick, 2016; Vanacker & Forbes, 2016; |
| 27     | 5     | 11    | 7     | Wang, Doucet, Waller, Sanders & Phillips, 2016; Sako, Chondrakis & Vaaler, 2016; Choi, Kumar & Zambuto, 2016; Souder, Reilly, Bromiley & Mitchell, 2016; Yang & Schwartz, 2016; Eesleym, 2016; Zhang, Marquis & Qiao, 2016; |
| Volume | Issue | Total | Count | Articles                                                                 |
|--------|-------|-------|-------|--------------------------------------------------------------------------|
| 27     | 4     | 8     | 6     | Stenard & Sauermann, 2016; Hahl, 2016; Montauti & Wezel, 2016; Lin , 2016; Burbano, 2016; Bhaskarabhatla, 2016; |
| 27     | 3     | 0     | 0     | -                                                                         |
| 27     | 2     | 8     | 5     | Szulanski, Ringov & Jensen, 2016; Zhang & Gimeno, 2016; Kilduff, Willer & Anderson, 2016; Khessina & Reis, 2016; Eesley, Li & Yang, 2016; |
| 27     | 1     | 8     | 5     | Adams, Fontana & Malerba, 2016; McDonell, 2016; Slavova, Fosfuri & De Castro, 2016; Cuypers, Koh & Wang, 2016; Kozhikode, 2016; |
| 26     | 6     | 9     | 4     | Hasan, Ferguson & Koning, 2015; Carnabuci, Operti & Kovács, 2015; Huang & Washington, 2015; Hiatt, Grandy, & Lee, 2015; |
| 26     | 5     | 12    | 3     | Cobb, 2015; Srivastava, 2015; Verhaal, Khessina & Dobre, 2015; |
| 26     | 4     | 10    | 5     | Sosa, Gargiulo & Rowles, 2015; Marino, Aversa, Mesquita & Anand, 2015; Vidal & Mitchell, 2015; Brands, Menges &b Kilduff, 2015; Kleinbaum, Jordan & Aufia, 2015; |
| 26     | 3     | 13    | 7     | Sterling, 2015; Dobrajska, Billinger & Karim, 2015; Piazza & Perretti, 2015; O’Reilly, Robinson, Berdahl & Banki, 2015; Lee & Lounsbury, 2015; Jacobides &Yae, 2015; Yang, Li & Delios, 2015; |
| 26     | 2     | 13    | 6     | Williams & Polman, 2015; Rider & Tan, 2015; Casciaro & Lobo, 2015; Tortoriello, McEvily & Krackhardt, 2015; Gambardella, Ganco & Honoré, 2015; Evans, Hendron & Oldroyd, 2015; |
| 26     | 1     | 14    | 7     | Jensen & Kim, 2015; Smith & Hou, 2015; Lo & Kennedy, 2015; Almeida, Phene & Li, 2015; Aggarwal & Wu, 2015; Zhou, 2015; Lui & Wezel, 2015; |
### B Simulation Results

Table 3: OLS regressions with varying adjustment sets

|       | Figure 1a | Figure 1b | Figure 1c | Figure 1d |
|-------|-----------|-----------|-----------|-----------|
|       | (1)       | (2)       | (3)       | (4)       |
| $X$   | 1.017     | 1.004     | 1.015     | 0.993     |
|       | (0.015)   | (0.006)   | (0.010)   | (0.012)   |
| $Z_1$ | 0.499     | -0.019    | 1.503     | 1.004     |
|       | (0.018)   | (0.013)   | (0.014)   | (0.014)   |
| $Z_2$ | 0.993     | 0.997     |           |           |
|       | (0.008)   | (0.008)   |           |           |
| $Z_3$ |           |           | 0.994     | 0.991     |
|       |           |           | (0.008)   | (0.010)   |
| $Z_4$ |           |           | 0.991     | 0.988     |
|       |           |           | (0.008)   | (0.010)   |
| $Z_5$ |           |           | 1.011     | 1.009     |
|       |           |           | (0.006)   | (0.008)   |

**Notes:** Simulation results (as discussed in the main text) using different backdoor-admissible adjustment sets for the causal models depicted in Figure 1. Bootstrapped standard errors (with 1,000 replications and $N = 10,000$) in parentheses. True effect sizes for all variables (except intercepts) are equal to one.