Poorly Measured Confounders are More Useful on the Left Than on the Right

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Abstract

Researchers frequently test identifying assumptions in regression based research designs (which include instrumental variables or difference-in-differences models) by adding additional control variables on the right hand side of the regression. If such additions do not affect the coefficient of interest (much) a study is presumed to be reliable. We caution that such invariance may result from the fact that the observed variables used in such robustness checks are often poor measures of the potential underlying confounders. In this case, a more powerful test of the identifying assumption is to put the variable on the left hand side of the candidate regression. We provide derivations for the estimators and test statistics involved, as well as power calculations, which can help applied researchers interpret their findings. We illustrate these results in the context of various strategies which have been suggested to identify the returns to schooling.

JEL classifications: C31, C52

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1 Introduction

Research on causal effects depends on implicit identifying assumptions, which typically form the core of a debate about the quality and credibility of a particular research design. In regression based strategies, this is the claim that variation in the regressor of interest is as good as random after conditioning on a sufficient set of control variables. In instrumental variables models it involves the assumption that the instrument is as good as randomly assigned. In panel or differences-in-differences designs it is the parallel trends assumption, possibly after suitable conditioning. The credibility of a design can be enhanced when researchers can show explicitly that potentially remaining sources of selection bias have been eliminated. This is often done through some form of balancing tests or robustness checks.

The research designs mentioned above can all be thought of as variants of regression strategies. If the researcher has access to a variable for a potentially remaining confounder, tests for the identifying assumption take two canonical forms. The variable can be added as a control on the right hand side of the regression. The identifying assumption is confirmed if the estimated causal effect of interest is insensitive to this variable addition—we call this the coefficient comparison test. Alternatively, the variable can be placed on the left hand side of the regression instead of the outcome variable. A zero coefficient on the causal variable of interest then confirms the identifying assumption. This is the balancing test which is typically carried out using baseline characteristics or pre-treatment outcomes in a randomized trial or in a regression discontinuity design.

Researchers often rely on one or the other of these tests. The main point of our paper is to show that the balancing test, using the proxy for the candidate confounder on the left hand side of the regression, is generally more powerful. This is particularly the case when the available variable is a noisy measure of the true underlying confounder. The attenuation due to

measurement error often implies that adding the candidate variable on the right hand side as a regressor does little to eliminate any omitted variables bias. The same measurement error does comparatively less damage when putting this variable on the left hand side. Regression strategies work well in finding small but relevant amounts of variation in noisy dependent variables.

These two testing strategies are intimately related through the omitted variables bias formula. The omitted variables bias formula shows that the coefficient comparison test involves two regression parameters, the coefficient from the balancing test and the coefficient from the added regressor in the outcome equation. If the researcher has a strong prior that the added regressor ought to matter for the outcome under study then the balancing test will provide the remaining information necessary to assess the research design. This maintained assumption is the ultimate source of the superior power of the balancing test. However, we show that quantitatively meaningful differences emerge particularly when there is some substantial amount of measurement error in the added regressor. We derive the relevant parameters in the presence of measurement error in Section 3.

Of course, sometimes researchers may be more agnostic about whether the added regressor matters for the outcome. In case it does not matter, rejecting balance for this variable is of no consequence for this particular research design. In this view, only the coefficient comparison test is really relevant while the balancing test provides no additional information. However, this strikes us as a narrow view and not one shared by many in the experimental community, where balancing tests are commonly used. Lack of balance is seen as an indictment of the randomization in an experiment irrespective of whether the variable in question affects the outcome. Lack of balance with respect to one or more observed covariates raises the possibility that there may also be lack of balance for other unobservables, and would lead a prudent researcher to reassess the credibility of their research design. The same should be true for quasi-experimental research based on observational

data.

A second point we are making is that the two strategies, coefficient comparison and balancing, both lead to explicit statistical tests. The balancing test is a simple t-test used routinely by researchers. When adding a covariate on the right hand side, comparing the coefficient of interest across the two regressions can be done using a generalized Hausman test. In practice, we have not seen this test carried out in applied papers, where researchers typically just eye-ball the results.[2] We provide the relevant test statistics and discuss how they behave under measurement error in Section 4. We also show how the coefficient comparison test is simple to implement for varying identification strategies. We demonstrate the superior power of the balancing test under a variety of scenarios in Section 5.

The principles underlying the points we are making are not new but the consequences do not seem to be fully appreciated in much applied work. Griliches (1977) is a classic reference for the issues arising when regression controls are measured with error. A subsequent literature, for example Rosenbaum and Rubin (1983) and Imbens (2003), has considered omitted variables bias in non-linear models without measurement error. More closely related is Battistin and Chesher (2014), as it discusses identification in the presence of a mismeasured covariate in non-linear models. Like in the literature following Rosenbaum and Rubin (1983), they discuss identification given assumptions about a missing parameter, namely the degree of measurement error in the covariate. We follow Griliches (1977) in framing our discussion around the omitted variables bias arising in linear regressions, the general framework used most widely in empirical studies. Unlike this literature, we are less interested in point identification in the presence of missing information. We go beyond the analysis in all of these papers in our explicit discussion of testing, which forms the core of our study.

[2] An exception is Gelbach (2016), who discusses the Hausman test in this context.

Altonji, Elder, and Taber (2005) discuss an alternative but closely related approach to the problem. As we noted above, applied researchers often argue that relative stability of regression coefficients when adding additional controls provides evidence for credible identification. Implicit in this argument is the idea that other confounders not controlled for are similar to the controls just added to the regression. The paper by Altonji, Elder, and Taber (2005) formalizes this argument. In practice, adding controls will typically move the coefficient of interest somewhat even if it is not by much. Altonji, Conley, Elder, and Taber (2013) and Oster (forthcoming) extend the original Altonji, Elder and Taber work by providing more precise conditions for bounds and point identification in this case. The approach in these papers relies on an assumption about how the omitted variables bias due to the observed regressor is related to any remaining omitted variables bias due to unobserved confounders.

The remaining unobserved confounders in this previous work can be thought of as the source of measurement error in the covariate which is added to the regression in our analysis. For example, in our empirical example below, we use mother's education as a measure for family background but this variable may only capture a small part of all the relevant family background information, a lot of which may be orthogonal to mother's education. In fact, we show that our formulation and Oster's (forthcoming) are isomorphic. This means that our framework is a useful starting point for researchers who are willing to make the type of assumptions in Altonji, Elder, and Taber (2005) and follow-up papers as well.

Another related strand of work is by Belloni, Chernozhukov, and Hansen (2014a, b), who tackle the opposite problem from Altonji, Elder, and Taber (2005), namely choosing the best controls when the researcher has a potentially bigger set of candidate controls available than is necessary. This large dimensional set comes from the fact that they consider possible nonlinearities and interactions among regressors. Belloni, Chernozhukov, and Hansen

(2014b) use Lasso to select regressors which are highly correlated with either the treatment or the outcome conditional on other covariates. They then estimate an outcome equation including as controls all the regressors selected in this preliminary step. In a sense, this is more closely related to our setup than the Altonji, Elder and Taber approach as Belloni, Chernozhukov, and Hansen (2014b) also postulate that identification can be achieved when using a subset of the available covariates as controls. Their variable selection problem is related to the two testing strategies we discuss in this paper. However, like Altonji, Conley, Elder, and Taber (2013) and Oster (forthcoming), their ultimate interest is in point identification and inference for the treatment effects parameter, not in testing whether a particular specification is subject to remaining confounders. Their setup is also not specifically geared towards dealing with control variables which are subject to error, which is our focus.

An older literature by Hausman (1978), Hausman and Taylor (1980), and Holly (1982) (see also the summary in MacKinnon, 1992, section II.9) considers the relative power of the Hausman test compared to alternatives, in particular an F-test for the added covariates in the outcome equation when potentially multiple covariates are added. This comparison effectively maintains that there is a lack of balance, and instead tests whether the added regressors matter for explaining the outcome. While this is a different exercise from ours, this literature highlights the potential power of the Hausman test when it succinctly transforms a test with multiple restrictions (like the F-test for the added covariates) into a test with a single restriction (the coefficient comparison test). We briefly discuss how to extend our framework to multiple added controls in Section 5.4. We also reach the conclusion that the Hausman test may be useful when the goal is to summarize a large number of restrictions.

Griliches (1977) uses estimates of the returns to schooling as example for the methodological points he makes. Such estimates have formed a staple of labor economics ever since. We use Griliches' data from the National Lon-

gitudinal Survey of Young Men to illustrate our power results in Section 6. In addition to Griliches (1977), this data set has been used in a well known study by Card (1995). It is well suited for our purposes because the data contain various test score measures which can be used as controls in a regression strategy (as investigated by Griliches, 1977), a candidate instrument for college attendance (investigated by Card, 1995), as well as a myriad of other useful variables on individual and family background. The empirical results support and illustrate our theoretical claims.

2 A Simple Framework

Consider the following simple framework starting with a population regression equation

$$y_i = \alpha^s + \beta^s s_i + e_i^s \tag{1}$$

where y_i is an outcome like log wages, s_i is the causal variable of interest, like years of schooling, and e_i^s is the regression residual. The researcher proposes this short regression model to be causal. This might be the case because the data come from a randomized experiment, so the simple bivariate regression is all we need. More likely, the researcher has a particular research design applied to observational data. For example, in the case of a regression strategy controlling for confounders, y_i and s_i would be residuals from regressions of the original outcome and treatment variables on the chosen controls. In the case of panel data or differences-in-differences designs the controls are sets of fixed effects. In the case of instrumental variables, s_i would be the predicted value from a first stage regression. In practice, (1) encompasses a wide variety of empirical approaches, and should be thought of as a short-hand for these.[3]

Now consider the possibility that the population regression parameter β^s from (1) may not actually capture a causal effect. There may be a candidate

[3]Of course, all subsequent regression equations and results also inherit the structure of the actual underlying research design.

confounder x_i, so that the causal effect of s_i on y_i would only be obtained conditional on x_i, as in the long regression

$$y_i = \alpha + \beta s_i + \gamma x_i + e_i \tag{2}$$

and the researcher would like to probe whether this is a concern. For example, in the returns to schooling context, x_i might be some remaining part of an individual's earnings capacity which is also related to schooling, like ability or family background.

Researchers who find themselves in a situation where they start with a proposed causal model (1) and a measure for a candidate confounder x_i typically do one of two things: They either regress x_i on s_i and check whether s_i is significant, or they include x_i on the right hand side of the original regression as in (2), and check whether the estimate of β changes materially when x_i is added to the regression of interest. The first strategy constitutes a test for "balance," a standard check for successful randomization in an experiment. In principle, the second strategy has the advantage that it goes beyond testing whether (1) qualifies as a causal regression. An appreciable change in β suggests that the original estimate β^s is biased. The results obtained with x_i as an additional control should be closer to the causal effect we seek to uncover. In particular, if x_i were the only relevant confounder and if we measure it without error, the β parameter from the controlled regression is the causal effect of interest. In practice, there is usually little reason to believe that these two conditions are met, and hence a difference between β and β^s again only indicates a flawed research design.

The relationship between these two strategies is easy to see. Write the regression of x_i on s_i, which we will call the balancing regression, as

$$x_i = \delta_0 + \delta s_i + u_i. \tag{3}$$

The change in the coefficient β from adding x_i to the regression (1) is given

by the omitted variables bias formula

$$\beta^s - \beta = \gamma\delta. \tag{4}$$

The change in the coefficient of interest β from adding x_i consists of two components, the coefficient γ on x_i in the outcome equation (2) and the coefficient δ from the balancing regression.

Here we consider the relationship between these two approaches: the balancing test, consisting of an investigation of the null hypothesis

$$H_0 : \delta = 0, \tag{5}$$

compared to the inspection of the coefficient movement $\beta^s - \beta$. The latter strategy of comparing β^s and β is often done informally, but it can be formalized as a statistical test of the null hypothesis

$$H_0 : \beta^s - \beta = 0, \tag{6}$$

which we will call the coefficient comparison (CC) test. From (4) it is clear that (6) amounts to

$$H_0 : \beta^s - \beta = 0 \Leftrightarrow \gamma = 0 \text{ or } \delta = 0. \tag{7}$$

This highlights that the two approaches formally test the same hypothesis under the maintained assumption $\gamma \neq 0$. We may often have a strong sense that $\gamma \neq 0$; i.e. we are dealing with a variable x_i which we believe affects the outcome, but we are unsure whether it is related to the regressor of interest s_i. In this case, both tests would seem equally suitable. Nevertheless, in other cases γ may be zero, or we may be unsure. In this case, the coefficient comparison test seems to dominate because it directly addresses the question we are after, namely whether the coefficient of interest β is affected by the inclusion of x_i in the regression.[4]

[4]Equations (4) and (7) highlight that a regressor ought to be included in the long regression when both $\gamma \neq 0$ and $\delta \neq 0$. This differs from the selection rule chosen by Belloni, Chernozhukov, and Hansen (2014b), who include a regressor when either $\gamma \neq 0$ or $\delta \neq 0$ is true.

Here we make the point that the balancing test adds valuable information particularly when the true confounder is measured with error. In general, x_i may not be easy to measure. If the available measure for x_i contains classical measurement error, the estimator of γ in (2) will be attenuated, and the comparison $\beta^s - \beta$ will be too small (in absolute value) as a result. The estimator of δ from the balancing regression is still consistent in the presence of measurement error; this regression simply loses precision because the mismeasured variable is on the left hand side. Under the maintained assumption that $0 < \gamma < \infty$, the balancing test is more powerful than the coefficient comparison test. In order to make these statements precise, we collect results for the relevant population parameters for the case of classical measurement error in the following section, before moving on to the test statistics.

3 Population Parameters in the Presence of Measurement Error

The candidate variable x_i is not observed. Instead, the researcher works with the mismeasured variable

$$x_i^m = x_i + m_i. \tag{8}$$

Here we assume the measurement error m_i is classical, i.e. $E(m_i) = 0$, $Cov(x_i, m_i) = 0$. In section 5 below we also investigate the impact of non-classical errors. As a result of the measurement error, the researcher compares the regressions

$$\begin{aligned} y_i &= \alpha^s + \beta^s s_i + e_i^s \\ y_i &= \alpha^m + \beta^m s_i + \gamma^m x_i^m + e_i^m. \end{aligned} \tag{9}$$

Notice that the short regression does not involve the mismeasured x_i, so that $\beta^s = \beta + \gamma\delta$ as before. However, the population regression coefficients

β^m and γ^m are now different from β and γ from equation (2), and they are related in the following way:

$$\begin{aligned} \beta^m &= \beta + \gamma\delta\frac{1-\lambda}{1-R^2} = \beta + \gamma\delta\theta \\ \gamma^m &= \gamma\frac{\lambda - R^2}{1-R^2} = \gamma\left(1-\theta\right) \end{aligned} \tag{10}$$

where R^2 is the population R^2 of the regression of s_i on x_i^m and

$$\lambda = \frac{Var\left(x_i\right)}{Var\left(x_i^m\right)}$$

is the reliability of x_i^m.[5] It measures the amount of measurement error present as the fraction of the variance in the observed x_i^m, which is due to the signal in the true x_i. λ is also the attenuation factor in a simple bivariate regression on x_i^m. In the multivariate model (9), an alternative way to parameterize the amount of measurement error is

$$\theta = \frac{1-\lambda}{1-R^2} = \frac{\sigma_m^2}{\sigma_u^2 + \sigma_m^2}.$$

where $\sigma_\bullet^2$ denotes the variance of the random variable in the subscript. $1-\theta$ is the multivariate attenuation factor. Recall that u_i is the residual from the balancing regression (3).

With the mismeasured x_i^m the balancing regression becomes

$$x_i^m = \delta_0^m + \delta^m s_i + u_i + m_i, \tag{11}$$

which implies that

$$\lambda = 1 - \frac{\sigma_m^2}{Var\left(x_i^m\right)} > 1 - \frac{\sigma_u^2 + \sigma_m^2}{Var\left(x_i^m\right)} = R^2.$$

As a result

$$\begin{aligned} 0 &< \frac{1-\lambda}{1-R^2} < 1 \\ 0 &< \frac{\lambda - R^2}{1-R^2} < \lambda. \end{aligned}$$

[5]Note R^2 is also the population R^2 of the regression of x_i^m on s_i.

θ is an alternative way to parameterize the degree of measurement error in x_i compared to λ and R^2. The θ parameterization uses only the variation in x_i^m which is orthogonal to s_i. This is the part of the variation in x_i^m relevant to the estimate of γ^m in regression (9), which also has s_i as a regressor. θ turns out to be a useful parameter in many of the derivations that follow.

The population coefficient β^m differs from β but less so than β^s. In fact, β^m lies between β^s and β, as can be seen from (10). The parameter γ^m is attenuated compared to γ; the attenuation is bigger than in the case of a bivariate regression of y_i on x_i^m without the regressor s_i if x_i^m and s_i are correlated ($R^2 > 0$).

These results highlight a number of issues. The gap $\beta^s - \beta^m$ is too small compared to the desired $\beta^s - \beta$, directly affecting the coefficient comparison test. In addition, γ^m is biased towards zero. Ceteris paribus, this is making the assessment of the hypothesis $\gamma = 0$ more difficult. Finally, the balancing regression (11) with the mismeasured x_i^m involves measurement error in the dependent variable and therefore no bias in the OLS estimator of δ^m, i.e. plim $\left(\widehat{\delta}^m\right) = \delta^m = \delta$, but simply a loss of precision as compared to $\hat{\delta}$.

The results here are also useful for thinking about the identification of β and γ in the presence of measurement error. Rearranging (10) yields

$$\begin{aligned} \gamma &= \gamma^m \frac{1 - R^2}{\lambda - R^2} \\ \beta &= \beta^m - \delta\gamma^m \frac{1 - \lambda}{\lambda - R^2}. \end{aligned} \tag{12}$$

Since R^2 can be estimated from the data, these expressions only involve the unknown parameter λ. If we are willing to make an assumption about the measurement error, we are able to point identify β. Even if λ is not known precisely, (12) can be used to bound β for a range of plausible reliabilities. Alternatively, (10) can be used to derive the value of λ for which $\beta = 0$. These calculations are similar in spirit to the ones suggested by Oster (forthcoming) in a setting that is closely related.

4 Inference

In this section, we consider how conventional standard errors and test statistics for the quantities of interest are affected in the homoskedastic case.[6] We present the theoretical power functions for the two alternative test statistics; derivations are in Appendix A, which also shows that our results carry over to robust standard errors. We extend the power results to the heteroskedastic case and non-classical measurement error in simulations. Our basic conclusions are the same in all these different scenarios.

Start with the standard error of estimator $\widehat{\delta}^m$ from the balancing regression:

$$\sqrt{n}\widehat{se}\left(\widehat{\delta}^m\right) \xrightarrow{p} \sqrt{\frac{\sigma_u^2+\sigma_m^2}{\sigma_s^2}} = \frac{1}{\sqrt{1-\theta}}\frac{\sigma_u}{\sigma_s},$$

where we use $\widehat{se}(\bullet)$ to denote the estimated standard error of a given estimator. Let $se(\bullet)$ denote the *asymptotic* standard error of an estimator, i.e., $se(\bullet) \equiv \frac{1}{\sqrt{n}}\text{plim}\{\sqrt{n}\widehat{se}(\bullet)\}$. In the case of $\widehat{\delta}^m$,

$$se\left(\widehat{\delta}^m\right) = \frac{1}{\sqrt{n}}\frac{1}{\sqrt{1-\theta}}\frac{\sigma_u}{\sigma_s}.$$

Comparing the asymptotic standard error of $\widehat{\delta}^m$ to its counterpart in the case with no measurement error,

$$se\left(\widehat{\delta}\right) = \frac{1}{\sqrt{n}}\frac{\sigma_u}{\sigma_s},$$

we have

$$se\left(\widehat{\delta}^m\right) = \frac{se\left(\widehat{\delta}\right)}{\sqrt{1-\theta}}.$$

Since $0 < \theta < 1$, the standard error is inflated compared to the case with no measurement error.

[6] See Appendix A for the precise setup of the model. The primitive disturbances are s_i, u_i, e_i, and m_i, which we assume to be uncorrelated with each other. Other variables are determined by (3), (2), and (8).

A test based on the t-statistic

$$t_{\delta^m} = \frac{\widehat{\delta}^m}{\widehat{se}\left(\widehat{\delta}^m\right)}$$

remains consistent because m_i is correctly accounted for in the residual of the balancing regression (11), but the t-statistic is asymptotically smaller than in the error free case: As $n \to \infty$

$$\frac{1}{\sqrt{n}} t_{\delta^m} \xrightarrow{p} \sqrt{1-\theta}\frac{\delta}{\left(\frac{\sigma_u}{\sigma_s}\right)} < \frac{\delta}{\left(\frac{\sigma_u}{\sigma_s}\right)} \xleftarrow{p} \frac{1}{\sqrt{n}} t_\delta$$

This means the null hypothesis (5) is rejected less often. The test is less powerful than in the error free case; the power loss is captured by the term $\sqrt{1-\theta}$.

We next turn to $\widehat{\gamma}^m$, the estimator for the coefficient on the mismeasured x_i^m in (9). The parameter γ is of interest since it determines the coefficient movement $\beta^s - \beta = \gamma\delta$ in conjunction with the result from the balancing regression. Let $\tilde{x}_i^m$ be the residual from the population regression of x_i^m on s_i. For ease of exposition, we impose conditional homoskedasticity of e_i^m given s_i and x_i^m here and leave the more general case to Appendix A.2.3. The standard error for $\widehat{\gamma}^m$ in the limit is

$$\begin{aligned} se\left(\widehat{\gamma}^m\right) &= \frac{1}{\sqrt{n}} \frac{\sqrt{Var\left(e_i^m\right)}}{\sqrt{Var\left(\widetilde{x}_i^m\right)}} \\ &= \frac{1}{\sqrt{n}} \sqrt{\frac{\gamma^2\theta\sigma_u^2 + \sigma_e^2}{\sigma_u^2 + \sigma_m^2}} \\ &= \frac{1}{\sqrt{n}} \sqrt{1-\theta} \sqrt{\theta\gamma^2 + \frac{\sigma_e^2}{\sigma_u^2}}, \end{aligned}$$

while

$$se\left(\widehat{\gamma}\right) = \frac{1}{\sqrt{n}} \sqrt{\frac{\sigma_e^2}{\sigma_u^2}}.$$

$se(\widehat{\gamma}^m)$ involves two terms: the first term is an attenuated version of $se(\widehat{\gamma})$ from the corresponding regression with the correctly measured x_i, while the second term depends on the value of γ. The parameters in the two terms are not directly related, so $se\left(\widehat{\gamma}^m\right) \gtrless se\left(\widehat{\gamma}\right)$. Measurement error does not necessarily inflate the standard error here.

The two terms have a simple, intuitive interpretation. Measurement error attenuates the parameter γ^m towards zero, the attenuation factor is $1-\theta$. The standard error is attenuated in the same direction; this is reflected in the $\sqrt{1-\theta}$ factor, which multiplies the remainder of the standard error calculation. The second influence from measurement error comes from the term $\theta\gamma^2$, which results from the fact that the residual variance $Var\left(e_i^m\right)$ is larger when there is measurement error. The increase in the variance is related to the true γ, which enters the residual.

The t-statistic for testing whether $\gamma^m = 0$ is

$$t_{\gamma^m} = \frac{\widehat{\gamma}^m}{\widehat{se}\left(\widehat{\gamma}^m\right)}$$

and it follows that

$$\frac{1}{\sqrt{n}} t_{\gamma^m} \xrightarrow{p} \sqrt{1-\theta}\frac{\gamma}{\sqrt{\theta\gamma^2 + \frac{\sigma_e^2}{\sigma_u^2}}} < \frac{\gamma}{\sqrt{\frac{\sigma_e^2}{\sigma_u^2}}} \xleftarrow{p} \frac{1}{\sqrt{n}} t_\gamma.$$

As in the case of $\widehat{\delta}^m$ from the balancing regression, the t-statistic for $\widehat{\gamma}^m$ is smaller than t_γ for the error free case. But in contrast to the balancing test statistic t_{δ^m}, measurement error reduces t_{γ^m} relatively more, namely due to the term $\theta\gamma^2$ in the denominator, in addition to the attenuation factor $\sqrt{1-\theta}$. This is due to the fact that measurement error in a regressor both attenuates the relevant coefficient towards zero and introduces additional variance into the residual. Though interestingly, $\theta\gamma^2$ captures the additional residual variance while the factor $\sqrt{1-\theta}$ now captures the attenuation of γ^m. In the balancing test statistic, $\sqrt{1-\theta}$ accounted for the residual variance. The upshot from this discussion is that classical measurement error makes

the assessment of whether $\gamma = 0$ comparatively more difficult compared to the assessment whether $\delta = 0$. As we will see, this is the source of the greater power of the balancing test statistic.

Finally, consider the quantity $\beta^s - \beta^m$, which enters the coefficient comparison test. To form a test statistic for this quantity we need the expression for the asymptotic variance of $\widehat{\beta}^s - \widehat{\beta}^m$, which we derive through an application of the delta method to the omitted variables bias formula

$$\widehat{\beta}^s - \widehat{\beta}^m = \widehat{\delta}^m \widehat{\gamma}^m.$$

Specifically, we can relate $Var(\widehat{\beta}^s - \widehat{\beta}^m)$ to the asymptotic variances of $\widehat{\delta}^m$ and $\widehat{\gamma}^m$ and their asymptotic covariance:

$$\begin{aligned} Var\left(\widehat{\beta}^s - \widehat{\beta}^m\right) = \gamma^2 (1-\theta)^2 Var\left(\widehat{\delta}^m\right) + \delta^2 Var\left(\widehat{\gamma}^m\right) \\ + 2\delta\gamma (1-\theta) Cov\left(\widehat{\delta}^m, \widehat{\gamma}^m\right). \end{aligned} \tag{13}$$

Using $Var\left(\widehat{\delta}^m\right)$ and $Var\left(\widehat{\gamma}^m\right)$, which we derived above, and the fact that $Cov\left(\widehat{\delta}^m, \widehat{\gamma}^m\right) = 0$, which we show in the Appendix, we get

$$Var\left(\widehat{\beta}^s - \widehat{\beta}^m\right) = \frac{1}{n}(1-\theta)\left(\gamma^2 \frac{\sigma_u^2}{\sigma_s^2} + \theta\delta^2\gamma^2 + \delta^2 \frac{\sigma_e^2}{\sigma_u^2}\right).$$

It is easy to see that, like $Var\left(\widehat{\gamma}^m\right)$, $Var\left(\widehat{\beta}^s - \widehat{\beta}^m\right)$ has both an attenuation factor as well as an additional positive term compared to the case where $\theta = 0$, i.e. $Var\left(\widehat{\beta}^s - \widehat{\beta}\right)$. Measurement error may therefore raise or lower the sampling variance for the coefficient comparison test.

Before we proceed to discuss the power of the coefficient comparison test, we note that the covariance term in

$$Var\left(\widehat{\beta}^s - \widehat{\beta}^m\right) = Var\left(\widehat{\beta}^s\right) + Var\left(\widehat{\beta}^m\right) - 2Cov\left(\widehat{\beta}^s, \widehat{\beta}^m\right)$$

reduces the sampling variance of $\widehat{\beta}^s - \widehat{\beta}^m$. In fact, this covariance term is positive, and it is generally sizable compared to $Var\left(\widehat{\beta}^s\right)$ and $Var\left(\widehat{\beta}^m\right)$

since the regression residuals e_i^s and e_i^m are highly correlated. Because $2Cov\left(\widehat{\beta}^s, \widehat{\beta}^m\right)$ gets subtracted, looking at the standard errors of $\widehat{\beta}^s$ and $\widehat{\beta}^m$ alone can potentially mislead the researcher into concluding that the two coefficients are not significantly different from each other when in fact they are.

The coefficient comparison test itself can be formulated as a t-test as well, since we are interested in the movement in a single parameter. Define

$$t_{(\beta^s-\beta^m)} \equiv \frac{\widehat{\beta}^s - \widehat{\beta}^m}{\widehat{se}(\widehat{\beta}^s - \widehat{\beta}^m)}$$

where $\widehat{se}(\widehat{\beta}^s - \widehat{\beta}^m)$ is a consistent standard error estimator. Since

$$\beta^s - \beta^m = \delta\gamma^m = \delta\gamma\left(1-\theta\right)$$

we have

$$\begin{aligned} \frac{1}{\sqrt{n}} t_{(\beta^s-\beta^m)} &\xrightarrow{p} \frac{\delta\gamma\left(1-\theta\right)}{\sqrt{\left(1-\theta\right)\left(\gamma^2\frac{\sigma_u^2}{\sigma_s^2} + \theta\delta^2\gamma^2 + \delta^2\frac{\sigma_e^2}{\sigma_u^2}\right)}} \\ &= \sqrt{1-\theta}\frac{\delta\gamma}{\sqrt{\gamma^2\frac{\sigma_u^2}{\sigma_s^2} + \theta\delta^2\gamma^2 + \delta^2\frac{\sigma_e^2}{\sigma_u^2}}}. \end{aligned} \tag{14}$$

Under the alternative hypothesis ($\delta \neq 0$) and the maintained assumption $\gamma \neq 0$, the limits for the other two test statistics can be written as

$$\begin{aligned} \frac{1}{\sqrt{n}} t_{\delta^m} &\xrightarrow{p} \sqrt{1-\theta}\frac{\delta\gamma}{\sqrt{\gamma^2\frac{\sigma_u^2}{\sigma_s^2}}} \\ \frac{1}{\sqrt{n}} t_{\gamma^m} &\xrightarrow{p} \sqrt{1-\theta}\frac{\delta\gamma}{\sqrt{\theta\delta^2\gamma^2 + \delta^2\frac{\sigma_e^2}{\sigma_u^2}}}. \end{aligned}$$

Hence, using (14), it is apparent that under these conditions the three tests are asymptotically related in the following way:

$$\text{plim}\left(\frac{1}{\frac{1}{\sqrt{n}}t_{(\beta^s-\beta^m)}}\right)^2 = \text{plim}\left(\frac{1}{\frac{1}{\sqrt{n}}t_{\delta^m}}\right)^2 + \text{plim}\left(\frac{1}{\frac{1}{\sqrt{n}}t_{\gamma^m}}\right)^2 \tag{15}$$

These results highlight a number of things. First of all, under the maintained hypothesis $\gamma \neq 0$, the balancing test alone is more powerful. This is not surprising at all, since the balancing test only involves estimating the parameter δ while the coefficient comparison test involves estimating both δ and γ. Imposing $\gamma \neq 0$ in the coefficient comparison test is akin to $t_{\gamma^m} \to \infty$, and this would restore the equivalence of the balancing and coefficient comparison tests. Note that the power advantage from imposing $\gamma \neq 0$ exists regardless of the presence of measurement error.

The second insight is that measurement error affects the coefficient comparison test in two ways. The test statistic is subject to both the attenuation factor $\sqrt{1-\theta}$ and the term $\theta\delta^2\gamma^2$ in the variance, which is inherited from the t-statistic for $\widehat{\gamma}^m$. Importantly, however, all these terms interact in the coefficient comparison test. In our numerical exercises below, it turns out that the way in which measurement error attenuates γ^m compared to γ is a major source of the power disadvantage of the coefficient comparison test. Our simulations demonstrate that the differences in power between the coefficient comparison and balancing tests can be substantial when there is considerable measurement error in x_i^m. Before we turn to these results, we briefly note how the coefficient comparison test can be implemented in practice.

4.1 Implementing the Coefficient Comparison Test

The balancing test is a straightforward t-test, which regression software calculates routinely. We noted that the coefficient comparison test is a generalized Hausman test. Regression software will typically calculate this as well if it allows for seemingly unrelated regression estimation (SURE). SURE takes $Cov\left(e_i^s, e_i^m\right)$ into account and therefore facilitates the test. In Stata, this is implemented via the `suest` command. Generically, the test would take the following form:

```
reg y s
```

```
est store reg1
reg y s x
est store reg2
suest reg1 reg2
test[reg1_mean]s=[reg2_mean]s
```

The test easily accommodates covariates or can be carried out with the variables `y`, `s`, and `x` being residuals from a previous regression (hence facilitating large numbers of fixed effects though degrees of freedom may have to be adjusted in this case).

As far as we can tell, the Stata `suest` or `3reg` commands don't work for the type of IV regressions we might be interested in here. An alternative, which also works for IV, is to take the regressions (1) and (2) and stack them:

$$\begin{bmatrix} y_i \\ y_i \end{bmatrix} = \begin{bmatrix} 1 & 0 \\ 0 & 1 \end{bmatrix} \begin{bmatrix} \alpha^s \\ \alpha \end{bmatrix} + \begin{bmatrix} s_i & 0 \\ 0 & s_i \end{bmatrix} \begin{bmatrix} \beta^s \\ \beta \end{bmatrix} + \begin{bmatrix} 0 & 0 \\ 0 & x_i \end{bmatrix} \begin{bmatrix} 0 \\ \gamma \end{bmatrix} + \begin{bmatrix} e_i^s \\ e_i \end{bmatrix}.$$

Testing $\beta^s - \beta = 0$ is akin to a Chow test across the two specifications (1) and (2). Of course, the data here are not two subsamples but rather duplicates of the original data set. To take account of this and allow for the correlation in the residuals across duplicates, it is crucial to cluster standard errors on the observation identifier i.

5 Power Comparisons

5.1 Asymptotic and Monte Carlo Results with Classical Measurement Error

The ability of a test to reject when the null hypothesis is false is described by the power function of the test. The power functions here are functions of d, the values the parameter δ might take on under the alternative hypothesis. Because the joint distribution between the coefficient and standard error

estimators is difficult to characterize, especially in the case of the coefficient comparison test, we abstract away from the sampling variation in estimating the standard errors in the theoretical derivations of this section. The resulting t-statistic for the null hypothesis that the coefficient δ is zero in the balancing test is

$$t_{\delta^m} = \frac{\widehat{\delta}^m}{se(\widehat{\delta}^m)} = \frac{\sqrt{n} \cdot \widehat{\delta}^m}{\frac{\sqrt{\sigma_u^2+\sigma_m^2}}{\sigma_s}} = \frac{\sqrt{n} \cdot \widehat{\delta}^m}{\frac{\sigma_u}{\sigma_s\sqrt{1-\theta}}}.$$

Similarly, we use

$$t_{(\beta^s-\beta^m)}(d;\gamma) = \frac{\widehat{\beta}^s - \widehat{\beta}^m}{se(\widehat{\beta}^s - \widehat{\beta}^m)} = \frac{\sqrt{n}(\widehat{\beta}^s - \widehat{\beta}^m)}{\sqrt{V_\beta\left(d;\gamma\right)}}$$

where

$$V_\beta\left(d;\gamma\right) = (1-\theta)\left(\frac{\gamma^2\sigma_u^2}{\sigma_s^2} + \theta d^2\gamma^2 + \frac{d^2\sigma_e^2}{\sigma_u^2}\right)$$

in the derivation of the power function for the coefficient comparison test.

As shown in Appendix A, the power function for a 5% critical value of the balancing test is

$$\begin{aligned} Power_{t_{\delta^m}}\left(d\right) = 1 &- \Phi\left(1.96 - d\frac{\sqrt{n}\sigma_s\sqrt{1-\theta}}{\sigma_u}\right) \\ &+ \Phi\left(-1.96 - d\frac{\sqrt{n}\sigma_s\sqrt{1-\theta}}{\sigma_u}\right), \end{aligned} \tag{16}$$

where $\Phi\left(\bullet\right)$ is the standard normal cumulative distribution function. The power function for the coefficient comparison test is

$$\begin{aligned} Power_{t_{(\beta^s-\beta^m)}}\left(d;\gamma\right) = 1 &- \Phi\left(1.96 - d\frac{\sqrt{n}\gamma\left(1-\theta\right)}{\sqrt{V_\beta\left(d;\gamma\right)}}\right) \\ &+ \Phi\left(-1.96 - d\frac{\sqrt{n}\gamma\left(1-\theta\right)}{\sqrt{V_\beta\left(d;\gamma\right)}}\right). \end{aligned} \tag{17}$$

Note that the power function for the balancing test does not involve the

parameter γ. Using our results above, for $0 < \gamma < \infty$ it can be written as

$$Power_{t_{\delta^m}}(d) = 1 - \Phi\left(1.96 - d\frac{\sqrt{n}\gamma(1-\theta)}{\sqrt{V_\delta(d;\gamma)}}\right) + \Phi\left(-1.96 - d\frac{\sqrt{n}\gamma(1-\theta)}{\sqrt{V_\delta(d;\gamma)}}\right). \quad (18)$$

where

$$V_\delta(d;\gamma) = (1-\theta)\frac{\gamma^2\sigma_u^2}{\sigma_s^2}.$$

It is hence apparent that $V_\beta(d;\gamma) > V_\delta(d;\gamma)$, i.e. the coefficient comparison test has a larger variance. As a result, when $d \neq 0$[7]

$$Power_{t_{\delta^m}}(d) > Power_{t_{(\beta^s-\beta^m)}}(d;\gamma). \quad (19)$$

In practice, this result may or may not be important. In addition, when the standard error is estimated, the powers of the two tests may differ from the theoretical results above. Therefore, we carry out a number of Monte Carlo simulations to assess the performance of the two tests. Table 1 displays the parameter values we use as well as the implied values of the population R^2 of regression (9). The values were chosen so that for intermediate amounts of measurement error in x_i^m the R^2s are reflective of regressions fairly typical of those in applied microeconomics, for example, a wage regression. Note that the amounts of measurement error we consider are comparatively large. In our empirical application we use mother's education and the presence of a library card in the household as measures of family background. We suspect that these variables pick up at most a minor part of the true variation of

[7]To see this, define $f(t) = 1 - \Phi(1.96 - t) + \Phi(-1.96 - t)$ and denote the probability density function of a standard normal distribution by ϕ. The f notation allows us to rewrite the expressions for the power functions $Power_{t_{\delta^m}}(d)$ and $Power_{t_{(\beta^s-\beta^m)}}(d;\gamma)$ in equations (17) and (18) simply as $f(t_1)$ and $f(t_2)$. When $d \neq 0$, $V_\beta(d;\gamma) > V_\delta(d;\gamma)$ implies that $|t_1| > |t_2| > 0$. Since $f'(t) = \phi(1.96 - t) - \phi(1.96 + t)$ is positive for all $t > 0$ and negative for all $t < 0$, $f(t_1) > f(t_2)$ given $|t_1| > |t_2| > 0$, and equation (19) follows.

family background, even in the presence of other covariates, so that values of $\theta = 0.7$ or $\theta = 0.85$ for the measurement error are not unreasonable.

In Figure 1, we plot the power functions for both tests for three different magnitudes of the measurement error.[8] The black/thin lines show the power functions with no measurement error. The power functions can be seen to increase quickly with d, and both tests reject with virtual certainty once d exceeds values of 1. The balancing test is slightly more powerful but this difference is small, and only visible in the figure for a small range of d.

The blue/medium thick lines correspond to $\theta = 0.7$, i.e. 70% of the variance of x_i^m is measurement error after partialling out s_i. Measurement error of that magnitude visibly affects the power of both tests. The balancing test still rejects with certainty for $d > 1.5$, while the coefficient comparison test does not reject with certainty for the parameter values considered in the figure. This discrepancy becomes even more pronounced when we set $\theta = 0.85$ (red/thick lines). The power of the coefficient comparison test does not rise above 0.65 in this case, while the balancing test still rejects with probability 1 when d is around 2.

The results in Figure 1 highlight that there are parameter combinations where the balancing test has substantially more power than the coefficient comparison test. In other regions of the parameter space, the two tests have more similar power, for example, when $d < 0.5$.

Before going on to simulations of more complicated cases, we contrast the theoretical power functions in Figure 1, based on asymptotic approximations, to simulated rejection rates of the same tests in Monte Carlo samples. Figure 2 shows the power functions for the two tests without measurement error ($\theta =$

[8]The power function for the balancing test in equation (16) is written using the normal distribution, but we actually calculate it using the t-distribution with $n - 2$ degrees of freedom. This is consistent with how Stata version 14 performs the balancing test following the command `reg x s` or `reg x s, r`, even though this distribution choice makes little difference given our sample size ($n = 100$).

0) and with ($\theta = 0.85$), as well as their simulated counterparts.[9] Without measurement error, the theoretical power functions are closely aligned with the empirical rejection rates (black lines). Adding measurement error, this is also true for the balancing test (solid red and blue/thick lines) but not for the coefficient comparison test (broken red and blue/thick lines).

Figure 2 reveals that the empirical rejection rates of the coefficient comparison test in the presence of measurement error deviate substantially from the power function calculation based on the asymptotic approximation. This discrepancy is almost completely explained by the fact that we use the asymptotic values of standard errors in the calculations but estimated standard errors in the simulations. The empirical test is severely distorted under the null; it barely rejects more than 1% of the time for a nominal size of 5%. While this problem leads to too few rejections under the null, it is important to note that the same issue arises for positive values of d until about $d < 1.5$. For larger values of d the relationship reverses. In other words, for moderate values of d the coefficient comparison test statistic is biased downwards under the alternative, and the test has too little power. This highlights another advantage of the balancing test—a standard t-test where no such problem arises. We note that this is a small sample problem, which goes away when we increase the sample size (in unreported simulations). We suspect that this problem is related to the way in which the coefficient comparison test effectively combines the simple t_{δ^m} and t_{γ^m} test statistics in a non-linear fashion, as can be seen in equation (15), and the fact that t_{γ^m} sometimes is close to 0 in small samples despite the fact that we fix γ substantially above 0.

[9] We did 25,000 replications in these simulations, and each repeated sample contains 100 observations.

5.2 Monte Carlo Results beyond the Benchmark Model

The homoskedastic case with classical measurement error might be highly stylized and not correspond well to the situations typically encountered in empirical practice. We therefore explore some other scenarios using simulations in this Section. Figure 3 shows the original theoretical power functions for the case with no measurement error from Figure 1. It adds empirical rejection rates from simulations with heteroskedastic errors u_i and e_i of the form

$$\begin{aligned} \sigma_{u,i}^2 &= \left(\frac{e^{|s_i|}}{1+e^{|s_i|}}\right)^2 \sigma_{0u}^2 \\ \sigma_{e,i}^2 &= \left(\frac{e^{|s_i|}}{1+e^{|s_i|}}\right)^2 \sigma_{0e}^2. \end{aligned}$$

We set the baseline variances σ_{0u}^2 and σ_{0e}^2 so that $\overline{\sigma}_u^2 = 3$ and $\overline{\sigma}_e^2 = 30$ match the variances in Figure 1. The test statistics used in the simulations employ robust standard errors. We plot the rejection rates for data with no measurement error and for the more severe measurement error scenario given by $\theta = 0.85$. As can be seen in Figure 3, both the balancing and the coefficient comparison tests lose some power when the residuals are heteroskedastic compared to the homoskedastic baseline (black/thin lines). Otherwise, the main findings look very similar to those in Figure 1. Heteroskedasticity does not seem to alter the basic conclusions appreciatively.

Next, we explore mean reverting measurement error (Bound, Brown, Duncan, and Rodgers, 1994). We generate measurement error as

$$m_i = \kappa x_i + \mu_i$$

where κ is a parameter and $Cov(x_i, \mu_i) = 0$, so that κx_i captures the error related to x_i and μ_i the unrelated part. When $-1 < \kappa < 0$, the error is mean reverting, i.e. the κx_i-part of the error reduces the variance in x_i^m compared

to x_i. Notice that x_i^m can now be written as

$$x_i^m = (1+\kappa)\,\delta_0 + (1+\kappa)\,\delta s_i + (1+\kappa)\,u_i + \mu_i,$$

so this parameterization directly affects the coefficient in the balancing regression, which will be smaller than δ for a negative κ.

The case of mean reverting measurement error captures a variety of ideas, including the one that we may observe only part of a particular confounder made up of multiple components. Imagine we would like to include in our regression a variable $x_i = w_{1i} + w_{2i}$, where w_{1i} and w_{2i} are two orthogonal variables. We observe $x_i^m = w_{1i}$. For example, x_i may be family background, w_{1i} is mother's education and other parts of family background correlated with it, and w_{2i} are all relevant parts of family background which are uncorrelated with mother's education. As long as selection bias due to w_{1i} and w_{2i} is the same, this amounts to the mean reverting measurement error formulation above. Note that $\lambda = Var\left(x_i\right)/Var\left(x_i^m\right) > 1$ in this case, so the mismeasured x_i^m has a lower variance than the true x_i. This scenario is also isomorphic to the model studied by Oster (forthcoming). See Appendix B for details.

For the simulations we set $\kappa = -0.5$, so the error is mean reverting. We also fix σ_μ^2 in the simulations. However, it is important to note that the nature of the measurement error will change as we change the value of d under the alternative hypotheses. x_i depends on δ and the correlated part of the measurement error depends in turn on x_i. We show results for two cases with $\sigma_\mu^2 = 0.75$ and $\sigma_\mu^2 = 2.25$. Under the null, these two parameter values correspond to $\lambda = 2$ and $\lambda = 1$, respectively. The case $\lambda = 2$ corresponds to the Oster (forthcoming) model just described with $Var\left(w_{1i}\right) = Var\left(w_{2i}\right)$. These models exhibit relatively large amounts of mean reversion. Figure 4 demonstrates that the balancing test again dominates. The gap is small for the $\sigma_\mu^2 = 0.75$ case but grows with σ_μ^2, the classical portion of the measurement error. This finding is not surprising as mean-reverting measurement

error does less damage in terms of biasing the estimate of γ.

A particular case of mean reverting measurement error is the one where x_i is a dummy variable, so we provide some simulation results for this case. In this case, the balancing equation is a binary choice model, and hence inherently non-linear. While we assume that the researcher continues to estimate (3) as a linear probability model, we generate x_i as follows:

$$\Pr\left(x_i = 1\right) = \Phi\left(\delta s_i\right), \tag{20}$$

where $\Phi\left(\bullet\right)$ is the normal distribution function as before. Measurement error takes the form of misclassification, and we assume the misclassification rate to be symmetric:

$$\Pr\left(x_i^m = 1 | x_i = 0\right) = \Pr\left(x_i^m = 0 | x_i = 1\right) = \tau.$$

Compared to the baseline parameters in Table 1, we set $\sigma_s^2 = 0.25$, and $\tau = 0.1$ in our simulations. The model remains the same in all other respects. We use robust standard errors in estimating (9) and (11).

Various issues arise from the nonlinear nature of (20). One is the fact that plim $\left(\hat{\delta}\right)$ from estimating (11) is not going to equal the δ we generated in equation (20). The relationship between plim $\left(\hat{\delta}\right)$ and δ is concave. In Figure 5, we plot rejection rates against δ, although the quantity plim $\left(\hat{\delta}\right)$ is probably more comparable to the values of d we have used in the linear models in the previous simulations. We note that results look qualitatively very similar when we plot rejection rates against the empirical averages of $\hat{\delta}$ from our simulations.

Another issue is that measurement error in x_i will now lead to a biased estimate of δ in estimating (11). This is true even if we were to use a probit and estimated a model like (20). The bias takes the form of attenuation, just as in the case of a binary regressor with measurement error (see Hausman, Abrevaya, and Scott-Morton, 1998). Hence, measurement error will now

also reduce the power of the balancing test. Of course, we know from the relationship (15) between the test statistics that the coefficient comparison test will also suffer from the same power loss.

The blue/thin lines in Figure 5 reveal a sizable power advantage for the balancing test even without any misclassification. This result is in stark contrast to the linear models we have analyzed, where a large power loss for the coefficient comparison test only resulted once we introduced measurement error. In fact, it is possible to think of the binary nature of x_i itself as a form of mismeasurement. Equation (20) defines $\Pr(x_i = 1)$ as a latent index, but the outcome regression (2) uses a coarse version of this variable in the form of the binary x_i.

In our parameterization, the coefficient comparison test never reaches a rejection rate of 1, and the power function levels off at a far lower level. As d increases, the power of the balancing test goes to 1. In the linear model, the rejection rate of t_γ is independent of d. Because of the nonlinear nature of (20) this is no longer true here, and the average value of t_γ across repeated samples actually falls for higher values of d. Drawing on (15), the power of the coefficient comparison test will equal the power of t_γ when $t_\delta \to \infty$. This is not a specific feature of the binary case but is generally true for the relationship between the three test statistics. However, in the binary case this implies that the power of the coefficient comparison test may decline with d.[10]

Adding measurement error to the binary regressor x_i makes things worse as is visible from the red/thick lines in Figure 5. The power loss of the

[10]The reason for the decline of t_γ with d in our parameterization is as follows: the standard error of $\hat{\gamma}$ depends on the residual variance of the long regression, which is independent of d, and on the variance of the residual from regressing x_i on s_i (because s_i is partialled out in the long regression). When $d = 0$, this latter residual is just equal to x_i itself, which is binary. But s_i is continuous, so as d increases, partialling out s_i transforms the binary x_i into a continuous variable, which has less variance than in the $d = 0$ case. As the effective variance in this regressor falls, the standard error of $\hat{\gamma}$ goes up and t_γ goes down.

balancing test is comparatively minor for the relatively low misclassification rate of $\tau = 0.1$ we are using. Much of the loss for the balancing test results from the binary nature of the x_i variable in the first place. The coefficient comparison test is affected by misclassification error to a much higher degree because t_γ is affected, the Hausman, Abrevaya, and Scott-Morton (1998) result notwithstanding.

5.3 Simulations with Actual Data

Starting with a simple linear model with homoskedastic errors, we have explored simulations of a few leading scenarios which we believe are of empirical relevance. Throughout these cases we have found an important power advantage for the balancing test in the presence of measurement error in the candidate control variable. There are of course many other possibilities and settings, and we have presented just a few parameterizations. But simulated data rarely capture the complexities of many of the variables we encounter in real data. Before turning to results from an empirical example, we briefly present a simulation not based on draws of random numbers but instead based on drawing observations from an actual data set.

For this exercise, we pooled data from the 2010 - 2014 American Community Surveys (ACS). Our data set consists of white and African American individuals aged 21 to 64 with non-missing annual earnings. This data set has 5,644,865 observations. Our outcome equation is a wage regression of the form

$$\ln(earnings_i) = \alpha + \beta black_i + \gamma x_i + other\ regressors + e_i.$$

The parameter of interest is the coefficient on a dummy for whether the respondent is African American and the added candidate control x_i is years of schooling. We chose years of schooling as the added regressor because the distribution of schooling is discrete, its support is wide, but a lot of

mass is concentrated at 12 and 16 years. It therefore does not resemble any particularly "nice" looking distribution. We also control for sex, age, age squared, a dummy for living in a metro area, and Census region and year dummies in all regressions.

We treat the ACS data as our effective population. For our simulations we draw samples of 1,000 observations with replacement from this universe. Introducing measurement error into the bounded schooling variable is tricky, so we use a fairly simple form of mismeasurement. We start with the original data and then replace a successively larger percentage of the schooling observations with random draws from the empirical distribution of schooling in the data. This means

$$x_i^m = \begin{cases} x_i & \text{with probability } 1-p \\ \check{x} & \text{with probability } p \end{cases}$$

where $\check{x}$ is the schooling level of a randomly sampled observation from the ACS data.

African Americans have lower levels of schooling than whites, so δ and γ are both nonzero in this exercise. $\beta = -0.236$ and $\beta^s = -0.333$ in the full ACS data. Figure 6 shows the rejection rates across 25,000 random draws from the ACS for the balancing and coefficient comparison tests. With the true schooling variable, both tests reject about 80 percent of the time in our samples. As p increases from 0 to 1, rejection rates fall but they decrease more precipitously for the coefficient comparison test. In fact, when $p = 1$, the balancing test rejects 5 percent of the time while rejection rates for the coefficient comparison test actually go to zero. This reflects the small sample bias in the coefficient comparison test again, which we have discussed above in the context of Figure 2. The power advantage of the balancing test is not as large in this case as in the simulations above, but it is noticeable.

5.4 Extension: Multiple Controls

So far we have concentrated on the case of a single added regressor x_i. Often in empirical practice we may want to add a set of additional covariates at once. It is straightforward to extend our framework to that setting, at least in principle. In this section, we describe this multivariate extension, and provide some simulation results. These results turn out to be more speculative than those in the rest of our paper.

Suppose there are k added regressors, i.e. $\mathbf{x}_i$ is a $k \times 1$ vector, and

$$\begin{aligned} y_i &= \alpha + \beta s_i + \mathbf{x}_i' \boldsymbol{\gamma} + e_i \\ \mathbf{x}_i &= \boldsymbol{\delta}_0 + \boldsymbol{\delta} s_i + \mathbf{u}_i \\ \beta^s - \beta &= \boldsymbol{\gamma}' \boldsymbol{\delta} \end{aligned} \tag{21}$$

where $\boldsymbol{\gamma}$, $\boldsymbol{\delta}_0$, $\boldsymbol{\delta}$ and $\mathbf{u}_i$ are $k \times 1$ vector analogs of their scalar counterparts in Section 2. Lee and Lemieux (2010) suggest a balancing test for multiple covariates in the context of evaluating regression discontinuity designs. Let $\mathbf{x}_{(j)}$ denote the $n \times 1$ vector of all the observations on the j-th x-variable. We can stack all the x-variables to obtain the regression

$$\begin{bmatrix} \mathbf{x}_{(1)} \\ \mathbf{x}_{(2)} \\ ... \\ \mathbf{x}_{(k)} \end{bmatrix} = \begin{bmatrix} \boldsymbol{\iota}\delta_{01} \\ \boldsymbol{\iota}\delta_{02} \\ ... \\ \boldsymbol{\iota}\delta_{0k} \end{bmatrix} + \begin{bmatrix} \mathbf{s} & \mathbf{0} & \mathbf{0} & \mathbf{0} \\ \mathbf{0} & \mathbf{s} & \mathbf{0} & \mathbf{0} \\ \mathbf{0} & \mathbf{0} & ... & \mathbf{0} \\ \mathbf{0} & \mathbf{0} & \mathbf{0} & \mathbf{s} \end{bmatrix} \begin{bmatrix} \delta_1 \\ \delta_2 \\ ... \\ \delta_k \end{bmatrix} + \begin{bmatrix} \mathbf{u}_{(1)} \\ \mathbf{u}_{(2)} \\ ... \\ \mathbf{u}_{(k)} \end{bmatrix},$$

where $\boldsymbol{\iota}$ is an $n \times 1$ vector of ones, $\mathbf{s} = [s_1, s_2, ..., s_n]'$, and $\mathbf{u}_{(j)}$ the vector or residuals corresponding to covariate $\mathbf{x}_{(j)}$. We can then perform an F-test for the joint significance of the $\boldsymbol{\delta}$ coefficients. This is similar to the way we implemented the coefficient comparison test above in section 4.1. An equivalent alternative is to estimate the k balancing equations jointly by SURE.

How does the balancing test perform with multiple covariates? Figure 7 shows simulations using a similar design as in Table 1 for all k balancing

equations. However, with multiple covariates there are different ways of specifying the alternative hypotheses now. The null may fail for one, various, or all of the k covariates now. We show rejection rates under two polar versions of the alternative hypothesis: first for the case where all covariates are unbalanced, i.e. $\delta_1 = \delta_2 = \ldots = \delta_k = d$ and then for the case where only the first covariate is unbalanced while the others remain balanced, i.e. $\delta_1 = d$, $\delta_2 = \ldots = \delta_k = 0$. We generate homoskedastic errors and impose homoskedasticity when performing the joint test of the δ_j's. There are four panels in Figure 7: the top row has 4 added covariates, and the bottom row 8; the left hand column shows the case where all covariates are unbalanced while the right hand column displays the case where only the first covariate is unbalanced.

Figure 7 highlights a number of results. When all covariates are unbalanced, the Hausman test turns out to be an efficient test in combining the k separate hypotheses into one single test-statistic, generated from the estimates of only two parameters, the long and short β's. The balancing test, on the other hand has to rely on the estimation of k equations, and combine the results from these.[11] Without measurement error, the rejection rates for the coefficient comparison test (blue/thin broken lines) therefore lie above the ones for the balancing test (blue/thin solid lines), as can be seen in the left-hand panels.

When only one covariate is unbalanced, as in the right hand panels, both tests are obviously less powerful. However, the coefficient comparison test now loses power much more quickly. This is particularly pronounced in the case with measurement error in the covariates (red/thick lines). We suspect that the empirically relevant case is most likely to lie in between these extremes. Researchers may be faced with a set of potential controls

[11] The analyses in Hausman (1978), Hausman and Taylor (1980), Holly (1982), and MacKinnon (1992) section II.9, which compare the power of the coefficient comparison test to the F-test for $\boldsymbol{\gamma} = \mathbf{0}$, highlight a similar result.

to investigate, some of which may be unbalanced with the treatment while others are not. Figure 7 demonstrates that the balancing test suggested by Lee and Lemieux (2010) can play an important role in such an exercise, while the coefficient comparison test will also be valuable.

The simulations reveal a number of further issues. With measurement error, the small sample issue of the coefficient comparison test which we highlighted in Figure 2, arises again. On top of this, another bias is visible in these figures. The balancing test has a size distortion under the null hypothesis and rejects too often. This distortion tends to get worse when more covariates are added. Note that this problem arises for a conventional covariance matrix, and hence is distinct from the small sample bias in the robust covariance matrix discussed by Chesher and Jewitt (1987). However, we also found in unreported simulations that using a robust covariance matrix makes the problem worse, and this also interacts with the number of covariates. Applied researcher will be most interested in the testing strategies discussed here when k is large (so that a series of single variable balancing tests is unattractive), and will want to rely on a robust covariance matrix. The bias problem in the balancing test lessens its usefulness. Research on remedies for these problems is therefore particularly important.[12]

The upshot is that it is straightforward to extend the balancing test to multiple covariates, at least in principle. Yet, at this point implementation issues may hamper our ability to confidently carry out a balancing test for multiple covariates. Moreover, sometimes we are interested in the robustness of the original results when the number of added regressors is very large. An example would be a differences-in-differences analysis in a state-year panel, where the researcher is interested in checking whether the results are robust

[12]There is an active literature on how to deal with the small sample bias of the robust or clustered covariance estimator. Young (2016) is a recent contribution. However, we suspect that these remedies may not be sufficient for solving the size distortion of the stacked F-test when the sample size is small relative to the number of covariates.

to the inclusion of state specific trends. The balancing test does not seem to be the right framework to deal with this situation. The coefficient comparison may have an important role to play in tackling this problem.

6 Empirical Analysis

We illustrate the theoretical results in the context of estimating the returns to schooling using data from the National Longitudinal Survey of Young Men (NLS). This is a panel study of about 5,000 male respondents interviewed from 1966 to 1981. The data set has featured in many prominent analyses of the returns to education, including Griliches (1977) and Card (1995). We use the NLS extract posted by David Card and augment it with the variable on body height measured in the 1973 survey. We estimate regressions similar to equation (2). The variable y_i is the log hourly wage in 1976 and s_i is the number of years of schooling reported by the respondent in 1976. Our samples are restricted to observations without missing values in any of the variables used in a particular table or set of tables.

We start in Table 2 by presenting simple OLS regressions controlling for experience, race, and past and present residence. The estimated return to schooling is 0.075. This estimate may not reflect the causal effect of education on income because important confounders, such as ability or family background, are not controlled for.

In columns (2) to (5) we include variables which might proxy for the respondent's family background. In column (2) we include mother's education, in column (3) whether the household had a library card when the respondent was 14, and in column (4) we add body height measured in inches. Each of these variables is correlated with earnings, and the coefficient on education moves moderately when these controls are included. Mother's education captures an important component of a respondent's family background. The library card measure has been used by researchers to proxy for important

parental attitudes (e.g. Farber and Gibbons, 1996). Body height is a variable determined by parents' genes and by nutrition and disease environment during childhood. It is unlikely a particularly powerful control variable but it is predetermined and correlated with family background, self-esteem, and ability (e.g. Persico, Postlewaite, and Silverman, 2004; Case and Paxson, 2008). The return to education falls by 0.1 to 0.2 log points when these controls are added. In column (5) we enter all three variables simultaneously. The coefficients on the controls are somewhat attenuated, and the return to education falls slightly further to 0.071.

It might be tempting to conclude from the relatively small change in the estimated returns to schooling that this estimate should be given a causal interpretation. We provide a variety of evidence that this conclusion is unlikely to be a sound one. Below the estimates in columns (2) to (5), we display the p-values from the coefficient comparison test, comparing each of the estimated returns to education to the one from column (1). Although the coefficient movements are small, the tests all reject at the 5% level, and in columns (4) and (5) they reject at the 1% level. These results might not be expected from the size of the coefficient movements and the individual standard errors on the years of education coefficients alone, highlighting the importance for the formal coefficient comparison test.

The results in columns (6) to (8), where we regress maternal education, the library card, and body height on education, further magnify the concern. The education coefficient is positive and strongly significant in all three regressions, with t-values ranging from 4.4 to 13.1, and a joint balancing test rejects the hypothesis that all three controls are balanced with a p-value of virtually zero. The magnitudes of the coefficients are substantively important. It is difficult to think of these results as causal effects: the respondent's education should not affect predetermined proxies of family background. Instead, these estimates reflect selection bias. Individuals with more education have significantly better educated mothers, were more likely to grow up in

a household with a library card, and experienced more body growth when young. Measurement error leads to attenuation bias when these variables are used on the right-hand side which renders them fairly useless as controls. The measurement error matters less for the estimates in columns (6) to (8), and these are informative about the role of selection. Comparing the p-values at the bottom of the table to the corresponding ones for the coefficient comparison test in columns (2) to (4) demonstrates the superior power of the balancing test.

The following tables have the same general layout. In Table 3 we repeat the regressions including a direct measure for ability, the respondent's score on the Knowledge of the World of Work test (KWW), a variable used by Griliches (1977) as a proxy for ability. The sample size is reduced due to the exclusion of missing IQ values in the test score for consistency with a subsequent table. Estimated returns without the KWW score in this restricted sample (unreported) are very similar to those in Table 2. Adding the KWW score reduces the coefficient on education by almost 20%, from 0.075 to 0.061. Adding maternal education, the library card, and body height does very little to the estimated returns to education now. The coefficient comparison test indicates that none of the small changes in the returns to education are statistically significant. Controlling for the KWW scores has largely knocked out the library card effect but done little to the coefficients on maternal education and body height. The relatively small and insignificant coefficient movements in columns (2) to (5) suggest that the specification controlling for the KWW score might solve the ability bias problem.

Columns (6) to (8), however, show that the regressions with the controls on the left hand side still mostly result in significant education coefficients even when the KWW score is in the regression. This raises the possibility that the estimated returns in columns (1) to (5) might remain biased by selection. The estimated coefficients on education for the three controls are on the order of half their value from Table 2, and the body height measure is

now only significant at the 10% level. But the relationship between mother's and own education is still sizable, so that this measure continues to indicate the possibility of important selection. Balance in library card ownership is rejected despite the fact that a comparison of the coefficients in columns (1) and (3) indicates no role for this variable at all. A joint balancing test with all three controls strongly rejects the hypothesis that they are balanced. The results in this table illustrate the message of our paper in a powerful fashion.

While the KWW score might be a potent control, it is likely also measured with substantial error. Griliches (1977) proposes to instrument this measure with an IQ test score variable, which is also contained in the NLS data, to eliminate at least some of the consequences of this measurement error. In Table 4 we repeat the schooling regressions with IQ as instrument for the KWW score. The coefficient on the KWW score almost triples, in line with the idea that an individual test score is a very noisy measure of ability. The education coefficient now falls to only about half its previous value from 0.061 to 0.034. This might be due to positive omitted variable bias present in the previous regressions which is eliminated by IQ-instrumented KWW (although there may be other possible explanations for the change as well, like measurement error in schooling). Both the coefficient comparison tests and the balancing tests (individual and joint) indicate no evidence of selection any more. This is due to a combination of lower point estimates and larger standard errors. The contrast between Tables 3 and 4 highlights the usefulness of the balancing test: it warns about the Table 3 results, while the coefficient comparison test delivers insignificant differences in either case.

Finding an instrumental variable for education is an alternative to control strategies, such as using test scores. In Table 5 we follow Card's (1995) analysis and instrument education using distance to the nearest college, while dropping the KWW score. We use the same sample as in Table 2, which

differs from Card's sample.[13] Our IV estimates of the return to education are slightly higher than in Table 2 but a lot lower than in Card (1995) at around 8%. The IV returns estimates are noisy, never quite reaching a t-statistic of 2. Columns 1-5 of Table 5 show that the IV estimate on education, while bouncing around a bit, does not change significantly when maternal education, the library card, or body height is included. In particular, if these three controls are included at the same time in column (5), the point estimate is indistinguishable from the unconditional estimate in column (1) both substantively and statistically.

IV regressions with pre-determined variables on the left hand side can be thought of as a test for random assignment of the instruments. Unfortunately, in this case the selection regressions in columns (6)-(8) are also much less precise and as a result less informative. The coefficients in the regressions for mother's education and body height have the wrong sign but confidence intervals cover anything ranging from zero selection to large positive amounts. Only the library card measure is large, positive, and significant around the 6% level, possibly indicative of some remaining potential for selection even in the IV regressions. However, with a p-value of 0.29, the joint balancing test fails to reject the null hypothesis that all three controls are balanced. While the data do not speak clearly in this particular case this does not render the methodology per se any less useful.

7 Conclusion

Using predetermined characteristics as dependent variables offers a useful specification check for a variety of identification strategies popular in empir-

[13]Unlike Card, who uses two dummies for proximity to a two- and a four-year college, we use a single dummy variable for whether there is a four-year college in the county as instrument, and we instrument experience and experience squared by age and age squared. We restrict Card's sample to non-missing values in maternal education, the library card, and body height.

ical economics. We argue that this is the case even for variables which might be poorly measured and are of little value as control variables. Such variables should be available in many data sets, and we encourage researchers to perform such balancing tests more frequently. We show that this is generally a more powerful strategy than adding the same variables on the right hand side of the regression as controls and looking for movement in the coefficient of interest.

We have illustrated our theoretical results with an application to the returns to education. Taking our assessment from this exercise at face value, a reader might conclude that the results in Table 4, returns around 3.5%, can safely be regarded as causal estimates. Of course, this is not the conclusion reached in the literature, where much higher IV estimates like those in Table 5 are generally preferred (see e.g. Card, 2001 or Angrist and Pischke, 2015, chapter 6). This serves as a reminder that the discussion here is focused on sharpening one particular tool in the kit of applied economists. Successfully passing the balancing test should be a necessary condition for a successful research design but it is not sufficient.

The balancing test and other statistics we discuss here are useful for gauging selection bias due to observed confounders, even when they are potentially measured poorly. It does not address any other issues which may also haunt a successful empirical investigation of causal effects. One possible issue is measurement error in the variable of interest, which is also exacerbated as more potent controls are added. Griliches (1977) shows that a modest amount of measurement error in schooling may be responsible for the patterns of returns we have displayed in Tables 2 to 4. Another issue, also discussed by Griliches, is that controls like test scores might themselves be influenced by schooling, which would make them bad controls. For all these reasons, IV estimates of the returns may be preferable.

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Figure 1: Theoretical Rejection Rates

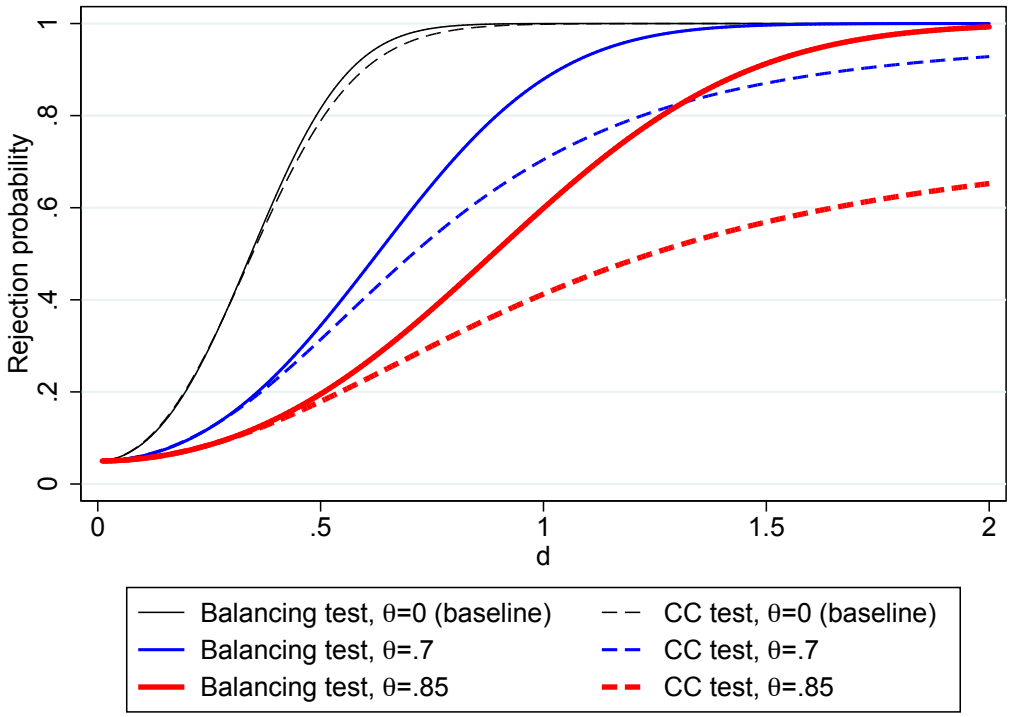
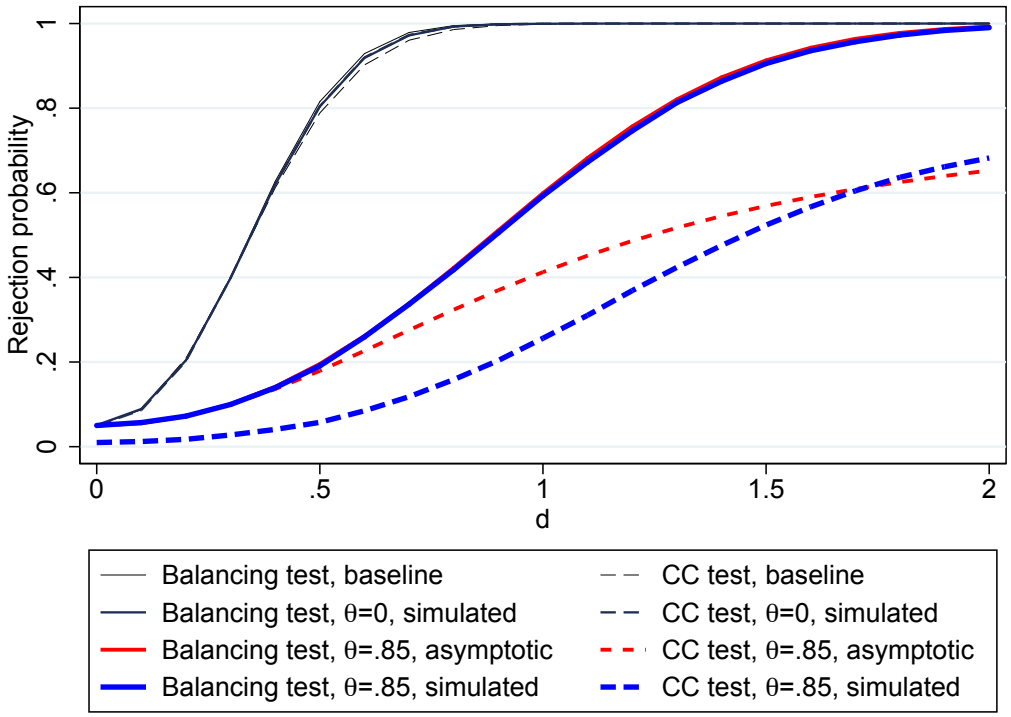
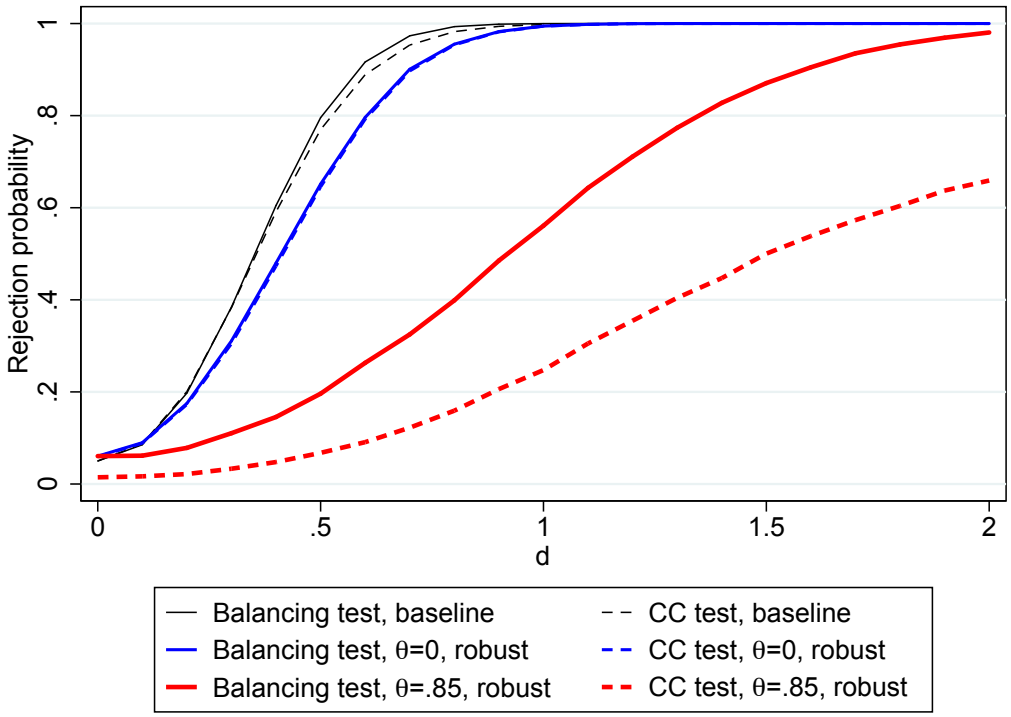


Figure 2: Theoretical and Simulated Rejection Rates



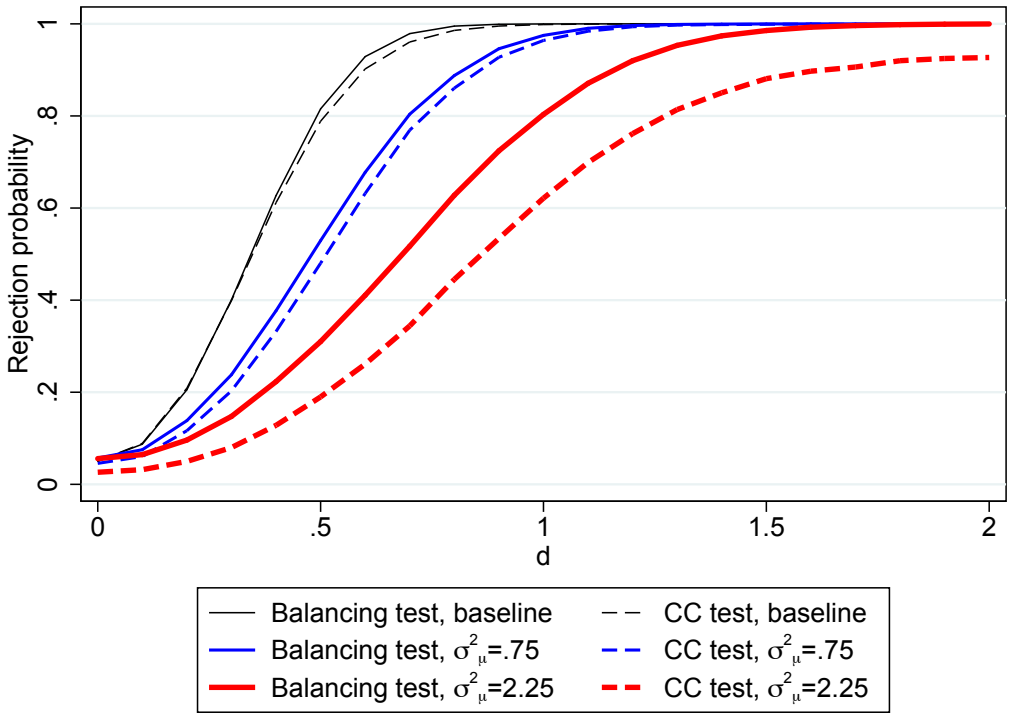
Note: Comparison of asymptotic rejection rates with rejection rates based on Monte Carlo simulations. Baseline refers to the theoretical rejection rates without measurement error.

Figure 3: Simulated Rejection Rates with Heteroskedasticity



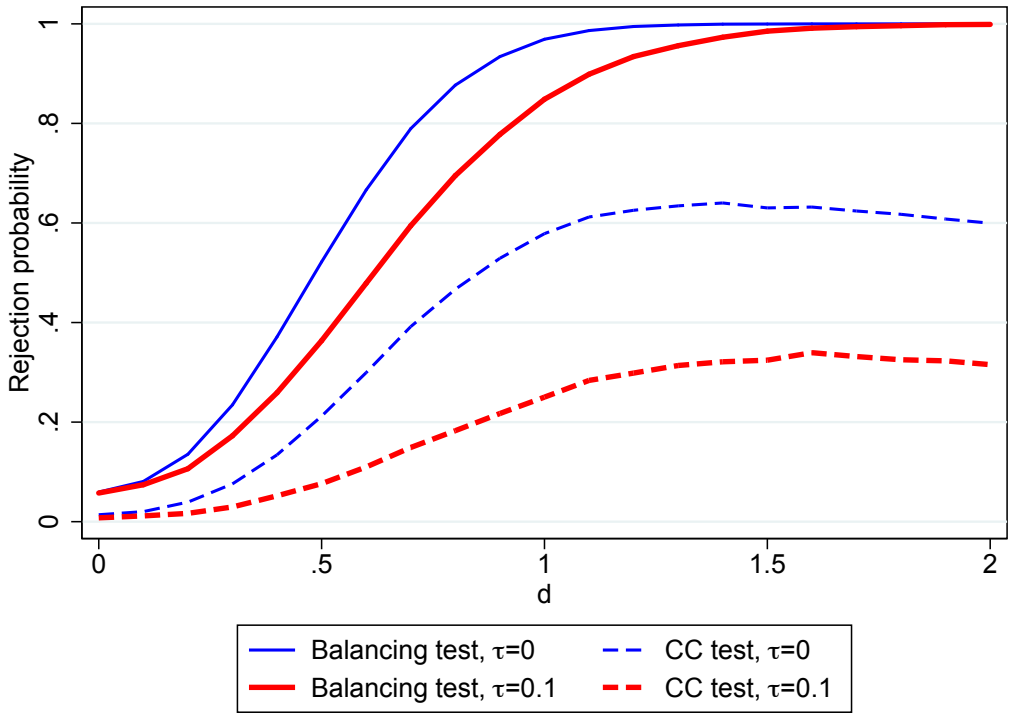
Note: Comparison of baseline rejection rates (from Figure 1) with simulated rejection rates based on heteroskedastic errors and robust standard errors.

Figure 4: Simulated Rejection Rates with Mean Reverting Measurement Error



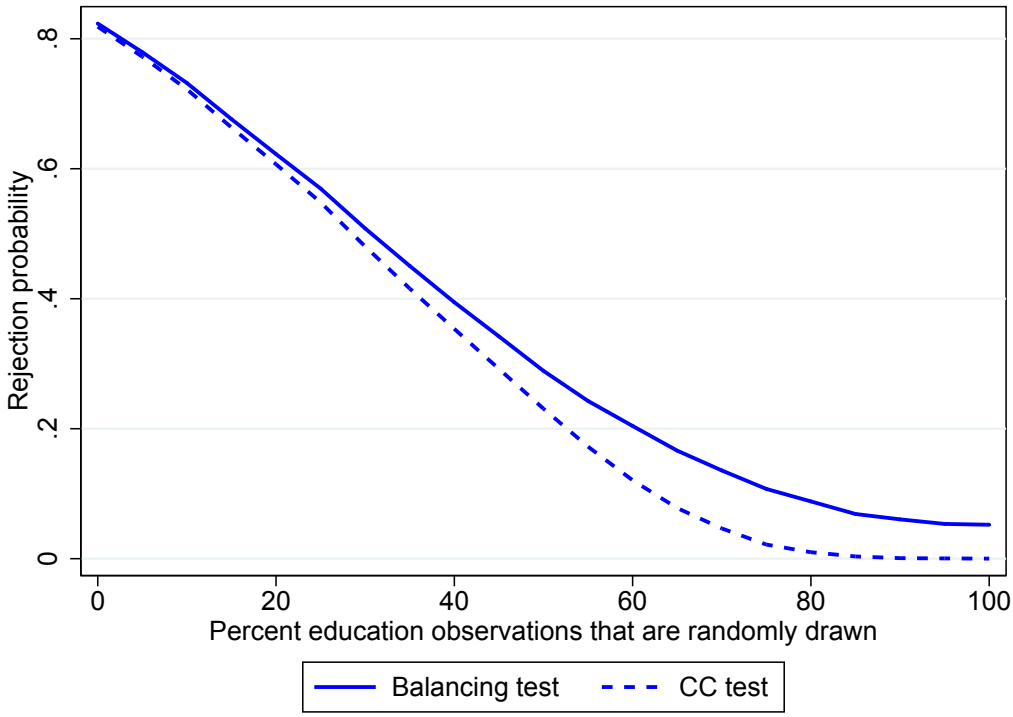
Note: Comparison of baseline rejection rates (from Figure 1) with simulated rejection rates based on mean reverting measurement error and robust standard errors.

Figure 5: Simulated Rejection Rates with Binary Control and Misclassification



Note: Rejection rates for a binary control variable that is misclassified (i.e. its binary value is flipped) with probability τ.

Figure 6: Rejection Rates in Actual Data from the ACS



Note: Rejection rates based on drawing random samples of size 1,000 from the American Community Surveys. Measurement error is generated by replacing different percentages of the schooling observations with random draws from the empirical distribution of schooling in the original data.

Figure 7: Simulated Rejection Rates with Multiple Controls

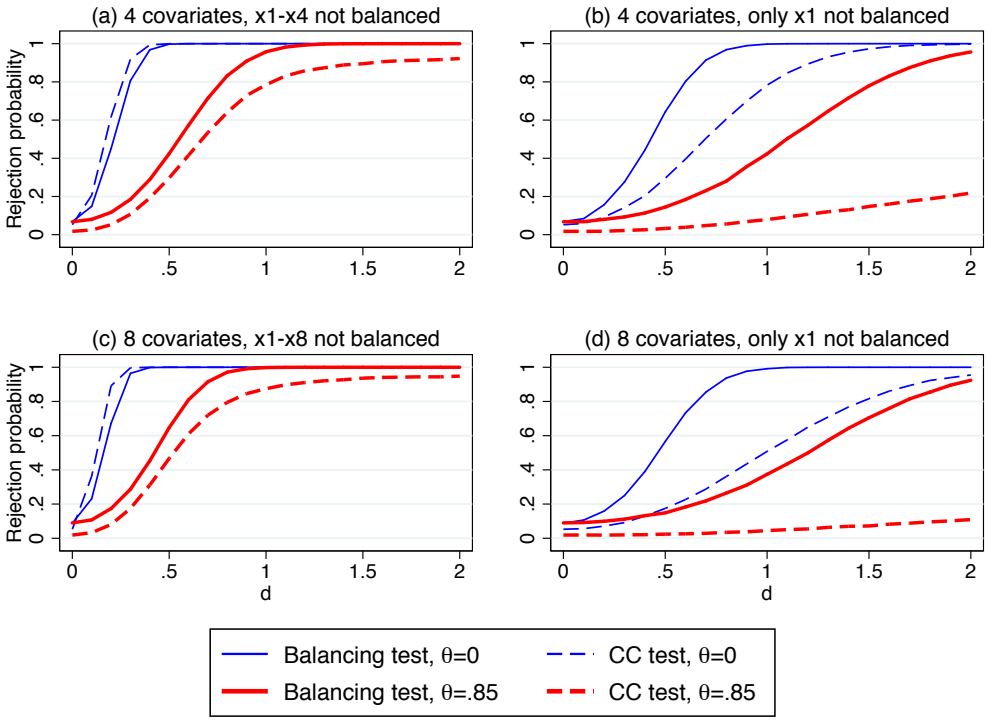


Table 1: Parameters for Power Calculations and Implied R^2s

$\sigma_s^2 = 1$		$\beta = 1$	
$\sigma_u^2 = 3$		$\gamma = 3$	
$\sigma_e^2 = 30$		$n = 100$	
		R^2	
d	$\theta = 0$	$\theta = 0.7$	$\theta = 0.85$
0	0.48	0.16	0.09
0.5	0.53	0.23	0.16
1.0	0.59	0.33	0.27
1.5	0.66	0.44	0.39
2.0	0.72	0.54	0.50

Note: The implied population R^2's do not depend on n, but the subsequent power calculations do.

Table 2: Baseline Regressions for Returns to Schooling and Specification Checks

	Dependent Variable							
	Log hourly earnings					Mother's years of education	Library card at age 14	Body height in inches
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years of education	0.0751	0.0728	0.0735	0.0740	0.0710	0.3946	0.0371	0.1204
	(0.0040)	(0.0042)	(0.0040)	(0.0040)	(0.0042)	(0.0300)	(0.0040)	(0.0273)
Mother's years of education		0.0059			0.0044			
		(0.0029)			(0.0030)			
Library card at age 14			0.0428		0.0361			
			(0.0183)		(0.0184)			
Body height in inches				0.0090	0.0084			
				(0.0027)	(0.0027)			
p-values								
Coefficient comparison test		0.044	0.022	0.009	0.002			
Balancing test: individual						0.000	0.000	0.000
Balancing test: joint							0.000	

Note: The number of observations is 2,500 in all regressions. Heteroskedasticity robust standard errors in parentheses. All regressions control for experience, experience-squared, indicators for black, for southern residence and residence in a standard metropolitan statistical area (SMSA) in 1976, indicators for region in 1966 and living in an SMSA in 1966.

Table 3: Regressions for Returns to Schooling and Specification Checks Controlling for the KWW Score

	Dependent Variable							
	Log hourly earnings					Mother's years of education	Library card at age 14	Body height in inches
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years of education	0.0609	0.0596	0.0608	0.0603	0.0591	0.2500	0.0133	0.0731
	(0.0059)	(0.0060)	(0.0059)	(0.0059)	(0.0060)	(0.0422)	(0.0059)	(0.0416)
KWW score	0.0070	0.0068	0.0069	0.0069	0.0067	0.0410	0.0076	0.0145
	(0.0015)	(0.0016)	(0.0016)	(0.0015)	(0.0016)	(0.0107)	(0.0016)	(0.0117)
Mother's years of education		0.0053			0.0048			
		(0.0037)			(0.0037)			
Library card at age 14			0.0097		0.0045			
			(0.0215)		(0.0216)			
Body height in inches				0.0078	0.0075			
				(0.0034)	(0.0034)			
p-values								
Coefficient comparison test		0.161	0.651	0.156	0.084			
Balancing test: individual						0.000	0.025	0.079
Balancing test: joint							0.000	

Note: The number of observations is 1,773 in all regressions, due to missing values in IQ. Heteroskedasticity robust standard errors in parentheses. All regressions control for experience, experience-squared, indicators for black, for southern residence and residence in an SMSA in 1976, indicators for region in 1966 and living in an SMSA in 1966.

Table 4: Regressions for Returns to Schooling and Specification Checks Instrumenting the KWW Score

	Dependent Variable							
	Log hourly earnings					Mother's years of education	Library card at age 14	Body height in inches
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years of education	0.0340	0.0339	0.0342	0.0343	0.0345	0.0234	0.0168	-0.0486
	(0.0139)	(0.0139)	(0.0138)	(0.0139)	(0.0138)	(0.0952)	(0.0134)	(0.0998)
KWW score instrumented by IQ	0.0199	0.0195	0.0200	0.0194	0.0191	0.1496	0.0060	0.0728
	(0.0062)	(0.0063)	(0.0063)	(0.0062)	(0.0064)	(0.0422)	(0.0060)	(0.0449)
Mother's years of education		0.0028			0.0026			
		(0.0039)			(0.0039)			
Library card at age 14			-0.0130		-0.0154			
			(0.0245)		(0.0243)			
Body height in inches				0.0070	0.0069			
				(0.0034)	(0.0034)			
p-values								
Coefficient comparison test		0.818	0.634	0.636	0.552			
Balancing test: individual						0.806	0.212	0.626
Balancing test: joint							0.593	

Note: The number of observations is 1,773 in all regressions, due to missing values in IQ. Heteroskedasticity robust standard errors in parentheses. All regressions control for experience, experience-squared, indicators for black, for southern residence and residence in an SMSA in 1976, indicators for region in 1966 and living in an SMSA in 1966.

Table 5: Regressions for Returns to Schooling and Specification Checks Instrumenting Schooling by Proximity to College

	Dependent Variable							
	Log hourly earnings					Mother's years of education	Library card at age 14	Body height in inches
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years of education instrumented by college proximity	0.0816	0.0818	0.0778	0.0845	0.0822	-0.0952	0.1015	-0.3658
	(0.0431)	(0.0417)	(0.0518)	(0.0418)	(0.0466)	(0.3594)	(0.0542)	(0.3681)
Mother's years of education		0.0030			0.0012			
		(0.0143)			(0.0140)			
Library card at age 14			0.0367		0.0237			
			(0.0886)		(0.0581)			
Body height in inches				0.0081	0.0079			
				(0.0044)	(0.0032)			
p-values								
Coefficient comparison test		0.873	0.686	0.380	0.908			
Balancing test: individual						0.791	0.061	0.321
Balancing test: joint							0.290	

Note: The number of observations is 2,500 in all regressions. Heteroskedasticity robust standard errors in parentheses. All regressions control for experience, experience-squared, indicators for black, for southern residence and residence in an SMSA in 1976, indicators for region in 1966 and living in an SMSA in 1966.

Appendix

A Power Functions

A.1 The Balancing Test

The desired balancing regression is

$$x_i = \delta_0 + \delta s_i + \ u_i,$$

but x_i is measured with error

$$x_i^m = x_i + m_i.$$

Effectively, we run the balancing regression

$$x_i^m = \delta_0^m + \delta^m s_i + \ u_i + m_i.$$

As mentioned in Section 5.1, in the theoretical derivation of the power functions we abstract away from the sampling variation in estimating the standard errors by treating σ_u, σ_m and σ_s as known constants. In this case, the asymptotic variance of $\widehat{\delta}^m$ can be directly calculated, and the resulting test statistic for the null hypothesis that the balancing coefficient δ is zero is

$$t_{\delta^m} = \frac{\widehat{\delta}^m}{se\left(\widehat{\delta}^m\right)} = \frac{\widehat{\delta}^m}{\frac{1}{\sqrt{n}}\frac{\sqrt{\sigma_u^2+\sigma_m^2}}{\sigma_s}}.$$

Define

$$\begin{aligned} \theta &= \frac{\sigma_m^2}{\sigma_u^2 + \sigma_m^2} \\ &\Rightarrow \ \sigma_u^2 + \sigma_m^2 = \frac{\sigma_u^2}{1-\theta} \end{aligned}$$

Hence

$$t_{\delta^m} = \widehat{\delta}^m \frac{\sqrt{n}\sigma_s\sqrt{1-\theta}}{\sigma_u}.$$

The rejection probability when $\delta = d$ and when using critical value C is

$$\begin{aligned}
\Pr\left(|t_{\delta^m}| > C\right) &= \Pr\left(t_{\delta^m} > C\right) + \Pr\left(t_{\delta^m} < -C\right) \\
&= \Pr\left(\frac{\widehat{\delta}^m}{se\left(\widehat{\delta}^m\right)} > C\right) + \Pr\left(\frac{\widehat{\delta}^m}{se\left(\widehat{\delta}^m\right)} < -C\right) \\
&= \Pr\left(\frac{\widehat{\delta}^m - d}{se\left(\widehat{\delta}^m\right)} > C - d\frac{\sqrt{n}\sigma_s\sqrt{1-\theta}}{\sigma_u}\right) \\
&\quad + \Pr\left(\frac{\widehat{\delta}^m - d}{se\left(\widehat{\delta}^m\right)} < -C - d\frac{\sqrt{n}\sigma_s\sqrt{1-\theta}}{\sigma_u}\right) \\
&\approx 1 - \Phi\left(C - d\frac{\sqrt{n}\sigma_s\sqrt{1-\theta}}{\sigma_u}\right) + \Phi\left(-C - d\frac{\sqrt{n}\sigma_s\sqrt{1-\theta}}{\sigma_u}\right)
\end{aligned}$$

when n is large. This is the power function of the balancing test

$$Power_{t_\delta}\left(d\right) = 1 - \Phi\left(1.96 - d\frac{\sqrt{n}\sigma_s\sqrt{1-\theta}}{\sigma_u}\right) + \Phi\left(-1.96 - d\frac{\sqrt{n}\sigma_s\sqrt{1-\theta}}{\sigma_u}\right).$$

A.2 The Coefficient Comparison Test

The short and long regressions are

$$\begin{aligned}
y_i &= \alpha^s + \beta^s s_i + e_i^s \\
y_i &= \alpha + \beta s_i + \gamma x_i + e_i,
\end{aligned}$$

and

$$x_i = \delta_0 + \delta s_i + u_i.$$

Adding measurement error in x_i:

$$x_i^m = x_i + m_i,$$

we have

$$\begin{aligned}
y_i &= \alpha^s + \beta^s s_i + e_i^s \\
y_i &= \alpha^m + \beta^m s_i + \gamma^m x_i^m + e_i^m \\
x_i^m &= \delta_0 + \delta s_i + u_i + m_i.
\end{aligned}$$

Treat s_i, u_i, e_i, and m_i as the underlying random variables which determine x_i, y_i, e_i^s and e_i^m. We normalize s_i to a mean zero variable. For the derivations in the remainder of this section, we make the following assumptions:

Assumption A1: s_i, u_i, e_i and m_i are mutually independent;

Assumption A2: $E[u_i^3] = 0$.

Note that Assumptions A1 and A2 are satisfied in the DGP's we adopt for the Monte Carlo simulations underlying Figure 2, that is, when s_i, u_i, e_i, m_i follow a joint normal distribution with the first two moments specified according to

$$\begin{matrix} s_i \\ u_i \\ e_i \\ m_i \end{matrix} \sim \left(\begin{bmatrix} 0 \\ 0 \\ 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \sigma_s^2 & 0 & 0 & 0 \\ 0 & \sigma_u^2 & 0 & 0 \\ 0 & 0 & \sigma_e^2 & 0 \\ 0 & 0 & 0 & \sigma_m^2 \end{bmatrix} \right). \tag{A1}$$

A.2.1 Population Parameters

In this subsection, we derive the expressions of population regression coefficients β^m and γ^m in terms of the model parameters, as discussed in Section 3. Performing an anatomy to the multiple regression (9), we have

$$\gamma^m = \frac{Cov(y_i, u_i + m_i)}{Var(u_i + m_i)} = \gamma \frac{\sigma_u^2}{\sigma_u^2 + \sigma_m^2}, \tag{A2}$$

where $u_i + m_i$ is the residual from the population regression of x_i^m on s_i. Using θ as defined above, equation (A2) becomes

$$\gamma^m = \gamma(1 - \theta). \tag{A3}$$

By the omitted variable bias formula, we have

$$\begin{aligned} \beta^s &= \beta + \gamma\delta \\ \beta^s &= \beta^m + \gamma^m\delta, \end{aligned}$$

and therefore

$$\beta^m = \beta + \gamma\delta\theta. \tag{A4}$$

As mentioned in the main text, an alternative representation of θ is

$$\theta = \frac{1-\lambda}{1-R^2}, \tag{A5}$$

where

$$\lambda = \frac{Var\left(x_i\right)}{Var\left(x_i^m\right)}$$

is the reliability of x_i^m, and R^2 is the population R^2 of the regression of x_i^m on s_i. To see why (A5) holds, notice that

$$\begin{aligned} Var(x_i) &= \delta^2\sigma_s^2 + \sigma_u^2 \\ Var(x_i^m) &= \delta^2\sigma_s^2 + \sigma_u^2 + \sigma_m^2 \\ R^2 &= 1 - \frac{\sigma_u^2 + \sigma_m^2}{\delta^2\sigma_s^2 + \sigma_u^2 + \sigma_m^2}, \end{aligned}$$

from which equation (A5) mechanically follows.

A.2.2 Asymptotic Variance in the Coefficient Comparison Test under Homoskedasticity

For the coefficient comparison test $\beta^s - \beta^m = 0$, the test statistic is

$$t_{\beta^s-\beta^m} = \frac{\widehat{\beta}^s - \widehat{\beta}^m}{\sqrt{Var(\widehat{\beta}^s - \widehat{\beta}^m)}},$$

which is asymptotically standard normal. As mentioned in section 4, we rely on the delta method equation (13) to derive $Var(\widehat{\beta}^s - \widehat{\beta}^m)$. We have already shown in the previous subsection that

$$Var(\widehat{\delta}^m) = \frac{1}{n}\frac{\sigma_u^2}{(1-\theta)\sigma_s^2}, \tag{A6}$$

and we derive $Var\left(\widehat{\gamma}^m\right)$ and $Cov\left(\widehat{\delta}^m, \widehat{\gamma}^m\right)$ in the remainder of this subsection. For simplicity of exposition, we make an additional assumption:

Assumption A3: $Var(e_i^m|s_i, x_i^m)$ is constant.

Like Assumptions A1 and A2, Assumption A3 is also satisfied in the DGP's underlying Figure 2. In the subsection below, we also derive the general expression of $Var(\widehat{\beta}^s - \widehat{\beta}^m)$ when Assumption A3 is relaxed.

In order to derive $Var(\widehat{\gamma}^m)$, first note that

$$Var\left(\widehat{\gamma}^m\right) = \frac{1}{n}\frac{Var\left(e_i^m\right)}{Var\left(u_i + m_i\right)}, \tag{A7}$$

where, as mentioned above, $u_i + m_i$ is the residual from the population regression of x_i^m on s_i. Since $Var\left(u_i + m_i\right) = \sigma_u^2 + \sigma_m^2$, the missing piece in equation (A7) is $Var\left(e_i^m\right)$. Plugging (A3) and (A4) into (9), we get

$$\begin{aligned} y_i &= \alpha^m + \beta^m s_i + \gamma^m x_i^m + e_i^m \\ &= \alpha^m + \left(\beta + \gamma\delta\theta\right) s_i + \gamma\left(1-\theta\right) x_i^m + e_i^m \\ &= \left(\alpha^m + \gamma\left(1-\theta\right)\delta_0\right) + \left(\beta + \gamma\delta\right) s_i + \gamma\left(1-\theta\right)\left(u_i + m_i\right) + e_i^m \end{aligned}$$

Since

$$\begin{aligned} y_i &= \alpha + \beta s_i + \gamma\left(\delta_0 + \delta s_i + \ u_i\right) + e_i \\ &= \left(\alpha + \gamma\delta_0\right) + \left(\beta + \gamma\delta\right) s_i + \gamma u_i + e_i, \end{aligned}$$

matching residuals yields

$$\begin{aligned} \gamma u_i + e_i &= \gamma\left(1-\theta\right)\left(u_i + m_i\right) + e_i^m \\ e_i^m &= \gamma\theta u_i - \gamma\left(1-\theta\right) m_i + e_i \\ Var\left(e_i^m\right) &= \gamma^2\theta^2\sigma_u^2 + \gamma^2\left(1-\theta\right)^2\sigma_m^2 + \sigma_e^2 \\ &= \gamma^2\left(\left(\frac{\sigma_m^2}{\sigma_u^2+\sigma_m^2}\right)^2\sigma_u^2 + \left(\frac{\sigma_u^2}{\sigma_u^2+\sigma_m^2}\right)^2\sigma_m^2\right) + \sigma_e^2 \\ &= \gamma^2\theta\sigma_u^2 + \sigma_e^2. \end{aligned}$$

So

$$\begin{aligned} Var\left(\widehat{\gamma}^m\right) &= \frac{1}{n}\frac{\gamma^2\theta\sigma_u^2 + \sigma_e^2}{\sigma_u^2 + \sigma_m^2} \\ &= \frac{1-\theta}{n}\left(\gamma^2\theta + \frac{\sigma_e^2}{\sigma_u^2}\right). \end{aligned} \tag{A8}$$

As for $Cov(\widehat{\delta}^m, \widehat{\gamma}^m)$, first note that

$$\widehat{\delta}^m - \delta = \frac{\sum_i (u_i + m_i)(s_i - \bar{s})}{\sum_i (s_i - \bar{s})^2} \tag{A9}$$

$$\widehat{\gamma}^m - \gamma^m = \frac{\sum e_i^m (\widetilde{x}_i^m - \bar{\widetilde{x}}^m)}{\sum \left(\widetilde{x}_i^m - \bar{\widetilde{x}}^m\right)^2} \tag{A10}$$

where $\bar{s}$ and $\bar{\widetilde{x}}^m$ are the sample averages of s_i and $\widetilde{x}_i^m$ with $\widetilde{x}_i^m = x_i^m - \widehat{\delta}_0 - \widehat{\delta}^m s_i$ being the residual from regressing x_i^m on s_i. By Assumption A1 along with the fact that $\hat{\delta}_0 \xrightarrow{p} \delta_0$ and $\hat{\delta}^m \xrightarrow{p} \delta$, the asymptotic joint distribution of the numerators in equations (A9) and (A10) is

$$\begin{aligned} &\frac{1}{\sqrt{n}} \begin{bmatrix} \sum_i (u_i + m_i)(s_i - \bar{s}) \\ \sum_i e_i^m (\widetilde{x}_i^m - \bar{\widetilde{x}}^m) \end{bmatrix} \\ &\xrightarrow{d} N\left(0, \begin{bmatrix} (\sigma_u^2 + \sigma_m^2)\,\sigma_s^2 & E[s_i (u_i + m_i)^2 e_i^m] \\ E[s_i (u_i + m_i)^2 e_i^m] & E[(u_i + m_i)^2 (e_i^m)^2] \end{bmatrix}\right). \end{aligned}$$

By Assumptions A1 and A2,

$$\begin{aligned} E[s_i (u_i + m_i)^2 e_i^m] &= E[s_i (u_i + m_i)^2 (\gamma\theta u_i - \gamma\,(1-\theta)\, m_i + e_i)] \\ &= 0. \end{aligned}$$

Since the denominators of equations (A9) and (A10) converge in probability to positive constants,

$$Cov(\widehat{\delta}^m, \widehat{\gamma}^m) = 0. \tag{A11}$$

Plugging equations (A6), (A8) and (A11) into (13) yields

$$\begin{aligned} Var(\widehat{\beta}^s - \widehat{\beta}^m) &\equiv \frac{1}{n} V_\beta\,(d; \gamma) \\ &= \frac{1}{n}\,(1-\theta) \left(\frac{\gamma^2 \sigma_u^2}{\sigma_s^2} + \theta \delta^2 \gamma^2 + \frac{\delta^2 \sigma_e^2}{\sigma_u^2}\right). \end{aligned} \tag{A12}$$

Recall that

$$\beta^s - \beta^m = \delta \gamma^m = \delta \gamma\,(1-\theta)\,,$$

so the power function of the coefficient comparison test is

$$Power_{t_\beta}\,(d; \gamma) = 1 - \Phi\left(1.96 - d \frac{\sqrt{n}\gamma\,(1-\theta)}{\sqrt{V_\beta\,(d; \gamma)}}\right) + \Phi\left(-1.96 - d \frac{\sqrt{n}\gamma\,(1-\theta)}{\sqrt{V_\beta\,(d; \gamma)}}\right).$$

A.2.3 Relaxing Assumption A3

In this subsection, we provide the expression for $Var(\widehat{\beta}^s - \widehat{\beta}^m)$ while relaxing the conditional homoskedasticity of e_i^m, i.e. Assumption A3. Our derivation of this asymptotic variance expression still relies on equation (13). Since equations (A6) and (A11) are not affected by Assumption A3, we will only need the general expression for $Var\left(\widehat{\gamma}^m\right)$.

Representing model (9) in matrix form,

$$y_i = \mathbf{W}_i'\mathbf{\Gamma} + e_i^m,$$

where $\mathbf{W}_i = (1, s_i, x_i^m)'$ and $\mathbf{\Gamma} = (\alpha^m, \beta^m, \gamma^m)'$. The asymptotic variance-covariance matrix of the regression estimator $\widehat{\mathbf{\Gamma}}$ is

$$\frac{1}{n} E[\mathbf{W}_i\mathbf{W}_i']^{-1} E[\mathbf{W}_i\mathbf{W}_i'(e_i^m)^2] E[\mathbf{W}_i\mathbf{W}_i']^{-1}.$$

Expressing $E[\mathbf{W}_i\mathbf{W}_i']$ in terms of the fundamental model parameters is straightforward:

$$\begin{aligned} E[\mathbf{W}_i\mathbf{W}_i'] &= E\begin{bmatrix} 1 & s_i & x_i^m \\ s_i & s_i^2 & s_i x_i^m \\ x_i^m & s_i x_i^m & (x_i^m)^2 \end{bmatrix} \\ &= \begin{bmatrix} 1 & 0 & \delta_0 \\ 0 & \sigma_s^2 & \delta\sigma_s^2 \\ \delta_0 & \delta\sigma_s^2 & \delta_0^2 + \delta^2\sigma_s^2 + \sigma_u^2 + \sigma_m^2 \end{bmatrix}. \end{aligned}$$

As before, we set $E[s_i] = 0$, which sacrifices no generality since the mean does not enter the variance calculation in any case.

Writing out the entries in the matrix $E[\mathbf{W}_i\mathbf{W}_i'(e_i^m)^2]$:

$$\begin{aligned} & E[\mathbf{W}_i\mathbf{W}_i'(e_i^m)^2] \\ = \; & E\begin{bmatrix} \underbrace{(e_i^m)^2}_{\text{(i)}} & \underbrace{s_i(e_i^m)^2}_{\text{(ii)}} & \underbrace{x_i^m(e_i^m)^2}_{\text{(iii)}} \\ s_i(e_i^m)^2 & \underbrace{s_i^2(e_i^m)^2}_{\text{(iv)}} & \underbrace{s_i x_i^m(e_i^m)^2}_{\text{(v)}} \\ x_i^m(e_i^m)^2 & s_i x_i^m(e_i^m)^2 & \underbrace{(x_i^m)^2(e_i^m)^2}_{\text{(vi)}} \end{bmatrix}. \end{aligned}$$

Below we express quantities (i) to (vi) in terms of the fundamental model parameters. Letting $\kappa_m = E[m_i^4]$ and $\kappa_u = E[u_i^4]$ and utilizing Assumptions 1 and 2, we have the expressions for (i) to (vi):

$$\begin{aligned} E[(e_i^m)^2] &= E[(\gamma\theta u_i - \gamma(1-\theta)m_i + e_i)^2] \\ &= \gamma^2\theta^2\sigma_u^2 + \gamma^2(1-\theta)^2\sigma_m^2 + \sigma_e^2, \end{aligned} \tag{i}$$

$$\begin{aligned} E[s_i(e_i^m)^2] &= E[s_i(\gamma\theta u_i - \gamma(1-\theta)m_i + e_i)^2] \\ &= 0, \end{aligned} \tag{ii}$$

$$\begin{aligned} E[x_i^m(e_i^m)^2] &= E[(\delta_0 + \delta s_i + u_i + m_i)(e_i^m)^2] \\ &= \delta_0 E[(e_i^m)^2] + \delta E[s_i(e_i^m)^2] \\ &= \delta_0(\gamma^2\theta^2\sigma_u^2 + \gamma^2(1-\theta)^2\sigma_m^2 + \sigma_e^2), \end{aligned} \tag{iii}$$

$$\begin{aligned} E[s_i^2(e_i^m)^2] &= E[s_i^2(\gamma\theta u_i - \gamma(1-\theta)m_i + e_i)^2] \\ &= \sigma_s^2(\gamma^2\theta^2\sigma_u^2 + \gamma^2(1-\theta)^2\sigma_m^2 + \sigma_e^2), \end{aligned} \tag{iv}$$

and

$$\begin{aligned} E[s_i x_i^m(e_i^m)^2] &= E[s_i(\delta_0 + \delta s_i + u_i + m_i)\cdot(e_i^m)^2] \\ &= \delta_0 E[s_i(e_i^m)^2] + \delta E[s_i^2(e_i^m)^2] \\ &+ E[s_i u_i(\gamma\theta u_i - \gamma(1-\theta)m_i + e_i)^2] \\ &+ E[s_i m_i(\gamma\theta u_i - \gamma(1-\theta)m_i + e_i)^2] \\ &= \delta\sigma_s^2(\gamma^2\theta^2\sigma_u^2 + \gamma^2(1-\theta)^2\sigma_m^2 + \sigma_e^2). \end{aligned} \tag{v}$$

Finally, for the expression of (vi)

$$\begin{aligned}
E[(x_i^m)^2(e_i^m)^2] &= E[(\delta_0+\delta s_i+u_i+m_i)^2(e_i^m)^2] \\
&= \delta_0^2 E[(e_i^m)^2]+\delta^2 E[s_i^2(e_i^m)^2] \\
&\quad +E[u_i^2(\gamma\theta u_i-\gamma(1-\theta)m_i+e_i)^2] \\
&\quad +E[m_i^2(\gamma\theta u_i-\gamma(1-\theta)m_i+e_i)^2] \\
&\quad +2\delta_0\delta E[s_i(e_i^m)^2]+2\delta_0 E[u_i(e_i^m)^2] \\
&\quad +2\delta_0 E[m_i(e_i^m)^2]+2\delta E[s_iu_i(e_i^m)^2] \\
&\quad +2\delta E[s_im_i(e_i^m)^2]+2E[u_im_i(e_i^m)^2].
\end{aligned}$$

Note that

$$\begin{aligned}
E[s_i(e_i^m)^2] &= 0 \\
E[u_i(e_i^m)^2] &= E[m_i(e_i^m)^2] = 0 \\
E[s_iu_i(e_i^m)^2] &= E[s_im_i(e_i^m)^2] = 0,
\end{aligned}$$

and we only need to find the expressions for

$$\begin{aligned}
& E[u_i^2(\gamma\theta u_i-\gamma(1-\theta)m_i+e_i)^2] \\
= \;& E[u_i^2\{\gamma^2\theta^2u_i^2+\gamma^2(1-\theta)^2m_i^2+e_i^2 \\
& -2\gamma^2\theta(1-\theta)u_im_i+2\gamma\theta u_ie_i-2\gamma(1-\theta)m_ie_i\}] \\
= \;& \gamma^2\theta^2E[u_i^4]+\gamma^2(1-\theta)^2\sigma_u^2\sigma_m^2+\sigma_u^2\sigma_e^2 \\
= \;& \gamma^2\theta^2\kappa_u+\gamma^2(1-\theta)^2\sigma_u^2\sigma_m^2+\sigma_u^2\sigma_e^2,
\end{aligned}$$

$$\begin{aligned}
& E[m_i^2(\gamma\theta u_i-\gamma(1-\theta)m_i+e_i)^2] \\
= \;& E[m_i^2\{\gamma^2\theta^2u_i^2+\gamma^2(1-\theta)^2m_i^2+e_i^2 \\
& -2\gamma^2\theta(1-\theta)u_im_i+2\gamma\theta u_ie_i-2\gamma(1-\theta)m_ie_i\}] \\
= \;& \gamma^2\theta^2\sigma_u^2\sigma_m^2+\gamma^2(1-\theta)^2\kappa_m+\sigma_m^2\sigma_e^2,
\end{aligned}$$

and

$$\begin{aligned} E[u_i m_i (e_i^m)^2] &= E[u_i m_i (\gamma\theta u_i - \gamma(1-\theta)m_i + e_i)^2] \\ &= E[u_i m_i \{\gamma^2\theta^2 u_i^2 + \gamma^2(1-\theta)^2 m_i^2 + e_i^2 \\ &\quad -2\gamma^2\theta(1-\theta)u_i m_i + 2\gamma\theta u_i e_i - 2\gamma(1-\theta)m_i e_i\}] \\ &= -2\gamma^2\theta(1-\theta)\sigma_u^2\sigma_m^2. \end{aligned}$$

Putting these terms together,

$$\begin{aligned} E[(x_i^m)^2(e_i^m)^2] &= \delta_0^2 E[(e_i^m)^2] + \delta^2 E[s_i^2(e_i^m)^2] \\ &\quad + E[u_i^2(\gamma\theta u_i - \gamma(1-\theta)m_i + e_i)^2] \\ &\quad + E[m_i^2(\gamma\theta u_i - \gamma(1-\theta)m_i + e_i)^2] \\ &\quad + 2E[u_i m_i (e_i^m)^2] \\ &= \delta_0^2\{\gamma^2\theta^2\sigma_u^2 + \gamma^2(1-\theta)^2\sigma_m^2 + \sigma_e^2\} \\ &\quad + \delta^2\sigma_s^2(\gamma^2\theta^2\sigma_u^2 + \gamma^2(1-\theta)^2\sigma_m^2 + \sigma_e^2) \\ &\quad + \{\gamma^2\theta^2\kappa_u + \gamma^2(1-\theta)^2\sigma_u^2\sigma_m^2 + \sigma_u^2\sigma_e^2\} \\ &\quad + \{\gamma^2\theta^2\sigma_u^2\sigma_m^2 + \gamma^2(1-\theta)^2\kappa_m + \sigma_m^2\sigma_e^2\} \\ &\quad - \{4\gamma^2\theta(1-\theta)\sigma_u^2\sigma_m^2\}. \end{aligned} \tag{vi}$$

Now that we have the expression for both $E[\mathbf{W}_i\mathbf{W}_i']$ and $E[\mathbf{W}_i\mathbf{W}_i'(e_i^m)^2]$, we can compute the asymptotic variance of $\widehat{\gamma}^m$

$$\begin{aligned} Var\left(\widehat{\gamma}^m\right) = \frac{1}{n}\Bigg\{ &(1-\theta)\left(\gamma^2\theta + \frac{\sigma_e^2}{\sigma_u^2}\right) \\ &+ \underbrace{\gamma^2\left[\frac{(\kappa_u - 3\sigma_u^4)\theta^2}{(\sigma_m^2+\sigma_u^2)^2} + \frac{(\kappa_m - 3\sigma_m^4)(1-\theta)^2}{(\sigma_m^2+\sigma_u^2)^2}\right]}_{(a)}\Bigg\}. \end{aligned}$$

Compared to its expression under homoskedasticity (A8), we have an extra term (a) that accounts for the excess kurtosis of the u and m distributions.

It follows that

$$\begin{aligned}\frac{1}{n}V_\beta(d;\gamma) &= Var\left(\widehat{\beta}^s - \widehat{\beta}^m\right) \\ &= \frac{1}{n}\left\{(1-\theta)\left(\frac{\gamma^2\sigma_u^2}{\sigma_s^2} + \theta\delta^2\gamma^2 + \frac{\delta^2\sigma_e^2}{\sigma_u^2}\right)\right. \\ &\quad \left. +\gamma^2\delta^2\left[\frac{(\kappa_u - 3\sigma_u^4)\theta^2}{(\sigma_m^2+\sigma_u^2)^2} + \frac{(\kappa_m - 3\sigma_m^4)(1-\theta)^2}{(\sigma_m^2+\sigma_u^2)^2}\right]\right\}.\end{aligned}$$

Note that when u_i and m_i are normal, $\kappa_u - 3\sigma_u^4 = 0$ and $\kappa_m - 3\sigma_m^4 = 0$, and the variance expression above simplifies to that of equation (A12). Since $Var\left(\widehat{\beta}^s - \widehat{\beta}^m\right)$ increases in κ_u and κ_m and that the balancing test is unaffected by the heteroskedasticity of e^m, the power advantage of the balancing test is larger when u_i and m_i have thicker tails than a normal distribution.

B Comparison with Oster (forthcoming)

The Oster (forthcoming) formulation of the causal regression takes the form

$$y_i = \alpha + \beta s_i + \rho w_{1i} + w_{2i} + e_i,$$

where w_{1i} is an observed covariate and w_{2i} is an unobserved covariate, uncorrelated with w_{1i}. To map this into our setup, think of the true x_i as capturing both w_{1i} and w_{2i}, i.e. $x_i = \rho w_{1i} + w_{2i}$. Furthermore, there is equal selection, i.e.

$$\frac{Cov(s_i, \rho w_{1i})}{\rho^2\sigma_1^2} = \frac{Cov(s_i, w_{2i})}{\sigma_2^2},$$

where σ_1^2 and σ_2^2 are the variances of w_{1i} and w_{2i}, respectively. Then, Oster's (forthcoming) regression can be written as

$$y_i = \alpha + \beta s_i + x_i + e_i,$$

which is our regression with $\gamma = 1$ (the scaling of x_i is arbitrary of course; it could be $x_i = w_{1i} + w_{2i}/\rho$ instead and $\gamma = \rho$ or anything else).

Our observed $x_i^m = \rho w_{1i}$, so measurement error $m_i = -w_{2i}$. Measurement error here is mean reverting, i.e.

$$m_i = \kappa x_i + \mu_i \tag{A13}$$

with $\kappa < 0$. Notice that

$$Cov\left(m_i, x_i\right) = -\sigma_2^2,$$

and hence

$$\kappa = \frac{-\sigma_2^2}{\rho^2\sigma_1^2 + \sigma_2^2} \tag{A14}$$

and

$$\begin{aligned}
\mu_i &= -w_{2i} - \kappa\left(\rho w_{1i} + w_{2i}\right) \\
&= -\kappa\rho w_{1i} - \left(1 + \kappa\right) w_{2i} \\
&= \frac{\sigma_2^2}{\rho^2\sigma_1^2 + \sigma_2^2}\rho w_{1i} - \frac{\rho^2\sigma_1^2}{\rho^2\sigma_1^2 + \sigma_2^2} w_{2i}.
\end{aligned}$$

It turns out that μ_i implicitly defined in (A13) and κ given by (A14) imply $Cov(x_i, \mu_i) = 0$ and $Cov(s_i, \mu_i) = 0$. Hence, these two equations represent mean reverting measurement error as defined in the body of the manuscript. However, note that $Cov(s_i, \mu_i) = 0$ depends on the equal selection assumption. With proportional selection, i.e.

$$\phi\frac{Cov(s_i, \rho w_{1i})}{\rho^2\sigma_1^2} = \frac{Cov(s_i, w_{2i})}{\sigma_2^2},$$

and $\phi \neq 1$ we would have $Cov(s_i, \mu_i) \neq 0$.