Investigating Instruments with Meta-Regressions*

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Abstract

Instrumental variable (IV) estimates are typically much larger than their ordinary least squares (OLS) counterparts, often suggesting implausible values of the omitted variable bias. We derive a meta-regression of OLS on IV estimates that can resolve this puzzle by separately identifying omitted variable bias from measurement error in the endogenous regressor, provided the instrument is valid. We then show that meta-regression estimates can also reveal when the underlying instruments are invalid. We conduct simulation exercises and apply our estimator to three empirical settings in finance. We include a Stata package, `metaps`, which implements the meta-regression.

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1 Introduction

Instrumental variable (IV) regressions are central to the "credibility revolution" in empirical economics (Angrist and Pischke, 2009, 2010). While valid instruments are powerful tools for resolving endogeneity problems and estimating causal effects, invalid instruments can lead to IV estimates that are even more biased than their ordinary least squares (OLS) counterparts. In this paper, we use meta-regressions to investigate both the problems that instruments solve, and the problems that instruments create.

In economics and finance, instruments are primarily used to address omitted variable bias, which is often expected to inflate OLS estimates. Yet, OLS estimates are almost always *smaller* than their IV counterparts (Jiang, 2017; Pancost and Schaller, 2026). However, the simple difference between IV and OLS coefficients is not equal to the omitted variable bias, even if the instrument is valid; rather, the difference reflects both omitted variable bias *and* measurement error, which can explain the discrepancy between IV and OLS coefficients.

When the instruments are valid, our meta-regression estimator separately identifies omitted variable bias and measurement error, thereby quantifying the importance of each source of endogeneity. Given a single pair of OLS and IV estimates, omitted variable bias and measurement error are indistinguishable. However, researchers often estimate the causal effects of the same endogenous regressor on a variety of dependent variables, which we can exploit to separately identify each bias, owing to the fact that measurement error bias is multiplicative while omitted variable bias is additive. In fact, our meta-regression is simply a regression of OLS coefficients on IV coefficients; the estimated slope identifies the measurement error, and the estimated intercept identifies the average omitted variable bias.

We run a simulation exercise to show that the meta-regression performs well even when the number of observed OLS-IV pairs is small. We then apply our meta-regression to three settings in finance. First, we show that measurement error can explain why IV estimates in Mian and Sufi (2014) exceed their OLS counterparts, even though economic theory suggests that the omitted variable bias is positive; once we account for measurement error, we find that the implied omitted variable bias is positive and quantitatively small. Second, using investment-q regressions, we show that meta-regression estimates of proxy quality are similar to higher-order moment and cumulant estimates reported in the literature (Erickson, Jiang and Whited, 2014; Peters and Taylor, 2017). Finally, we apply the meta-regression to a set of reported coefficients and standard errors from Adelino, Ma and Robinson (2017), without using the underlying data, which again reveals that measurement error bias is quantitatively more important than omitted variable bias.

Identifying omitted variable bias and measurement error with our meta-regression is subject to the same identification challenges faced by the underlying IV regressions, including the possibility of instrument invalidity. It is well known that correlation between the instruments and the omitted variables leads to biased IV estimates. Less well-known is the fact that instruments that are correlated with the measurement error in the endogenous regressor are *also* invalid, and lead to biased IV estimates, even in the absence of omitted variable bias (Roberts and Whited, 2013). Correlation between the instrument and measurement error, which we call *measurement invalidity*, will be reflected in the meta-regression slope coefficient; correlation between the instrument and omitted variables, which we call *economic invalidity*, will be reflected in the meta-regression intercept.

When instruments are subject to these identification issues, we can then use the meta-

regression to infer the extent of invalidity. In particular, because the variance of the measurement error is bounded by the variance of the endogenous regressor itself, large or negative values of the meta-regression slope suggest that the instrument is subject to measurement invalidity. In addition, when the sign of the omitted variable bias is known, the meta-regression intercept can indicate that the instrument is subject to economic invalidity. We derive distinct monotonic relationships between measurement error and measurement invalidity, and between omitted variable bias and economic invalidity, that are pinned down by the meta-regression slope and intercept terms. In Appendix B, we also extend our method to allow for heterogeneous treatment effects, and derive the relevance of the sub-population that is affected by the instrument.

The meta-regression relies on the assumptions that the measurement error is classical, there is no simultaneity bias, and treatment effects are homogeneous. We discuss these assumptions in section 3.3, and give special treatment to heterogeneity in Appendix B, though it is worth keeping in mind that none of these assumptions differ from standard measurement error estimators (Erickson and Whited, 2012; Erickson, Jiang and Whited, 2014) or econometric methods that compare estimates across specifications (Hansen, 1982). The meta-regression estimates may also be subject to publication bias when applied to OLS and IV estimates reported in published work.

Instrumental variables techniques have a long history. The earliest published use of an instrumental variables regression is Appendix B of Wright (1928), which employed IV in the context of a simultaneous equation system (Stock and Trebbi, 2003). Subsequent developments in IV methods were motivated by measurement error (Wald, 1940; Reiersøl, 1941; Geary, 1943; Durbin, 1954); see Aldrich (1993) for a history of econometric research in the

1940s as it relates to modern instrumental variables techniques and measurement. The surveys of Bound, Brown and Mathiowetz (2001) and Hausman (2001) note the usefulness of instrumental variables in combating errors in variables. Hausman (1978) derives an expression for the variance of $\hat{\beta}_j^{\text{OLS}} - \hat{\beta}_j^{\text{IV}}$, which can be used to test for the presence of endogeneity. Griliches and Hausman (1986) propose IV estimators for dealing with measurement error in panel data when the measurement error terms are i.i.d.; Biørn (2000) extends their estimator to deal with non-i.i.d. errors. See Schennach (2016, 2020) for recent surveys on measurement error in econometrics.

A number of recent papers analyze popular econometric methods and practices with fundamental insights from econometric theory (Borusyak, Hull and Jaravel, 2018; Grieser and Hadlock, 2019; Oster, 2019; Goldsmith-Pinkham, Sorkin and Swift, 2020; Berg, Reisinger and Streitz, 2021; Cohn, Liu and Wardlaw, 2022). DiTraglia and García-Jimeno (2021) show how prior beliefs on measurement error, omitted variable bias, and instrument validity can be incorporated into an IV estimation in a Bayesian framework. Our analysis of instrument invalidity is similar in spirit to theirs, in that we show how differences between our baseline meta-regression estimates and a researcher's beliefs about measurement error and omitted variable bias can indicate instrument invalidity.

Pancost and Schaller (2026) likewise show that a meta-regression can quantify measurement error bias based on reported OLS and IV coefficients. Our contribution complements that analysis. Pancost and Schaller (2026) ask a cross-literature question using 2,848 IV regressions from 391 papers: averaging across regressors, samples, instruments, and designs, how noisy is the average proxy variable? By contrast, in this paper, we ask a within-study question: what does the joint pattern of OLS-IV coefficients from a single design reveal about

that particular instrument, proxy, and omitted variable bias? Our within-design focus also allows us to gauge instrument validity and, in the appendix, extend our analysis to account for heterogeneous treatment effects.

Heath et al. (2023) document the reuse of natural experiments across many outcome variables, and since any natural experiment can be cast in an IV framework, our papers are two sides of the same coin: they treat the accumulated family of estimates across outcomes as an inference problem and correct the critical values, showing that more than half of statistically significant findings in reused natural experiment settings may be false positives, while we treat the same family as identifying variation and extract structural parameters from it. Heath et al. (2023) catalog 114 and 434 IV-outcome pairs in their two empirical settings, showing that the many-pairs requirement of the meta-regression is far less restrictive than it may at first appear. Moreover, the fact that researchers are using critical values that are too low, and therefore have inflated false positive rates, may lessen the impact of publication bias on the meta-regression; on the other hand, selecting on significance also distorts the sampling errors and biases the meta-regression corrections for sampling variation, making the net effect on our estimates ambiguous. Our simulation exercise also mirrors the simulations in Heath et al. (2023): we use actual independent variables and simulate outcomes, while they use actual outcomes and simulate independent variables.

In the absence of omitted variable bias, higher-order moment and cumulant estimators can be used to correct for measurement error (Erickson and Whited, 2000; Whited, 2001; Erickson and Whited, 2002, 2012; Erickson, Jiang and Whited, 2014). These estimators also deliver direct estimates of the extent of measurement error. For example, Erickson, Jiang and Whited (2014) estimate that only 30%–35% of the variance of Tobin's q comes from

variation in marginal q (Table 4). Our meta-regression requires a valid instrument instead of higher-order moments, but also allows for the identification of the average omitted variable bias in addition to the extent of measurement error.

There is a long tradition of using meta-regressions to infer publication bias across studies (Card and Krueger, 1995; Stanley, 2008; Doucouliagos and Stanley, 2009; Stanley and Doucouliagos, 2014; Christensen and Miguel, 2018); see Roberts (2005) for a brief survey. The first paper to advocate for the use of meta-regression as a tool is likely Stanley and Jarrell (1989). Rather than focus on publication bias by looking at results across studies, our meta-regression instead uses the multiple reported coefficients within a study—coming from multiple dependent variables—to analyze measurement error, omitted variable bias, instrument invalidity, and the importance of heterogeneous treatment effects.

2 Theory

In this section, we present the theory behind our meta-regression estimator. In section 2.1, we show how pairs of OLS and IV estimates can be used to disentangle omitted variable bias from measurement error. In section 2.2, we discuss the robustness of our estimator. In section 2.3, we introduce a restricted meta-regression estimator. In section 2.4, we allow for instrument invalidity. In Appendix B, we allow for heterogeneous treatment effects.

Throughout this section, we assume that there are J outcome variables $(y_{i1}, y_{i2}, ..., y_{iJ})$, one regressor x_i, and one instrument z_i; we include control variables in Appendix A. For ease of exposition, we assume that variables are i.i.d. across i and parameters are i.i.d. across j, which we relax in our empirical applications.

2.1 Meta-regression

Suppose there are J regressions with the same regressor x_i and different outcome variables y_{ij}:

$$y_{ij} = \alpha_j^{\text{OLS}} + \beta_j^{\text{OLS}} x_i + v_{ij}^{\text{OLS}}, \tag{1}$$

where $i = 1, ..., N$ indexes the observation and $j = 1, ..., J$ indexes the outcome variable. If x_i is a proxy for an unobserved economic variable of interest χ_i, where

$$y_{ij} = \alpha_j + \beta_j \chi_i + u_{ij}, \tag{2}$$

then the OLS estimand is

$$\beta_j^{\text{OLS}} = \frac{\text{cov}_j\left(y_{ij}, x_i\right)}{\text{var}\left(x_i\right)} = \underbrace{\frac{\text{cov}\left(x_i, \chi_i\right)}{\text{var}\left(x_i\right)}}_{\theta} \beta_j + \underbrace{\frac{\text{cov}_j\left(x_i, u_{ij}\right)}{\text{var}\left(x_i\right)}}_{\gamma_j}. \tag{3}$$

Thus, β_j^{OLS} differs from the true causal effect β_j both because of the measurement error $\varepsilon_i \equiv x_i - \chi_i$ and because of the omitted variable u_{ij}. On the other hand, an IV estimand satisfies $\beta_j^{\text{IV}} = \beta_j$ provided the instrument z_i is valid, which requires both $\text{cov}_j\left(z_i, u_{ij}\right) = 0$ and $\text{cov}\left(z_i, \varepsilon_i\right) = 0$.

Under classical measurement error, $\text{cov}\left(\chi_i, \varepsilon_i\right) = \text{cov}_j\left(\varepsilon_i, u_{ij}\right) = 0$ and equation (3) simplifies to

$$\beta_j^{\text{OLS}} = \tau^2 \beta_j^{\text{IV}} + \gamma_j, \tag{4}$$

where

$$\tau^2 \equiv \frac{\text{var}\left(\chi_i\right)}{\text{var}\left(x_i\right)} = 1 - \frac{\text{var}\left(\varepsilon_i\right)}{\text{var}\left(x_i\right)} \tag{5}$$

is a measure of proxy quality. In fact, τ^2 is the R-squared from a hypothetical regression of the unobserved economic variable of interest χ_i on the observed proxy x_i. If x_i is a perfect proxy for χ_i, then $\tau^2 = 1$; if x_i contains no information about χ_i, then $\tau^2 = 0$.

Equation (4) implies that a meta-regression of β_j^{OLS} on β_j^{IV} will recover the proxy quality τ^2 as the slope coefficient and the average omitted variable bias $\mathbb{E}\left[\gamma_j\right]$ as the intercept, provided $\text{cov}\left(\beta_j, \gamma_j\right) = 0$. Proposition 1 formalizes this result.

Proposition 1. *If* $cov\left(\beta_j, \gamma_j\right) = 0$*, then we can consistently estimate* τ^2 *and* $\mathbb{E}\left[\gamma_j\right]$ *with the following meta-regression of* β_j^{OLS} *on* β_j^{IV}*:*

$$\beta_j^{OLS} = a + b\beta_j^{IV} + v_j, \tag{6}$$

where

$$b = \frac{cov\left(\beta_j^{OLS}, \beta_j^{IV}\right)}{var\left(\beta_j^{IV}\right)} = \tau^2,$$
$$a = \mathbb{E}\left[\beta_j^{OLS}\right] - b\mathbb{E}\left[\beta_j^{IV}\right] = \mathbb{E}\left[\gamma_j\right].$$

As the following corollary shows, we can also use the meta-regression to compute the omitted variable bias for each j.

Corollary 1. *Under Proposition 1, we can consistently estimate γ_j using*

$$\beta_j^{OLS} - b\beta_j^{IV} = \gamma_j. \tag{7}$$

Crucially, the omitted variable bias is not equal to the simple difference between the OLS and IV coefficients: $\beta_j^{\text{OLS}} - \beta_j^{\text{IV}} \neq \gamma_j$. When $\beta_j^{\text{IV}}/\beta_j^{\text{OLS}} > 1$, which is typically what researchers find (Jiang, 2017; Pancost and Schaller, 2026), the difference between β_j^{OLS} and β_j^{IV} does not even reveal the *sign* of the omitted variable bias. Inferring the omitted variable bias requires knowledge of the proxy quality τ^2, which is what the slope b delivers.

However, there is an important caveat to Proposition 1 and Corollary 1: β_j^{OLS} and β_j^{IV} are *asymptotic* OLS and IV coefficients, while in practice we observe OLS and IV *estimates* that are subject to sampling variation. Fortunately, as Proposition 2 and Corollary 2 show, we can correct for sampling variation using the OLS and IV standard errors.

Proposition 2. *If $cov\left(\beta_j, \gamma_j\right) = 0$, then we can consistently estimate τ^2 and $\mathbb{E}\left[\gamma_j\right]$ with the following meta-regression of $\hat{\beta}_j^{OLS}$ on $\hat{\beta}_j^{IV}$:*

$$\hat{\beta}_j^{OLS} = \hat{a} + \hat{b}\hat{\beta}_j^{IV} + \hat{v}_j, \tag{8}$$

where

$$\begin{aligned} \hat{b} &= \frac{cov\left(\hat{\beta}_j^{OLS}, \hat{\beta}_j^{IV}\right)}{var\left(\hat{\beta}_j^{IV}\right)} = \tau^2 \hat{q}\hat{\rho}^2, \\ \hat{a} &= \mathbb{E}\left[\hat{\beta}_j^{OLS}\right] - \hat{b}\mathbb{E}\left[\hat{\beta}_j^{IV}\right] = \mathbb{E}\left[\gamma_j\right] + \tau^2\left(1 - \hat{q}\hat{\rho}^2\right)\mathbb{E}\left[\hat{\beta}_j^{IV}\right], \end{aligned} \tag{9}$$

and

$$\hat{\rho}^2 \equiv 1 - \frac{\mathbb{E}\left[\left(\varsigma_j^{IV}\right)^2\right]}{var\left(\hat{\beta}_j^{IV}\right)}$$

$$\hat{q} \equiv \left(1 - \frac{\mathbb{E}\left[\varsigma_j^{OLS}\varsigma_j^{IV}\right]}{cov\left(\hat{\beta}_j^{OLS}, \hat{\beta}_j^{IV}\right)}\right)^{-1}$$

where $\varsigma_j^k \equiv \hat{\beta}_j^k - \hat{\mu}_k - \left(\beta_j^k - \mu_k\right)$ *for* $k \in \{OLS, IV\}$.

Note that $\hat{\rho}^2$ adjusts for sampling variation in $\hat{\beta}_j^{\text{IV}}$, $\hat{q}$ adjusts for correlated sampling variation between $\hat{\beta}_j^{\text{IV}}$ and $\hat{\beta}_j^{\text{OLS}}$, and both $\hat{\rho}^2$ and $\hat{q}$ are trivial to estimate using the full variance-covariance matrix of the J OLS and IV estimates.

Furthermore, we can still estimate individual γ_j as follows.

Corollary 2. *Under Proposition 2, we can consistently estimate* γ_j *using*

$$\hat{\beta}_j^{OLS} - \left(\frac{\hat{b}}{\hat{q}\hat{\rho}^2}\right)\hat{\beta}_j^{IV} \equiv \hat{\gamma}_j. \tag{10}$$

Granted, estimates of individual γ_j may be noisy if the OLS and IV coefficients are imprecisely estimated. We discuss this further in our simulation exercise in section 4.

All of our results are robust to the inclusion of controls. Proposition 2' in Appendix A extends both Proposition 2 and Corollary 2 to allow for perfectly measured control variables in the underlying J regressions, which implies that we are measuring the measurement error of the proxy variable x_i after controlling for other factors.

2.2 Robustness

In this section, we show that we can relax the assumption that $\text{cov}\left(\beta_j, \gamma_j\right) = 0$ provided β_j and γ_j are not *perfectly* correlated with each other. Suppose that u_{ij} in equation (2) takes the form[1]

$$u_{ij} = o_i \zeta_j + u_{ij}^*,$$

where o_i is an omitted variable with causal effect ζ_j, $\text{cov}\left(x_i, o_i\right) \neq 0$, and $\text{cov}_j\left(x_i, u_{ij}^*\right) = 0$. Put differently, o_i is an omitted variable that is responsible for the omitted variable bias:

$$\gamma_j = \zeta_j \frac{\text{cov}\left(x_i, o_i\right)}{\text{var}\left(x_i\right)}.$$

In this case, assuming $\text{cov}\left(\beta_j, \gamma_j\right) = 0$ is equivalent to assuming $\text{cov}\left(\beta_j, \zeta_j\right) = 0$; i.e., that the causal effect of x_i is uncorrelated with the causal effect of o_i across the J different outcomes. If $\text{cov}\left(\beta_j, \zeta_j\right) \neq 0$, then $\text{cov}\left(\beta_j, \gamma_j\right) \neq 0$, and the meta-regression estimates of τ^2 and $\mathbb{E}\left[\gamma_j\right]$ will be biased. For example, suppose x_i is house price growth, o_i is an (unobservable) aggregate demand shock in county i, and the y_{ij} are employment growth in county i for a collection of J different industries. If industries that have a higher exposure to demand shocks (high ζ_j) also have higher exposure to house price shocks (high β_j), then $\text{cov}\left(\beta_j, \gamma_j\right) > 0$. In this case, the meta-regression will overstate τ^2, implying that the measurement error problem is more severe than what is implied by Proposition 2.

Fortunately, as the following proposition shows, the R-squared from the meta-regression (8)

[1] In Proposition 3, we allow for a general u_{ij}; i.e., u_{ij} does not have to take the form shown in this specific example.

can be used to bound our estimates of τ^2 and $\mathbb{E}\left[\gamma_j\right]$ even when $\text{cov}\left(\beta_j, \gamma_j\right) \neq 0$. Intuitively, if β_j and γ_j are not perfectly correlated, then a strong linear relationship between $\hat{\beta}_j^{\text{OLS}}$ and $\hat{\beta}_j^{\text{IV}}$ puts an upper bound on the variance of γ_j, and therefore limits any bias that could arise from $\text{cov}\left(\beta_j, \gamma_j\right) \neq 0$. If the meta-regression R-squared is close to 1, as we see in the Mian and Sufi example of section 5.1, then correlation between β_j and γ_j will have little effect on our estimates of τ^2 and $\mathbb{E}\left[\gamma_j\right]$.

Proposition 3. *If* $cov\left(\beta_j, \gamma_j\right) \neq 0$*, then the meta-regression coefficients (9) will instead be:*

$$\hat{b} \equiv \frac{cov\left(\hat{\beta}_j^{OLS}, \hat{\beta}_j^{IV}\right)}{var\left(\hat{\beta}_j^{IV}\right)} = \hat{q}\hat{\rho}^2\left(\tau^2 + \kappa\right),$$

$$\hat{a} \equiv \mathbb{E}\left[\gamma_j\right] + \left(\tau^2\left(1 - \hat{q}\hat{\rho}^2\right) - \kappa\hat{q}\hat{\rho}^2\right)\mathbb{E}\left[\hat{\beta}_j^{IV}\right],$$

where the bias κ *denotes the slope coefficient from a hypothetical meta-regression of* γ_j *on* β_j*:*

$$\gamma_j = \iota + \beta_j\kappa + v_j. \tag{11}$$

Moreover, this bias satisfies

$$\kappa^2 = \left(\frac{\tilde{R}^2}{1 - \tilde{R}^2}\right)\left(1 - \frac{\hat{r}^2}{\hat{q}^2\hat{\rho}^2\hat{p}^2}\right)\left(\frac{\hat{p}^2}{\hat{\rho}^2}\right)\left(\frac{var\left(\hat{\beta}_j^{OLS}\right)}{var\left(\hat{\beta}_j^{IV}\right)}\right), \tag{12}$$

where $\hat{p}^2 \equiv 1 - \frac{\mathbb{E}\left[\left(\varsigma_j^{OLS}\right)^2\right]}{var\left(\hat{\beta}_j^{OLS}\right)}$ *is the R-squared from a hypothetical meta-regression of* β_j^{OLS} *on* $\hat{\beta}_j^{OLS}$*,* $\tilde{R}^2$ *is the R-squared from the hypothetical meta-regression (11), and* $\hat{r}^2$ *is the R-squared*

from the meta-regression (8).

Equation (12) shows that, for a given $\tilde{R}^2 < 1$, we can use the R-squared from the meta-regression $\hat{r}^2$ to infer the bias κ resulting from correlation between β_j and γ_j. In an OLS regression, the coefficient of determination approaches 1 as the variance of the residuals goes to zero; in our setting, after accounting for the standard errors on $\hat{\beta}_j^{\text{IV}}$ and $\hat{\beta}_j^{\text{OLS}}$, the meta-regression $\hat{r}^2$ goes to 1 as the variance of γ_j approaches zero. Thus the bias κ becomes negligible as $\hat{r}^2$ approaches 1.

2.3 Restricted Meta-regression

As equation (10) demonstrates, it is important to account for proxy quality when estimating omitted variable bias. The converse is also true: it is important to account for omitted variable bias when estimating proxy quality.

If there were no omitted variable bias, the meta-regression would still recover τ^2; Proposition 4 shows that a restricted (no-constant) meta-regression would *also* recover τ^2.

Proposition 4. *If $\gamma_j = 0$ for $j = 1, 2, ..., J$, then we can consistently estimate τ^2 with the following restricted meta-regression of $\hat{\beta}_j^{OLS}$ on $\hat{\beta}_j^{IV}$:*

$$\hat{\beta}_j^{OLS} = \hat{b}_R \hat{\beta}_j^{IV} + \hat{v}_j^R, \tag{13}$$

where

$$\hat{b}_R = \frac{\mathbb{E}\left[\hat{\beta}_j^{OLS}\hat{\beta}_j^{IV}\right]}{\mathbb{E}\left[\left(\hat{\beta}_j^{IV}\right)^2\right]} = \tau^2 \hat{q}_R \hat{\rho}_R^2, \tag{14}$$

$$\hat{\rho}_R^2 \equiv 1 - \frac{\mathbb{E}\left[\left(\varsigma_j^{IV}\right)^2\right]}{\mathbb{E}\left[\left(\hat{\beta}_j^{IV}\right)^2\right]} \text{ and } \hat{q}_R \equiv \left(1 - \frac{\mathbb{E}\left[\varsigma_j^{OLS}\varsigma_j^{IV}\right]}{\mathbb{E}\left[\hat{\beta}_j^{OLS}\hat{\beta}_j^{IV}\right]}\right)^{-1}.$$

Comparing the meta-regression (8) and restricted meta-regression (13) estimates of τ^2 to each other will demonstrate the importance of accounting for omitted variable bias when estimating proxy quality.

Furthermore, if there is no omitted variable bias, then higher-order moment and cumulant estimators (Erickson and Whited, 2012; Erickson, Jiang and Whited, 2014) can recover τ^2. In section 5.2 we compare the meta-regression, restricted meta-regression, and higher-order moment and cumulant estimates of τ^2 to each other in a setting in which it is standard to assume no omitted variable bias.

2.4 Invalidity

The results thus far rely on the strong assumption that the instrument z_i satisfies the exclusion restriction in each regression j. In this section, we allow for violations of the exclusion restriction and show how they affect our interpretation of the meta-regression coefficients. We then demonstrate that the meta-regression can be used to detect violations of the exclusion restriction.

The instrument z_i may be invalid for two reasons. First, the instrument may be invalid for addressing omitted variable bias, $\text{cov}_j\left(z_i, u_{ij}\right) \neq 0$, which we refer to as *economic invalidity*. Economic invalidity includes cases in which z_i has a direct effect on y_{ij}; Jiang (2017) notes that such failures of the exclusion restriction can be greatly exacerbated by a weak-instrument problem.

Second, the instrument may be invalid for addressing measurement error, $\text{cov}\left(z_i, \varepsilon_i\right) \neq$

0, which we refer to as *measurement invalidity*. As Roberts and Whited (2013) note, an instrument that is chosen to deal with omitted variable bias need not also be valid for measurement error. Given the paucity of finance papers that mention measurement error as an identification issue (Erickson, Jiang and Whited, 2014; Pancost and Schaller, 2026), it seems plausible that some published instruments suffer from measurement invalidity.

The following proposition shows that the meta-regression can be a useful tool for assessing both types of invalidity.[2]

Proposition 5. *If* $cov_j\left(z_i, u_{ij}\right) \neq 0$ *and* $cov\left(z_i, \varepsilon_i\right) \neq 0$*, then the IV estimand is*

$$\beta_j^{IV} = \left(1 - \delta_1\right)\beta_j + \delta_{j0}, \tag{15}$$

where

$$\delta_1 \equiv \frac{cov\left(z_i, \varepsilon_i\right)}{cov\left(x_i, z_i\right)},$$
$$\delta_{j0} \equiv \frac{cov_j\left(z_i, u_{ij}\right)}{cov\left(x_i, z_i\right)},$$

and we assume $\left(\beta_j, \gamma_j, \delta_{j0}\right)$ *are mutually independent.*

[2] In Proposition 5 we assume there is a single instrument, which is also the case for the three example applications we describe in section 5. However, given multiple potential instruments a researcher could run the meta-regression once per candidate instrument, comparing not only the estimated $\hat{\beta}_j^{\mathrm{IV}}$ across instruments (Hansen, 1982) but also the implied τ^2, which should be invariant to the instrument used under the null hypothesis of validity. Such an approach, combined with Proposition 5, could thus be used as a tool for instrument selection. We view this topic as a promising avenue for future research that is beyond the scope of the current paper.

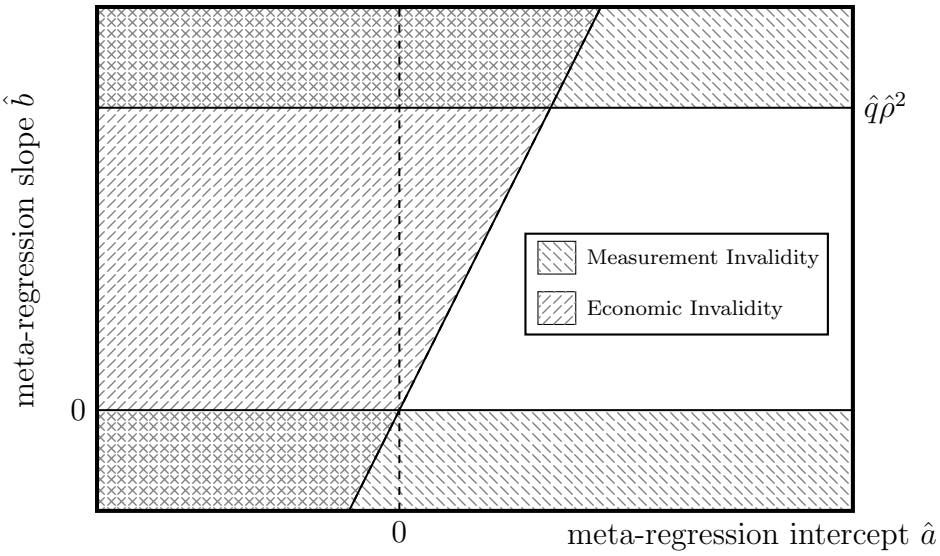


Figure 1. Recognizing Instrument Invalidity

The figure plots the five regions in which the meta-regression implies instrument invalidity, where the diagonal line is $\hat{a} = \hat{b}\mathbb{E}\left[\hat{\beta}_j^{\text{IV}}\right]\left(\frac{1}{\hat{q}\hat{\rho}^2} - 1\right)$. If $\hat{b} < 0$ or $\hat{b} > \hat{q}\hat{\rho}^2$, then the instrument suffers from measurement invalidity. If $\mathbb{E}\left[\gamma_j\right] > 0$ and $\hat{a}$ is to the left of the diagonal line (pictured here), or if $\mathbb{E}\left[\gamma_j\right] < 0$, and $\hat{a}$ is to the right of the diagonal line, then the instrument suffers from economic invalidity.

The meta-regression estimator from equation (8) converges to

$$\begin{aligned}\hat{b} &= \frac{\tau^2}{1-\delta_1}\hat{q}\left(\hat{\rho}^2 - \phi^2\right), \\ \hat{a} &= \mathbb{E}\left[\gamma_j\right] + \frac{\tau^2}{1-\delta_1}\left(\left(1 - \hat{q}\left(\hat{\rho}^2 - \phi^2\right)\right)\mathbb{E}\left[\hat{\beta}_j^{IV}\right] - \mathbb{E}\left[\delta_{j0}\right]\right),\end{aligned} \tag{16}$$

where $\phi^2 \equiv \frac{var\left(\delta_{j0}\right)}{var\left(\hat{\beta}_j^{IV}\right)} \in \left[0, \hat{\rho}^2\right]$ *is the R-squared from a hypothetical regression of* δ_{j0} *on* $\hat{\beta}_j^{IV}$.

In Proposition 5, we parameterize invalidity with δ_{j0} and δ_1. If $\delta_{j0} \neq 0$ for any j, then the instrument z_i suffers from economic invalidity; if $\delta_1 \neq 0$, then the instrument z_i suffers from measurement invalidity.[3]

The meta-regression can reveal invalidity for certain values of $\left(\hat{a}, \hat{b}\right)$. If we observe $\hat{b} \notin$

[3]The measurement invalidity δ_1 is also subject to bounds on τ^2, which we derive in Corollary 3 in Appendix A.

$\left[0, \hat{q}\hat{\rho}^2\right]$, then there must be *measurement* invalidity. It is worth noting that an instrument cannot be invalid for addressing measurement error if there is no measurement error to begin with; thus, measurement invalidity necessarily implies the existence of measurement error, $\tau^2 < 1$.

Likewise, if the average omitted variable bias is positive and we observe $\hat{a} < \hat{b}\mathbb{E}\left[\hat{\beta}_j^{\text{IV}}\right]\left(\frac{1}{\hat{q}\hat{\rho}^2} - 1\right)$, or if the average omitted variable bias is negative and we observe $\hat{a} > \hat{b}\mathbb{E}\left[\hat{\beta}_j^{\text{IV}}\right]\left(\frac{1}{\hat{q}\hat{\rho}^2} - 1\right)$, then there must be *economic* invalidity. On the other hand, if researchers have no prior on the sign of the omitted variable bias, then the meta-regression coefficients are uninformative about economic invalidity. Figure 1 plots the values of $\left(\hat{a}, \hat{b}\right)$ that imply violations of the exclusion restriction when the omitted variable bias is positive.

To summarize, when researchers are concerned about instrument invalidity:

(i) If we observe $\hat{b} \notin \left[0, \hat{q}\hat{\rho}^2\right]$, then we can infer $\text{cov}\left(z_i, \varepsilon_i\right) \neq 0$, hence the instrument z_i suffers from measurement invalidity.

(ii) If we assume $\mathbb{E}\left[\gamma_j\right] > 0$ and observe $\hat{a} < \hat{b}\mathbb{E}\left[\hat{\beta}_j^{\text{IV}}\right]\left(\frac{1}{\hat{q}\hat{\rho}^2} - 1\right)$, then we can infer that $\text{cov}\left(z_i, u_{ij}\right) \neq 0$, hence the instrument z_i suffers from *economic* invalidity.

(iii) If we assume $\mathbb{E}\left[\gamma_j\right] < 0$ and observe $\hat{a} > \hat{b}\mathbb{E}\left[\hat{\beta}_j^{\text{IV}}\right]\left(\frac{1}{\hat{q}\hat{\rho}^2} - 1\right)$, then we can infer that $\text{cov}\left(z_i, u_{ij}\right) \neq 0$, hence the instrument z_i suffers from *economic* invalidity.

3 Discussion and Limitations

In this section we offer practical guidance and discuss various caveats and limitations involved with using the meta-regression. These limitations fall into two general categories:

those related specifically to technical assumptions required by the meta-regression estimator described in section 2, and those related to practical implementation of the estimator, especially in cases where the researcher does not have access to replication code and data.

3.1 Guidance for Researchers

We recommend researchers run the meta-regression whenever they have multiple outcome variables with the same endogenous regressor x_i, instrument z_i, control variables, and sample. The meta-regression can disentangle the endogeneity problems that necessitated the use of an instrument in the first place, and can explain why IV estimates exceed their OLS counterparts even when the omitted variable bias has the same sign as $\hat{\beta}_j^{\text{IV}}$. We recommend that researchers report all of the OLS and IV regressions that they run, to avoid introducing selection biases into their estimates.

In some cases, multiple outcome variables are readily available, as we show in sections 5.1 and 5.3. In other cases, there may be a single outcome variable of interest, and researchers will need to select additional outcome variables to run the meta-regression, as we show in section 5.2. These additional outcome variables do not need to be important contributions of the paper, but they should be outcomes for which χ_i has a different causal effect and for which z_i does not violate the exclusion restriction. In the context of forecasting regressions, a researcher can select additional outcomes by including progressively longer-horizon forecasts, e.g., regressing $y_{i,t+1}$, $y_{i,t+2}$, ..., $y_{i,t+J}$ on x_{it}. In other contexts, a researcher can select additional outcomes that are similar to the original outcome of interest; in section 5.2, for example, we use different types of investment as outcome variables.

The number of different outcome variables does not need to be large. The simulations that we conduct in section 4 show that the meta-regression performs well with as few as five OLS-IV pairs. In our empirical examples, we can reject the hypothesis that $\tau^2 = 1$ with six outcomes in section 5.1, and with five outcomes in section 5.2.

3.2 Guidance for Referees and Discussants

Running the meta-regression only requires a set of *reported* OLS and IV estimates; it does not require the data that were used to compute those OLS and IV estimates. Referees and discussants can therefore apply the meta-regression to OLS-IV pairs that appear in a paper without having to replicate the paper itself. Indeed, in section 5.3, we run the meta-regression on the set of reported estimates in Adelino, Ma and Robinson (2017).

However, authors control what they report. Some authors do not report the full set of OLS estimates that correspond to their IV estimates, which reduces J and therefore the statistical power of the meta-regression. In addition, some authors may *selectively* report their OLS and IV estimates, which both reduces the statistical power of the meta-regression and potentially introduces selection bias. On the other hand, if researchers are using critical values that are too low (Heath et al., 2023), then the selection bias may be less severe; however, selecting on statistical significance also implies selecting on sampling variation, which biases the corrections for sampling variation, making the net effect of this selection on the meta-regression ambiguous.

Moreover, researchers do not typically report the full variance-covariance matrix of their OLS and IV estimates. Given the underlying data that produced the IV and OLS estimates,

it is trivial to compute $\hat{\rho}^2$ and $\hat{q}$ using the variance-covariance matrix, which accounts for correlated sampling variation across j. If only the reported coefficients and standard errors are available, we can approximate the adjustments for sampling variation by setting $\mathbb{E}\left[\left(\varsigma_j^{\text{IV}}\right)^2\right] = \mathbb{E}\left[se\left(\hat{\beta}_j^{\text{IV}}\right)^2\right]$, and setting $\hat{q}$ to the midpoint of the following bounds.

Corollary 4. *For* $cov\left(\hat{\beta}_j^{OLS}, \hat{\beta}_j^{IV}\right) > 0$, $\hat{q}$ *satisfies*

$$\hat{q} \in [\hat{q}_-, \hat{q}_+] \tag{17}$$

where $\hat{q}_-$ *and* $\hat{q}_+$ *are defined in Appendix A.*

It is possible for the bounds in Corollary 4 to be infeasible. Indeed, in Appendix E.3 we find that $\hat{q}_- > \hat{q}_+$ when we run the meta-regression on ten OLS-IV pairs in Duranton, Morrow and Turner (2014); in that paper, the OLS standard errors are slightly larger than the IV standard errors, even though the OLS coefficients are smaller than their IV counterparts.

3.3 Caveats

Section 2 assumes that the measurement error is classical, y_{ij} does not have a direct causal effect on χ_i, and the treatment effects are homogeneous.

If measurement error is non-classical, then θ in equation (3) is not equal to τ^2 in equation (5), hence θ cannot be interpreted as the R-squared from a hypothetical regression of χ_i on x_i. Furthermore, the measurement invalidity regions in Figure 1 rely on the fact that $\tau^2 \in [0, 1]$; a meta-regression slope coefficient that lies outside of $\left[0, \hat{q}\hat{\rho}^2\right]$ might indicate non-classical measurement error as opposed to a failure of the exclusion restriction. It is worth

noting, however, that θ is still a measure of proxy quality (Pancost and Schaller, 2026), though the resulting measurement error bias may not be attenuative.

If y_{ij} has a direct causal effect on χ_i, then a regression of y_{ij} on χ_i suffers from simultaneity bias. There are, without question, empirical settings in which simultaneity matters; for example, when regressing price (at time t) on quantity (at time t).[4] If y_{ij} has a direct causal effect on χ_i, then it is straightforward to show that β_j and γ_j are mechanically correlated. While Proposition 3 can account for correlation between β_j and γ_j, simultaneity bias makes it difficult to discern the appropriate direction and magnitude of the correlation. Thus, we do not recommend using the meta-regression in settings where simultaneity is a material concern.

If treatment effects are heterogeneous, then the causal effect depends on the observation i, hence β_j should be replaced with β_{ij}. Different estimation procedures may reflect different weighted averages of the causal effects $\left(\beta_{ij}\right)_{i=1}^{N}$, and differences in these weighted averages can drive a wedge between OLS and IV estimates (Imbens and Angrist, 1994; Angrist, Imbens and Rubin, 1996; Mogstad, Torgovitsky and Walters, 2021; Mogstad and Torgovitsky, 2024). As we show in Appendix B, the meta-regression will pick up terms associated with heterogeneous treatment effects, and can therefore be used to gauge the empirical relevance of the local average treatment effects that IV approaches estimate. However, this requires a fundamental departure from the convenience of section 2, especially given the prevalence of continuous treatments (x_i) in economics and finance.

[4] A common setting in finance that does *not* involve simultaneity is as follows. Suppose a regressor χ_{it} is chosen in anticipation of a future outcome $y_{i,t+1,j}$. The future cannot affect the past, hence $y_{i,t+1,j}$ cannot affect χ_{it}; however, $\mathbb{E}_t\left[y_{i,t+1,j}\right]$ might affect both $y_{i,t+1,j}$ and χ_{it} jointly. In this case, a regression of $y_{i,t+1,j}$ on χ_{it} suffers from omitted variable bias, not simultaneity bias, and the meta-regression is still valid.

4 Simulations

In this section, we conduct a simulation exercise to illustrate the performance of the meta-regression. We parameterize our simulations to match Mian and Sufi (2014), which is the first paper that we discuss in section 5. Across all simulations, we set the true proxy quality to the value we estimate in section 5.1: $\tau^2 = 0.445$. For each simulation s, we draw $\beta_{j,s}$ and $\gamma_{j,s}$ from mutually independent normal distributions to match the observed mean and variance of $\hat{\gamma}_j$ and $\hat{\beta}_j^{\text{IV}}$. We also draw clusters of the proxy, instrument, and weight variables from the actual data. We then generate an arbitrary number of outcome variables, which lets us implement the meta-regression for different meta-regression sample sizes, i.e., for different numbers of OLS-IV regression pairs. The simulated outcome variables are exposed to both cluster-level and unit-level shocks, with a correlation structure that we estimate to match the outcome variables in Mian and Sufi (2014). Appendix C details the construction of these variables and shocks, and shows similar results using the investment-q data.

In each simulation s, we use the drawn proxy, instrument, and weight variables from Mian and Sufi (2014), alongside our simulated outcomes, to estimate $\left(\hat{\beta}_{j,s}^{\text{OLS}}, \hat{\beta}_{j,s}^{\text{IV}}\right)$. We then run meta-regressions according to Proposition 2 to estimate proxy quality $\hat{\tau}_s^2$, average omitted variable bias $\hat{\mu}_{\gamma,s}$, and the omitted variable biases for each outcome $\hat{\gamma}_{j,s}$. We also run the restricted meta-regression of Proposition 4 to estimate $\hat{\tau}_{R,s}^2$. Table 1 then reports statistics across all simulations s, both with and without omitted variable bias; Table 2 reports simulation results when the instruments are invalid.

The meta-regression performs well in our baseline simulation. Panel A of Table 1 reports the baseline simulation results, where each column reports a different number of observed

OLS-IV pairs J. The meta-regression estimates of proxy quality $\hat{\tau}^2$ and average omitted variable bias $\hat{\mu}_\gamma$ exhibit low median bias for all J, and the median absolute deviation (MAD) decreases uniformly as J increases. The same is true for the individual omitted variable biases $\hat{\gamma}_j$, although the individual estimates $\hat{\gamma}_j$ remain noisier than the average estimate $\hat{\mu}_\gamma$. Note also that the percentage of $\hat{\mu}_\gamma$ and $\hat{\gamma}_j$ estimates that are the correct sign increases uniformly as J increases. The coverage is close to 95% across all columns.

As expected, the restricted meta-regression estimator $\hat{\tau}^2_R$ performs poorly in the presence of omitted variable bias, as evidenced by Panel A of Table 1; in Panel B we conduct simulations in which there is no omitted variable bias, and the performance of $\hat{\tau}^2_R$ greatly improves. Note that both the restricted and unrestricted meta-regression estimators are consistent in the simulations in Panel B; both estimators have a similar median bias, although the MAD is slightly lower for the restricted estimator.

The simulations in Table 2 allow for instrument invalidity. In Panel A we allow for economic invalidity, which biases the estimates of omitted variable bias. But as can be seen from equation (16), the unrestricted meta-regression still performs well in identifying τ^2 even when $\delta_{j0} \neq 0$; indeed, Panel A shows negligible median bias for $\hat{\tau}^2$, although the coverage is slightly worse than in Table 1. Panel B reports simulation results for measurement invalidity, which biases the estimated proxy quality more, and the estimated omitted variable bias less, as J increases. In both panels, the restricted estimator is consistently wrong due to the presence of instrument invalidity and omitted variable bias.

Panel A: Baseline						
	(1)	(2)	(3)	(4)	(5)	(6)
Median Bias ($\hat{\tau}^2$)	0.004	0.007	0.009	0.011	0.010	0.009
MAD ($\hat{\tau}^2$)	0.138	0.122	0.112	0.095	0.074	0.065
Coverage ($\hat{\tau}^2$)	0.948	0.950	0.948	0.950	0.949	0.949
Out of Range ($\hat{\tau}^2$)	0.118	0.080	0.062	0.031	0.002	0.000
Median Bias ($\hat{\tau}^2_{\mathrm{R}}$)	0.085	0.085	0.084	0.082	0.083	0.082
MAD ($\hat{\tau}^2_{\mathrm{R}}$)	0.140	0.131	0.124	0.113	0.103	0.099
Coverage ($\hat{\tau}^2_{\mathrm{R}}$)	0.896	0.894	0.895	0.891	0.874	0.870
Out of Range ($\hat{\tau}^2_{\mathrm{R}}$)	0.080	0.058	0.043	0.019	0.002	0.000
Median Bias ($\hat{\mu}_\gamma$)	-0.001	-0.002	-0.003	-0.003	-0.003	-0.003
MAD ($\hat{\mu}_\gamma$)	0.042	0.037	0.034	0.028	0.022	0.019
Coverage ($\hat{\mu}_\gamma$)	0.944	0.948	0.947	0.947	0.948	0.948
Same Sign ($\hat{\mu}_\gamma$)	0.786	0.808	0.817	0.863	0.925	0.953
Median Bias ($\hat{\gamma}_j$)	-0.001	-0.002	-0.003	-0.003	-0.003	-0.003
MAD ($\hat{\gamma}_j$)	0.059	0.056	0.054	0.051	0.048	0.047
Coverage ($\hat{\gamma}_j$)	0.945	0.944	0.942	0.938	0.932	0.931
Same Sign ($\hat{\gamma}_j$)	0.712	0.718	0.721	0.735	0.748	0.753
Number of Outcomes J	5	6	7	10	20	30
Panel B: No Omitted Variable Bias						
	(1)	(2)	(3)	(4)	(5)	(6)
Median Bias ($\hat{\tau}^2$)	0.010	0.015	0.016	0.017	0.013	0.013
MAD ($\hat{\tau}^2$)	0.117	0.106	0.097	0.083	0.066	0.059
Coverage ($\hat{\tau}^2$)	0.963	0.961	0.962	0.958	0.957	0.957
Out of Range ($\hat{\tau}^2$)	0.106	0.078	0.060	0.025	0.001	0.000
Median Bias ($\hat{\tau}^2_{\mathrm{R}}$)	0.014	0.014	0.014	0.015	0.011	0.010
MAD ($\hat{\tau}^2_{\mathrm{R}}$)	0.093	0.085	0.082	0.072	0.061	0.056
Coverage ($\hat{\tau}^2_{\mathrm{R}}$)	0.945	0.947	0.946	0.944	0.946	0.944
Out of Range ($\hat{\tau}^2_{\mathrm{R}}$)	0.051	0.040	0.028	0.009	0.000	0.000
Median Bias ($\hat{\mu}_\gamma$)	-0.002	-0.003	-0.003	-0.004	-0.004	-0.003
MAD ($\hat{\mu}_\gamma$)	0.035	0.031	0.029	0.024	0.019	0.017
Coverage ($\hat{\mu}_\gamma$)	0.966	0.965	0.965	0.959	0.953	0.949
Median Bias ($\hat{\gamma}_j$)	-0.002	-0.003	-0.004	-0.004	-0.003	-0.003
MAD ($\hat{\gamma}_j$)	0.053	0.052	0.051	0.049	0.046	0.046
Coverage ($\hat{\gamma}_j$)	0.951	0.947	0.944	0.939	0.932	0.930
Number of Outcomes J	5	6	7	10	20	30

Table 1. Baseline Simulation Results

The table shows results from the simulation exercise. The median bias and median absolute deviation (MAD) are reported in levels. Coverage is the probability that the true parameter lies within the 95% confidence interval. Out of Range is the probability that the estimate lies outside of $[0, 1]$. Same Sign is the probability that the estimate has the same sign as the true parameter; in the absence of omitted variable bias, $\mu_\gamma = \gamma_j = 0$ for all j, hence we do not include this probability in Panel B.

Panel A: Economic Invalidity						
	(1)	(2)	(3)	(4)	(5)	(6)
Median Bias ($\hat{\tau}^2$)	-0.037	-0.037	-0.032	-0.033	-0.035	-0.035
MAD ($\hat{\tau}^2$)	0.145	0.129	0.119	0.102	0.077	0.068
Coverage ($\hat{\tau}^2$)	0.925	0.927	0.929	0.926	0.922	0.922
Out of Range ($\hat{\tau}^2$)	0.120	0.088	0.063	0.025	0.003	0.000
Median Bias ($\hat{\tau}^2_{\mathrm{R}}$)	-0.126	-0.125	-0.125	-0.123	-0.123	-0.121
MAD ($\hat{\tau}^2_{\mathrm{R}}$)	0.130	0.127	0.128	0.124	0.123	0.122
Coverage ($\hat{\tau}^2_{\mathrm{R}}$)	0.682	0.674	0.670	0.663	0.657	0.657
Out of Range ($\hat{\tau}^2_{\mathrm{R}}$)	0.009	0.004	0.002	0.001	0.000	0.000
Median Bias ($\hat{\mu}_\gamma$)	-0.120	-0.120	-0.122	-0.122	-0.121	-0.120
MAD ($\hat{\mu}_\gamma$)	0.135	0.130	0.128	0.123	0.121	0.120
Coverage ($\hat{\mu}_\gamma$)	0.754	0.723	0.694	0.603	0.408	0.304
Same Sign ($\hat{\mu}_\gamma$)	0.258	0.227	0.203	0.159	0.085	0.057
Median Bias ($\hat{\gamma}_j$)	-0.120	-0.121	-0.121	-0.122	-0.121	-0.121
MAD ($\hat{\gamma}_j$)	0.140	0.135	0.133	0.128	0.124	0.123
Coverage ($\hat{\gamma}_j$)	0.781	0.763	0.747	0.705	0.632	0.601
Same Sign ($\hat{\gamma}_j$)	0.386	0.376	0.369	0.361	0.354	0.354
Number of Outcomes J	5	6	7	10	20	30
Panel B: Measurement Invalidity						
	(1)	(2)	(3)	(4)	(5)	(6)
Median Bias ($\hat{\tau}^2$)	0.071	0.131	0.174	0.277	0.433	0.503
MAD ($\hat{\tau}^2$)	0.620	0.642	0.646	0.651	0.679	0.686
Coverage ($\hat{\tau}^2$)	0.973	0.974	0.974	0.976	0.973	0.969
Out of Range ($\hat{\tau}^2$)	0.571	0.584	0.582	0.585	0.595	0.604
Median Bias ($\hat{\tau}^2_{\mathrm{R}}$)	0.322	0.374	0.412	0.499	0.675	0.730
MAD ($\hat{\tau}^2_{\mathrm{R}}$)	0.851	0.861	0.873	0.884	0.918	0.919
Coverage ($\hat{\tau}^2_{\mathrm{R}}$)	0.928	0.928	0.925	0.924	0.911	0.890
Out of Range ($\hat{\tau}^2_{\mathrm{R}}$)	0.677	0.686	0.688	0.697	0.738	0.763
Median Bias ($\hat{\mu}_\gamma$)	0.053	0.050	0.047	0.044	0.037	0.033
MAD ($\hat{\mu}_\gamma$)	0.087	0.082	0.079	0.072	0.061	0.055
Coverage ($\hat{\mu}_\gamma$)	0.932	0.936	0.936	0.939	0.927	0.923
Same Sign ($\hat{\mu}_\gamma$)	0.823	0.829	0.833	0.838	0.847	0.855
Median Bias ($\hat{\gamma}_j$)	0.051	0.049	0.046	0.043	0.034	0.030
MAD ($\hat{\gamma}_j$)	0.129	0.130	0.130	0.129	0.127	0.124
Coverage ($\hat{\gamma}_j$)	0.922	0.921	0.920	0.922	0.930	0.934
Same Sign ($\hat{\gamma}_j$)	0.691	0.688	0.685	0.681	0.671	0.670
Number of Outcomes J	5	6	7	10	20	30

Table 2. Simulation results with invalidity

The table shows results from the simulation exercise. The median bias and median absolute deviation are reported in levels. Coverage is the probability that the true parameter lies within the 95% confidence interval. Out of Range is the probability that the estimate lies outside of $[0, 1]$. Same Sign is the probability that the estimate has the same sign as the true parameter. In Panel A, we set $\mu_\delta = 0.3$ and $\sigma_\delta = 0.1$. In Panel B, we set $\delta_1 = 0.7$.

5 Meta-regression in Practice

In this section we illustrate the meta-regression in three different finance applications. First, we run a meta-regression on the OLS and IV regressions reported in Mian and Sufi (2014), a highly cited paper in finance for which we have replication data and code.[5] Second, we estimate standard investment-q regressions and compare the meta-regression estimate of τ^2 to other estimators proposed in the literature. Third, we apply the meta-regression to the reported estimates of Adelino, Ma and Robinson (2017), for which we do not have replication code and data, in order to illustrate the meta-regression's usefulness for referee reports and discussions. See also Appendix E for two other example applications of the meta-regression.

5.1 Mian and Sufi (2014)

Mian and Sufi (2014) are interested in establishing a causal relationship between county-level housing net worth and employment during the Great Recession from 2007 to 2009. Our meta-regression point estimates suggest that if the Saiz instrument is valid, then measurement error in housing net worth is substantial, while omitted variable bias is negligible.

Mian and Sufi regress county-level employment growth on growth in housing net worth, though they note that OLS estimates will likely be biased upwards due to omitted variables:

> [Growth in housing net worth] may be spuriously correlated with supply-side industry-specific shocks that impact both employment and housing net worth. In particular, certain industries may be harder hit during the recession, and counties with greater exposure to these industries may naturally experience both

[5] We thank Amir Sufi for making the replication code and data available on his website.

a larger decline in housing net worth and larger fall in employment. (page 2207)

To combat this omitted variable bias, Mian and Sufi use the Saiz (2010) housing supply elasticity as an instrument for growth in housing net worth.

The quote above suggests that the authors are worried about positive omitted variable bias: $\gamma > 0$. Absent measurement error, equation (3) implies that we should then observe IV estimates that are *smaller* than their OLS counterparts. Yet, among the ten OLS-IV pairs reported in Tables III and IV of Mian and Sufi, seven of the IV estimates are *larger* than their OLS counterparts.[6] Measurement error can explain this discrepancy. If Mian and Sufi's proxy for growth in housing net worth is subject to measurement error, then Proposition 2 implies that the average omitted variable bias is identified by the intercept of the meta-regression; crucially, this meta-regression intercept, and therefore the average omitted variable bias, is not equal to the average difference between the OLS and IV coefficients.

Our meta-regression estimates suggest that measurement error bias is quantitatively more important than omitted variable bias in this setting. Table 3 reports the six OLS-IV pairs in the first two columns, and the meta-regression estimates of the omitted variable bias and proxy quality in the last two columns.[7] Figure 2 plots the six OLS-IV pairs against each other for comparison. The estimated value of $\tau^2 = 0.445$ implies a signal-to-noise ratio of about 0.8, while the average omitted variable bias is negligible and not statistically significant. Even the individual estimated γ_j are small and, in most cases, statistically insignificant. For example, consider the estimated elasticity of employment growth to housing net worth

[6] In the analysis below, we omit the four OLS-IV pairs reported in Tables III and IV that include different control variables or are run on different samples.

[7] The IV regressions Mian and Sufi report in Table IV are run on smaller samples than their OLS counterparts due to missing values of the Saiz instrument. We re-estimate their OLS regressions on the same sample as their IV estimates; our results are nearly identical if we instead use the coefficients reported in their paper.

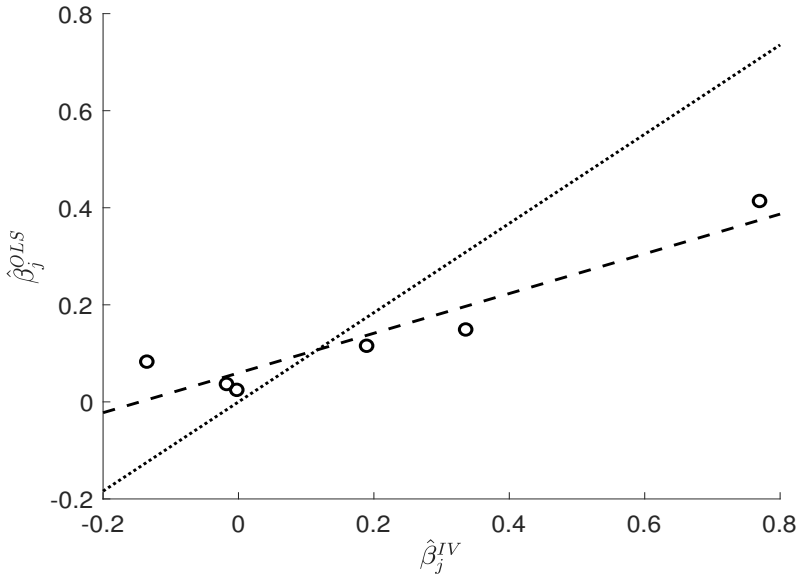


Figure 2. Meta-regression for Mian and Sufi (2014)
The figure plots $\hat{\beta}_j^{\text{OLS}}$ against $\hat{\beta}_j^{\text{IV}}$ for six OLS-IV regression pairs reported in Table IV of Mian and Sufi (2014), where x_i is a proxy for county-level growth in housing net worth and each y_{ij} is a measure of county-level employment growth. The dashed line corresponds to the meta-regression, which has an intercept of $\hat{a} = 0.059$, slope of $\hat{b} = 0.409$, and coefficient of determination of $\hat{r}^2 = 0.885$. The dotted line has a slope of $\hat{q}\hat{\rho}^2 = 0.919$.

at large firms. In the absence of measurement error, the implied OLS coefficient would be $0.770 + 0.071 = 0.841$, very close to the actual IV estimate (0.770); in the absence of omitted variable bias, the implied OLS coefficient would be $0.770 \times 0.445 = 0.343$, very close to the actual OLS estimate (0.414). The meta-regression estimates are robust to correlation between β_j and γ_j: even a correlation between β_j and γ_j as high as 0.9 has little effect on these results, as shown in Figure 3.

Meta-regression can also be used to quantitatively analyze instrument invalidity. Following Proposition 5, if the average omitted variable bias differs from 0.053, then it must be the case that the instrument suffers from economic invalidity; likewise, if the proxy quality differs from 0.445, then it must be the case that the instrument suffers from measurement invalidity. While Saiz (2010) and Davidoff (2016) argue that the Saiz instrument suffers

Dependent Variable	Estimated β		Meta-Regression	
	OLS	IV	γ_j	τ^2
Average	0.137	0.190	0.053	0.445
	(0.034)	(0.083)	(0.039)	(0.128)
Emp Growth (1 to 4)	0.083	-0.135	0.143	
	(0.024)	(0.145)	(0.062)	
Emp Growth (5 to 9)	0.025	-0.003	0.026	
	(0.037)	(0.125)	(0.035)	
Emp Growth (10 to 19)	0.037	-0.018	0.045	
	(0.049)	(0.108)	(0.030)	
Emp Growth (20 to 49)	0.116	0.189	0.031	
	(0.032)	(0.086)	(0.041)	
Emp Growth (50 to 99)	0.149	0.336	-0.000	
	(0.102)	(0.188)	(0.075)	
Emp Growth (100+)	0.414	0.770	0.071	
	(0.062)	(0.206)	(0.066)	

Table 3. Mian and Sufi (2014) Meta-Regression Results

The table reports meta-regression results for six OLS-IV regressions from Table IV of Mian and Sufi (2014). The first row reports averages across the dependent variable results reported in subsequent rows. Each subsequent row reports results for a different dependent variable. The first two columns report the estimated β from estimating their regressions via OLS and IV, respectively. Column 3 reports the estimated omitted variable bias γ_j from Corollary 2. Column 4 reports the estimated proxy quality τ^2 from Proposition 2. Standard errors are reported below each estimate in parentheses (Appendix D.1). The results in this table use only the 531 observations that are common to all six OLS and six IV regressions reported in Panels A and B of Table IV of Mian and Sufi (2014), and thus the OLS and IV estimates here differ slightly from the printed estimates.

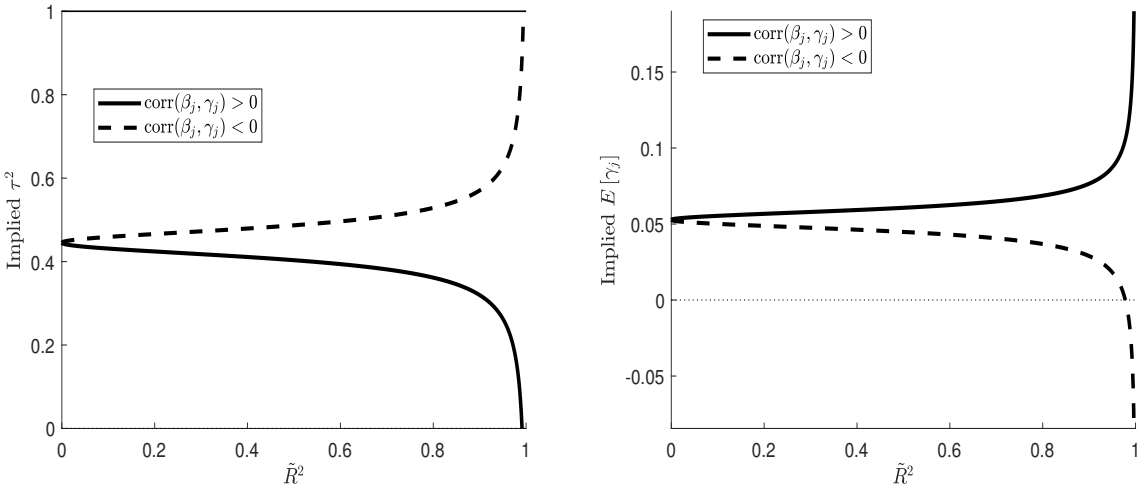


Figure 3. Proposition 3 in Mian and Sufi (2014)
The figure plots the values of τ^2 (left panel) and $\mathbb{E}\left[\gamma_j\right]$ (right panel) as a function of $\tilde{R}^2$, from Proposition 3, for six OLS-IV pairs reported in Table IV of Mian and Sufi (2014). The solid lines plot the values when γ_j and β_j are positively correlated, and the dashed lines plot the values when γ_j and β_j are negatively correlated. In the right panel we set the y-axis scale to be twice the average OLS coefficient.

from economic invalidity due to omitted demand factors, we are the first to highlight the potential for measurement invalidity in this instrument.

The left panel of Figure 4 plots the relationship between economic invalidity $\mathbb{E}\left[\delta_{j0}\right]$ and omitted variable bias $\mathbb{E}\left[\gamma_j\right]$; the right panel plots the relationship between measurement invalidity δ_1 and proxy quality τ^2. Relative to the baseline estimates, the higher the proxy quality, the worse the measurement invalidity; the higher the omitted variable bias, the worse the economic invalidity. In both cases, the IV estimates are on average biased upwards. If the omitted variable bias or proxy quality is lower than the values implied by Proposition 2, then this again implies instrument invalidity, but the resulting IV estimates will instead be biased downwards.

Reasonable values of the average omitted variable bias or proxy quality imply that, on average, the OLS estimates are closer to the truth than the IV estimates. Figure 5 plots

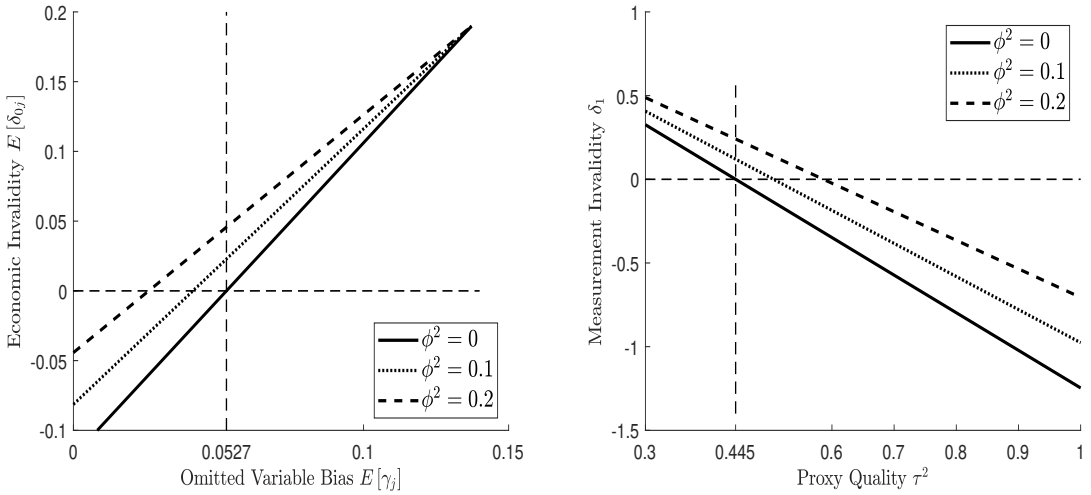


Figure 4. Instrument Invalidity in Mian and Sufi (2014)

The figure plots the implied values of average economic invalidity $\mathbb{E}\left[\delta_{j0}\right]$ and measurement invalidity δ_1 from equation (16) for Mian and Sufi (2014). The left panel plots $\mathbb{E}\left[\delta_{j0}\right]$ as a function of $\mathbb{E}\left[\gamma_j\right]$, and the right panel plots the value of δ_1 as a function of τ^2. The vertical dashed lines correspond to the baseline estimates from Proposition 2. In the right panel, each line ends at the maximum value of τ^2 implied by equation (21).

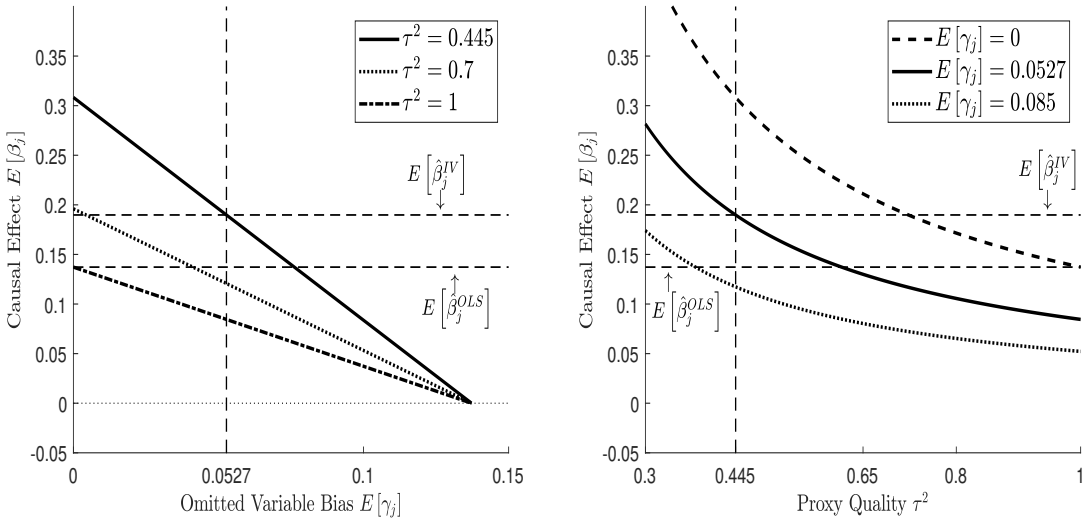


Figure 5. Average Causal Effect in Mian and Sufi (2014)

The figure plots the implied average causal effect $\mathbb{E}\left[\beta_j\right]$ when the instrument is invalid for Mian and Sufi (2014). The left panel plots the value of $\mathbb{E}\left[\beta_j\right]$ as a function of $\mathbb{E}\left[\gamma_j\right]$ given τ^2, and the right panel plots the value of $\mathbb{E}\left[\beta_j\right]$ as a function of τ^2 given $\mathbb{E}\left[\gamma_j\right]$. The pair of horizontal dashed lines report the average values of $\hat{\beta}_j^{\text{IV}}$ and $\hat{\beta}_j^{\text{OLS}}$, respectively. The vertical dashed lines correspond to the baseline estimates from Proposition 2.

the average causal effect $\mathbb{E}\left[\beta_j\right]$ as a function of both omitted variable bias $\mathbb{E}\left[\gamma_j\right]$ and proxy quality τ^2. The OLS estimates are closer to the true causal effect even for a minuscule amount of omitted variable bias, as low as 0.06 at our baseline estimates of τ^2. Likewise, increasing proxy quality above 0.445 moves the true causal effect closer to the average OLS estimate, no matter the value of the omitted variable bias.

This analysis demonstrates a surprising and counterintuitive result: if the omitted variable bias is quantitatively important, then it is the *OLS* estimates that are less biased than the IV estimates. In fact, the greater the omitted variable bias, the worse the IV estimator performs relative to the OLS estimator. In other words, Propositions 2 and 5 together imply that:

1. If the Saiz instrument is valid, then our meta-regression implies that measurement error is substantial, while omitted variable bias is negligible.

2. If the omitted variable bias is *not* negligible, then the Saiz instrument is invalid, and the OLS estimates are less biased than the IV estimates.

5.2 Investment-q Regressions

A long literature in finance going back to at least Fazzari, Hubbard and Petersen (1988) has regressed measures of investment on Tobin's q and cash flow; in a seminal contribution, Erickson and Whited (2000) argued that measurement error in q explained many of the empirical facts in this literature. Several papers have estimated τ^2 for q specifically using higher-order moments (Erickson and Whited, 2002, 2005, 2012) and cumulants (Erickson, Jiang and Whited, 2014; Peters and Taylor, 2017). In this section we compare these estimates

Variable	Mean	Std Dev	p10	p50	p90	Skewness
Total q	0.962	1.502	-0.023	0.567	2.185	3.996
Total Investment	0.167	0.130	0.051	0.138	0.308	2.748
Total Cash Flow	0.134	0.167	-0.032	0.131	0.299	0.165
Acquisitions	0.029	0.099	0.000	0.000	0.069	5.351
Dividends	0.014	0.028	0.000	0.000	0.037	4.084
Net Equity Iss	0.023	0.146	-0.035	0.000	0.066	6.678
Net LT Debt Iss	0.023	0.138	-0.058	0.000	0.116	3.726

Table 4. Investment-q Summary Statistics

The table reports summary statistics for our sample of 151,030 annual observations of 14,466 unique Compustat firms from 1978 to 2024. The denominator for all variables is lagged Total Capital K^{tot} from Peters and Taylor (2017). The first three rows report Total q, Total Investment, and Total Cash Flow, respectively, from Peters and Taylor (2017). The last four rows report Acquisitions (`aqc`), Dividends (`dvc`+`dvp`), Net Equity Issuance (`sstk`−`prstkc`), and Net Long-Term Debt Issuance (`dltis`−`dltr`), respectively. We replace missing values for all numerators with zeroes.

to what we find with a meta-regression.

We begin with an annual sample of Compustat firms from 1978 to 2024. We drop firms with SIC codes from 4900–4999 (regulated utilities), 6000–6999 (financial firms), and greater than 9000 (firms categorized as public service, international affairs, or non-operating establishments). We drop firms with negative or missing values of assets or sales, and firms with less than $5 million in gross property, plant, and equipment (using 2017 dollars, according to the BEA's gross private domestic investment deflator). Our final sample consists of 151,030 annual observations of 14,466 unique firms. Table 4 reports summary statistics for this sample.

Following Peters and Taylor (2017), we estimate regressions of the form

$$y_{it} = q_{i,t-1}\beta + c_{it}\alpha + \delta_i + u_{it} \tag{18}$$

where q_{it} and c_{it} are Total q and Total Cash Flow at firm i in year t, y_{it} is a proxy for investment at firm i in year t, and δ_i is a firm fixed effect.[8,9] To construct an instrumental-variables version of equation (18), we follow Almeida, Campello and Galvao, Jr (2010) and Erickson and Whited (2012) and eliminate the firm fixed effects δ_i by differencing. We then use thrice-lagged q in levels as an instrument for the lagged difference in q:

$$\Delta y_{it} = \Delta \hat{q}_{i,t-1}\beta + \Delta c_{it}\alpha + \nu_{it} \tag{19}$$

where $\Delta \hat{q}_{i,t-1}$ is the fitted value from the first stage regression:

$$\Delta q_{i,t-1} = \gamma_0 + q_{i,t-3}\gamma_1 + \Delta c_{it}\gamma_2 + \xi_{i,t-1}. \tag{20}$$

We use $q_{i,t-3}$ in equation (20) to ensure that the instrument is not mechanically correlated with the measurement error in $\Delta q_{i,t-1} = q_{i,t-1} - q_{i,t-2}$. More broadly, validity requires $q_{i,t-3}$ to be uncorrelated with both the differenced measurement error and the differenced structural disturbance.

To implement the meta-regression, we estimate equation (19) using both OLS and IV for multiple dependent variables Δy_{it}. The standard dependent variable in the literature on measurement error in Tobin's q is investment, and so the first variable we use is Total Investment from Peters and Taylor (2017), which includes investment in intangibles. In order to generate the variation in $\hat{\beta}_j^{\text{IV}}$ needed to identify τ^2 from a meta-regression, we construct other variables in Compustat which can also be interpreted as forms of corporate

[8] We thank Luke Taylor for making these data available on his website.

[9] As in Erickson and Whited (2012) and Erickson, Jiang and Whited (2014), we accommodate the fixed effects in estimation by applying the within transformation to the data.

investment (or disinvestment): acquisitions, dividends, and net debt and equity issuance. These variables are simple to construct in Compustat and often appear in regressions like equation (18) (Hovakimian, Opler and Titman, 2001; Gatchev, Pulvino and Tarhan, 2010; Lewellen and Lewellen, 2016).

After estimating equation (19) via OLS and IV for each dependent variable, we apply Proposition 2 and Corollary 2 to the estimated OLS and IV coefficients and standard errors to estimate proxy quality τ^2 and the omitted variable bias γ_j for each dependent variable. We report the results of this exercise in Table 5, which also reports the estimated values of β and τ^2 for each dependent variable using a third-order cumulant estimator in columns 3 and 4, respectively.[10] These estimators assume that there is no omitted variable bias; while the unrestricted meta-regression allows us to test this assumption, we also estimate a restricted meta-regression (Proposition 4) that imposes this assumption.

Figure 6 plots the five OLS-IV pairs reported in the first two columns of Table 5. The top row of the table reports meta-regression results and averages across the five regressions, and subsequent rows report results that pertain to each dependent variable.[11] The first three columns report estimates of β from OLS, IV, and cumulant regressions, respectively. The fourth column reports estimates of τ^2 assuming no omitted variable bias; the top row reports the restricted meta-regression estimate, and subsequent rows report the cumulant estimate for each outcome.[12] The last two columns report estimates of omitted variable bias γ_j and

[10] The 3rd-order cumulant estimator is identical to the classic 3rd-order moments estimator of Geary (1942); at higher orders, cumulant estimators are superior to moment estimators because they can be computed in closed form (Erickson, Jiang and Whited, 2014).

[11] The results in Table 5 are similar if we use longer lags of $q_{i,t-h}$ as instruments for $\Delta q_{i,t-1}$, if we include cumulants of order higher than three, or if we include the change in cash holdings as an additional outcome variable.

[12] In the notation of Erickson, Jiang and Whited (2014), we compute $\tau^2 = \mathbb{E}\left[\eta^2\right] / \left(\mathbb{E}\left[\eta^2\right] + \mathbb{E}\left[\varepsilon^2\right]\right)$, which partials out the variation in control variables that appears in their equation (35).

Dependent Variable	Estimated β			$\gamma_j = 0$	$\gamma_j \neq 0$	
	OLS	IV	EJW	τ^2	γ_j	τ^2
Average	0.011	0.022	0.456	0.441†	0.002	0.394
	(0.000)	(0.001)		(0.020)	(0.004)	(0.154)
Total Investment	0.027	0.054	0.077	0.400	0.006	
	(0.001)	(0.001)	(0.005)	(0.029)	(0.008)	
Acquisitions	0.009	0.008	0.195	0.033	0.006	
	(0.001)	(0.001)	(0.028)	(0.004)	(0.001)	
Dividends	0.001	0.000	0.028	0.052	0.000	
	(0.000)	(0.000)	(0.006)	(0.012)	(0.000)	
Net Equity Iss	0.005	0.032	1.707	0.014	-0.007	
	(0.001)	(0.002)	(1.673)	(0.014)	(0.005)	
Net LT Debt Iss	0.010	0.016	0.274	0.039	0.004	
	(0.001)	(0.002)	(0.035)	(0.005)	(0.003)	

Table 5. Investment-q Meta-Regression Results

The table reports meta-regression results for the investment-q regressions in equations (18) and (19). The first row reports averages across the dependent variable results reported in subsequent rows, apart from the result marked with † which reports the restricted meta-regression estimate of τ^2 from Proposition 4. Each subsequent row reports results for a different dependent variable. The first two columns report the estimated β from estimating equation (19) via OLS and IV, respectively. The third column reports the estimated β from estimating equation (18) using the 3rd-order cumulant method of Erickson, Jiang and Whited (2014). The top row of column 4 reports the estimated τ^2 from the restricted meta-regression estimator; subsequent rows of column 4 report the τ^2 from the 3rd-order cumulant estimator. Column 5 reports the estimated omitted variable bias γ_j from Corollary 2. Column 6 reports the estimated proxy quality τ^2 from Proposition 2. Standard errors are reported below each estimate in parentheses (Appendix D.1).

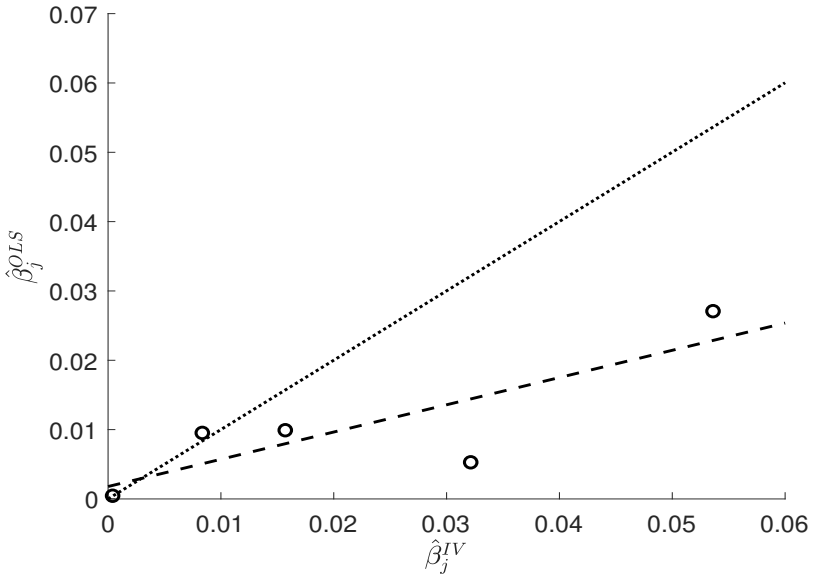


Figure 6. Scatterplot for Investment-q Regressions

The figure plots $\hat{\beta}_j^{\text{OLS}}$ against $\hat{\beta}_j^{\text{IV}}$ for the five OLS-IV regression pairs reported in the first two columns of Table 5. The dashed line corresponds to the meta-regression, which has an intercept of $\hat{a} = 0.002$, slope of $\hat{b} = 0.393$, and coefficient of determination of $\hat{r}^2 = 0.685$. The dotted line has a slope of $\hat{q}\hat{\rho}^2 = 0.997$.

proxy quality τ^2, respectively, from the unrestricted meta-regression.

The meta-regression estimates of τ^2 are similar to what one obtains from a cumulant estimator using Total Investment, and we cannot reject the hypothesis of no omitted variable bias on average, although both of these conclusions may be due to lack of power. Both the restricted and unrestricted estimates of τ^2 reported in Table 5 are similar to each other, and to the cumulant estimator of τ^2 for Total Investment, which is the outcome variable of interest in the literature using higher-order moment estimators (Erickson and Whited, 2012; Peters and Taylor, 2017); however, it is worth noting that this is a consistency check, and not a test of the cumulant estimator's assumptions. Moreover, the OLS and IV estimates are in first differences, while the cumulant estimates are in levels after a within transformation, which is a common feature of this literature (Almeida, Campello and Galvao, Jr, 2010). The cumulant

estimates of τ^2 for the other four outcomes are substantially lower, though the papers which regress dividends, acquisitions, etc. on q have not used those outcomes to measure τ^2.[13] Only the omitted variable biases for Acquisitions and Dividends are statistically significant, although the point estimate of γ_j for Dividends is zero to three decimal places. While the standard error on the unrestricted meta-regression estimate of τ^2 is high, it is the only estimator which accounts for omitted variable bias, and thus we view it as our main measure of proxy quality.

The IV estimates of β in the second column of Table 5 differ substantially from the cumulant estimates in the third column. The difference is more extreme for outcome variables that have not previously been explored with cumulant estimators, such as acquisitions and debt or equity issuance. One explanation could be serial correlation in the measurement error in q (Erickson and Whited, 2012), which would invalidate an instrument with an insufficient number of lags. Alternatively, it may be the case that some outcome variables exhibit conditional heteroskedasticity that induces correlation among higher-order moments, which would invalidate a cumulant estimator of β and τ^2 for those outcomes. A full exploration of these issues is beyond the scope of the present paper.

5.3 Adelino, Ma and Robinson (2017)

For our final example, we apply the meta-regression to Adelino, Ma and Robinson (2017), for which we do not have access to the underlying census data. We therefore rely solely on the reported OLS coefficients, IV coefficients, and standard errors that we infer from the

[13] In Appendix E.1, we report leave-one-out estimates which show that Total Investment is the most influential outcome in the meta-regression. With more outcome variables, outlying outcomes would become less important.

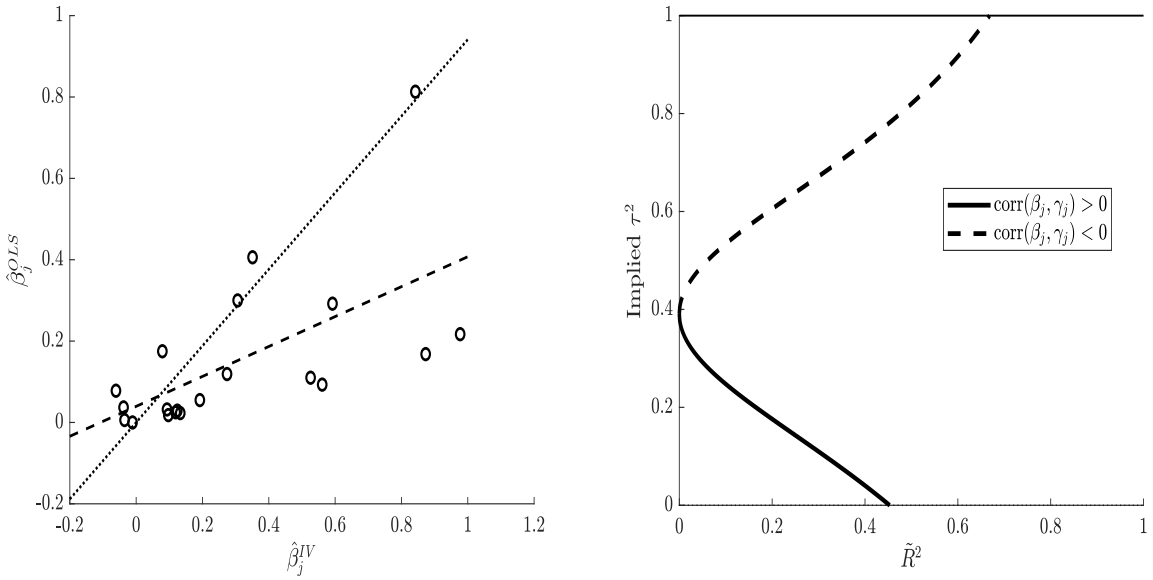


Figure 7. Meta-regression for Adelino, Ma and Robinson (2017)
The left panel plots $\hat{\beta}_j^{\text{OLS}}$ against $\hat{\beta}_j^{\text{IV}}$ for 20 OLS-IV regression pairs reported in Adelino, Ma and Robinson (2017), where x_i is CZ-level income growth and each y_{ij} is a measure of CZ-level job creation and destruction. The dashed line corresponds to the meta-regression, which has an intercept of $\hat{a} = 0.040$, slope of $\hat{b} = 0.368$, and coefficient of determination of $\hat{r}^2 = 0.379$. The dotted line has a slope of $\hat{q}\hat{\rho}^2 = 0.941$. The right panel plots the values of τ^2 as a function of $\tilde{R}^2$, from Proposition 3. The solid lines plot the values when γ_j and β_j are positively correlated, and the dashed lines plot the values when γ_j and β_j are negatively correlated.

t-statistics.

Adelino, Ma and Robinson (2017) estimate the causal impact of shocks to investment opportunities on employment growth by regressing various measures of commuting zone (CZ) non-tradable employment growth, including net job creation, gross job creation, and gross job destruction, both in the aggregate and for firms of various age classes, on CZ-level income growth. They note that regressing measures of employment growth on income growth is problematic because any omitted variable that affects local income growth would also mechanically affect non-tradable employment growth. Therefore, they instrument for income growth using a Bartik (1991) instrument that interacts a CZ's pre-existing exposure to the manufacturing sector with national trends in manufacturing employment.

Adelino, Ma and Robinson are concerned that their OLS estimates will be too large; a positive correlation between employment growth and income growth might primarily reflect omitted variable bias rather than the causal effect of income on employment. And yet, 14 of the 20 IV estimates reported in the paper are larger than their OLS counterparts.[14] Moreover, this difference is substantial: the average IV estimate is over *two times larger* than the average OLS estimate.

Once again, the meta-regression can resolve this puzzle; Proposition 2 implies that the average omitted variable bias is indeed positive, $\mathbb{E}\left[\gamma_j\right] = 0.033$, but their regressor is subject to substantial measurement error, $\tau^2 = 0.391$. Figure 7 plots the OLS-IV pairs and the meta-regression line of best fit for this paper. To compute these estimates, we use equation (17) to bound $\hat{q} \in [0.876, 1.166]$, and take the midpoint of this interval. Note that using any $\hat{q}$ in this range would only vary τ^2 from 0.34 to 0.46; measurement error in this paper's proxy for investment opportunities is substantial. On the other hand, omitted variable bias is not: one of the key estimates of the paper, the elasticity of the net change in employment in new firms to investment opportunities, has an IV coefficient of 0.274; the implied average omitted variable bias is an order of magnitude smaller.

Compared to Mian and Sufi (2014), the meta-regression $\hat{r}^2$ is quite low (0.379 vs. 0.885; compare the left panel of Figure 7 with Figure 2); hence correlation between β_j and γ_j can substantively bias our estimates of τ^2 and $\mathbb{E}\left[\gamma_j\right]$. The right panel of Figure 7 plots the bounds on τ^2 implied by Proposition 3. For example, if γ_j is positively correlated with β_j,

[14] There are 28 pairs of OLS and IV coefficients reported in Adelino, Ma and Robinson (2017), across Tables III–VI. We omit the three OLS-IV pairs in Table IV because they are estimated on a much smaller sample than the others; we omit the five OLS-IV pairs in Panel C of Table V because the dependent variable is the difference between the dependent variables in panels A and B. Results are nearly identical if we include these observations.

it can "explain" at most about 45% of its variation, at which point the true proxy quality is zero. On the other hand, a negative correlation of that magnitude would imply that the true proxy quality would be twice as high as our baseline estimate, though this would also imply that the omitted variable bias is negative, contrary to economic theory.

6 Conclusion

We derive a meta-regression estimator that separates measurement error from the average omitted variable bias, provided the instrument is valid. This estimator can be used whenever different dependent variables are regressed on the same endogenous regressor with the same instrument. Crucially, the covariance between the OLS and IV estimates is what allows us to separate measurement error from the omitted variable bias. By contrast, neither the difference between, nor the ratio of, the OLS and IV estimates will recover the omitted variable bias. The most direct way to measure these biases, and thus understand why OLS and IV estimates differ, is to run a meta-regression.

The meta-regression can also detect when the underlying instrument is invalid. Note that an instrument can be invalid because it is correlated with the omitted variables (economic invalidity) or because it is correlated with the measurement error in the endogenous regressor (measurement invalidity). Economic invalidity is reflected in the meta-regression intercept, while measurement invalidity is reflected in the meta-regression slope, hence in some cases we can distinguish the type of invalidity that affects the instrument. We also show that the meta-regression can be informative about the external validity of the IV estimates when treatment effects are heterogeneous and the IV is a local average treatment effect. Thus, the

meta-regression is a useful diagnostic tool for interpreting IV estimates.

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A Proofs

Proof of Proposition 1

We can relax equation (2) by only requiring it to hold in expectation:

$$\mathbb{E}_j\left[y_{ij}\left|\chi_i\right.\right] = \beta_j\chi_i + \mathbb{E}_j\left[u_{ij}\left|\chi_i\right.\right].$$

An OLS regression of y_{ij} on x_i yields

$$\begin{aligned}\beta_j^{\text{OLS}} &\equiv \frac{\text{cov}_j\left(y_{ij}, x_i\right)}{\text{var}\left(x_i\right)} \\ &= \frac{\text{cov}_j\left(\beta_j\chi_i + u_{ij}, \chi_i + \varepsilon_i\right)}{\text{var}\left(x_i\right)} \\ &= \theta\beta_j + \gamma_j,\end{aligned}$$

where $\theta \equiv \text{cov}\left(x_i, \chi_i\right)/\text{var}\left(x_i\right)$ and $\gamma_j \equiv \text{cov}_j\left(x_i, u_{ij}\right)/\text{var}\left(x_i\right)$. Under classical measurement error, $\theta = \text{var}\left(\chi_i\right)/\text{var}\left(x_i\right) \equiv \tau^2$. Given a valid instrument z_i, the corresponding IV estimator converges to

$$\begin{aligned}\beta_j^{\text{IV}} &\equiv \frac{\text{cov}_j\left(y_{ij}, z_i\right)}{\text{cov}\left(x_i, z_i\right)} \\ &= \beta_j\frac{\text{cov}_j\left(\chi_i, z_i\right)}{\text{cov}\left(\chi_i, z_i\right)} \\ &= \beta_j.\end{aligned}$$

If $\text{cov}\left(\beta_j, \gamma_j\right) = 0$, then the slope of the meta-regression converges to

$$\begin{aligned} b &\equiv \frac{\text{cov}\left(\beta_j^{\text{OLS}}, \beta_j^{\text{IV}}\right)}{\text{var}\left(\beta_j^{\text{IV}}\right)} \\ &= \frac{\text{cov}\left(\tau^2\beta_j + \gamma_j, \beta_j\right)}{\text{var}\left(\beta_j\right)} \\ &= \tau^2, \end{aligned}$$

and the intercept of the meta-regression converges to

$$\begin{aligned} a &\equiv \mathbb{E}\left[\beta_j^{\text{OLS}}\right] - b\mathbb{E}\left[\beta_j^{\text{IV}}\right] \\ &= \tau^2\mathbb{E}\left[\beta_j\right] + \mathbb{E}\left[\gamma_j\right] - \tau^2\mathbb{E}\left[\beta_j\right] \\ &= \mathbb{E}\left[\gamma_j\right]. \end{aligned}$$

□

Proof of Corollary 1

Following Proposition 1: $\beta_j^{\text{OLS}} - b\beta_j^{\text{IV}} = \tau^2\beta_j + \gamma_j - \tau^2\beta_j = \gamma_j$. □

Proof of Proposition 2

The slope from a meta-regression of $\hat{\beta}_j^{\text{OLS}}$ on $\hat{\beta}_j^{\text{IV}}$ converges to

$$\begin{aligned}\hat{b} &\equiv \frac{\text{cov}\left(\hat{\beta}_j^{\text{OLS}}, \hat{\beta}_j^{\text{IV}}\right)}{\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)} \\ &= \tau^2 \frac{\text{var}\left(\beta_j\right)}{\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)} \frac{\text{cov}\left(\hat{\beta}_j^{\text{OLS}}, \hat{\beta}_j^{\text{IV}}\right)}{\text{cov}\left(\beta_j^{\text{OLS}}, \beta_j^{\text{IV}}\right)} \\ &= \tau^2 \hat{q} \hat{\rho}^2,\end{aligned}$$

where $\hat{\rho}^2 \equiv 1 - \frac{\mathbb{E}\left[\left(\varsigma_j^{\text{IV}}\right)^2\right]}{\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)}$ and $\hat{q} \equiv \left(1 - \frac{\mathbb{E}\left[\varsigma_j^{\text{OLS}} \varsigma_j^{\text{IV}}\right]}{\text{cov}\left(\hat{\beta}_j^{\text{OLS}}, \hat{\beta}_j^{\text{IV}}\right)}\right)^{-1}$, and the intercept converges to

$$\begin{aligned}\hat{a} &\equiv \mathbb{E}\left[\hat{\beta}_j^{\text{OLS}}\right] - \hat{b}\mathbb{E}\left[\hat{\beta}_j^{\text{IV}}\right] \\ &= \tau^2 \mathbb{E}\left[\beta_j\right] + \mathbb{E}\left[\gamma_j\right] - \tau^2 \hat{q}\hat{\rho}^2 \mathbb{E}\left[\hat{\beta}_j^{\text{IV}}\right] \\ &= \mathbb{E}\left[\gamma_j\right] + \tau^2\left(1 - \hat{q}\hat{\rho}^2\right)\mathbb{E}\left[\hat{\beta}_j^{\text{IV}}\right].\end{aligned}$$

□

Proof of Corollary 2

Following Proposition 2: $\hat{\beta}_j^{\text{OLS}} - \left(\frac{\hat{b}}{\hat{q}\hat{\rho}^2}\right)\hat{\beta}_j^{\text{IV}} = \gamma_j + \varsigma_j^{\text{OLS}} - \left(\frac{\hat{b}}{\hat{q}\hat{\rho}^2}\right)\varsigma_j^{\text{IV}} \equiv \hat{\gamma}_j$, hence $\hat{\gamma}_j$ is asymptotically normally distributed with a mean of γ_j. □

Statement and Proof of Proposition 2'

We now extend Proposition 2 and Corollary 2 to allow for a vector of perfectly measured control variables $\vec{w}_i$. In this case, we will be referencing the measurement error in the proxy variable after controlling for other factors. It is therefore helpful to define

$$\dot{\tau}^2 \equiv \frac{\text{var}\left(\dot{\chi}_i\right)}{\text{var}\left(\dot{x}_i\right)},$$

where $\dot{\chi}_i$ denotes the residual from a hypothetical regression of χ_i on $\vec{w}_i$, and $\dot{x}_i$ denotes the residual from a regression of x_i on $\vec{w}_i$. Under classical measurement error and instrument validity, if

$$y_{ij} = \alpha_j + \chi_i \beta_j + \vec{w}_i \vec{\Psi}_j + u_{ij},$$

then the relevant OLS estimand from a regression of y_{ij} on x_i and $\vec{w}_i$ is

$$\beta_j^{\text{OLS}} = \dot{\tau}^2 \beta_j^{\text{IV}} + \dot{\gamma}_j,$$

where $\dot{\gamma}_j = \frac{\text{cov}_j\left(\dot{x}_i, \dot{u}_{ij}\right)}{\text{var}(\dot{x}_i)}$ is the omitted variable bias and $\dot{u}_{ij}$ denotes the residual from a hypothetical regression of u_{ij} on $\vec{w}_i$. The meta-regression of $\hat{\beta}_j^{\text{OLS}}$ on $\hat{\beta}_j^{\text{IV}}$ will therefore have a familiar representation.

Proposition 2'. *The meta-regression estimates converge to*

$$\hat{b} = \dot{\tau}^2 \hat{q}\hat{\rho}^2,$$

$$\hat{a} = \mathbb{E}\left[\dot{\gamma}_j\right] + \dot{\tau}^2 \left(1 - \hat{q}\hat{\rho}^2\right) \mathbb{E}\left[\hat{\beta}_j^{IV}\right],$$

provided $cov\left(\beta_j, \dot{\gamma}_j\right) = 0$. *Furthermore, we can consistently estimate* $\dot{\gamma}_j$ *using*

$$\hat{\beta}_j^{OLS} - \left(\frac{\hat{b}}{\hat{q}\hat{\rho}^2}\right) \hat{\beta}_j^{IV} \equiv \hat{\dot{\gamma}}_j.$$

Proof. The proof is analogous to the previous proofs, but in this case, each OLS coefficient converges to

$$\begin{aligned}
\beta_j^{\text{OLS}} &\equiv \frac{\text{cov}_j\left(\dot{y}_{ij}, \dot{x}_i\right)}{\text{var}\left(\dot{x}_i\right)} \\
&= \frac{\text{cov}_j\left(\dot{\chi}_i\beta_j + \dot{u}_{ij}, \dot{\chi}_i + \varepsilon_i\right)}{\text{var}\left(\dot{x}_i\right)} \\
&= \dot{\tau}^2\beta_j + \dot{\gamma}_j,
\end{aligned}$$

while the corresponding IV coefficient converges to

$$\begin{aligned}
\beta_j^{\text{IV}} &\equiv \frac{\text{cov}_j\left(\dot{y}_{ij}, \dot{z}_i\right)}{\text{cov}\left(\dot{x}_i, \dot{z}_i\right)} \\
&= \frac{\text{cov}_j\left(\dot{x}_i\beta_j + \dot{u}_{ij}, \dot{z}_i\right)}{\text{cov}\left(\dot{x}_i, \dot{z}_i\right)} \\
&= \beta_j.
\end{aligned}$$

The slope of the meta-regression estimator therefore converges to

$$\begin{aligned}\hat{b} &\equiv \frac{\operatorname{cov}\left(\hat{\beta}_j^{\mathrm{OLS}}, \hat{\beta}_j^{\mathrm{IV}}\right)}{\operatorname{var}\left(\hat{\beta}_j^{\mathrm{IV}}\right)} \\ &= \frac{\operatorname{cov}\left(\dot{\tau}^2\beta_j + \dot{\gamma}_j, \beta_j\right)}{\operatorname{var}\left(\beta_j\right)}\hat{q}\hat{\rho}^2 \\ &= \dot{\tau}^2\hat{q}\hat{\rho}^2,\end{aligned}$$

and the intercept converges to

$$\begin{aligned}\hat{a} &\equiv \mathbb{E}\left[\hat{\beta}_j^{\mathrm{OLS}}\right] - \mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right]\hat{b} \\ &= \dot{\tau}^2\mathbb{E}\left[\beta_j\right] + \mathbb{E}\left[\dot{\gamma}_j\right] - \dot{\tau}^2\mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right]\hat{q}\hat{\rho}^2 \\ &= \mathbb{E}\left[\dot{\gamma}_j\right] + \dot{\tau}^2\left(1 - \hat{q}\hat{\rho}^2\right)\mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right].\end{aligned}$$

Finally, note that $\hat{\beta}_j^{\mathrm{OLS}} - \left(\frac{\hat{b}}{\hat{q}\hat{\rho}^2}\right)\hat{\beta}_j^{\mathrm{IV}} = \dot{\gamma}_j + \varsigma_j^{\mathrm{OLS}} - \left(\frac{\hat{b}}{\hat{q}\hat{\rho}^2}\right)\varsigma_j^{\mathrm{IV}} \equiv \hat{\dot{\gamma}}_j$, hence $\hat{\dot{\gamma}}_j$ is asymptotically normally distributed with a mean of $\dot{\gamma}_j$. □

Proof of Proposition 3

The meta-regression slope coefficient converges to

$$\begin{aligned}
\hat{b} &\equiv \frac{\operatorname{cov}\left(\hat{\beta}_j^{\mathrm{OLS}}, \hat{\beta}_j^{\mathrm{IV}}\right)}{\operatorname{var}\left(\hat{\beta}_j^{\mathrm{IV}}\right)} \\
&= \frac{\operatorname{cov}\left(\tau^2\beta_j, \beta_j\right) + \operatorname{cov}\left(\gamma_j, \beta_j\right)}{\operatorname{var}\left(\beta_j\right)} \left(\frac{\operatorname{var}\left(\beta_j\right)}{\operatorname{var}\left(\hat{\beta}_j^{\mathrm{IV}}\right)}\right) \left(\frac{\operatorname{cov}\left(\hat{\beta}_j^{\mathrm{OLS}}, \hat{\beta}_j^{\mathrm{IV}}\right)}{\operatorname{cov}\left(\beta_j^{\mathrm{OLS}}, \beta_j^{\mathrm{IV}}\right)}\right) \\
&= \left(\tau^2 + \kappa\right) \frac{\operatorname{var}\left(\beta_j\right)}{\operatorname{var}\left(\hat{\beta}_j^{\mathrm{IV}}\right)} \left(\frac{\operatorname{cov}\left(\hat{\beta}_j^{\mathrm{OLS}}, \hat{\beta}_j^{\mathrm{IV}}\right)}{\operatorname{cov}\left(\beta_j^{\mathrm{OLS}}, \beta_j^{\mathrm{IV}}\right)}\right) \\
&= \left(\tau^2 + \kappa\right) \hat{q}\hat{\rho}^2,
\end{aligned}$$

where $\kappa \equiv \frac{\operatorname{cov}\left(\beta_j, \gamma_j\right)}{\operatorname{var}\left(\beta_j\right)}$.

The meta-regression intercept converges to

$$\begin{aligned}
\hat{a} &\equiv \mathbb{E}\left[\hat{\beta}_j^{\mathrm{OLS}}\right] - \mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right] \hat{b} \\
&= \tau^2 \mathbb{E}\left[\beta_j\right] + \mathbb{E}\left[\gamma_j\right] - \left(\tau^2 + \kappa\right) \hat{q}\hat{\rho}^2 \mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right] \\
&= \mathbb{E}\left[\gamma_j\right] + \left(\tau^2\left(1 - \hat{q}\hat{\rho}^2\right) - \kappa\hat{q}\hat{\rho}^2\right) \mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right].
\end{aligned}$$

Moreover, we can square the bias κ to obtain

$$\begin{aligned}
\kappa^2 &= \frac{\operatorname{cov}\left(\beta_j, \gamma_j\right)^2}{\operatorname{var}\left(\beta_j\right)^2} \\
&= \tilde{R}^2 \frac{\operatorname{var}\left(\gamma_j\right)}{\operatorname{var}\left(\beta_j\right)},
\end{aligned}$$

where $\tilde{R}^2 \equiv \frac{\text{cov}\left(\beta_j,\gamma_j\right)^2}{\text{var}\left(\beta_j\right)\text{var}\left(\gamma_j\right)}$ is the coefficient of determination from the hypothetical meta-regression (11), and var $\left(\gamma_j\right)$ satisfies

$$\text{var}\left(\hat{\beta}_j^{\text{OLS}}\right) = \frac{\tau^4\text{var}\left(\beta_j\right) + \text{var}\left(\gamma_j\right) + 2\tau^2\text{corr}\left(\beta_j,\gamma_j\right)\sqrt{\text{var}\left(\beta_j\right)\text{var}\left(\gamma_j\right)}}{\hat{p}^2},$$

$$\hat{r}^2 = \hat{q}^2\hat{\rho}^2\left(\frac{\tau^4\text{var}\left(\beta_j\right) + \text{corr}\left(\beta_j,\gamma_j\right)^2\text{var}\left(\gamma_j\right) + 2\tau^2\text{corr}\left(\beta_j,\gamma_j\right)\sqrt{\text{var}\left(\beta_j\right)\text{var}\left(\gamma_j\right)}}{\text{var}\left(\hat{\beta}_j^{\text{OLS}}\right)}\right),$$

where $\hat{p}^2 \equiv 1 - \frac{\mathbb{E}\left[\left(\varsigma_j^{\text{OLS}}\right)^2\right]}{\text{var}\left(\hat{\beta}_j^{\text{OLS}}\right)}$, and $\hat{r}^2 \equiv \frac{\text{cov}\left(\hat{\beta}_j^{\text{OLS}},\hat{\beta}_j^{\text{IV}}\right)^2}{\text{var}\left(\hat{\beta}_j^{\text{OLS}}\right)\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)}$ is the coefficient of determination from the meta-regression (8). Plugging the previous equation for var $\left(\hat{\beta}_j^{\text{OLS}}\right)$ into the equation for $\hat{r}^2$ yields

$$\hat{r}^2 = \hat{q}^2\hat{\rho}^2\hat{p}^2\left(1 + \frac{\left(\tilde{R}^2 - 1\right)\text{var}\left(\gamma_j\right)}{\text{var}\left(\beta_j^{\text{OLS}}\right)}\right),$$

which we can rearrange to obtain

$$\text{var}\left(\gamma_j\right) = \frac{\text{var}\left(\beta_j^{\text{OLS}}\right)}{1 - \tilde{R}^2}\left(1 - \frac{\hat{r}^2}{\hat{q}^2\hat{\rho}^2\hat{p}^2}\right).$$

Combining this expression with the previous equation for κ^2 produces

$$\begin{aligned}\kappa^2 &= \left(\frac{\tilde{R}^2}{1 - \tilde{R}^2}\right)\left(\frac{\text{var}\left(\beta_j^{\text{OLS}}\right)}{\text{var}\left(\beta_j\right)}\right)\left(1 - \frac{\hat{r}^2}{\hat{q}^2\hat{\rho}^2\hat{p}^2}\right) \\ &= \left(\frac{\tilde{R}^2}{1 - \tilde{R}^2}\right)\left(1 - \frac{\hat{r}^2}{\hat{q}^2\hat{\rho}^2\hat{p}^2}\right)\left(\frac{\hat{p}^2}{\hat{\rho}^2}\right)\left(\frac{\text{var}\left(\hat{\beta}_j^{\text{OLS}}\right)}{\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)}\right),\end{aligned}$$

where $\frac{\hat{r}^2}{\hat{q}^2\hat{\rho}^2\hat{p}^2}$ is equal to the coefficient of determination from a hypothetical regression of β_j^{OLS} on β_j^{IV}, i.e., the coefficient of determination from a hypothetical meta-regression that is not subject to sampling variation in the underlying regression estimates:

$$\frac{\hat{r}^2}{\hat{q}^2\hat{\rho}^2\hat{p}^2} = \text{corr}\left(\beta_j^{\text{OLS}}, \beta_j^{\text{IV}}\right)^2,$$

hence $\frac{\hat{r}^2}{\hat{q}^2\hat{\rho}^2\hat{p}^2} \leq 1$. □

Proof of Proposition 4

If $\gamma_j = 0$ for all j, then the slope coefficient from a restricted (no-constant) meta-regression of $\hat{\beta}_j^{\text{OLS}}$ on $\hat{\beta}_j^{\text{IV}}$ converges to

$$\begin{aligned}
\hat{b}_{\text{R}} &\equiv \frac{\mathbb{E}\left[\hat{\beta}_j^{\text{OLS}}\hat{\beta}_j^{\text{IV}}\right]}{\mathbb{E}\left[\left(\hat{\beta}_j^{\text{IV}}\right)^2\right]} \\
&= \tau^2 \frac{\mathbb{E}\left[\beta_j^2\right]}{\mathbb{E}\left[\left(\hat{\beta}_j^{\text{IV}}\right)^2\right]} \frac{\mathbb{E}\left[\hat{\beta}_j^{\text{OLS}}\hat{\beta}_j^{\text{IV}}\right]}{\mathbb{E}\left[\beta_j^{\text{OLS}}\beta_j^{\text{IV}}\right]} \\
&= \tau^2 \hat{q}_{\text{R}} \hat{\rho}_{\text{R}}^2,
\end{aligned}$$

where $\hat{\rho}_{\text{R}}^2 \equiv 1 - \frac{\mathbb{E}\left[\left(\varsigma_j^{\text{IV}}\right)^2\right]}{\mathbb{E}\left[\left(\hat{\beta}_j^{\text{IV}}\right)^2\right]}$ and $\hat{q}_{\text{R}} \equiv \left(1 - \frac{\mathbb{E}\left[\varsigma_j^{\text{OLS}}\varsigma_j^{\text{IV}}\right]}{\mathbb{E}\left[\hat{\beta}_j^{\text{OLS}}\hat{\beta}_j^{\text{IV}}\right]}\right)^{-1}$. □

Proof of Proposition 5

The IV estimand is given by

$$\begin{aligned}
\beta_j^{\text{IV}} &\equiv \frac{\text{cov}_j\left(y_{ij}, z_i\right)}{\text{cov}\left(x_i, z_i\right)} \\
&= \frac{\text{cov}_j\left(\chi_i \beta_j + u_{ij}, z_i\right)}{\text{cov}\left(\chi_i + \varepsilon_i, z_i\right)} \\
&= \frac{\text{cov}\left(\chi_i, z_i\right)\beta_j + \text{cov}_j\left(z_i, u_{ij}\right)}{\text{cov}\left(\chi_i, z_i\right) + \text{cov}\left(z_i, \varepsilon_i\right)} \\
&= \left(1 - \delta_1\right)\beta_j + \delta_{j0},
\end{aligned}$$

where $\delta_1 \equiv \frac{\text{cov}(z_i, \varepsilon_i)}{\text{cov}(x_i, z_i)}$ and $\delta_{j0} \equiv \frac{\text{cov}_j\left(z_i, u_{ij}\right)}{\text{cov}(x_i, z_i)}$.

Thus, the slope of the meta-regression converges to

$$\begin{aligned}
\hat{b} &\equiv \frac{\text{cov}\left(\hat{\beta}_j^{\text{OLS}}, \hat{\beta}_j^{\text{IV}}\right)}{\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)} \\
&= \frac{\text{cov}\left(\tau^2\beta_j + \gamma_j, \left(1 - \delta_1\right)\beta_j + \delta_{j0}\right)}{\text{var}\left(\beta_j^{\text{IV}}\right) - \text{var}\left(\delta_{j0}\right)} \left(\frac{\text{var}\left(\beta_j^{\text{IV}}\right) - \text{var}\left(\delta_{j0}\right)}{\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)}\right)\hat{q} \\
&= \frac{\tau^2\left(1 - \delta_1\right)\text{var}\left(\beta_j\right)}{\left(1 - \delta_1\right)^2\text{var}\left(\beta_j\right)} \left(\frac{\text{var}\left(\beta_j^{\text{IV}}\right) - \text{var}\left(\delta_{j0}\right)}{\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)}\right)\hat{q} \\
&= \frac{\tau^2}{1 - \delta_1}\hat{q}\left(\hat{\rho}^2 - \phi^2\right),
\end{aligned}$$

where $\phi^2 \equiv \frac{\text{var}\left(\delta_{j0}\right)}{\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)}$.

The intercept of the meta-regression converges to

$$\begin{aligned}
\hat{a} &\equiv \mathbb{E}\left[\hat{\beta}_j^{\text{OLS}}\right] - \mathbb{E}\left[\hat{\beta}_j^{\text{IV}}\right]\hat{b} \\
&= \tau^2\mathbb{E}\left[\beta_j\right] + \mathbb{E}\left[\gamma_j\right] - \mathbb{E}\left[\hat{\beta}_j^{\text{IV}}\right]\hat{b} \\
&= \frac{\tau^2}{1-\delta_1}\left(\mathbb{E}\left[\hat{\beta}_j^{\text{IV}}\right] - \mathbb{E}\left[\delta_{j0}\right]\right) + \mathbb{E}\left[\gamma_j\right] - \frac{\tau^2}{1-\delta_1}\hat{q}\left(\hat{\rho}^2 - \phi^2\right)\mathbb{E}\left[\hat{\beta}_j^{\text{IV}}\right] \\
&= \mathbb{E}\left[\gamma_j\right] + \frac{\tau^2}{1-\delta_1}\left(\left(1 - \hat{q}\left(\hat{\rho}^2 - \phi^2\right)\right)\mathbb{E}\left[\hat{\beta}_j^{\text{IV}}\right] - \mathbb{E}\left[\delta_{j0}\right]\right).
\end{aligned}$$

□

Statement and Proof of Corollary 3

The measurement invalidity δ_1 is subject to the following restriction on τ^2:

Corollary 3. *Given the meta-regression slope $\hat{b}$ and a value of ϕ^2, the proxy quality τ^2 satisfies*

$$\tau^2 \leq \frac{-2k_\phi r_{xz}^2 - 1 + \sqrt{4k_\phi\left(k_\phi + 1\right)r_{xz}^2 + 1}}{2k_\phi^2 r_{xz}^2} \tag{21}$$

where $r_{xz} \equiv corr\left(x_i, z_i\right)$ and $k_\phi \equiv \frac{\hat{q}\left(\phi^2 - \hat{\rho}^2\right)}{\hat{b}}$.

If $\phi^2 = \hat{\rho}^2 - \hat{b}\hat{q}^{-1}$, then $k_\phi = -1$, and (21) reduces to $\tau^2 \leq 1$.

Proof. The Cauchy-Schwarz inequality implies

$$|\delta_1| \leq \frac{\sqrt{1-\tau^2}}{|r_{xz}|},$$

where $r_{xz} \equiv \text{corr}\left(x_i, z_i\right)$. Given the meta-regression slope coefficient $\hat{b}$, we can write this

inequality as

$$\left|1+\tau^2 k_\phi\right| \leq \frac{\sqrt{1-\tau^2}}{|r_{xz}|},$$

where $k_\phi \equiv \frac{\hat{q}\left(\phi^2-\hat{\rho}^2\right)}{\hat{b}}$. Since $\tau^2 \geq 0$, the above inequality is equivalent to (21).

□

Proof of Corollary 4

If $\hat{\beta}_j^{\text{OLS}}| \left(\beta_j^{\text{OLS}}, se\left(\hat{\beta}_j^{\text{OLS}}\right)^2\right)$ and $\hat{\beta}_j^{\text{IV}}| \left(\beta_j^{\text{IV}}, se\left(\hat{\beta}_j^{\text{IV}}\right)^2\right)$ are normally distributed,

$$\hat{\beta}_j^{\text{OLS}}|\beta_j^{\text{OLS}} \sim \mathcal{N}\left(\beta_j^{\text{OLS}}, se\left(\hat{\beta}_j^{\text{OLS}}\right)^2\right),$$
$$\hat{\beta}_j^{\text{IV}}|\beta_j^{\text{IV}} \sim \mathcal{N}\left(\beta_j^{\text{IV}}, se\left(\hat{\beta}_j^{\text{IV}}\right)^2\right),$$

and i.i.d. across j, then

$$\mathbb{E}\left[\left(\varsigma_j^{\text{IV}}\right)^2\right] = \mathbb{E}\left[se\left(\hat{\beta}_j^{\text{IV}}\right)^2\right],$$
$$\mathbb{E}\left[\varsigma_j^{\text{OLS}}\varsigma_j^{\text{IV}}\right] = \mathbb{E}\left[\rho_j^{\text{OLS-IV}} se\left(\hat{\beta}_j^{\text{OLS}}\right) se\left(\hat{\beta}_j^{\text{IV}}\right)\right],$$

which implies that the adjustments for sampling variation simplify to

$$\hat{p}^2 \equiv 1 - \frac{\mathbb{E}\left[se\left(\hat{\beta}_j^{\text{OLS}}\right)^2\right]}{\text{var}\left(\hat{\beta}_j^{\text{OLS}}\right)},$$

$$\hat{\rho}^2 \equiv 1 - \frac{\mathbb{E}\left[se\left(\hat{\beta}_j^{\text{IV}}\right)^2\right]}{\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)},$$

$$\hat{q} \equiv \left(1 - \frac{\mathbb{E}\left[\rho_j^{\text{OLS-IV}} se\left(\hat{\beta}_j^{\text{OLS}}\right) se\left(\hat{\beta}_j^{\text{IV}}\right)\right]}{\text{cov}\left(\hat{\beta}_j^{\text{OLS}}, \hat{\beta}_j^{\text{IV}}\right)}\right)^{-1}.$$

Note that $\mathbb{E}\left[\rho_j^{\text{OLS-IV}} se\left(\hat{\beta}_j^{\text{OLS}}\right) se\left(\hat{\beta}_j^{\text{IV}}\right)\right]$ is a linear combination of the product of standard errors $se\left(\hat{\beta}_j^{\text{IV}}\right) se\left(\hat{\beta}_j^{\text{OLS}}\right) \geq 0$, hence this expectation is maximized when $\rho_j^{\text{OLS-IV}} = 1$ for all j, and minimized when $\rho_j^{\text{OLS-IV}} = -1$ for all j. For $\text{cov}\left(\hat{\beta}_j^{\text{OLS}}, \hat{\beta}_j^{\text{IV}}\right) > 0$, $\hat{q}$ therefore satisfies

$$\hat{q} \in \left[\left(1 + \frac{\mathbb{E}\left[se\left(\hat{\beta}_j^{\text{OLS}}\right) se\left(\hat{\beta}_j^{\text{IV}}\right)\right]}{\text{cov}\left(\hat{\beta}_j^{\text{OLS}}, \hat{\beta}_j^{\text{IV}}\right)}\right)^{-1}, \left(1 - \frac{\mathbb{E}\left[se\left(\hat{\beta}_j^{\text{OLS}}\right) se\left(\hat{\beta}_j^{\text{IV}}\right)\right]}{\text{cov}\left(\hat{\beta}_j^{\text{OLS}}, \hat{\beta}_j^{\text{IV}}\right)}\right)^{-1}\right]. \tag{22}$$

Furthermore, we can tighten this bound by writing $\hat{q}$ in terms of $\hat{r}^2 \equiv \frac{\text{cov}\left(\hat{\beta}_j^{\text{OLS}}, \hat{\beta}_j^{\text{IV}}\right)^2}{\text{var}\left(\hat{\beta}_j^{\text{OLS}}\right)\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)}$,

the coefficient of determination from the meta-regression (8):

$$\begin{aligned}
\hat{r}^2 &\equiv \frac{\text{cov}\left(\hat{\beta}_j^{\text{OLS}}, \hat{\beta}_j^{\text{IV}}\right)^2}{\text{var}\left(\hat{\beta}_j^{\text{OLS}}\right)\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)} \\
&= \frac{\hat{q}^2\text{cov}\left(\beta_j^{\text{OLS}}, \beta_j^{\text{IV}}\right)^2}{\text{var}\left(\hat{\beta}_j^{\text{OLS}}\right)\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)} \\
&= \frac{\hat{q}^2\hat{p}^2\hat{\rho}^2\text{cov}\left(\beta_j^{\text{OLS}}, \beta_j^{\text{IV}}\right)^2}{\text{var}\left(\beta_j^{\text{OLS}}\right)\text{var}\left(\beta_j^{\text{IV}}\right)} \\
&= \hat{q}^2\hat{p}^2\hat{\rho}^2 R^2,
\end{aligned}$$

where R^2 denotes the coefficient of determination from a hypothetical meta-regression of β_j^{OLS} on β_j^{IV}. We can therefore rearrange this equation to obtain

$$\hat{q}^2 = \frac{\hat{r}^2}{\hat{\rho}^2\hat{p}^2} R^{-2},$$

where $R^2 \leq 1$ implies $\hat{q}^2 \geq \frac{\hat{r}^2}{\hat{\rho}^2\hat{p}^2}$. Combining this lower bound with (22) yields

$$\hat{q} \in [\hat{q}_-, \hat{q}_+],$$

where

$$\hat{q}_{-} \equiv \max\left(\sqrt{\frac{\hat{r}^2}{\hat{\rho}^2\hat{p}^2}}, \left(1 + \frac{\mathbb{E}\left[se\left(\hat{\beta}_j^{\text{OLS}}\right) se\left(\hat{\beta}_j^{\text{IV}}\right)\right]}{\text{cov}\left(\hat{\beta}_j^{\text{OLS}}, \hat{\beta}_j^{\text{IV}}\right)}\right)^{-1}\right),$$

$$\hat{q}_{+} \equiv \left(1 - \frac{\mathbb{E}\left[se\left(\hat{\beta}_j^{\text{OLS}}\right) se\left(\hat{\beta}_j^{\text{IV}}\right)\right]}{\text{cov}\left(\hat{\beta}_j^{\text{OLS}}, \hat{\beta}_j^{\text{IV}}\right)}\right)^{-1}.$$

□

Proof of Proposition 6

Given Assumptions B.1(i)-B.1(iv), we can run an OLS regression of y_{ij} on x_i for $j = 1, 2, ..., J$.

For each j, the OLS estimator converges to

$$\begin{aligned}
\beta_j^{\text{OLS}} &\equiv \frac{\text{cov}_j\left(y_{ij}, x_i\right)}{\text{var}\left(x_i\right)} \\
&= \frac{\text{cov}_j\left(\chi_i\beta_{ij} + u_{ij}, \chi_i + \varepsilon_i\right)}{\text{var}\left(x_i\right)} \\
&= \frac{\mathbb{E}_j\left[\chi_i^2\beta_{ij}\right] + \mathbb{E}_j\left[x_i u_{ij}\right]}{\mathbb{E}_j\left[x_i^2\right]} \\
&= \tau^2\beta_j^{\chi} + \gamma_j.
\end{aligned}$$

For each j, Assumption B.1(v) implies that the corresponding IV estimator converges to

$$\begin{aligned}
\beta_j^{\text{IV}} &\equiv \frac{\text{cov}_j\left(y_{ij}, z_i\right)}{\text{cov}\left(x_i, z_i\right)} \\
&= \frac{\text{cov}_j\left(\chi_i\beta_{ij} + u_{ij}, z_i\right)}{\text{cov}\left(\chi_i + \varepsilon_i, z_i\right)} \\
&= \frac{\mathbb{E}_j\left[\chi_i z_i \beta_{ij}\right] + \mathbb{E}_j\left[z_i u_{ij}\right]}{\mathbb{E}\left[\chi_i z_i\right] + \mathbb{E}\left[z_i\varepsilon_i\right]} \\
&= \left(1-\delta_1\right)\beta_j^z + \delta_{j0}.
\end{aligned}$$

Thus, under Assumption B.1(vi), the slope of the meta-regression converges to

$$\begin{aligned}
\hat{b} &\equiv \frac{\text{cov}\left(\hat{\beta}_j^{\text{OLS}}, \hat{\beta}_j^{\text{IV}}\right)}{\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)} \\
&= \frac{\text{cov}\left(\tau^2\beta_j^\chi + \gamma_j, \left(1-\delta_1\right)\beta_j^z + \delta_{j0}\right)}{\text{var}\left(\beta_j^{\text{IV}}\right) - \text{var}\left(\delta_{j0}\right)} \left(\frac{\text{var}\left(\beta_j^{\text{IV}}\right) - \text{var}\left(\delta_{j0}\right)}{\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)}\right)\hat{q} \\
&= \frac{\tau^2\left(1-\delta_1\right)\text{cov}\left(\beta_j^\chi, \beta_j^z\right)}{\left(1-\delta_1\right)^2\text{var}\left(\beta_j^z\right)} \left(\frac{\text{var}\left(\hat{\beta}_j^{\text{IV}}\right) - \mathbb{E}\left[\left(\varsigma_j^{\text{IV}}\right)^2\right] - \text{var}\left(\delta_{j0}\right)}{\text{var}\left(\hat{\beta}_j^{\text{IV}}\right)}\right)\hat{q} \\
&= \frac{\tau^2}{1-\delta_1}\hat{q}\left(\hat{\rho}^2 - \phi^2\right)\psi_1,
\end{aligned}$$

where $\psi_1 \equiv \frac{\text{cov}\left(\beta_j^\chi, \beta_j^z\right)}{\text{var}\left(\beta_j^z\right)}$ is the slope coefficient from a hypothetical regression of β_j^χ on β_j^z:

$$\beta_j^\chi = \psi_0 + \beta_j^z\psi_1 + v_j.$$

The intercept of the meta-regression converges to

$$
\begin{aligned}
\hat{a} &\equiv \mathbb{E}\left[\hat{\beta}_j^{\mathrm{OLS}}\right] - \mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right]\hat{b} \\
&= \tau^2\mathbb{E}\left[\beta_j^{\chi}\right] + \mathbb{E}\left[\gamma_j\right] - \frac{\tau^2}{1-\delta_1}\hat{q}\left(\hat{\rho}^2-\phi^2\right)\psi_1\mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right] \\
&= \mathbb{E}\left[\gamma_j\right] + \tau^2\psi_0 + \tau^2\psi_1\mathbb{E}\left[\beta_j^{z}\right] - \frac{\tau^2}{1-\delta_1}\hat{q}\left(\hat{\rho}^2-\phi^2\right)\psi_1\mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right] \\
&= \mathbb{E}\left[\gamma_j\right] + \tau^2\psi_0 + \frac{\tau^2}{1-\delta_1}\psi_1\left(\mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right] - \mathbb{E}\left[\delta_{j0}\right]\right) - \frac{\tau^2}{1-\delta_1}\hat{q}\left(\hat{\rho}^2-\phi^2\right)\psi_1\mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right] \\
&= \mathbb{E}\left[\gamma_j\right] + \tau^2\psi_0 + \frac{\tau^2}{1-\delta_1}\psi_1\left(\left(1-\hat{q}\left(\hat{\rho}^2-\phi^2\right)\right)\mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right] - \mathbb{E}\left[\delta_{j0}\right]\right) \\
&= \mathbb{E}\left[\gamma_j\right] + \tau^2\left(\psi_0 + \frac{\psi_1}{1-\delta_1}\left(\left(1-\hat{q}\left(\hat{\rho}^2-\phi^2\right)\right)\mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right] - \mathbb{E}\left[\delta_{j0}\right]\right)\right).
\end{aligned}
$$

□

Proof of Corollary 5

If $\mathrm{cov}_j\left(\chi_i^2, \beta_{ij}\right) = 0$, then the weighted average treatment effect β_j^{χ} is given by

$$
\begin{aligned}
\beta_j^{\chi} &= \frac{\mathbb{E}_j\left[\chi_i^2\beta_{ij}\right]}{\mathbb{E}\left[\chi_i^2\right]} \\
&= \mathbb{E}_j\left[\beta_{ij}\right].
\end{aligned}
$$

If $\text{cov}_j\left(\chi_i z_i, \beta_{ij}\right) = 0$, then the weighted average treatment effect β_j^z is given by

$$\begin{aligned}\beta_j^z &= \frac{\mathbb{E}_j\left[\chi_i z_i \beta_{ij}\right]}{\mathbb{E}\left[\chi_i z_i\right]} \\ &= \mathbb{E}_j\left[\beta_{ij}\right] \\ &= \beta_j^\chi.\end{aligned}$$

Thus, $\psi_1 = 1$, $\psi_0 = 0$, and (23) is equivalent to (16).

□

Proof of Corollary 6

In this setting, the weighted average treatment effect β_j^χ is equal to

$$\begin{aligned}\beta_j^\chi &\equiv \frac{\mathbb{E}_j\left[\chi_i^2 \beta_{ij}\right]}{\mathbb{E}\left[\chi_i^2\right]} \\ &= \frac{\mathbb{P}\left(\pi_i = 1\right)\mathbb{E}\left[\chi_i^2 | \pi_i = 1\right]\beta_{j1} + \left(1 - \mathbb{P}\left(\pi_i = 1\right)\right)\mathbb{E}\left[\chi_i^2 | \pi_i = 0\right]\beta_{j0}}{\mathbb{E}\left[\chi_i^2\right]} \\ &= \lambda\beta_{j1} + \left(1 - \lambda\right)\beta_{j0},\end{aligned}$$

where $\lambda \equiv \frac{\mathbb{P}(\pi_i=1)\mathbb{E}\left[\chi_i^2|\pi_i=1\right]}{\mathbb{E}\left[\chi_i^2\right]}$. Therefore, following Proposition 6, the OLS estimator converges to

$$\beta_j^{\text{OLS}} = \tau^2\left(\lambda\beta_{j1} + \left(1 - \lambda\right)\beta_{j0}\right) + \gamma_j.$$

Similarly, the weighted average treatment effect β_j^z is given by

$$\begin{aligned}
\beta_j^z &\equiv \frac{\mathbb{E}_j\left[\chi_i z_i \beta_{ij}\right]}{\mathbb{E}\left[\chi_i z_i\right]} \\
&= \frac{\mathbb{P}\left(\pi_i = 1\right)\mathbb{E}\left[\chi_i z_i | \pi_i = 1\right]\beta_{j1} + \left(1 - \mathbb{P}\left(\pi_i = 1\right)\right)\mathbb{E}\left[\chi_i z_i | \pi_i = 0\right]\beta_{j0}}{\mathbb{P}\left(\pi_i = 1\right)\mathbb{E}\left[\chi_i z_i | \pi_i = 1\right] + \left(1 - \mathbb{P}\left(\pi_i = 1\right)\right)\mathbb{E}\left[\chi_i z_i | \pi_i = 0\right]} \\
&= \frac{\mathbb{P}\left(\pi_i = 1\right)\mathrm{var}\left(z_i | \pi_i = 1\right)\alpha\beta_{j1}}{\mathbb{P}\left(\pi_i = 1\right)\mathrm{var}\left(z_i | \pi_i = 1\right)\alpha} \\
&= \beta_{j1}.
\end{aligned}$$

Again, following Proposition 6, the corresponding IV estimator converges to

$$\beta_j^{\mathrm{IV}} = \left(1 - \delta_1\right)\beta_{j1} + \delta_{j0}.$$

Finally, note that

$$\begin{aligned}
\psi_1 &\equiv \frac{\mathrm{cov}\left(\beta_j^\chi, \beta_j^z\right)}{\mathrm{var}\left(\beta_j^z\right)} \\
&= \frac{\mathrm{cov}\left(\lambda\beta_{j1} + \left(1 - \lambda\right)\beta_{j0}, \beta_{j1}\right)}{\mathrm{var}\left(\beta_{j1}\right)} \\
&= \lambda + \left(1 - \lambda\right)\theta_1,
\end{aligned}$$

and

$$\begin{aligned}
\psi_0 &\equiv \mathbb{E}\left[\beta_j^\chi\right] - \mathbb{E}\left[\beta_j^z\right]\psi_1 \\
&= \lambda\mathbb{E}\left[\beta_{j1}\right] + \left(1 - \lambda\right)\mathbb{E}\left[\beta_{j0}\right] - \mathbb{E}\left[\beta_{j1}\right]\left(\lambda + \left(1 - \lambda\right)\theta_1\right) \\
&= \left(1 - \lambda\right)\theta_0,
\end{aligned}$$

where θ_1 and θ_0 are the slope and intercept coefficients, respectively, from a hypothetical regression of β_{j0} on β_{j1}.

Thus, following Proposition 6, the meta-regression estimator converges to

$$\hat{b} = \frac{\tau^2}{1-\delta_1}\hat{q}\left(\hat{\rho}^2 - \phi^2\right)\left(\lambda + (1-\lambda)\,\theta_1\right),$$
$$\hat{a} = \mathbb{E}\left[\gamma_j\right] + \tau^2\left((1-\lambda)\,\theta_0 + \frac{\lambda + (1-\lambda)\,\theta_1}{1-\delta_1}\left(\left(1 - \hat{q}\left(\hat{\rho}^2 - \phi^2\right)\right)\mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right] - \mathbb{E}\left[\delta_{j0}\right]\right)\right).$$

□

B Heterogeneity

The analysis in sections 2–5 assumes that treatment effects across observations i are homogeneous; in this section we consider the impact of heterogeneous treatment effects. We derive an expression for the meta-regression coefficients that allows for heterogeneity, invalidity, and endogeneity, thereby nesting all previous expressions. We then show that heterogeneity is irrelevant to the meta-regression if heterogeneity in the first stage is independent of heterogeneity in the second stage. Finally, we show that the meta-regression can be used to analyze the external validity of IV estimates that capture local average treatment effects. We apply these results to three of our example papers and find that reasonable departures from our baseline estimates of the measurement error and omitted variable bias suggest that the IV estimates apply to an irrelevant sub-population, even if the instruments themselves are valid.

B.1 Theory

We begin by making the following assumption.

Assumption B.1

(i) $(y_{i1}, y_{i2}, ..., y_{iJ}, x_i, z_i)$ is a sequence of mean-zero observable variables for $i = 1, 2, ..., N$.

(ii) $(\chi_i, u_{i1}, u_{i2}, ..., u_{iJ}, \varepsilon_i)$ is an i.i.d. sequence of *unobservable* mean-zero variables for $i = 1, 2, ..., N$.

(iii) $\left(y_{ij}, x_i\right)$ and $\left(\chi_i, u_{ij}, \varepsilon_i\right)$ are related to each other as follows:

$$y_{ij} = \chi_i \beta_{ij} + u_{ij},$$

$$x_i = \chi_i + \varepsilon_i.$$

(iv) $\text{cov}\left(\chi_i, \varepsilon_i\right) = \text{cov}_j\left(u_{ij}, \varepsilon_i\right) = 0$ for each j.

(v) $\text{cov}\left(\chi_i, z_i\right) \neq 0$.

(vi) $\left(\beta_j^{\chi}, \beta_j^{z}\right)$, γ_j, and δ_{j0} are mutually independent, where

$$\beta_j^m \equiv \mathbb{E}_j\left[\omega_{im}\beta_{ij}\right],$$

$$\omega_{im} \equiv \frac{\chi_i m_i}{\mathbb{E}\left[\chi_i m_i\right]},$$

$$m \in \{\chi, z\}.$$

Assumption B.1(iii) allows for heterogeneous treatment effects: β_{ij} can vary across observations i for each regression j. Consequently, Assumption B.1(vi) references weighted

average treatment effects β_j^{χ} and β_j^{z}, which are the weighted averages that appear in the OLS and IV coefficients, respectively.[15]

It is worth emphasizing that "heterogeneous treatment effects" refers to variation in β_{ij} across i for a given j; that is, heterogeneity across observations i in the effects of treatment χ_i on a particular outcome variable y_{ij}. By contrast, the heterogeneity in β_{ij} across j, which derives from the use of different outcome variables y_{ij} across j, is precisely the variation that identifies the meta-regression coefficients.

The following proposition shows how the meta-regression captures heterogeneous treatment effects, invalidity, and endogeneity.

Proposition 6. *Suppose Assumption B.1 holds. Then the OLS estimator converges to*

$$\beta_j^{OLS} = \tau^2 \beta_j^{\chi} + \gamma_j,$$

while the IV estimator converges to

$$\beta_j^{IV} = (1 - \delta_1)\, \beta_j^{z} + \delta_{j0}.$$

The meta-regression estimator converges to:

$$\begin{aligned} \hat{b} &= \frac{\tau^2}{1-\delta_1} \hat{q} \left(\hat{\rho}^2 - \phi^2\right) \psi_1, \\ \hat{a} &= \mathbb{E}\left[\gamma_j\right] + \tau^2 \left(\psi_0 + \frac{\psi_1}{1-\delta_1} \left(\left(1 - \hat{q}\left(\hat{\rho}^2 - \phi^2\right)\right) \mathbb{E}\left[\hat{\beta}_j^{IV}\right] - \mathbb{E}\left[\delta_{j0}\right] \right) \right), \end{aligned} \tag{23}$$

[15] By construction, $\mathbb{E}\left[\omega_{im}\right] = 1$, and the weights are always non-negative when computing β_j^{χ}. The weights *may* be negative when computing β_j^{z}, but are always non-negative if the following monotonicity condition is satisfied for each j: either $\mathbb{E}_j\left[\chi_i z_i | \beta_{ij}\right] \geq 0$ for all i, or $\mathbb{E}_j\left[\chi_i z_i | \beta_{ij}\right] \leq 0$ for all i.

where (ψ_1, ψ_0) *are the slope and intercept coefficients, respectively, from a hypothetical regression of* β_j^{χ} *on* β_j^{z}.

If treatment effects are not heterogeneous, then $\psi_1 = 1$ and $\psi_0 = 0$, hence (23) is equivalent to (16). As the following corollary shows, even if there is heterogeneity, it may still be irrelevant to the meta-regression.

Corollary 5. *Suppose Assumption B.1 holds and*

$$cov_j\left(\chi_i^2, \beta_{ij}\right) = cov_j\left(\chi_i z_i, \beta_{ij}\right) = 0 \tag{24}$$

for all j. *Then* $\beta_j^{\chi} = \beta_j^{z} = \mathbb{E}_j\left[\beta_{ij}\right]$ *for all* j, *which implies that* $\psi_0 = 0$ *and* $\psi_1 = 1$. *The meta-regression estimator is therefore unaffected by heterogeneity: (23) is equivalent to (16).*

In a linear environment, equation (24) will hold if heterogeneity in the first-stage coefficient $\alpha_i \equiv \frac{\text{cov}\left(\chi_i, z_i | \pi_i\right)}{\text{var}\left(z_i | \pi_i\right)}$ is independent of heterogeneity in the treatment effect β_{ij} for each j, where π_i denotes the group that individual i belongs to. This is trivially satisfied if, for instance, there is no heterogeneity in the first stage, so that $\alpha_i = \alpha$ for all i. More generally, if heterogeneity in the first stage is independent of heterogeneity in the second stage, then the weighted average treatment effects β_j^{χ} and β_j^{z} will both be equal to the average treatment effect. For the meta-regression, this heterogeneity is then irrelevant.

While Corollary 5 gives an intuitive sufficient condition under which heterogeneity is irrelevant, in the rest of the paper, we *do not* assume equation (24) holds. In what follows, we allow for heterogeneity that affects the IV estimates and, therefore, the meta-regression estimates. For practical purposes, it is helpful to cast heterogeneity in a simple setting with

compliant and non-compliant sub-populations.[16]

Corollary 6. *Suppose Assumption B.1 holds and observation i's type is indexed by the unobservable variable $\pi_i \in \{0,1\}$ such that*

$$\beta_{ij} = \begin{cases} \beta_{j0} & \text{if } \pi_i = 0 \\ \beta_{j1} & \text{if } \pi_i = 1. \end{cases}$$

Further suppose that

$$\alpha_i = \begin{cases} 0 & \text{if } \pi_i = 0 \\ \alpha & \text{if } \pi_i = 1, \end{cases}$$

where $\alpha_i \equiv \frac{cov\left(\chi_i, z_i | \pi_i\right)}{var\left(z_i | \pi_i\right)}$. Then the IV estimand is given by

$$\beta_j^{IV} = (1 - \delta_1)\, \beta_{j1} + \delta_{j0}.$$

The meta-regression estimator converges to

$$\begin{aligned} \hat{b} &= \frac{\tau^2}{1 - \delta_1} \hat{q} \left(\hat{\rho}^2 - \phi^2\right) \left(\lambda + (1 - \lambda)\, \theta_1\right), \\ \hat{a} &= \mathbb{E}\left[\gamma_j\right] + \tau^2 \left[(1 - \lambda)\, \theta_0 + \frac{\lambda + (1 - \lambda)\, \theta_1}{1 - \delta_1} \left(\left(1 - \hat{q} \left(\hat{\rho}^2 - \phi^2\right)\right) \mathbb{E}\left[\hat{\beta}_j^{IV}\right] - \mathbb{E}\left[\delta_{j0}\right]\right)\right], \end{aligned} \tag{25}$$

[16] We thank Andrew Y. Chen for this concise formulation of heterogeneous treatment effects.

where

$$\lambda \equiv \frac{\mathbb{P}(\pi_i = 1)\,\mathbb{E}\left[\chi_i^2 | \pi_i = 1\right]}{\mathbb{E}\left[\chi_i^2\right]},$$

and (θ_1, θ_0) *are the slope and intercept coefficients, respectively, from a hypothetical regression of* β_{j0} *on* β_{j1}.

Corollary 6 is a special case of Proposition 6 that allows us to discuss the arbitrary heterogeneity in Assumption B.1 in terms of compliers and non-compliers (Angrist, Imbens and Rubin, 1996). Assumption B.1(v) implies that $\alpha \neq 0$, hence the treatment χ_i and instrument z_i are only correlated in the sub-population for which $\pi_i = 1$. We therefore refer to observations of type $\pi_i = 1$ as compliers, and observations of type $\pi_i = 0$ as non-compliers.

The setup in Corollary 6 differs slightly from the standard definition of compliers and non-compliers in the literature (e.g. Mogstad and Torgovitsky 2024) because we assume that treatment is continuous, which is the case in all of our empirical examples. In our setting, as in a binary setting with monotonicity (see, for example, the denominator of equation 15 in Mogstad and Torgovitsky 2024), an individual i is a complier if there exist values of the instrument $z \neq z'$ such that

$$\mathbb{E}\left[\chi_i | z_i = z, \pi_i\right] - \mathbb{E}\left[\chi_i | z_i = z', \pi_i\right] \neq 0. \tag{26}$$

In other words, an individual is a complier if their treatment χ_i can be affected by the instrument. Corollary 6 also satisfies monotonicity since $\alpha_i \in \{0, \alpha\}$, and the IV estimator identifies a local average treatment effect (LATE) in the absence of invalidity ($\delta_{j0} = \delta_1 = 0$).

The distinction between compliers and non-compliers is commonplace in discussions of 2SLS estimation (Angrist and Pischke, 2009; Mogstad, Torgovitsky and Walters, 2021; Blandhol et al., 2022); Jiang (2017) uses this terminology to explain why 2SLS estimates are often larger than their OLS counterparts. If the instrument is valid, then the IV estimator identifies the treatment effect for compliers, i.e., the local average treatment effect β_{j1}. The parameter $\lambda \in [0, 1]$ denotes the extent to which the compliant sub-population drives variation in χ_i, hence it directly measures the external validity of the IV estimate. If λ is close to zero, then the estimated causal effect, though well-identified, applies to an inconsequential amount of variation in the data.

Following section 2.4, we can use the meta-regression coefficients to infer the relationship between external validity λ, proxy quality τ^2, and the average omitted variable bias $\mathbb{E}\left[\gamma_j\right]$. If the instrument is valid, then Corollary 6 implies that

$$\begin{aligned} \lambda &= \frac{1}{1-\theta_1}\left(\frac{\hat{b}}{\tau^2 \hat{q}\hat{\rho}^2} - \theta_1\right), \\ \lambda &= \frac{\theta_0 \hat{b} + \theta_1\left(\hat{q}\hat{\rho}^2\left(\mathbb{E}\left[\gamma_j\right] - \hat{a}\right) + \left(1 - \hat{q}\hat{\rho}^2\right)\mathbb{E}\left[\hat{\beta}_j^{IV}\right]\hat{b}\right)}{\theta_0 \hat{b} + (\theta_1 - 1)\left(\hat{q}\hat{\rho}^2\left(\mathbb{E}\left[\gamma_j\right] - \hat{a}\right) + \left(1 - \hat{q}\hat{\rho}^2\right)\mathbb{E}\left[\hat{\beta}_j^{IV}\right]\hat{b}\right)}. \end{aligned} \tag{27}$$

When $\theta_1 = 1$, θ_0 indexes the average difference (across regressions) between complier and non-complier effects. In this case, equation (27) reduces to

$$\lambda = 1 + \frac{\hat{q}\hat{\rho}^2\left(\mathbb{E}\left[\gamma_j\right] - \hat{a}\right) + \left(1 - \hat{q}\hat{\rho}^2\right)\mathbb{E}\left[\hat{\beta}_j^{IV}\right]\hat{b}}{\theta_0 \hat{b}}.$$

Given the meta-regression estimates, the relationship between λ and τ^2 does not depend

on $\mathbb{E}\left[\gamma_j\right]$, and the relationship between λ and $\mathbb{E}\left[\gamma_j\right]$ does not depend on τ^2, thereby allowing us to separately analyze the impact of each of the two fundamental endogeneity problems on the external validity of local average treatment effects.

B.2 Results

In section 5 we applied Proposition 5 to explore how the meta-regression estimates can be informative about instrument invalidity. In this section, we do the same for heterogeneous treatment effects using Proposition 6: even if the instrument is valid, the IV reflects a local average treatment effect whereas the OLS reflects a (biased) average treatment effect. Here we show how to use Corollary 6 to determine how "local" the LATE is, and find that if the heterogeneity is important enough to make the implied omitted variable bias large, then IV estimates are too local to be useful.

We first explore the possibility that the instrument in Mian and Sufi (2014) is valid, but proxy quality and omitted variable bias depart from Proposition 2 due to heterogeneous treatment effects, which has implications for the external validity of the IV estimates. Following Corollary 6, complier counties are those in which the Saiz instrument correlates with housing net worth shocks, while non-complier counties are those in which the Saiz instrument does not correlate with housing net worth shocks. For example, if the treatment effects for complier and non-complier counties are uncorrelated with each other across regressions ($\theta_1 = 0$), and there is no measurement error ($\tau^2 = 1$), then our meta-regression estimates imply that $\lambda = 0.445$. In other words, the IV estimates are identified based on a compliant sub-population that accounts for 45% of the variation in housing net worth shocks.

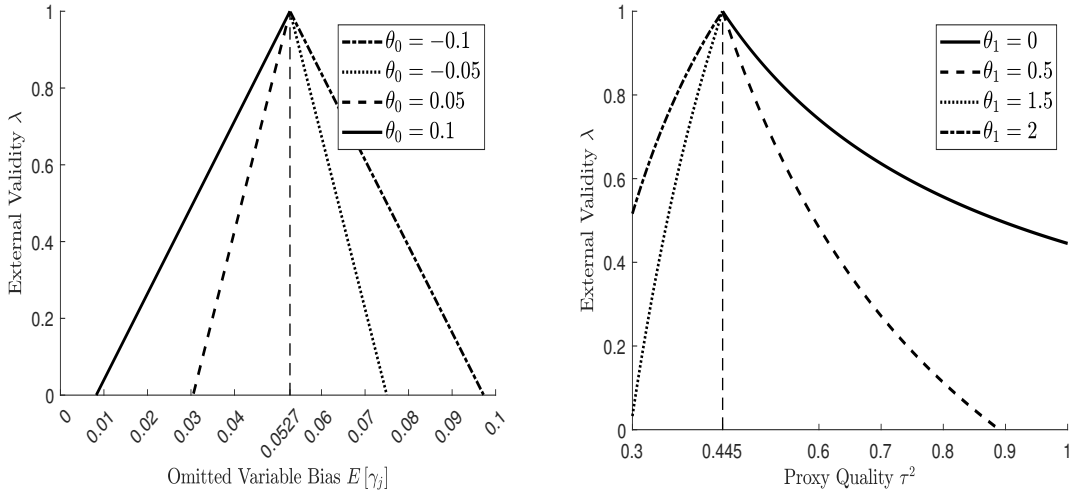


Figure 8. Estimated λ for Mian and Sufi (2014)

The figure plots equation (27) for Mian and Sufi (2014). The left panel plots λ as a function of $\mathbb{E}\left[\gamma_j\right]$ given θ_0 for $\theta_1 = 1$, and the right panel plots λ as a function of τ^2 given θ_1. The vertical dashed lines correspond to the baseline estimates from Proposition 2.

Figure 8 plots the implied value of λ as a function of average omitted variable bias in the left panel, and proxy quality in the right panel. In both panels, greater departures from the proxy quality and average omitted variable bias implied by Proposition 2 suggest that the IV is identified by an increasingly irrelevant compliant sub-population. Values of θ_1 closer to 1, or θ_0 closer to 0, amplify the impact of these departures on λ. Intuitively, if $\theta_1 = 1$ and $\theta_0 = 0$, then the treatment effects for compliers are, on average, equal to the treatment effects for non-compliers; as θ_1 and θ_0 depart from 1 and 0, respectively, the complier effects become less representative of the non-complier effects, hence the IV estimate of the local average treatment effect becomes less representative of the entire population.

It is difficult to increase the average omitted variable bias much without making the IV estimates irrelevant. In the left panel of Figure 8, we assume that $\theta_1 = 1$, so that θ_0 indexes the average difference between complier and non-complier effects. Moreover, because $\lambda \in [0, 1]$, the value of θ_0 imposes an upper bound on $\mathbb{E}\left[\gamma_j\right]$. For example, if $\theta_0 = -0.05$,

which implies that on average the complier effects exceed non-complier effects by 0.05, then the omitted variable bias cannot exceed about 0.075; if $\theta_0 = -0.1$, then the omitted variable bias cannot be more than about 0.1. As the omitted variable bias approaches these bounds, λ approaches 0, and the IV estimates approach irrelevance. If θ_0 is instead positive, such that complier effects are lower than non-complier effects, then the omitted variable bias is bounded above by 0.053. Similarly, as can be seen in the right panel of Figure 8, θ_1 imposes bounds on τ^2: $\theta_1 < 1$ implies that $\tau^2 > 0.445$, and as proxy quality increases, λ decreases.

Thus, our meta-regression point estimates offer the following interpretation of the results in Mian and Sufi (2014):

1. If the Saiz instrument is valid, then our meta-regression implies that measurement error is substantial, while omitted variable bias is negligible.

2. If the omitted variable bias is *not* negligible, then either the Saiz instrument is invalid, or heterogeneity of treatment effects is important.

    (a) If the Saiz instrument is invalid, then our meta-regression implies that the OLS estimates are less biased than the IV estimates.

    (b) If treatment effects are heterogeneous, then our meta-regression implies that the IV estimates apply to an irrelevant sub-population.

Corollary 6 implies a similar trade-off between external validity and either proxy quality or omitted variable bias in our other two example applications, as well. The left and right panels of Figure 9 plot the implied λ as a function of omitted variable bias for the investment-q regressions of section 5.2 and the Adelino, Ma and Robinson (2017) meta-regression of

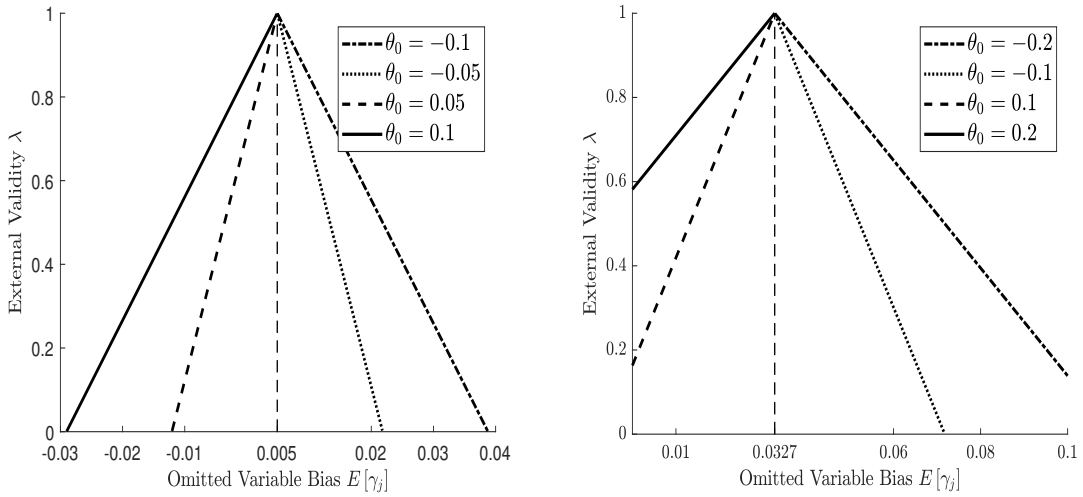


Figure 9. Estimated λ for Investment-q Regressions and Adelino, Ma and Robinson (2017)

The figure plots λ as a function of $\mathbb{E}\left[\gamma_j\right]$, given θ_0 and assuming $\theta_1 = 1$, using equation (27), for either the investment-q regressions of section 5.2 (left panel) or Adelino, Ma and Robinson (2017) (right panel). The vertical dashed lines correspond to the baseline estimates from Proposition 2.

section 5.3, respectively. The left panel of Figure 9 shows that if the average difference between complier and non-complier effects is 0.1, the average omitted variable bias could be as high as 0.04, which is of the same order of magnitude as the treatment effect of q on Total Investment. An average omitted variable bias of -0.03 is also possible if compliers—which in this case means firms for whom lagged q is a good predictor of changes in q—have on average lower treatment effects than non-compliers.

The right panel of Figure 9 shows that if the average difference between complier and non-complier effects in Adelino, Ma and Robinson (2017) is 0.10, then the average omitted variable bias could at most be about 0.07. While this is substantially higher than the omitted variable bias of 0.0327 implied by Proposition 2, it is still small compared to the reported IV estimates. Moreover, even this modest amount of heterogeneity would imply that many of the reported IV estimates are irrelevant: if $\theta_0 = -0.10$, then the non-complier effect on

net job creation for entering firms (Table V, column 5, panel C) is close to zero, and the positive IV coefficient applies to a compliant sub-population that accounts for almost none of the variation in income growth.

C Simulation Details

In section 4, we conduct a simulation exercise to test the performance of the meta-regression. In this section, we describe that simulation in detail.

In order to simulate outcome variables, we use x_i and z_i to generate a pair of random variables, $\hat{f}_i$ and $\hat{r}_i$, that respectively capture the orthogonal variation in x_i and z_i for $i = 1, 2, ..., N$. The variable $\hat{f}_i$ captures variation that is correlated with x_i but orthogonal to the instrument:

$$\hat{f}_i = \frac{\widehat{\text{var}}_\omega(x_i)}{\widehat{\text{cov}}_\omega\left(x_i, \hat{v}_i^{\text{FS}}\right)} \hat{v}_i^{\text{FS}},$$

where $\hat{v}_i^{\text{FS}}$ is the residual from the ω_i-weighted first-stage regression of x_i on z_i, hence $\widehat{\text{cov}}_\omega\left(z_i, \hat{f}_i\right) = 0$ and $\widehat{\text{cov}}_\omega\left(x_i, \hat{f}_i\right) / \widehat{\text{var}}_\omega(x_i) = 1$. Conversely, the variable $\hat{r}_i$ captures variation that is correlated with z_i but orthogonal to the proxy:

$$\hat{r}_i = \frac{\widehat{\text{cov}}_\omega(x_i, z_i)}{\widehat{\text{cov}}_\omega\left(z_i, \hat{v}_i^{\text{RV}}\right)} \hat{v}_i^{\text{RV}},$$

where $\hat{v}_i^{\text{RV}}$ is the residual from an ω_i-weighted regression of z_i on x_i, hence $\widehat{\text{cov}}_\omega(x_i, \hat{r}_i) = 0$ and $\widehat{\text{cov}}_\omega(z_i, \hat{r}_i) / \widehat{\text{var}}_\omega(z_i)$ is equal to the first-stage coefficient.

We additionally use $(y_{i1}, y_{i2}, ..., y_{iJ})$ to discern the correlation structure of cluster-level

and unit-level shocks to the outcomes in the data. We compute $\hat{\sigma}_j^{\text{MC}}$, the weighted standard deviation of the residuals $\hat{v}_{ij}^{\text{MC}}$ from the weighted OLS regression

$$y_{ij} = \hat{\alpha}_j^{\text{MC}} + \hat{\beta}_j^{\text{MC}} x_i + \hat{\delta}_j^{\text{MC}} \hat{f}_i + \hat{v}_{ij}^{\text{MC}},$$

and apportion $\hat{\sigma}_j^{\text{MC}}$ to cluster-level, unit-level, and idiosyncratic shocks. In particular, for the correlation between outcomes of units that are in the same cluster, we compute

$$\hat{\rho}_{\text{clust}} = \frac{1}{J} \sum_{j=1}^{J} \frac{\max\left(\left(SS_{B,j}/D_B - SS_{W,j}/D_W\right), 0\right)}{\max\left(\left(SS_{B,j}/D_B - SS_{W,j}/D_W\right), 0\right) + SS_{W,j} A / \left(D_B D_W\right)},$$

where $SS_{B,j}$ is the between-cluster sum of squared residuals for outcome j

$$SS_{B,j} = \sum_{g=1}^{G} \left(\sum_{i \in g} \omega_i \right) \left(\frac{\sum_{i \in g} \omega_i \hat{v}_{ij}^{\text{MC}}}{\sum_{i \in g} \omega_i} - \frac{\sum_{i=1}^{N} \omega_i \hat{v}_{ij}^{\text{MC}}}{\sum_{i=1}^{N} \omega_i} \right)^2,$$

$g \in \{1, 2, ..., G\}$ denotes a cluster, $SS_{W,j}$ is the within-cluster sum of squared residuals for outcome j

$$SS_{W,j} = \sum_{g=1}^{G} \sum_{i \in g} \omega_i \left(\hat{v}_{ij}^{\text{MC}} - \frac{\sum_{k \in g} \omega_k \hat{v}_{kj}^{\text{MC}}}{\sum_{k \in g} \omega_k} \right)^2,$$

D_B is the weighted between-cluster degrees of freedom

$$D_B = \sum_{g=1}^{G} \frac{\sum_{i \in g} \omega_i^2}{\sum_{i \in g} \omega_i} - \frac{\sum_{i=1}^{N} \omega_i^2}{\sum_{i=1}^{N} \omega_i},$$

D_W is the weighted within-cluster degrees of freedom

$$D_W = \sum_{g=1}^{G} \left(\sum_{i \in g} \omega_i - \frac{\sum_{i \in g} \omega_i^2}{\sum_{i \in g} \omega_i} \right),$$

and the term A is

$$A = \sum_{i=1}^{N} \omega_i - \frac{\sum_{g=1}^{G} \left(\sum_{i \in g} \omega_i \right)^2}{\sum_{i=1}^{N} \omega_i}.$$

For the correlation between the outcomes for a given unit i, we compute

$$\hat{\rho}_{\text{unit}} = \frac{2}{J\left(J-1\right)} \sum_{j=2}^{J} \sum_{k=1}^{j-1} \frac{\sum_{i=1}^{N} \omega_i \left(\hat{v}_{ij}^{\text{MC}} - \hat{\mu}_j^{\text{MC}} \right) \left(\hat{v}_{ik}^{\text{MC}} - \hat{\mu}_k^{\text{MC}} \right)}{\sqrt{\left(\sum_{i=1}^{N} \omega_i \left(\hat{v}_{ij}^{\text{MC}} - \hat{\mu}_j^{\text{MC}} \right)^2 \right) \left(\sum_{i=1}^{N} \omega_i \left(\hat{v}_{ik}^{\text{MC}} - \hat{\mu}_k^{\text{MC}} \right)^2 \right)}},$$

where $\hat{\mu}_j^{\text{MC}}$ is the weighted average of the jth outcome's residuals $\hat{v}_{ij}^{\text{MC}}$.

To evaluate the small-J properties of the meta-regression, we choose an ascending set of $(J_1, J_2, ..., J_K) \in \mathbb{N}^K$. Each simulation has J_K simulated outcome variables, and we compute estimates based on the first J_k outcome variables for $k = 1, 2, ..., K$. Across all $S = 10,000$ simulations, we set the proxy quality τ^2 equal to the $\hat{\tau}^2$ that we estimate in the real data.

For each simulation s, we draw $\beta_{j,s}$, $\gamma_{j,s}$, and $\delta_{j0,s}$ from mutually independent normal distributions for $j = 1, 2, ..., J_K$. When testing the performance of our estimators in the absence of omitted variable bias, we set $\mu_\gamma = \sigma_\gamma = 0$; otherwise, we set $\left(\mu_\gamma, \sigma_\gamma\right)$ equal to the mean and standard deviation of the set of $\hat{\gamma}_j$ that we estimate from the data. We likewise set $\left(\mu_\beta, \sigma_\beta\right)$ equal to the mean and standard deviation of the set of $\hat{\beta}_j^{\text{IV}}$ that we estimate from the data.

We draw clusters with replacement from the data, which determine the set of observations $\left(x_{i,s}, z_{i,s}, \hat{f}_{i,s}, \hat{r}_{i,s}, \omega_{i,s}\right)$ that we use for simulation s, and treat each simulated cluster as a distinct cluster with a distinct set of observations. With unequal cluster sizes, the total number of observations N_s can therefore differ across simulations, but the total number of clusters G always matches the number of clusters in the data.

Each simulated outcome variable $y_{ij,s}$ is a function of the drawn observations and generated shocks. In particular, we generate $\left(\eta^{\text{clust}}_{g,j,s}, \eta^{\text{unit}}_{i,s}, \eta^{\text{idio}}_{ij,s}\right)$ that follow mutually independent standard normal distributions for $i = 1, 2, ..., N_s$, $j = 1, 2, ..., J_K$, and $g = 1, 2, ..., G$, where g corresponds to a cluster for a given simulation. We assign each generated outcome to an actual outcome, which determines the simulated outcome's exposure to the shocks, by drawing $\left(D_{j,s}\right)_{j=1}^{J_K}$ where $D_{j,s} \overset{iid}{\sim} \text{DiscreteUniform}\{1, 2, ..., J\}$. Each generated outcome is then

$$y_{ij,s} = \hat{\alpha}^{\text{MC}}_{D_{j,s}} + \beta_{j,s} x_{i,s} + \left(\gamma_{j,s} - \left(1 - \tau^2\right)\beta_{j,s}\right)\hat{f}_{i,s} + \left(\delta_{j0,s} - \delta_1 \beta_{j,s}\right)\hat{r}_{i,s} + \eta^{\text{tot}}_{ij,s},$$

where total shock $\eta^{\text{tot}}_{ij,s}$ is a function of the cluster-level, unit-level, and idiosyncratic shocks:

$$\eta^{\text{tot}}_{ij,s} = \hat{\sigma}^{\text{MC}}_{D_{j,s}}\left(\eta^{\text{clust}}_{g(i),j,s}\sqrt{\hat{\rho}_{\text{clust}}} + \eta^{\text{unit}}_{i,s}\sqrt{\hat{\rho}_{\text{unit}}} + \eta^{\text{idio}}_{ij,s}\sqrt{1 - \hat{\rho}_{\text{clust}} - \hat{\rho}_{\text{unit}}}\right),$$

which implies $\text{var}\left(\eta^{\text{tot}}_{ij,s}\right) = \left(\hat{\sigma}^{\text{MC}}_{D_{j,s}}\right)^2$. Thus, each generated outcome is exposed to both unit-level shocks and cluster-level shocks such that

$$\text{corr}\left(\eta^{\text{tot}}_{ij,s}, \eta^{\text{tot}}_{ij',s}\right) = \hat{\rho}_{\text{unit}},$$
$$\text{corr}\left(\eta^{\text{tot}}_{ij,s}, \eta^{\text{tot}}_{i'j,s}\right) = \hat{\rho}_{\text{clust}},$$

for $j \neq j'$ and $g(i) = g(i')$.

Tables 1 and 2 show the simulation results based on Mian and Sufi (2014). Tables 6 and 7 show the simulation results based on the investment-q exercise in section 5.2, for which we partial the control variable out of $\left(y_{i1,s}, ..., y_{iJ_k,s}, y_{i1}, ..., y_{iJ}, x_i, z_i, \hat{f}_i, \hat{r}_i\right)$, and do not include weights. The results in Tables 6 and 7 are similar to the results in Tables 1 and 2; see section 4 for a discussion of the latter two tables.

D Standard Errors

In this section we describe how we compute standard errors for the meta-regression. The computation of standard errors depends on whether we have access to the underlying data (section D.1), or only the reported coefficients and standard errors (section D.2).

D.1 Standard errors with data

When we have access to the data, as in sections 5.1 and 5.2, we compute standard errors using the full variance-covariance matrix of the estimated OLS and IV coefficients. Let $\hat{T} \in \{\hat{b}, \hat{a}, \hat{\tau}^2, \hat{\mu}_\gamma, \hat{\gamma}_1, ..., \hat{\gamma}_J, \hat{b}_{\text{R}}, \hat{\tau}^2_{\text{R}}\}$ denote an estimate from the data. The standard error of $\hat{T}$ is given by

$$\text{se}\left(\hat{T}\right) = \sqrt{\widehat{\text{E}}\left[\text{Var}\left(\hat{T}\,|\gamma_1, \gamma_2, ..., \gamma_J\right)\right] + \widehat{\text{Var}}\left[\text{E}\left(\hat{T}\,|\gamma_1, \gamma_2, ..., \gamma_J\right)\right]}.$$

The first term accounts for sampling variation:

$$\widehat{\mathrm{E}}\left[\mathrm{Var}\left(\hat{T}\,|\gamma_1,\gamma_2,...,\gamma_J\right)\right]=\hat{g}_T'\hat{\Omega}\hat{g}_T,$$

where $\hat{\Omega}$ denotes the variance-covariance matrix of all $2J$ OLS and IV coefficients $\hat{\beta}=\left(\hat{\beta}_1^{\mathrm{OLS}},...,\hat{\beta}_J^{\mathrm{OLS}},\hat{\beta}_1^{\mathrm{IV}},...,\hat{\beta}_J^{\mathrm{IV}}\right)'$, and $\hat{g}_T=\partial\hat{T}/\partial\hat{\beta}$ is the vector of partial derivatives of $\hat{T}$ with respect to the $2J$ coefficients holding $\hat{\Omega}$ fixed. The second term accounts for outcome heterogeneity:

$$\widehat{\mathrm{Var}}\left[\mathrm{E}\left(\hat{T}\,|\gamma_1,\gamma_2,...,\gamma_J\right)\right]=\hat{\sigma}_\gamma^2\hat{K}_T,$$

where the dispersion of $\hat{\gamma}_j$, net of sampling variation, is given by

$$\hat{\sigma}_\gamma^2=\max\left(\frac{\sum_j\left(\hat{\gamma}_j-\hat{\mu}_\gamma\right)^2-\sum_j\hat{\Omega}_{j,j}^\gamma+J^{-1}\sum_j\sum_{j'}\hat{\Omega}_{j,j'}^\gamma}{J-2},0\right),$$

the term $\hat{\Omega}^{\gamma}_{j,j'} = \hat{\Omega}_{j,j'} + \hat{\tau}^4\hat{\Omega}_{J+j,J+j'} - \hat{\tau}^2\left(\hat{\Omega}_{j,J+j'} + \hat{\Omega}_{j',J+j}\right)$ denotes the sampling covariance between $\hat{\gamma}_j$ and $\hat{\gamma}_{j'}$, and the relevant parameter $\hat{K}_T$ for each estimate is

$$\hat{K}_T = \begin{cases} \left(\sum_j \left(\hat{\beta}_j^{\text{IV}} - \hat{\mu}_{\text{IV}}\right)^2\right)^{-1} & \text{if } T = b, \\ J^{-1} + \hat{\mu}_{\text{IV}}^2 \hat{K}_b & \text{if } T = a, \\ \hat{\rho}^{-2}\hat{K}_b & \text{if } T = \tau^2, \\ J^{-1} + \hat{\mu}_{\text{IV}}^2 \hat{K}_{\tau^2} & \text{if } T = \mu_\gamma, \\ \left(\hat{\beta}_j^{\text{IV}}\right)^2 \hat{K}_{\tau^2} & \text{if } T \in \{\gamma_1, ..., \gamma_J\}, \\ 0 & \text{if } T \in \{b_{\text{R}}, \tau^2_{\text{R}}\}. \end{cases}$$

Note that the restricted meta-regression estimator assumes $\gamma_j = 0$ for all j, hence $\hat{K}_{b_{\text{R}}} = \hat{K}_{\tau^2_{\text{R}}} = 0$ implements the assumption that $\sigma^2_\gamma = 0$.

The p-value and 95% confidence interval, $\hat{T} \pm t_{\nu\left(\hat{T}\right),0.975} \times \text{se}\left(\hat{T}\right)$, use the Satterthwaite degrees of freedom

$$\nu\left(\hat{T}\right) = \frac{\text{se}\left(\hat{T}\right)^4}{(G-1)^{-1}\,\widehat{\text{E}}\left[\text{Var}\left(\hat{T}\,|\gamma_1, \gamma_2, ..., \gamma_J\right)\right]^2 + (J-2)^{-1}\,\widehat{\text{Var}}\left[\text{E}\left(\hat{T}\,|\gamma_1, \gamma_2, ..., \gamma_J\right)\right]^2},$$

where G is the number of clusters.

D.2 Standard errors with reported coefficients

When we only have access to the reported coefficients and standard errors, as in section 5.3 and Appendix E, we compute standard errors by resampling $\hat{\beta}_j^{\text{IV}}$ from its reported asymptotic

distribution and resampling $\hat{\beta}_j^{\text{OLS}}$ using the meta-regression residuals. In particular, for each bootstrap sample $s \in \{1, 2, ..., S\}$ and OLS-IV pair $j \in \{1, 2, ..., J\}$, the IV coefficient is drawn according to

$$\hat{\beta}_{j,s}^{\text{IV}} \sim \mathcal{N}\left(\hat{\beta}_j^{\text{IV}}, \text{se}\left(\hat{\beta}_j^{\text{IV}}\right)^2\right),$$

and the corresponding OLS coefficient is drawn according to

$$\hat{\beta}_{j,s}^{\text{OLS}} = \hat{a} + \hat{b}\hat{\beta}_j^{\text{IV}} + \tilde{v}_{j,s},$$

where the residuals $\tilde{v}_{j,s}$ are drawn from leverage-adjusted meta-regression residuals:

$$\begin{aligned}
\tilde{v}_{j,s} &\sim \text{DiscreteUniform}\left\{\tilde{v}_1, \tilde{v}_2, ..., \tilde{v}_J\right\}, \\
\tilde{v}_j &= \frac{\hat{\beta}_j^{\text{OLS}} - \hat{a} - \hat{b}\hat{\beta}_j^{\text{IV}}}{\sqrt{1 - h_j}}, \\
h_j &= \min\left(\frac{1}{J} + \frac{\left(\hat{\beta}_j^{\text{IV}} - \hat{\mu}_{\text{IV}}\right)^2}{\sum_{j'}\left(\hat{\beta}_{j'}^{\text{IV}} - \hat{\mu}_{\text{IV}}\right)^2}, 0.99\right).
\end{aligned}$$

For the restricted meta-regression, the OLS coefficient is drawn according to

$$\hat{\beta}_{\text{R},j,s}^{\text{OLS}} = \hat{b}_{\text{R}}\hat{\beta}_j^{\text{IV}} + \tilde{v}_{\text{R},j,s},$$

where the restricted residuals $\tilde{v}_{\mathrm{R},j,s}$ are drawn from demeaned leverage-adjusted restricted meta-regression residuals:

$$\begin{aligned}
\tilde{v}_{\mathrm{R},j,s} &\sim \text{DiscreteUniform}\left\{\tilde{v}_{\mathrm{R},1}, \tilde{v}_{\mathrm{R},2}, ..., \tilde{v}_{\mathrm{R},J}\right\}, \\
\tilde{v}_{\mathrm{R},j} &= \frac{\hat{\beta}_j^{\mathrm{OLS}} - \hat{b}_{\mathrm{R}}\hat{\beta}_j^{\mathrm{IV}}}{\sqrt{1-h_{\mathrm{R},j}}} - J^{-1}\sum_j \frac{\hat{\beta}_j^{\mathrm{OLS}} - \hat{b}_{\mathrm{R}}\hat{\beta}_j^{\mathrm{IV}}}{\sqrt{1-h_{\mathrm{R},j}}}, \\
h_{\mathrm{R},j} &= \min\left(\frac{\left(\hat{\beta}_j^{\mathrm{IV}}\right)^2}{\sum_{j'}\left(\hat{\beta}_{j'}^{\mathrm{IV}}\right)^2}, 0.99\right).
\end{aligned}$$

For bootstrap sample s, the meta-regression is

$$\hat{\beta}_{j,s}^{\mathrm{OLS}} = \hat{a}_s + \hat{b}_s\hat{\beta}_{j,s}^{\mathrm{IV}},$$

the restricted meta-regression is

$$\hat{\beta}_{\mathrm{R},j,s}^{\mathrm{OLS}} = \hat{b}_{\mathrm{R},s}\hat{\beta}_{j,s}^{\mathrm{IV}},$$

and the resulting coefficients are used to compute

$$\begin{aligned}
\hat{\tau}_s^2 &= \frac{\hat{b}_s}{\hat{q}\hat{\rho}^2}, \\
\hat{\mu}_{\gamma,s} &= J^{-1}\sum_j \hat{\beta}_{j,s}^{\mathrm{OLS}} - \hat{\tau}_s^2 J^{-1}\sum_j \hat{\beta}_{j,s}^{\mathrm{IV}}, \\
\hat{\gamma}_{j,s} &= \hat{\beta}_{j,s}^{\mathrm{OLS}} - \hat{\tau}_s^2\hat{\beta}_{j,s}^{\mathrm{IV}}, \\
\hat{\tau}_{\mathrm{R},s}^2 &= \frac{\hat{b}_{\mathrm{R},s}}{\hat{q}_{\mathrm{R}}\hat{\rho}_{\mathrm{R}}^2}.
\end{aligned}$$

The standard error is the standard deviation of the bootstrapped estimates:

$$\text{se}\left(\hat{T}\right) = \sqrt{(S-1)^{-1}\sum_{s}\left(\hat{T}_s - s^{-1}\sum_{s'}\hat{T}_{s'}\right)^2},$$

where $\hat{T}_s \in \left\{\hat{b}_s, \hat{a}_s, \hat{\tau}_s^2, \hat{\mu}_{\gamma,s}, \hat{\gamma}_{1,s}, \hat{\gamma}_{2,s}, ..., \hat{\gamma}_{J,s}, \hat{b}_{\text{R},s}, \hat{\tau}_{\text{R},s}^2\right\}$.

The bootstrapped estimates $\left\{\hat{T}_s\right\}_{s=1}^{s}$ are then used to compute a median-centered bootstrapped confidence interval for $\hat{T}$, $\left[\hat{L}_0\left(\hat{T}\right), \hat{U}_0\left(\hat{T}\right)\right]$, where

$$\hat{L}_0\left(\hat{T}\right) = \hat{T} + Q_{0.025}\left(\hat{T}_s - \tilde{T}\right),$$
$$\hat{U}_0\left(\hat{T}\right) = \hat{T} + Q_{0.975}\left(\hat{T}_s - \tilde{T}\right),$$

$\hat{Q}_p\left(\cdot\right)$ denotes the pth quantile, and $\tilde{T} = \hat{Q}_{0.5}\left(\hat{T}_s\right)$ is the median bootstrapped $\hat{T}$. If the true T were known, then a scale $c\left(\hat{T}\right)$ could be computed so that the confidence interval contains the true parameter 95% of the time. In other words, $c\left(\hat{T}\right)$ could be computed such that

$$Pr\left(T \in \left[\hat{T} + c\left(\hat{T}\right) \times Q_{0.025}\left(\hat{T}_s - \tilde{T}\right), \hat{T} + c\left(\hat{T}\right) \times Q_{0.975}\left(\hat{T}_s - \tilde{T}\right)\right]\right) = 95\%,$$

and therefore the true 95% confidence interval could be recovered while preserving any potential asymmetry between the lower interval length $-Q_{0.025}\left(\hat{T}_s - \tilde{T}\right)$ and the upper interval length $Q_{0.975}\left(\hat{T}_s - \tilde{T}\right)$. In particular, the scale would be given by

$$c\left(\hat{T}\right) = Q_{0.95}\left(d\left(T, \hat{T}\right)\right),$$

where

$$d\left(T,\hat{T}\right)=\begin{cases}\frac{\hat{T}-T}{\hat{T}-\hat{L}_0\left(\hat{T}\right)} & \text{if } T<\hat{T},\\ \frac{T-\hat{T}}{\hat{U}_0\left(\hat{T}\right)-\hat{T}} & \text{if } T\geq\hat{T}.\end{cases}$$

Of course, the true parameter T is not observed, hence the scale $c\left(\hat{T}\right)$ cannot be computed. However, a t distribution can be used to compute an estimate $\hat{c}\left(\hat{T}\right)$ of $c\left(\hat{T}\right)$ as follows. Suppose $\left(\hat{T}-T\right)/\text{se}\left(\hat{T}\right)\sim t_{J-K}$, where $K=2$ for the meta-regression estimates and $K=1$ for the restricted meta-regression estimates, and define

$$\hat{c}\left(\hat{T}\right)=Q_{0.95}\left(\hat{d}\left(T,\hat{T}\right)\right),$$

where

$$\hat{d}\left(T,\hat{T}\right)=\begin{cases}\frac{\hat{T}-T}{1.96\times\text{se}\left(\hat{T}\right)} & \text{if } T<\hat{T},\\ \frac{T-\hat{T}}{1.96\times\text{se}\left(\hat{T}\right)} & \text{if } T\geq\hat{T}.\end{cases}$$

Then

$$\hat{d}\left(T,\hat{T}\right)=\frac{|t_{J-K}|}{1.96},$$

hence

$$\hat{c}\left(\hat{T}\right)=\frac{t_{J-K,0.975}}{1.96}.$$

The estimated scale $\hat{c}\left(\hat{T}\right)$ is used to compute the bootstrapped confidence interval for $\hat{T}$, $\left[\hat{L}\left(\hat{T}\right), \hat{U}\left(\hat{T}\right)\right]$, where

$$\hat{L}\left(\hat{T}\right) = \hat{T} + \hat{c}\left(\hat{T}\right) \times Q_{0.025}\left(\hat{T}_s - \tilde{T}\right),$$
$$\hat{U}\left(\hat{T}\right) = \hat{T} + \hat{c}\left(\hat{T}\right) \times Q_{0.975}\left(\hat{T}_s - \tilde{T}\right).$$

E Additional Results

In this section we provide additional empirical applications of the meta-regression to published OLS and IV estimates. In section E.1 we report the results of a leave-one-out analysis for the investment-q meta-regression of section 5.2. In sections E.2 and E.3 we estimate meta-regressions on Schlenker and Walker (2015) and Duranton, Morrow and Turner (2014), respectively. In both cases, as in Adelino, Ma and Robinson (2017) we observe only the printed OLS and IV coefficients and standard errors, and do not have access to the underlying data that produced those coefficients. In section E.4 we present some additional results on measurement and economic invalidity in Adelino, Ma and Robinson (2017).

E.1 Investment-q

It is apparent from Figure 6 that the most influential outcome in the investment-q meta-regression of section 5.2 is Total Investment. Table 8 reports leave-one-out estimates of both the restricted and unrestricted τ^2; the four unrestricted estimates that include Total Investment are all similar to the value reported in Table 5; on the other hand, the unrestricted estimate that excludes Total Investment (0.089) is similar to the four cumulant estimates

of τ^2 using the four outcome variables that are not Total Investment (column 4 of Table 5, rows 3–6).

E.2 Schlenker and Walker (2015)

Schlenker and Walker (2015) analyze the relationship between air pollution levels and health outcomes at the zip-code-level. They note that simply correlating pollution with sickness rates will not identify a causal estimate because "both avoidance behaviour and classical forms of measurement error...lead to significant downward bias in conventional dose-response estimates." (page 771) In addition, on page 775 they describe in detail how they aggregate hourly-level air pollution readings at airports, many of which are often missing, to daily-level zip-code pollution levels; thus, measurement error in their proxy may stem not from faulty pollution-measuring equipment, but from how the information from the instruments is used to construct their proxy.[17] Applying Proposition 2 to seven OLS-IV pairs reported in Tables 3 and 4, our estimates conform with their expectations: we find a negative, though statistically insignificant, omitted variable bias and a very low estimate of $\tau^2 = 0.100$.

Figure 10 plots the seven pairs of OLS and IV coefficients we use from this paper. The authors regress different health outcomes on the same measure of carbon monoxide, with the same control variables, and using the same instrument: airplane taxi time due to congestion, which affects carbon monoxide production at the airport. The wide range of IV estimates is unsurprising given that some health outcomes are more directly affected by carbon monoxide than others; for example, the IV estimate for all respiratory sickness is 0.761, while the IV

[17] To be clear, we are not criticizing the proxy used by Schlenker and Walker; proxies are, by their nature, noisy. Rather, we are merely pointing out that the measurement error in their proxy can be significant, as the authors themselves note.

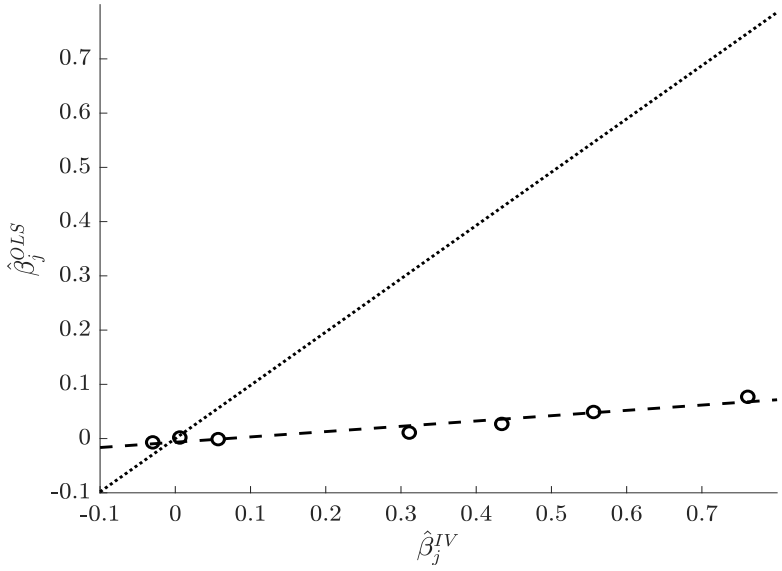


Figure 10. Meta-regression for Schlenker and Walker (2015)
The figure plots $\hat{\beta}_j^{\text{OLS}}$ against $\hat{\beta}_j^{\text{IV}}$ for seven OLS-IV regression pairs reported in Tables 3 and 4 of Schlenker and Walker (2015), where x_i is measured carbon monoxide levels and each y_{ij} is a zip-code-level sickness rate for a different health outcome j. The dashed line corresponds to the meta-regression, which has an intercept of $\hat{a} = -0.00671$, slope of $\hat{b} = 0.0978$, and coefficient of determination of $\hat{r}^2 = 0.930$. The dotted line has a slope of $\hat{q}\hat{\rho}^2 = 0.982$.

estimate for appendicitis is 0.006. The meta-regression identifies τ^2 from the covariation (or lack thereof, as is evident from Figure 10) between IV and OLS estimates, rather than their difference or ratio. In this case, the corresponding OLS estimates are 0.077 for all respiratory sickness and 0.002 for appendicitis.

While this example shows that meta-regression can deliver precise estimates of τ^2 with very few OLS-IV pairs, researchers often have additional OLS results which go unreported. For example, Schlenker and Walker (2015) show a multitude of IV results without their OLS counterparts, including the causal impact of carbon monoxide for patients in four different age categories. The seven OLS-IV pairs plotted in Figure 10 are for all ages combined; had the authors reported all the OLS estimates which go with these IV coefficients, we could have included a total of $4 \times 7 = 28$ observations in the meta-regression (dropping the "all

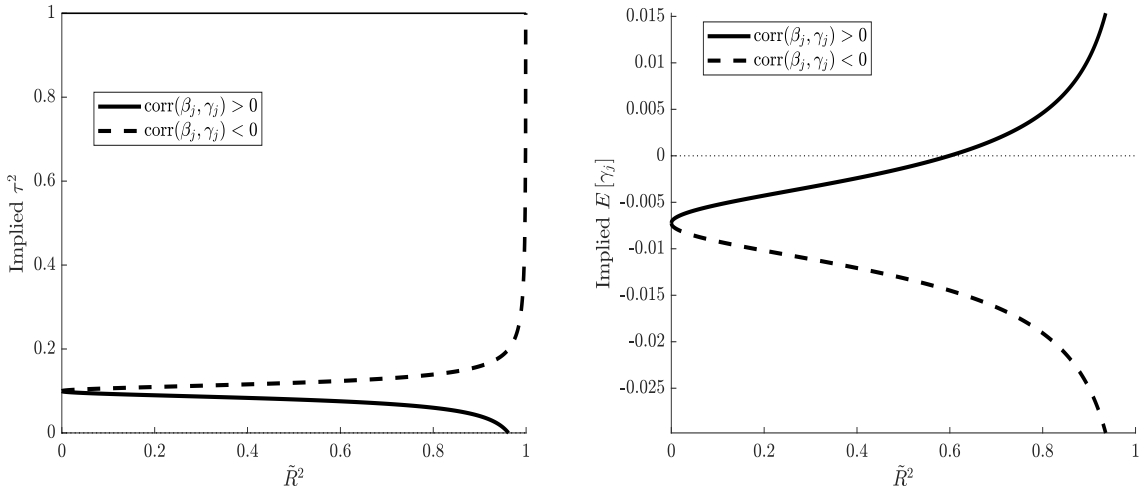


Figure 11. Proposition 3 in Schlenker and Walker (2015)
The figure plots the values of τ^2 (left panel) and $\mathbb{E}\left[\gamma_j\right]$ (right panel) as a function of $\tilde{R}^2$, from Proposition 3, for seven OLS-IV regression pairs reported in Tables 3 and 4 of Schlenker and Walker (2015). The solid lines plot the values when γ_j and β_j are positively correlated, and the dashed lines plot the values when γ_j and β_j are negatively correlated. In the right panel we set the y-axis scale to be twice the average OLS coefficient.

ages" regressions for collinearity). While we are restricted here to the *reported* OLS-IV pairs, researchers using our meta-regression in their own work face no such restrictions.

As Figure 10 shows, the meta-regression $\hat{r}^2$ is very high, which helps us pin down $\hat{q}$ for this particular paper. Because we have neither the underlying data nor the replication code, we cannot estimate $\hat{q}$ directly. However, because the $\hat{r}^2$ is so high, the bounds on $\hat{q}$ implied by Corollary 4 are very tight: $\hat{q} \in [1.126, 1.170]$. We use the midpoint of this interval to estimate τ^2 and $\mathbb{E}\left[\gamma_j\right]$, but our estimates are virtually unchanged if we use either endpoint. Proxy quality in this paper is on the order of 10% regardless of how we correct for sampling variation.

The high value of $\hat{r}^2$ also implies that correlation between β_j and γ_j is unlikely to materially bias our estimates of proxy quality or omitted variable bias. Figure 11 plots the implied ranges of τ^2 and $\mathbb{E}\left[\gamma_j\right]$ as a function of $\tilde{R}^2$ using Proposition 3. Because there is so little

variation in $\hat{\beta}_j^{\text{OLS}}$ that is *not* explained by $\hat{\beta}_j^{\text{IV}}$, even if omitted variable bias can "explain" half of the variation in β_j across regressions, τ^2 would still be between 0.08 and 0.12, and the average omitted variable bias would range from only -0.0013 to -0.0132. At higher values of $\tilde{R}^2$, τ^2 is still quite close to our baseline estimates; $\mathbb{E}\left[\gamma_j\right]$ varies a bit more, but is still less than 10% of the size of the average IV estimates.

E.3 Duranton, Morrow and Turner (2014)

Duranton, Morrow and Turner (2014) estimate the causal impact of interstate highways on trade between U.S. cities, using instruments to address the fact that omitted variables that increase trade are also likely to affect the construction of highways. Applying Proposition 2 to the largest set of OLS-IV pairs reported in the paper with the same controls, instruments, and sample, we estimate that $\tau^2 = 0.900$ and $\mathbb{E}\left[\gamma_j\right] = -0.0545$; Figure 12 plots these results.[18] Unlike Mian and Sufi (2014), Adelino, Ma and Robinson (2017), or Schlenker and Walker (2015), Proposition 2 therefore implies that the regressor is well-measured.

Proposition 2 implies that the average omitted variable bias in this setting is *negative*, which contrasts with the paper's model, in which unobserved city productivity increases both trade and highway construction even in the absence of a causal relationship between the two. One possibility is that other factors, which affect city trade and road construction in opposite directions, are more important than city productivity. If so, identifying what

[18] We use two OLS-IV pairs reported in Tables 4–5, six in Table 7, and another two in Table 8. In fact, there are two sets of ten OLS-IV pairs in these same tables depending on whether or not log percent manufacturing employment is included as a control variable. In Figure 12, we use the pairs that do include the control; however, the meta-regression results are nearly identical if we use the other ten pairs. In total, there are 59 separate OLS-IV pairs reported in Duranton, Morrow and Turner (2014), from Table 3 through Table 11; however, many of these regressions are run on different samples, include different controls, or use different instruments.

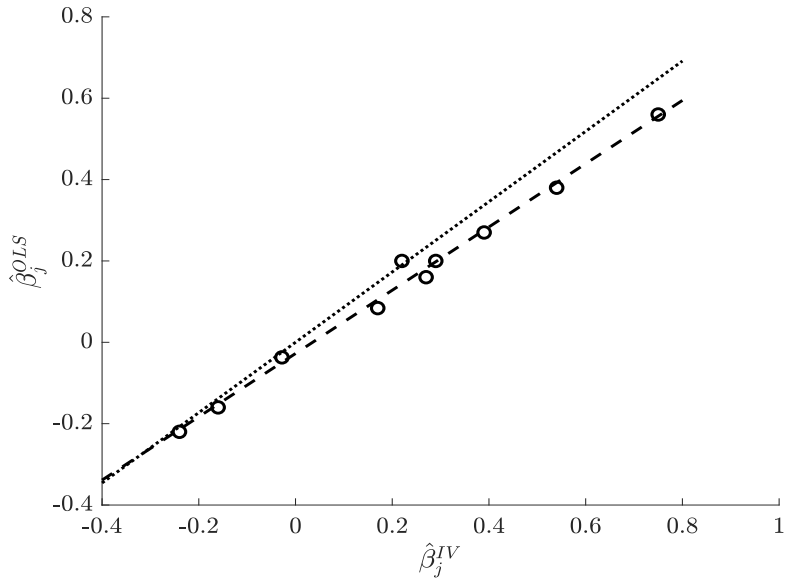


Figure 12. Meta-regression for Duranton, Morrow and Turner (2014)
The figure plots $\hat{\beta}_j^{\text{OLS}}$ against $\hat{\beta}_j^{\text{IV}}$ for ten OLS-IV regression pairs reported in Tables 4, 5, 7, and 8 of Duranton, Morrow and Turner (2014), where x_i is the log of within-city highway kilometers and each y_{ij} is a measure of trade. All ten regressions use all three instruments, have 66 observations, and include log employment, market access, log 1920, 1950, 2000 population, and log percent manufacturing employment as controls. The dashed line corresponds to the meta-regression, which has an intercept of $\hat{a} = -0.0276$, slope of $\hat{b} = 0.778$, and coefficient of determination of $\hat{r}^2 = 0.991$. The dotted line has a slope of $\hat{q}\hat{\rho}^2 = 0.864$.

these factors are, and precisely how they affect trade and road construction, could be an important part of the empirical analysis. Another possibility, which we explore below, is that the instruments are invalid.

It is also worth noting that equation (17) when applied to the OLS-IV pairs in Duranton, Morrow and Turner (2014) implies that $\hat{q}_- > \hat{q}_+$. Thus, it is impossible to simultaneously satisfy $R^2 \leq 1$ and $\rho_j^{\text{OLS-IV}} \in [-1, 1]$ using the paper's published estimates. In order to estimate τ^2 and $\mathbb{E}\left[\gamma_j\right]$, we therefore compute $\hat{q}$ as the midpoint of (22); however, we cannot apply Proposition 3 because the right-hand-side of equation (12) would be negative. Mechanically, this issue arises from the fact that $\hat{r}^2$ is extremely close to 1, despite the substantial sampling variation in $\hat{\beta}_j^{\text{OLS}}$ and $\hat{\beta}_j^{\text{IV}}$, and in particular the fact that the OLS standard errors are (unusually) a bit higher than the IV standard errors. This outcome could simply be the result of sampling variation in the standard errors or $\hat{r}^2$ itself, or potentially indicative of other issues with their estimator.

Proposition 2 applied to this paper implies that the average omitted variable bias is *negative*, which suggests that the relevant omitted variables either reduce highway construction at the same time that they increase trade, or increase highway construction at the same time that they reduce trade. However, in Duranton, Morrow and Turner's economic model, the propensity to trade directly depends on city productivity, which is an omitted variable. If highway construction is positively correlated with city productivity, then the omitted variable bias ought to be *positive*. Thus, Proposition 5 suggests that we are in the left shaded portion of Figure 1; the instruments may be invalid.

Figure 13 explores how the average causal effect in Duranton, Morrow and Turner (2014) changes as a function of the omitted variable bias. If the true average omitted variable bias

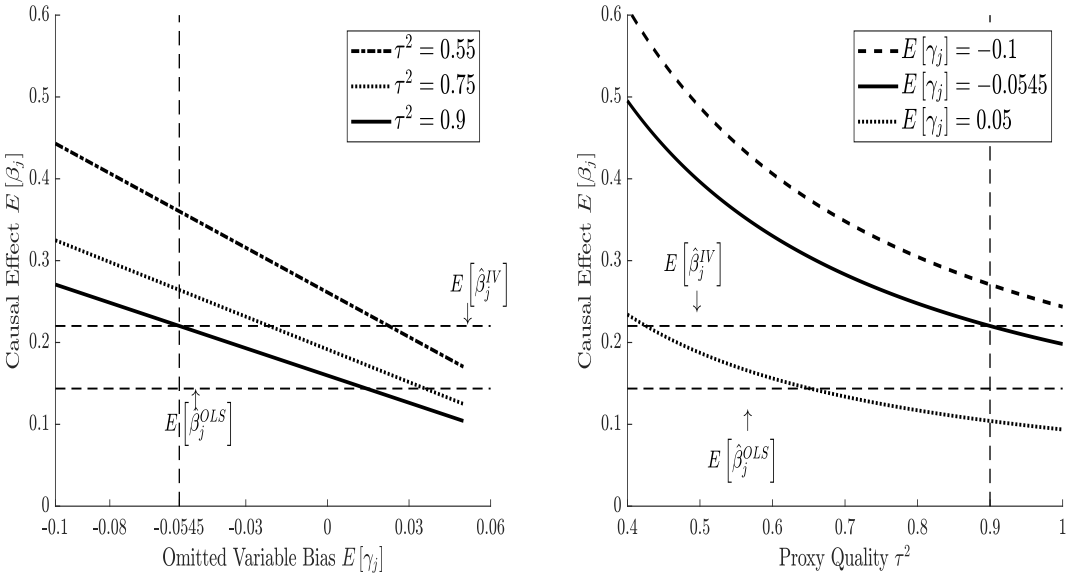


Figure 13. Average Causal Effect in Duranton, Morrow and Turner (2014)
The figure plots the implied average causal effect $\mathbb{E}\left[\beta_j\right]$ when the instrument is invalid for ten OLS-IV pairs reported in Duranton, Morrow and Turner (2014). The left panel plots the value of $\mathbb{E}\left[\beta_j\right]$ as a function of $\mathbb{E}\left[\gamma_j\right]$ given τ^2, and the right panel plots the value of $\mathbb{E}\left[\beta_j\right]$ as a function of τ^2 given $\mathbb{E}\left[\gamma_j\right]$. The pair of horizontal dashed lines report the average values of $\hat{\beta}_j^{\text{IV}}$ and $\hat{\beta}_j^{\text{OLS}}$, respectively. The vertical dashed lines correspond to the baseline estimates from Proposition 2.

is *zero*, then the OLS estimates are close to unbiased when $\tau^2 = 0.9$; the IV estimates only outperform the OLS estimates once $\tau^2 < 0.75$. On the other hand, if the average omitted variable bias is positive, even as low as 0.05, then the OLS estimates outperform the IV estimates regardless of proxy quality. In this case, the meta-regression reveals that if the omitted variable bias is positive, then the researchers are better off without the instrument.

E.4 Invalidity in Adelino, Ma and Robinson (2017)

As in Mian and Sufi (2014), the omitted variable bias we estimate in Adelino, Ma and Robinson (2017) is quantitatively small, while measurement error bias is quantitatively important; allowing for measurement or economic invalidity in their instrument in order to increase the omitted variable bias or proxy quality quickly implies that the OLS estimator outperforms the IV estimator. Figure 14 plots the average true causal effect in their setting as a function of the average omitted variable bias (left panel) or proxy quality (right panel). If $\tau^2 = 0.391$, then the IV estimates only outperform their OLS counterparts if the omitted variable bias is less than about 0.06. As the omitted variable bias becomes more severe, the IV performs even worse. If omitted variable bias is a problem in this paper, this instrument is not the answer. Nevertheless, in both Mian and Sufi (2014) and Adelino, Ma and Robinson (2017), the implied bias in section 5 is not inconsistent with the economic stories in each of their papers. The instruments may be valid; our meta-regressions merely imply that, if they are valid and the omitted variable bias is of the sign the authors expect, then the omitted variable bias must be quantitatively small.

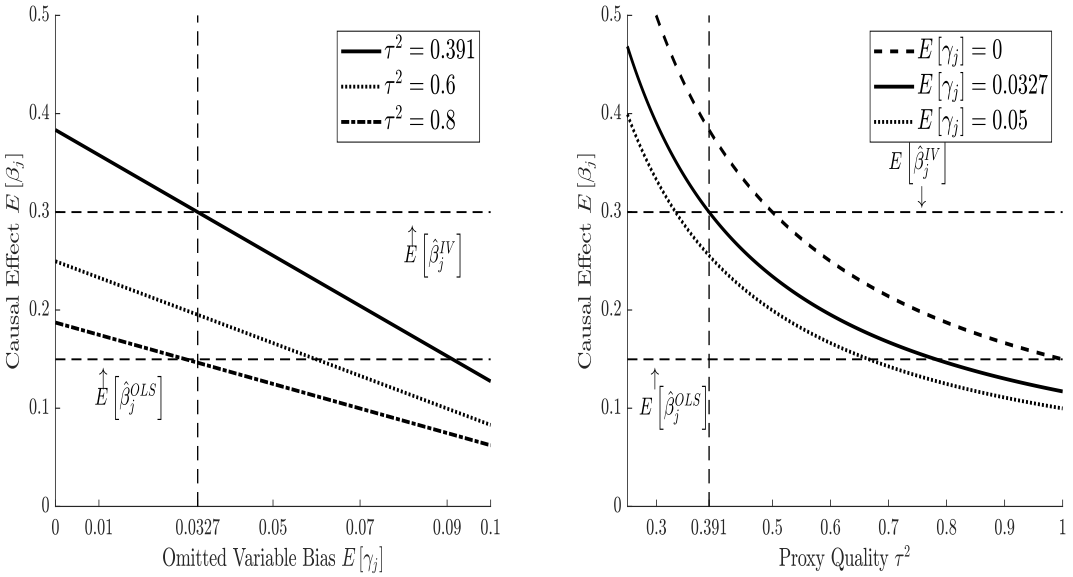


Figure 14. Average Causal Effect in Adelino, Ma and Robinson (2017)

The figure plots the implied average causal effect $\mathbb{E}\left[\beta_j\right]$ when the instrument is invalid for Adelino, Ma and Robinson (2017). The left panel plots the value of $\mathbb{E}\left[\beta_j\right]$ as a function of $\mathbb{E}\left[\gamma_j\right]$ given τ^2, and the right panel plots the value of $\mathbb{E}\left[\beta_j\right]$ as a function of τ^2 given $\mathbb{E}\left[\gamma_j\right]$. The pair of horizontal dashed lines report the average values of $\hat{\beta}_j^{\text{IV}}$ and $\hat{\beta}_j^{\text{OLS}}$, respectively. The vertical dashed lines correspond to the baseline estimates from Proposition 2.

Panel A: Baseline						
	(1)	(2)	(3)	(4)	(5)	(6)
Median Bias ($\hat{\tau}^2$)	0.005	0.003	0.005	0.004	0.001	-0.000
MAD ($\hat{\tau}^2$)	0.100	0.087	0.080	0.063	0.042	0.034
Coverage ($\hat{\tau}^2$)	0.940	0.942	0.943	0.950	0.948	0.951
Out of Range ($\hat{\tau}^2$)	0.031	0.013	0.008	0.001	0.000	0.000
Median Bias ($\hat{\tau}^2_{\mathrm{R}}$)	0.048	0.048	0.050	0.048	0.047	0.046
MAD ($\hat{\tau}^2_{\mathrm{R}}$)	0.067	0.062	0.061	0.053	0.048	0.047
Coverage ($\hat{\tau}^2_{\mathrm{R}}$)	0.185	0.186	0.187	0.188	0.188	0.166
Out of Range ($\hat{\tau}^2_{\mathrm{R}}$)	0.001	0.001	0.000	0.000	0.000	0.000
Median Bias ($\hat{\mu}_\gamma$)	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
MAD ($\hat{\mu}_\gamma$)	0.003	0.002	0.002	0.002	0.001	0.001
Coverage ($\hat{\mu}_\gamma$)	0.939	0.946	0.945	0.948	0.947	0.949
Same Sign ($\hat{\mu}_\gamma$)	0.662	0.679	0.696	0.739	0.829	0.880
Median Bias ($\hat{\gamma}_j$)	-0.000	-0.000	-0.000	-0.000	-0.000	0.000
MAD ($\hat{\gamma}_j$)	0.002	0.002	0.002	0.001	0.001	0.001
Coverage ($\hat{\gamma}_j$)	0.940	0.944	0.944	0.950	0.950	0.950
Same Sign ($\hat{\gamma}_j$)	0.789	0.811	0.826	0.859	0.902	0.918
Number of Outcomes J	5	6	7	10	20	30
Panel B: No Omitted Variable Bias						
	(1)	(2)	(3)	(4)	(5)	(6)
Median Bias ($\hat{\tau}^2$)	0.001	0.000	0.001	0.000	0.000	0.000
MAD ($\hat{\tau}^2$)	0.010	0.009	0.008	0.007	0.006	0.005
Coverage ($\hat{\tau}^2$)	0.962	0.962	0.962	0.958	0.955	0.953
Out of Range ($\hat{\tau}^2$)	0.000	0.000	0.000	0.000	0.000	0.000
Median Bias ($\hat{\tau}^2_{\mathrm{R}}$)	0.000	0.000	0.000	0.000	0.000	0.000
MAD ($\hat{\tau}^2_{\mathrm{R}}$)	0.008	0.007	0.007	0.006	0.006	0.006
Coverage ($\hat{\tau}^2_{\mathrm{R}}$)	0.954	0.951	0.952	0.952	0.951	0.950
Out of Range ($\hat{\tau}^2_{\mathrm{R}}$)	0.000	0.000	0.000	0.000	0.000	0.000
Median Bias ($\hat{\mu}_\gamma$)	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
MAD ($\hat{\mu}_\gamma$)	0.000	0.000	0.000	0.000	0.000	0.000
Coverage ($\hat{\mu}_\gamma$)	0.964	0.964	0.965	0.958	0.958	0.952
Median Bias ($\hat{\gamma}_j$)	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
MAD ($\hat{\gamma}_j$)	0.000	0.000	0.000	0.000	0.000	0.000
Coverage ($\hat{\gamma}_j$)	0.962	0.961	0.960	0.956	0.952	0.950
Number of Outcomes J	5	6	7	10	20	30

Table 6. Baseline Simulation Results: Investment-q

The table shows results from the simulation exercise calibrated to the investment-q data. The median bias and median absolute deviation (MAD) are reported in levels. Coverage is the probability that the true parameter lies within the 95% confidence interval. Out of Range is the probability that the estimate lies outside of $[0, 1]$. Same Sign is the probability that the estimate has the same sign as the true parameter; in the absence of omitted variable bias, $\mu_\gamma = \gamma_j = 0$ for all j, hence we do not include this probability in Panel B.

Panel A: Economic Invalidity						
	(1)	(2)	(3)	(4)	(5)	(6)
Median Bias ($\hat{\tau}^2$)	-0.033	-0.032	-0.031	-0.033	-0.035	-0.035
MAD ($\hat{\tau}^2$)	0.107	0.095	0.087	0.071	0.051	0.044
Coverage ($\hat{\tau}^2$)	0.937	0.943	0.944	0.936	0.920	0.895
Out of Range ($\hat{\tau}^2$)	0.037	0.019	0.011	0.002	0.000	0.000
Median Bias ($\hat{\tau}^2_{\mathrm{R}}$)	-0.127	-0.126	-0.126	-0.126	-0.126	-0.126
MAD ($\hat{\tau}^2_{\mathrm{R}}$)	0.127	0.126	0.126	0.126	0.126	0.126
Coverage ($\hat{\tau}^2_{\mathrm{R}}$)	0.018	0.011	0.008	0.002	0.000	0.000
Out of Range ($\hat{\tau}^2_{\mathrm{R}}$)	0.001	0.001	0.000	0.000	0.000	0.000
Median Bias ($\hat{\mu}_\gamma$)	-0.006	-0.007	-0.007	-0.006	-0.006	-0.006
MAD ($\hat{\mu}_\gamma$)	0.007	0.007	0.007	0.007	0.006	0.006
Coverage ($\hat{\mu}_\gamma$)	0.870	0.847	0.816	0.730	0.474	0.290
Same Sign ($\hat{\mu}_\gamma$)	0.246	0.220	0.195	0.152	0.067	0.035
Median Bias ($\hat{\gamma}_j$)	-0.006	-0.006	-0.007	-0.006	-0.006	-0.006
MAD ($\hat{\gamma}_j$)	0.007	0.007	0.007	0.007	0.006	0.006
Coverage ($\hat{\gamma}_j$)	0.778	0.742	0.704	0.614	0.416	0.308
Same Sign ($\hat{\gamma}_j$)	0.553	0.558	0.561	0.570	0.574	0.574
Number of Outcomes J	5	6	7	10	20	30
Panel B: Measurement Invalidity						
	(1)	(2)	(3)	(4)	(5)	(6)
Median Bias ($\hat{\tau}^2$)	0.944	0.942	0.944	0.932	0.927	0.922
MAD ($\hat{\tau}^2$)	0.959	0.946	0.947	0.932	0.927	0.922
Coverage ($\hat{\tau}^2$)	0.629	0.553	0.478	0.296	0.044	0.007
Out of Range ($\hat{\tau}^2$)	0.774	0.788	0.799	0.840	0.924	0.960
Median Bias ($\hat{\tau}^2_{\mathrm{R}}$)	1.089	1.084	1.088	1.083	1.078	1.076
MAD ($\hat{\tau}^2_{\mathrm{R}}$)	1.089	1.084	1.089	1.083	1.078	1.076
Coverage ($\hat{\tau}^2_{\mathrm{R}}$)	0.010	0.005	0.002	0.000	0.000	0.000
Out of Range ($\hat{\tau}^2_{\mathrm{R}}$)	0.947	0.964	0.974	0.990	0.999	1.000
Median Bias ($\hat{\mu}_\gamma$)	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
MAD ($\hat{\mu}_\gamma$)	0.003	0.003	0.002	0.002	0.001	0.001
Coverage ($\hat{\mu}_\gamma$)	0.915	0.925	0.929	0.938	0.946	0.950
Same Sign ($\hat{\mu}_\gamma$)	0.642	0.660	0.674	0.717	0.804	0.853
Median Bias ($\hat{\gamma}_j$)	-0.000	-0.000	-0.000	-0.000	0.000	0.000
MAD ($\hat{\gamma}_j$)	0.002	0.002	0.002	0.002	0.001	0.001
Coverage ($\hat{\gamma}_j$)	0.922	0.930	0.933	0.941	0.946	0.948
Same Sign ($\hat{\gamma}_j$)	0.758	0.781	0.796	0.827	0.866	0.881
Number of Outcomes J	5	6	7	10	20	30

Table 7. Simulation results with invalidity: Investment-q

The table shows results from the simulation exercise calibrated to the investment-q data. The median bias and median absolute deviation are reported in levels. Coverage is the probability that the true parameter lies within the 95% confidence interval. Out of Range is the probability that the estimate lies outside of $[0, 1]$. Same Sign is the probability that the estimate has the same sign as the true parameter. In Panel A, we set $\mu_\delta = 0.02$ and $\sigma_\delta = 0.006$. In Panel B, we set $\delta_1 = 0.7$.

Excluding:	τ^2_{R}	τ^2
None	0.441 (0.020)	0.394 (0.154)
Total Investment	0.302 (0.041)	0.089 (0.225)
Acquisitions	0.429 (0.020)	0.443 (0.177)
Dividends	0.441 (0.020)	0.361 (0.226)
Net Equity Iss	0.529 (0.016)	0.463 (0.066)
Net LT Debt Iss	0.428 (0.020)	0.403 (0.187)

Table 8. Investment-q Proxy Quality: Leave-One-Out Results

The table reports meta-regression estimates of τ^2, but leaving out each outcome variable in turn. The first column reports results for the restricted estimator in which $\gamma_j = 0$, and the second column reports results for the unrestricted estimator. Standard errors are below each estimate in parentheses.