Investigating Instruments with Meta-Regressions*

N. Aaron Pancost† and Garrett Schaller‡

This version: October 25, 2024

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. A meta-regression of OLS on IV estimates can resolve this puzzle by separating omitted variable bias from measurement error, detecting instrument invalidity, and assessing the relevance of heterogeneous treatment effects. We apply the meta-regression to three published papers. In the first two papers, omitted variable bias is quantitatively less important than measurement error; in the third paper, the instrument appears to be invalid. Our estimates imply that if heterogeneity is relevant, the IV estimates are irrelevant.

Keywords: Measurement error, instrumental variables, endogeneity, heterogeneity
JEL Classifications: C26, C36.

*We thank Tobias Berg (discussant), Andrew Y. Chen (discussant), Lars Peter Hansen, Michael Hasler (discussant), John Hatfield, Wei Jiang, Travis Johnson, Samuel Kruger, Daniel Neuhann, Clemens Sialm, and Toni Whited, as well as seminar participants at the University of Texas, the 2020 Midwest Finance Association Annual Meeting, Florida State University, the 2020 Lone Star Finance Conference, the University of Maryland (Smith), the Federal Reserve Bank of Boston, the Federal Reserve Bank of Chicago, the Federal Reserve Board, McGill University (Desautels), the 2022 AFA Annual Meeting, and Michigan State University (Broad). Any remaining errors are our own. We are grateful to Changyong Song and Minjoo Kim for outstanding research assistance.

†Corresponding author. University of Texas at Austin, McCombs School of Business. Email address: aaron.pancost@mccombs.utexas.edu

‡Colorado State University, College of Business. Email address: garrett.schaller@colostate.edu

1 Introduction

Instrumental variable (IV) regressions are central to the "credibility revolution" in empirical economics (Angrist and Pischke, 2009, 2010). In economics and finance, instruments are primarily used to address omitted variable bias, which is often expected to inflate ordinary least squares (OLS) estimates. Yet, OLS estimates are almost always *smaller* than their IV counterparts (Jiang, 2017; Pancost and Schaller, 2022). Classical measurement error—which was a central motivating factor in the development of IV methods (Wald, 1940; Reiersøl, 1941; Geary, 1943; Durbin, 1954)—can explain this discrepancy, but neither measurement error nor omitted variable bias can be estimated directly from the difference between OLS and IV coefficients.

In this paper, we derive a meta-regression estimator that separately identifies omitted variable bias and measurement error using pairs of OLS and IV estimates. Given a single OLS-IV pair, 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.

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 and heterogeneity in treatment effects. 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. Heterogeneity in treatment effects drives an additional wedge between OLS and IV estimates, which likewise affects the meta-regression estimates.

When instruments are subject to these identification issues, we can then use the meta-regression to infer the extent of invalidity and heterogeneity. 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, certain values of the meta-regression intercept term 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.

As with other tests of instrument validity, there remains the possibility that the instrument is valid but treatment effects are heterogeneous. We provide a general characterization of heterogeneity and instrument invalidity, and show that heterogeneity is only relevant when heterogeneity in the first stage is related to heterogeneity in the second stage. We then analyze a commonly-used case in which observations are grouped into compliers and non-compliers. In this case, both the meta-regression slope and intercept can be informative about the external validity of the IV estimates. Furthermore, information on the relationship between complier and non-complier effects can imply bounds on the measurement error and omitted variable bias.

We apply our meta-regression to three studies in which most of the IV estimates are larger than the corresponding OLS estimates (Mian and Sufi, 2014; Adelino, Ma and Robinson, 2017; Duranton, Morrow and Turner, 2014). Our meta-regression estimates reveal that, in the first two examples, measurement error offers a concise explanation of this disparity: the measurement error bias dwarfs the omitted variable bias, despite the fact that omitted variable bias motivated their use of instruments in the first place. On the other hand, if the omitted variable bias is significant, then our meta-regression implies that either the instrument are invalid, or heterogeneity renders the IV estimates irrelevant. In the third paper, we find very little measurement error; however, the omitted variable bias is of the wrong sign. The latter suggests that either the instrument is invalid, or that heterogeneity renders the IV estimates irrelevant.

Our meta-regression only requires reported coefficients and standard errors. Because this information is almost always reported in published work, this allows referees, discussants, or even seminar participants to easily analyze measurement error, omitted variable bias, instrument validity, and external validity in the underlying regressions, without needing replication code or data.

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.

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. We describe the underlying endogeneity problem in Section 2.1, analyze invalid instruments in Section 2.2, and allow for heterogeneous treatment effects in Section 2.3. In each section, we derive expressions for the meta-regression coefficients, which can be used to disentangle the underlying endogeneity problems, detect violations of the exclusion restriction, and assess the external validity of IV estimates. All proofs are in Appendix A.

2.1 Endogeneity

To start, we first describe the effects of endogeneity on OLS and IV estimates, then show how pairs of OLS and IV estimates can be used to disentangle omitted variable bias from measurement error. Assumption 1 details the baseline environment.

Assumption 1

(i) (y_i, x_i, z_i) is a sequence of observable variables for $i = 1, 2, ..., n$.

(ii) $(\chi_i, u_i, \varepsilon_i)$ is a mean-zero i.i.d. sequence of *unobservable* variables for $i = 1, 2, ..., n$.

(iii) (y_i, x_i) and $(\chi_i, u_i, \varepsilon_i)$ are related to each other as follows:

$$y_i = \chi_i \beta + u_i, \tag{1}$$
$$x_i = \chi_i + \varepsilon_i. \tag{2}$$

(iv) $\text{cov}(\chi_i, \varepsilon_i) = \text{cov}(u_i, \varepsilon_i) = 0.$

(v) $\text{cov}(z_i, u_i) = \text{cov}(z_i, \varepsilon_i) = 0.$

(vi) $\text{cov}(\chi_i, z_i) \neq 0.$

Equation (2) implies that x_i is an observable proxy for the unobservable χ_i, where ε_i represents measurement error. We do not assume that $\text{cov}(\chi_i, u_i) = 0$; hence, u_i in equation (1) is an omitted variable. Plugging (2) into (1) yields

$$y_i = x_i \beta + u_i - \varepsilon_i \beta,$$

where x_i is correlated with the unobservable variables u_i and ε_i. Consequently, an OLS regression of y_i on x_i would fail to recover β, both because of the omitted variable u_i and because of the measurement error ε_i. Assumptions 1(v) and 1(vi) are the standard exclusion restriction and relevance condition for a valid instrument, which we relax in Section 2.2. For notational convenience, we assume that there is a single instrument z_i; it is straightforward to extend our results to accommodate a vector of instruments.

Proposition 1. *Given Assumption 1, we can consistently estimate β by using z_i as an instrument for x_i:*

$$\beta^{IV} \equiv \frac{cov(y_i, z_i)}{cov(x_i, z_i)} = \beta, \tag{3}$$

where the covariance is computed over the joint distribution of $(\chi_i, z_i, u_i, \varepsilon_i)$. By contrast, the OLS estimator converges to

$$\beta^{OLS} \equiv \frac{cov(y_i, x_i)}{var(x_i)} = \tau^2 \beta + \gamma, \tag{4}$$

where $\gamma \equiv \frac{cov(\chi_i, u_i)}{var(x_i)}$ denotes omitted variable bias, and τ^2 is the R^2 from a hypothetical regression of x_i on χ_i:

$$\tau^2 \equiv \frac{var(\chi_i)}{var(x_i)} = 1 - \frac{var(\varepsilon_i)}{var(x_i)}.$$

Proposition 1 defines a measure of proxy quality, τ^2. If $\tau^2 = 1$, x_i is a perfect proxy for χ_i; if $\tau^2 = 0$, x_i is pure noise. The OLS-IV bias, which we define as the difference between (3) and (4), depends on both proxy quality and omitted variable bias:

$$\beta^{\text{OLS}} - \beta^{\text{IV}} = -\left(1 - \tau^2\right)\beta + \gamma. \tag{5}$$

In some settings, we can use economic intuition to infer the direction of the OLS-IV bias. To borrow an example from Jiang (2017), suppose that y_i is adulthood wages and x_i is years of education. The usual omitted variable in this regression is ability, which is unobservable and has a positive effect on both wages and years of education. In this setting, $\beta > 0$ and $\gamma > 0$, hence β^{OLS} should be biased upwards; in other words, we should find that $\beta^{\text{OLS}} > \beta^{\text{IV}}$.

Despite this intuition, OLS estimates of returns to schooling tend to be biased *downwards* (Card, 2001). One possibility is that some hitherto unknown effect overwhelms the ability effect and results in $\gamma < 0$. Another explanation is measurement error: as equation (5) shows, if returns to schooling are positive ($\beta > 0$) but subject to measurement error $\left(\tau^2 < 1\right)$, then β^{OLS} can be biased downwards $\left(\beta^{\text{OLS}} - \beta^{\text{IV}} < 0\right)$ even if education and ability are positively correlated ($\gamma > 0$).

Note the OLS coefficient in equation (4) is a mixture of the true effect β and the omitted variable bias γ, where the true effect is diluted by measurement error bias when $\tau^2 < 1$. There is a pernicious interaction between these biases: as proxy quality decreases, the observed OLS coefficient will be less reflective of the true effect and more reflective of the omitted variable bias. This is particularly concerning when the true effect and omitted variable bias have opposite signs: if the measurement error is sufficiently severe, then the OLS coefficient will take on the sign of the omitted variable bias instead of the true effect.

Substituting (3) into (4) yields

$$\beta^{\text{OLS}} = \tau^2\beta^{\text{IV}} + \gamma. \tag{6}$$

Given a single OLS estimate β^{OLS} and a single IV estimate β^{IV}, equation (6) is one equation in two unknowns, τ^2 and γ. Observing a single pair of coefficients $\left(\beta^{\text{OLS}}, \beta^{\text{IV}}\right)$ is therefore insufficient to separately identify the proxy quality τ^2 and omitted variable bias γ.

On the other hand, if we observe *multiple* OLS and IV estimates, then we can separately identify both the proxy quality and average omitted variable bias. In particular, suppose we observe J pairs of OLS and IV coefficients $\left(\beta_j^{\text{OLS}}, \beta_j^{\text{IV}}\right)$, where each pair corresponds to a different outcome variable $(y_{i1}, y_{i2}, ..., y_{iJ})$. Assumption 2 then replaces Assumption 1.

Assumption 2

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

(ii) $(\chi_i, u_{i1}, u_{i2}, ..., u_{iJ}, \varepsilon_i)$ is a mean-zero i.i.d. sequence of *unobservable* 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:

$$\begin{aligned} y_{ij} &= \chi_i \beta_j + u_{ij}, \\ x_i &= \chi_i + \varepsilon_i. \end{aligned} \tag{7}$$

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

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

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

(vii) β_j and γ_j are independent, where $\gamma_j \equiv \frac{\text{cov}_j\left(x_i, u_{ij}\right)}{\text{var}(x_i)}$.

Note that the covariance operator $\text{cov}_j(\cdot)$ in 2(iv) and 2(v) is relative to the joint distribution of $(\chi_i, z_i, u_{i1}, u_{i2}, ...u_{iJ}, \varepsilon_i)$ across i for a given j, whereas the independence in 2(vii) is relative to the joint distribution of $\left(\beta_j, \gamma_j\right)$ across j.

Assumption 2 replaces the single equation (1) with J equations (7), each corresponding to one of the J dependent variables $(y_{i1}, y_{i2}, ..., y_{iJ})$ and coefficients $(\beta_1, \beta_2, ..., \beta_J)$. We can therefore write the following system of J equations, each resembling (6):

$$\begin{aligned} \beta_1^{\text{OLS}} &= \tau^2 \beta_1^{\text{IV}} + \gamma_1, \\ \beta_2^{\text{OLS}} &= \tau^2 \beta_2^{\text{IV}} + \gamma_2, \\ &\vdots \\ \beta_J^{\text{OLS}} &= \tau^2 \beta_J^{\text{IV}} + \gamma_J. \end{aligned}$$

Following Assumption 2, we can estimate τ^2 by running an OLS regression of β_j^{OLS} on β_j^{IV}, where 2(vii) is analogous to the usual OLS assumption that the regressor β_j^{IV} is uncorrelated with the error term γ_j. Because the outcome and explanatory variables are themselves regression coefficients, we call this a *meta-regression*. We formalize the meta-regression in the proposition below.

Proposition 2. *Given Assumption 2, 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}:*

$$\beta_j^{OLS} = a + b\beta_j^{IV} + v_j, \tag{8}$$

where

$$b \equiv \frac{cov\left(\beta_j^{OLS}, \beta_j^{IV}\right)}{var\left(\beta_j^{IV}\right)} = \tau^2, \qquad (9)$$
$$a \equiv \mathbb{E}\left[\beta_j^{OLS}\right] - b\mathbb{E}\left[\beta_j^{IV}\right] = \mathbb{E}\left[\gamma_j\right].$$

The slope coefficient b identifies proxy quality τ^2, while the intercept coefficient a identifies the average omitted variable bias $\mathbb{E}\left[\gamma_j\right]$ across the J regressions. The meta-regression is therefore particularly useful in cases where the OLS-IV bias is unexpectedly positive or unexpectedly negative; by regressing OLS coefficients on IV coefficients, researchers can effectively control for measurement error, and recover both the sign and magnitude of the average omitted variable bias.

While Proposition 2 demonstrates the intuition behind our meta-regression estimator, it assumes that we can observe β_j^{IV} directly. In practice, however, we typically observe a noisy estimate of β_j^{IV}, which implies that the meta-regression is itself subject to sampling variation. Define

$$\hat{\beta}_j^{\mathrm{IV}} \equiv \beta_j^{\mathrm{IV}} + \varsigma_j^{\mathrm{IV}} \varepsilon_j^{\mathrm{IV}}, \qquad (10)$$

where $\varsigma_j^{\mathrm{IV}}$ is the standard error of the estimate $\hat{\beta}_j^{\mathrm{IV}}$, $\varepsilon_j^{\mathrm{IV}}$ is an unobservable mean-zero random variable with unit variance, and the individual elements of $\left(\beta_j, \varepsilon_j^{\mathrm{IV}}, \varsigma_j^{\mathrm{IV}}\right)$ are mutually independent of each other. Under Assumption 2, equation (10) implies that

$$\hat{\beta}_j^{\mathrm{IV}} \sim \mathcal{N}\left(\beta_j, \left(\varsigma_j^{\mathrm{IV}}\right)^2\right),$$

which captures the notion that the observed IV estimate $\hat{\beta}_j^{\mathrm{IV}}$ is asymptotically normally distributed with mean β_j.

Using $\hat{\beta}_j^{\mathrm{IV}}$ in lieu of β_j^{IV} introduces noise into our meta-regression.[1] Fortunately, given the reported standard errors of $\hat{\beta}_j^{\mathrm{IV}}$, we can correct for this bias following Proposition 3.

Proposition 3. *Given Assumption 2, we can consistently estimate τ^2 and $\mathbb{E}\left[\gamma_j\right]$ with the following meta-regression of $\hat{\beta}_j^{OLS}$ on $\hat{\beta}_j^{IV}$:*

$$\hat{\beta}_j^{OLS} = \hat{a} + \hat{b}\hat{\beta}_j^{IV} + v_j, \qquad (11)$$

[1]By contrast, we can use $\hat{\beta}_j^{\mathrm{OLS}}$ and β_j^{OLS} interchangeably, since noise in $\hat{\beta}_j^{\mathrm{OLS}}$ will not affect the meta-regression (Hausman, 2001).

where

$$\hat{b} \equiv \frac{cov\left(\hat{\beta}_j^{OLS}, \hat{\beta}_j^{IV}\right)}{var\left(\hat{\beta}_j^{IV}\right)} = \tau^2 \hat{\rho}^2,$$
$$\hat{a} \equiv \mathbb{E}\left[\hat{\beta}_j^{OLS}\right] - \hat{b}\mathbb{E}\left[\hat{\beta}_j^{IV}\right] = \mathbb{E}\left[\gamma_j\right] + \tau^2\left(1-\hat{\rho}^2\right)\mathbb{E}\left[\hat{\beta}_j^{IV}\right], \tag{12}$$

and $\hat{\rho}^2 \equiv 1 - \frac{\mathbb{E}\left[\left(\varsigma_j^{IV}\right)^2\right]}{var\left(\hat{\beta}_j^{IV}\right)}$ *is the coefficient of determination from a hypothetical regression of* $\hat{\beta}_j^{IV}$ *on* β_j*.*

Intuitively, noise in $\hat{\beta}_j^{\mathrm{IV}}$ attenuates the meta-regression estimate of τ^2. However, we can use the standard error of $\hat{\beta}_j^{\mathrm{IV}}$ to estimate $\hat{\rho}^2$ and correct for the sampling variation. In other words, we can still use the meta-regression coefficients in (12) to recover τ^2 and $\mathbb{E}\left[\gamma_j\right]$.

All of our results are robust to the inclusion of controls. Proposition 3' in Appendix A extends Proposition 3 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 Invalidity

Proposition 3 relies 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 that affects our interpretation of the meta-regression coefficients. We then demonstrate that the meta-regression can be used to detect violations of the exclusion restriction.

Assumption 3

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

(ii) $(\chi_i, u_{i1}, u_{i2}, ..., u_{iJ}, \varepsilon_i)$ is a mean-zero i.i.d. sequence of *unobservable* 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:

$$y_{ij} = \chi_i \beta_j + u_{ij},$$
$$x_i = \chi_i + \varepsilon_i.$$

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

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

(vi) The individual elements of $\left(\beta_j, \gamma_j, \delta_{j0}\right)$ are mutually independent, where

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

Assumption 3 dispenses with the exclusion restriction from Assumption 2(v), thereby allowing the instrument to 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}, which underpins the discussion of weak instruments in Jiang (2017).

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), 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.

Proposition 4. *Suppose Assumption 3 holds. Following equation (10), we observe*

$$\hat{\beta}_j^{IV} = \left(1 - \delta_1\right)\beta_j + \delta_{j0} + \varsigma_j^{IV}\varepsilon_j^{IV}, \tag{13}$$

where

$$\delta_1 \equiv \frac{cov\left(z_i, \varepsilon_i\right)}{cov\left(x_i, z_i\right)}.$$

The meta-regression estimator from equation (11) converges to

$$\begin{aligned} \hat{b} &= \frac{\tau^2}{1-\delta_1}\left(\hat{\rho}^2 - \phi^2\right), \\ \hat{a} &= \mathbb{E}\left[\gamma_j\right] + \frac{\tau^2}{1-\delta_1}\left(\left(1 - \hat{\rho}^2 + \phi^2\right)\mathbb{E}\left[\hat{\beta}_j^{IV}\right] - \mathbb{E}\left[\delta_{j0}\right]\right), \end{aligned} \tag{14}$$

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 coefficient of determination from a hypothetical regression of δ_{j0} on $\hat{\beta}_j^{IV}$.

In Proposition 4, 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

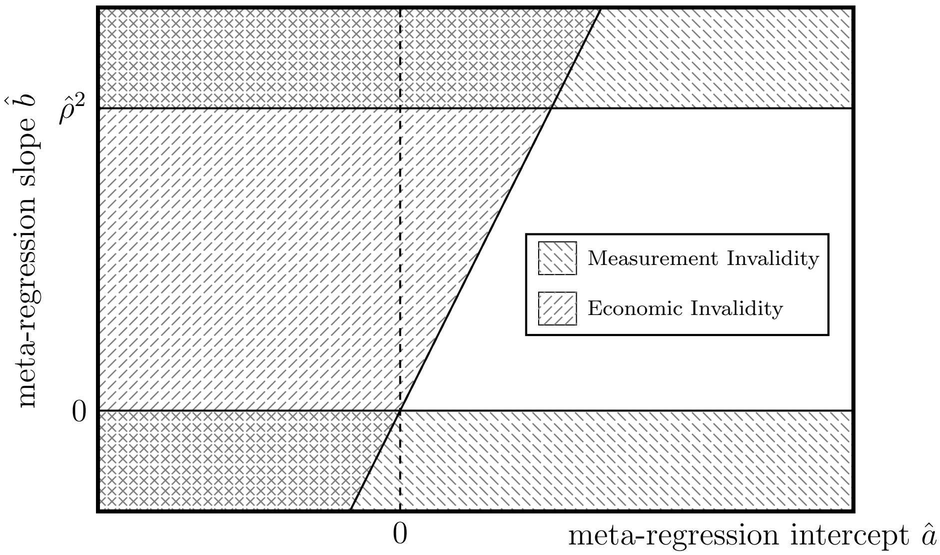

Figure 1. Recognizing Instrument Invalidity

The figure plots the five regions in which the meta-regression implies instrument invalidity, where the diagonal line is $\hat{a} = \hat{b}\mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right]\left(\frac{1}{\hat{\rho}^2} - 1\right)$. If $\hat{b} < 0$ or $\hat{b} > \hat{\rho}^2$, 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.

from measurement invalidity.

The meta-regression can reveal invalidity for certain values of $\left(\hat{a}, \hat{b}\right)$. If we observe $\hat{b} \notin \left[0, \hat{\rho}^2\right]$, 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, $\tau^2 < 1$. In fact, we can use Proposition 4 to derive an upper bound on τ^2, as the following corollary shows.

Corollary 4.1. *Given the meta-regression slope $\hat{b}$ and a value of ϕ^2, the proxy quality τ^2 satisfies:*

$$\tau^2 \leq \frac{-2k_\phi r_{xz}^2 - 1 + \sqrt{4k_\phi\left(k_\phi + 1\right) r_{xz}^2 + 1}}{2k_\phi^2 r_{xz}^2} \tag{15}$$

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

If $\phi^2 = \hat{\rho}^2 - \hat{b}$, then $k_\phi = -1$, and (15) reduces to $\tau^2 \leq 1$.

Likewise, if the average omitted variable bias is positive and we observe $\hat{a} < \hat{b}\mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right]\left(\frac{1}{\hat{\rho}^2} - 1\right)$, or if the average omitted variable bias is negative and we observe $\hat{a} > \hat{b}\mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right]\left(\frac{1}{\hat{\rho}^2} - 1\right)$, then there must be *economic* invalidity. Figure 1 plots the values of $\left(\hat{a}, \hat{b}\right)$ that imply violations of the exclusion restriction.

Furthermore, we can rearrange the terms in Proposition 4 to obtain:

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

Once we condition on the meta-regression coefficients, the relationship between τ^2 and δ_1 does not depend on $\left(\mathbb{E}\left[\gamma_j\right], \mathbb{E}\left[\delta_{j0}\right]\right)$; likewise, the relationship between $\mathbb{E}\left[\gamma_j\right]$ and $\mathbb{E}\left[\delta_{j0}\right]$ does not depend on $\left(\tau^2, \delta_1\right)$. We can therefore use (16) to show how departures from the estimates of τ^2 and $\mathbb{E}\left[\gamma_j\right]$ in Proposition 3 translate into measurement invalidity and economic invalidity, respectively.

While (16) conditions on the meta-regression coefficients to isolate τ^2 from $\mathbb{E}\left[\gamma_j\right]$, we can alternatively isolate the average causal effect from the terms governing invalidity:

$$\mathbb{E}\left[\beta_j\right] = \frac{1}{\tau^2}\left(\hat{a} + \hat{b}\mathbb{E}\left[\hat{\beta}_j^{\mathrm{IV}}\right] - \mathbb{E}\left[\gamma_j\right]\right). \tag{17}$$

By the same token, we can use equation (17) to show how departures from the estimates of τ^2 and $\mathbb{E}\left[\gamma_j\right]$ in Proposition 3 translate into the average causal effect $\mathbb{E}\left[\beta_j\right]$.

2.3 Heterogeneity

We now 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.

Assumption 4

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

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

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

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

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

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

(vi) $\left(\beta_j^\chi, \beta_j^z\right)$ and (γ_j, 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 4(iii) allows for heterogeneous treatment effects: β_{ij} can vary across observations i for each regression j. Consequently, Assumption 4(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.[2]

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 5. *Suppose Assumption 4 holds. Then the OLS estimator converges to*

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

while the observed IV estimator is given by

$$\hat{\beta}_j^{IV} = (1 - \delta_1)\beta_j^z + \delta_{j0} + \varsigma_j^{IV}\varepsilon_j^{IV}.$$

[2] By construction, $\mathbb{E}[\omega_{im}] = 1$, 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.

The meta-regression estimator converges to:

$$\begin{aligned} \hat{b} &= \frac{\tau^2}{1-\delta_1}\left(\hat{\rho}^2 - \phi^2\right)\psi_1, \\ \hat{a} &= \mathbb{E}\left[\gamma_j\right] + \tau^2\left(\psi_0 + \frac{\psi_1}{1-\delta_1}\left(\left(1-\hat{\rho}^2+\phi^2\right)\mathbb{E}\left[\hat{\beta}_j^{IV}\right] - \mathbb{E}\left[\delta_{j0}\right]\right)\right), \end{aligned} \tag{18}$$

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 (18) is equivalent to (14). As the following corollary shows, even if there is heterogeneity, it may still be irrelevant to the meta-regression.

Corollary 5.1. *Suppose Assumption 4 holds and*

$$cov_j\left(\chi_i^2, \beta_{ij}\right) = cov_j\left(\chi_i z_i, \beta_{ij}\right) = 0 \tag{19}$$

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: (18) is equivalent to (14).*

In a linear environment, equation (19) will hold if heterogeneity in the first-stage coefficient α_i is independent of heterogeneity in the treatment effect β_{ij} for each j. This is trivially satisfied if, for example, 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.

For practical purposes, it is helpful to cast heterogeneity in a more familiar setting with compliant and non-compliant subpopulations.[3]

Corollary 5.2. *Suppose Assumption 5 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

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

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

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

$$\hat{\beta}_j^{IV} = (1-\delta_1)\,\beta_{j1} + \delta_{j0} + \varsigma_j^{IV}\varepsilon_j^{IV}.$$

The meta-regression estimator converges to

$$\begin{aligned}\hat{b} &= \frac{\tau^2}{1-\delta_1}\left(\hat{\rho}^2 - \phi^2\right)\left(\lambda + (1-\lambda)\,\theta_1\right), \\ \hat{a} &= \mathbb{E}\left[\gamma_j\right] + \tau^2\left[(1-\lambda)\,\theta_0 + \frac{\lambda + (1-\lambda)\,\theta_1}{1-\delta_1}\left(\left(1-\hat{\rho}^2+\phi^2\right)\mathbb{E}\left[\hat{\beta}_j^{IV}\right] - \mathbb{E}\left[\delta_{j0}\right]\right)\right],\end{aligned} \tag{20}$$

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 5.2 is a special case of Proposition 5 that allows us to discuss the arbitrary heterogeneity in Assumptions 4 in terms of compliers and non-compliers (Angrist, Imbens and Rubin, 1996). Assumption 4(v) implies that $\alpha \neq 0$, hence the treatment χ_i and instrument z_i are only correlated in the subpopulation 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 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 subpopulation 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 2.2, we can condition on 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 5.2 implies:

$$\begin{aligned}\lambda &= \frac{1}{1-\theta_1}\left(\frac{\hat{b}}{\tau^2\hat{\rho}^2}-\theta_1\right),\\ \lambda &= \frac{\theta_0\hat{b}+\theta_1\left(\hat{\rho}^2\left(\mathbb{E}\left[\gamma_j\right]-\hat{a}\right)+\left(1-\hat{\rho}^2\right)\mathbb{E}\left[\hat{\beta}_j^{IV}\right]\hat{b}\right)}{\theta_0\hat{b}+\left(\theta_1-1\right)\left(\hat{\rho}^2\left(\mathbb{E}\left[\gamma_j\right]-\hat{a}\right)+\left(1-\hat{\rho}^2\right)\mathbb{E}\left[\hat{\beta}_j^{IV}\right]\hat{b}\right)}\end{aligned} \tag{21}$$

When $\theta_1 = 1$, θ_0 indexes the average difference (across regressions) between complier and non-complier effects. In this case, equation (21) reduces to

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

After conditioning on 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.

3 Meta-regression in practice

We now run the meta-regression on pairs of OLS and IV coefficients from three published papers: Mian and Sufi (2014), Adelino, Ma and Robinson (2017), and Duranton, Morrow and Turner (2014). In all three papers, the average magnitude of the IV coefficients vastly exceeds the average magnitude of the OLS coefficients, which is surprising, since economic intuition suggests that the omitted variable bias should be positive. Provided their instruments are valid, Proposition 3 implies that the meta-regression coefficients separately identify proxy quality τ^2 and the average omitted variable bias $\mathbb{E}\left[\gamma_j\right]$ for all three papers. Alternative values of τ^2 and $\mathbb{E}\left[\gamma_j\right]$ directly imply invalidity and/or heterogeneity, using Propositions 4 and 5.

Figure 2 plots the implied measurement error and omitted variable bias for all three papers under the assumptions of Proposition 3. For the first two papers, the meta-regression implies a value of τ^2 that is substantially less than one, which suggests that measurement error is pervasive in their respective explanatory variables; by contrast, there seems to be little measurement error in Duranton, Morrow and Turner (2014). Note that we use Figure 2 mainly as a convenient way to describe the meta-regression results of all three papers together; because each paper's value of $\mathbb{E}\left[\hat{\beta}_j^{\text{IV}}\right]\left(\frac{1}{\hat{\rho}^2}-1\right)$ is different, they cannot all be plot-

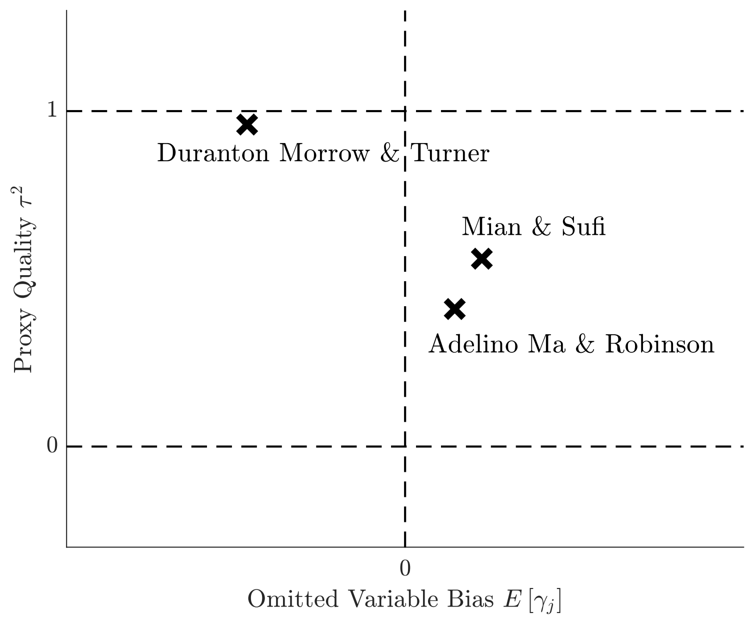

Figure 2. Three meta-regressions
The figure plots the estimated values of τ^2 and $\mathbb{E}\left[\gamma_j\right]$ from (12) in Proposition 3 using the respective OLS and IV coefficients from Table IV in Mian and Sufi (2014), Tables III, V, and VI in Adelino, Ma and Robinson (2017), and Table 7 in Duranton, Morrow and Turner (2014).

ted together in the $\left(\hat{a}, \hat{b}\right)$ plane as in Figure 1. We discuss the possibility that each paper's estimates may be invalid, or subject to heterogeneity, in detail below.

The estimated meta-regression coefficients also imply that the average omitted variable bias is positive for the first two papers, which resolves the puzzling direction of their OLS-IV bias. Indeed, the severity of the measurement error problem is precisely what deflates the OLS coefficients and therefore drives down the OLS-IV bias in both of these papers. On the other hand, because we find little measurement error in Duranton, Morrow and Turner (2014), the omitted variable bias still appears to be strongly negative, counter to economic intuition. And, while the estimated omitted variable bias is the correct sign for the first two papers, it seems quantitatively insignificant, which is surprising given the amount of attention afforded to it by the authors.

The low, and in one case negative, average omitted variable bias implied by Proposition 3 suggest that the instruments may be invalid. Following Proposition 4, we show that reasonable values of the average omitted variable bias, which differ from the estimates implied by Proposition 3, imply that the OLS estimates for each paper are, on average, closer to the truth than the IV estimates. In fact, the more omitted variable bias there is, the *worse* the IV estimator performs.

It may also be the case that the instruments are valid, but there is substantial heterogeneity in the underlying treatment effects for each regression. However, Corollary 5.2 applied to our then implies that there may be limited external validity for the IV estimates. In particular, if the instruments are valid, then the local average treatment effects that they identify may be substantially different from the average treatment effects across all observations.

3.1 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 offers the following interpretation of their results:

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.

Mian & 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 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 regressions they run are of the form:

$$\Delta \mathrm{E}_i^{\mathrm{NT}} = \alpha + \beta \Delta HNW_i + \varepsilon_i, \tag{22}$$

where $\Delta \mathrm{E}_i^{\mathrm{NT}}$ is employment growth from 2006–2009 in non-tradable industries in county i and ΔHNW_i is their measured shock to housing net worth in county i over the same period, defined as the change in house prices times the value of the housing stock, divided by total net worth in 2006:

$$\Delta HNW_i \equiv \frac{\Delta p^{H,i}_{2006-2009} \times H^i_{2006}}{S^i_{2006} + B^i_{2006} + H^i_{2006} - D^i_{2006}},$$

where $p^{H,i}_{2006-2009}$ is a measure of house price growth in county i, and S^i_{2006}, B^i_{2006}, H^i_{2006}, and D^i_{2006} are county-level stock, bond, house, and debt values in dollars. Mian and Sufi estimate S^i_{2006} and B^i_{2006} using IRS data on county-level dividend and interest income, assuming that households in each county hold the market portfolios of stocks and bonds. Mian, Rao and Sufi (2013) point out that any cross-sectional variation in portfolio holdings across counties introduces measurement error into ΔHNW_i. Moreover, ΔHNW_i unavoidably picks up noise from projecting house prices, population, and homeownership rates forward to 2006 and 2009 using the 2000 Decennial Census.

The quote above suggests that the authors are worried about positive omitted variable bias: $\gamma > 0$. Absent measurement error, Proposition 1 implies that we should then observe IV estimates that are *smaller* than their OLS counterparts. Yet, among the pairs of OLS and IV estimates that we observe, seven of the ten IV estimates are *larger* than their OLS counterparts. Measurement error can explain this discrepancy. If ΔHNW_i 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.

While many of the IV estimates are larger than their OLS counterparts, the meta-regression indicates that the average omitted variable bias is indeed positive. In Figure 3, we plot the six OLS-IV regression pairs reported in Table IV of Mian and Sufi (2014) which differ only in the outcome variable of interest; we omit the OLS-IV pairs reported in Tables III and IV that include different control variables or are run on different samples.[4] We find $\hat{a} = 0.057$, $\hat{b} = 0.44$, and $\hat{\rho}^2 = 0.79$; accounting for sampling variation in the reported coefficients, Proposition 3 then suggests that $\tau^2 = 0.56$ and $\mathbb{E}\left[\gamma_j\right] = 0.034$. In other words, it does appear that the OLS estimates are biased upward due to omitted variable bias, consistent with the authors' economic intuition.

Yet, omitted variable bias is quantitatively less important than measurement error bias. For example, the estimated elasticity of employment growth to housing net worth at large

[4] The IV regressions Mian & Sufi report in Table IV are run on slightly smaller samples than their OLS counterparts due to missing values of the Saiz instrument; our results are nearly identical if we re-estimate the OLS regressions on the same sample as the IV estimates.

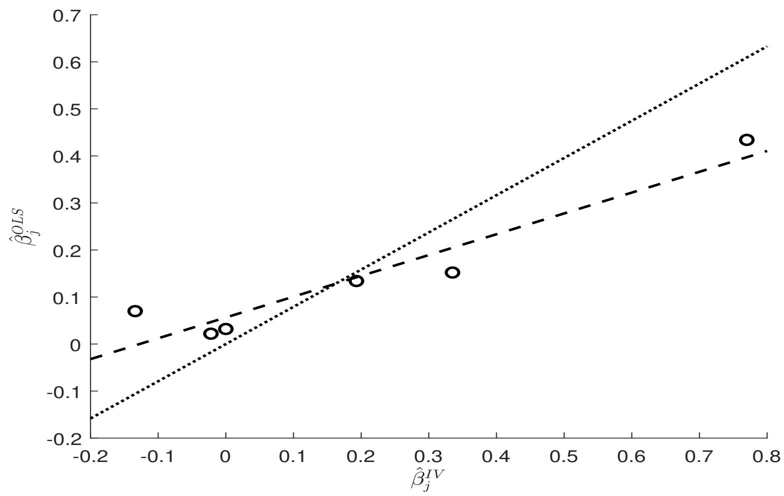

Figure 3. Meta-regression for Mian and Sufi (2014)
The figure plots $\hat{\beta}_j^{\mathrm{OLS}}$ against $\hat{\beta}_j^{\mathrm{IV}}$ for six regressions reported in Table IV of Mian and Sufi (2014), where x_i is a proxy for county-level growth in housing net worth and each y_{ij} is a measure of county-level employment growth. The dashed line corresponds to the meta-regression, which has an intercept of $\hat{a} = 0.057$ and a slope of $\hat{b} = 0.44$. The dotted line has a slope of $\hat{\rho}^2 = 0.79$.

firms is 0.770 (Table IV, column 6). The corresponding OLS estimate of 0.434 is about half that, almost entirely due to measurement error: $0.770 \times 0.56 = 0.43$. Were the proxy perfectly measured, the meta-regression estimates suggest that the OLS estimate would have been $0.770 + 0.034 = 0.804$. If their instrument is valid, then it is mainly addressing a substantial measurement error problem, while the omitted variable bias is insubstantial.

It may also be the case that the average omitted variable bias is a substantive problem, but the instrument is invalid. Following Proposition 4, if the average omitted variable bias differs from 0.034, then it must be the case that the instrument suffers from economic invalidity; likewise, if the measurement error differs from 0.56, 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 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 3, then this again implies instrument invalidity, but the resulting IV estimates will instead be

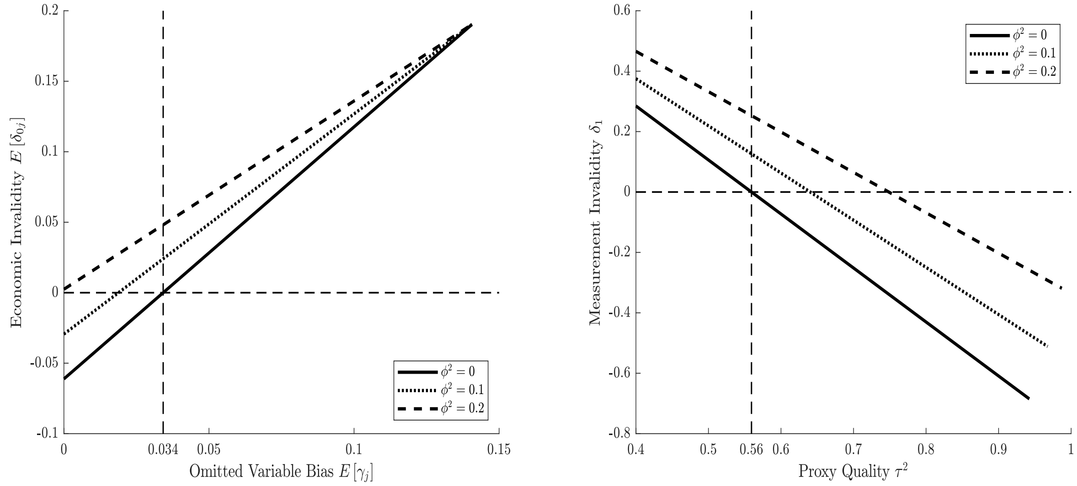

Figure 4. Instrument Invalidity in Mian and Sufi (2014)

The figure plots (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 3. In the right panel, each line ends at the maximum value of τ^2 implied by equation (15).

biased downwards.

Reasonable values of the average omitted variable bias or proxy quality imply that, on average, the OLS estimates outperform the IV estimates. Figure 5 plots 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 0.05 at our baseline estimates of τ^2. Likewise, if the proxy quality is higher than 0.56, then at our baseline estimates of the omitted variable bias, the OLS estimates are closer to the truth than the IV estimates.

It is also possible that the instrument is valid, but proxy quality and omitted variable bias depart from Proposition 3 due to heterogeneous treatment effects in equation (22), which has implications for the external validity of the IV estimates. Following Corollary 5.2, 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 $\lambda = 0.56$. In other words, the IV estimates are identified based on a compliant subpopulation that accounts for 56% of the variation in housing net worth shocks.

Figure 6 plots the implied value of λ as a function of average omitted variable bias in the

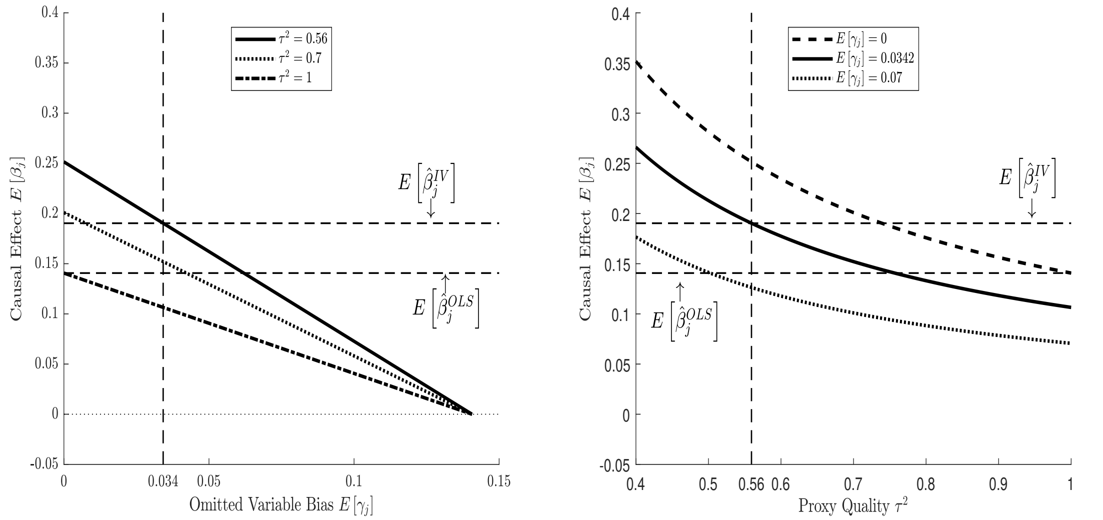

Figure 5. Average Causal Effect in Mian and Sufi (2014)

The figures plots equation (17) 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 $\hat{\beta}_j^{\mathrm{IV}}$ and $\hat{\beta}_j^{\mathrm{OLS}}$, respectively. The vertical dashed lines correspond to the baseline estimates from Proposition 3.

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 3 suggest that the IV is identified by an increasingly irrelevant compliant subpopulation. 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 without making the IV estimates irrelevant. In the left panel of Figure 6, 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 0.065; if $\theta_0 = -0.1$, then the omitted variable bias cannot be more than 0.09. 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 0.034. Similarly, as can be seen in the right panel of Figure 6, θ_1 imposes bounds on τ^2: $\theta_1 < 1$ implies that $\tau^2 > 0.56$, and as proxy quality increases, λ decreases.

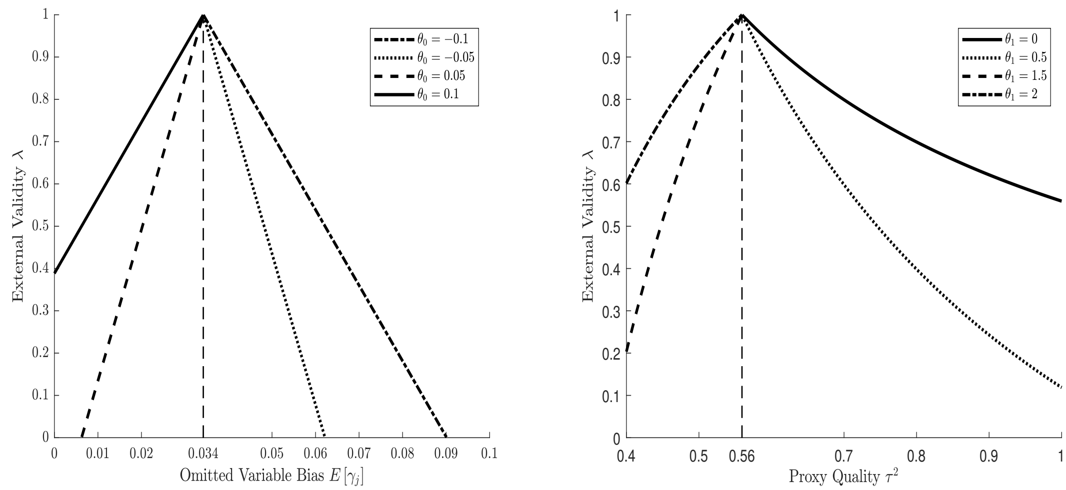

Figure 6. Estimated λ for Mian and Sufi (2014)
The figures plots (21) for Mian and Sufi (2014). The left panel plots λ as a function of $\mathbb{E}\left[\gamma_j\right]$ given θ_0 for $\theta_1 = 1$, and the right panel plots λ as a function of τ^2 given θ_1. The vertical dashed lines correspond to the baseline estimates from Proposition 3.

3.2 Adelino, Ma and Robinson (2017)

Applying the meta-regression to 20 reported OLS-iV pairs in Adelino, Ma and Robinson (2017) leads to broadly similar conclusions as Mian and Sufi (2014): if the instruments are valid and treatment effects are homogeneous, then the quantitative effect of omitted variable bias is small, while measurement error leads to a severe bias in the OLS estimates. On the other hand, increasing the implied omitted variable bias by invoking instrument invalidity quickly makes the IV more biased than the OLS (Proposition 4), while invoking heterogeneity implies that the reported IV estimates are largely irrelevant (Corollary 5.2).

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

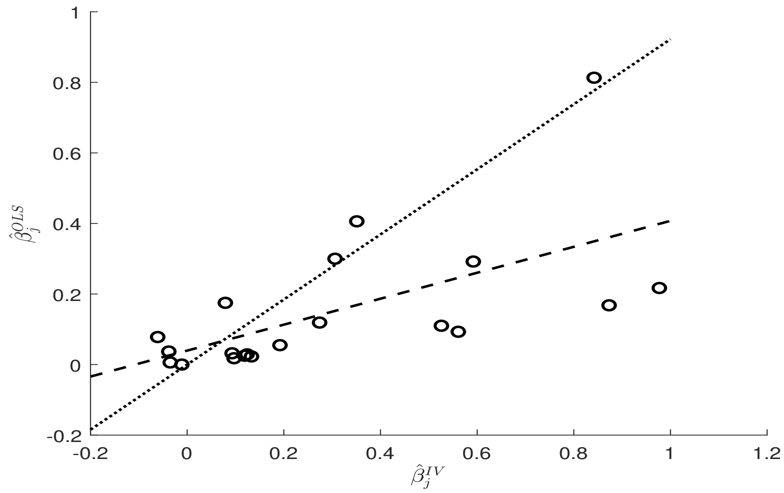

Figure 7. Meta-regression for Adelino, Ma and Robinson (2017)
The figure plots $\hat{\beta}_j^{\text{OLS}}$ against $\hat{\beta}_j^{\text{IV}}$ for 20 regressions reported in Adelino, Ma and Robinson (2017), where x_i is CZ-level wage growth and each y_{ij} is a measure of CZ-level job creation and destruction. The dashed line corresponds to the meta-regression, which has an intercept of $\hat{a} = 0.03$ and a slope of $\hat{b} = 0.38$. The dotted line has slope of $\hat{\rho}^2 = 0.93$.

the 20 IV estimates reported in the paper are larger than their OLS counterparts.[5] Moreover, this difference is substantial: on average, each IV estimate is *two and a half times larger* than the corresponding OLS estimate. Once again, the meta-regression can resolve this puzzle; Proposition 3 implies that the average variable bias is indeed positive, $\mathbb{E}\left[\gamma_j\right] = 0.030$, but their regressor is subject to substantial measurement error, $\tau^2 = 0.40$. Figure 7 plots these results.

However, as in Mian and Sufi (2014), the average omitted variable bias implied by Proposition 3 is negligible. For example, consider the IV estimate reported in column 5 of Table III, which reports the key estimate of the paper: the elasticity of the net change in employment in new firms to investment opportunities. The reported IV estimate is 0.274; omitted variable bias alone would imply a corresponding OLS estimate of $0.304 = 0.274 + 0.030$, whereas measurement error alone would imply a corresponding OLS estimate of $0.11 = 0.274 \times 0.40$. The actual OLS estimate is 0.119—approximately equal to what measurement error alone would predict.

Following Proposition 4, the true τ^2 and $\mathbb{E}\left[\gamma_j\right]$ can differ from the estimates implied by Proposition 3 provided the instrument is *in*valid; Figure 8 plots the implications for

[5]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.

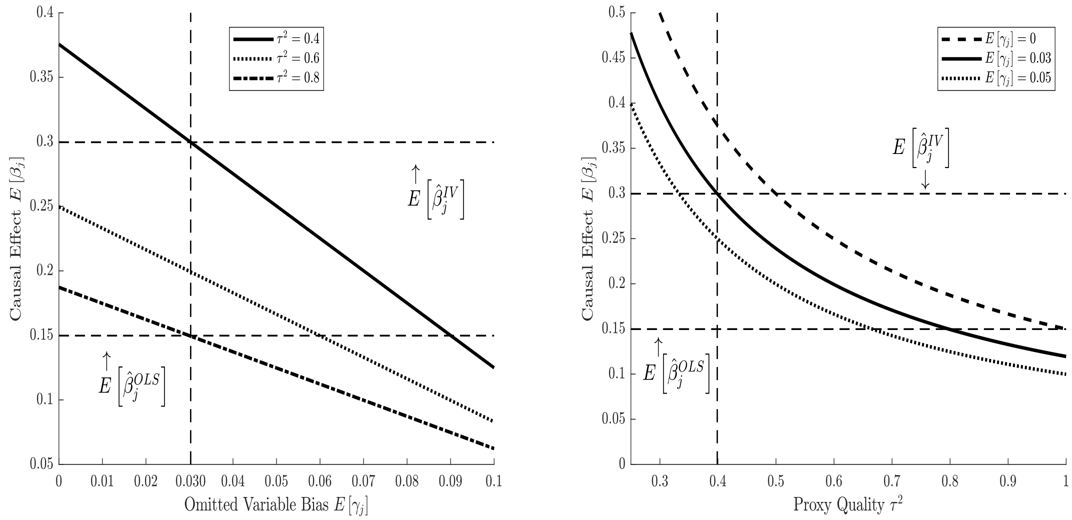

Figure 8. Average Causal Effect in Adelino, Ma and Robinson (2017)
The figures plots equation (17) for Adelino, Ma and Robinson (2017). The left panel plots the value of $\mathbb{E}\left[\beta_j\right]$ as a function of $\mathbb{E}\left[\gamma_j\right]$ given τ^2, and the right panel plots the value 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 3.

the average causal effect $\mathbb{E}\left[\beta_j\right]$. Starting with the left panel, if $\tau^2 = 0.40$, then the IV estimates outperform their OLS counterparts on average only if the omitted variables bias is less than about 0.06. Furthermore, if proxy quality is actually higher than the 0.40 implied by Proposition 3, then the OLS estimates are closer to unbiased even for other values of the omitted variable bias (right panel, and dotted lines in the left panel). Of course, if the true proxy quality is lower than 0.40, the reverse could be true; using an instrument that addresses a severe measurement error bias might be worth failing to address or even aggravating the omitted variable bias in that case.

Following Proposition 5, it is also possible that the average omitted variable bias is higher than 0.030, but the meta-regression is confounded by heterogeneity, as opposed to invalidity; however, as in Mian and Sufi (2014), our estimates then imply that the IV estimates have limited external validity. For example, the left panel of Figure 9 shows that if the average difference between complier and non-complier effects 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 0.030 implied by Proposition 3, 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

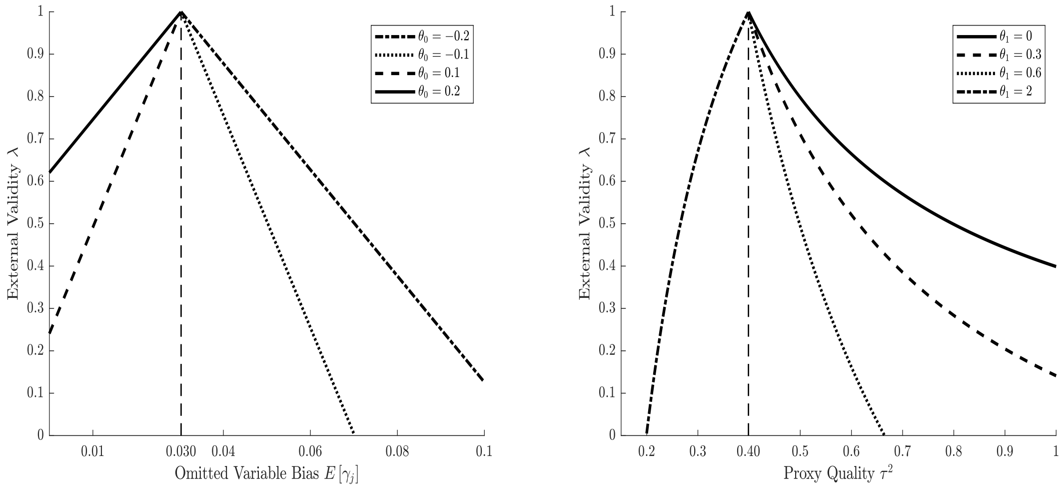

Figure 9. Estimated λ for Adelino, Ma and Robinson (2017)
The figures plots equation (21) for Adelino, Ma and Robinson (2017). The left panel plots λ as a function of $\mathbb{E}\left[\gamma_j\right]$ given θ_0 for $\theta_1 = 1$, and the right panel plots λ as a function of τ^2 given θ_1. The vertical dashed lines correspond to the baseline estimates from Proposition 3.

IV coefficient applies to a compliant subpopulation that accounts for almost none of the variation in income growth. Likewise, it is possible that τ^2 differs from the estimates implied by Proposition 3, but this again implies that the IV estimates are increasingly irrelevant, as shown in the right panel of Figure 9.

3.3 Duranton, Morrow and Turner (2014)

Duranton, Morrow and Turner (2014) estimate the causal impact of interstate highways on trade between US cities, using instruments to address the fact that omitted variables that increase trade are also likely to affect the construction of highways. Applying Proposition 3 to six OLS-IV pairs reported in their Table 7, we estimate that $\tau^2 = 0.96$ and $\mathbb{E}\left[\gamma_j\right] = -0.068$; Figure 10 plots these results.[6] Unlike the previous two examples, Proposition 3 therefore implies that the regressor is well-measured.

But Proposition 3 also 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 model, the propensity to trade directly depends on city productivity, which is an omitted variable. If highway construction

[6] In fact, there are two sets of six OLS-IV pairs in Table 7 of Duranton, Morrow and Turner (2014), depending on whether or not manufacturing employment is included as a control variable. In Figure 10, we use the six pairs that do not include the control; however, the meta-regression results are nearly identical if we use the other six pairs.

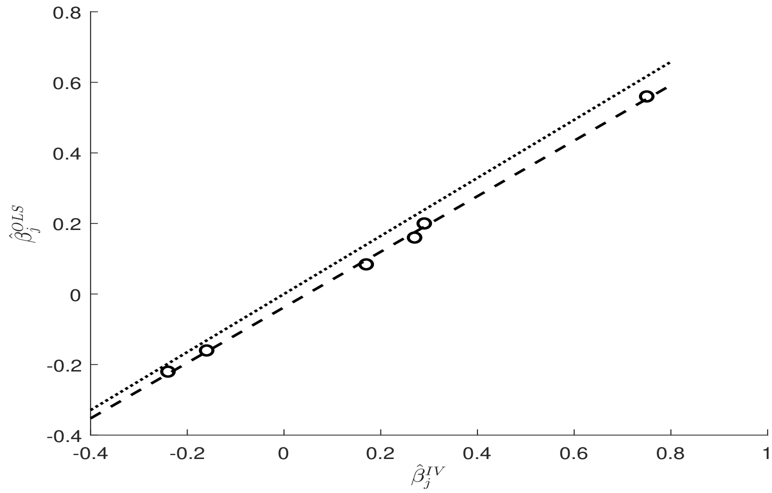

Figure 10. Meta-regression for Duranton, Morrow and Turner (2014)
The figure plots $\hat{\beta}_j^{\text{OLS}}$ against $\hat{\beta}_j^{\text{IV}}$ for six regressions reported in Table 7 of Duranton, Morrow and Turner (2014), where x_i is the log of within-city highway kilometers and each y_{ij} is a measure of trade. The dashed line corresponds to the meta-regression, which has an intercept of $\hat{a} = -0.038$ and a slope of $\hat{b} = 0.79$. The dotted line has a slope of $\hat{\rho}^2 = 0.82$.

is positively correlated with city productivity, then the omitted variable bias ought to be *positive*. Thus, Proposition 4 suggests that we are in the left shaded portion of Figure 1; the instruments may be invalid.

The left panel of Figure 11 explores how the average causal effect changes as a function of the omitted variable bias and proxy quality. If the true average omitted variable bias is zero, then the OLS estimates are close to unbiased when $\tau^2 = 0.96$; the IV estimates only outperform the OLS estimates once $\tau^2 < 0.6$, roughly in line with the proxy quality in Mian and Sufi (2014). 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.

The right panel of Figure 11 shows that, absent invalidity, the omitted variable bias can only be positive if there are heterogeneous treatment effects which make the IV estimates irrelevant. A positive omitted variable bias requires a sufficiently negative value of θ_0, which implies that the treatment effects for compliers are, on average, larger than the treatment effect for non-compliers. However, it would take an implausibly large spread θ_0 between these treatment effects—almost as large as the average IV coefficient itself—to obtain both a positive omitted variable bias and a reasonably large value of λ. As in Adelino, Ma and Robinson (2017), such a value of θ_0 implies that the average non-complier effect is

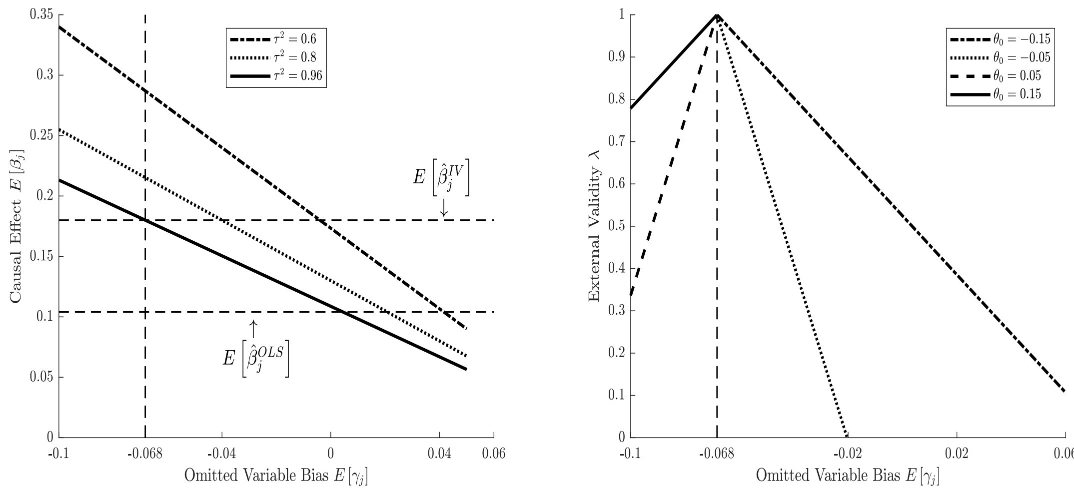

Figure 11. Invalidity or Heterogeneity for Duranton, Morrow and Turner (2014)
The figure plots $\mathbb{E}\left[\beta_j\right]$ and λ for Duranton, Morrow and Turner (2014). The left panel plots equation (17), and the right panel plots the second equation in (21) for $\theta_1 = 1$. The pair of horizontal dashed lines in the left panel report the average values of $\hat{\beta}_j^{\mathrm{IV}}$ and $\hat{\beta}_j^{\mathrm{OLS}}$, respectively. The vertical dashed lines in both panels correspond to the baseline estimates from Proposition 3.

roughly zero, at the same time that non-compliers account for most of the variation in the endogeneous regressor.

4 Conclusion

We derive a meta-regression estimator that separates measurement error from the average omitted variable bias, assuming instrument validity and homogeneous treatment effects. This estimator can be used whenever different dependent variables are regressed on the same endogenous regressor with the same instrument. When instruments are invalid or treatment effects are heterogeneous, the estimated slope and intercept terms from the meta-regression reveal information about the extent of heterogeneity or invalidity. Thus, our meta-regression is a useful diagnostic tool for interpreting IV estimates.

We apply our meta-regression to three published papers and find that measurement error is quantitatively more important than omitted variable bias in two out of three cases; in these two papers, measurement error explains why the IV estimates are, on average, so much larger than their OLS counterparts. Moreover, if the average omitted variable bias is larger than our baseline estimates, then the IV estimates are even more biased than the OLS estimates. In the third paper, we find very little measurement error, though the meta-regression intercept suggests that the instruments suffer from economic invalidity.

In all three papers, we use the meta-regression estimates to analyze heterogeneous treatment effects and the external relevance of the instrument. In particular, we find that reasonable departures from our baseline estimates of the measurement error and omitted variable bias imply that the IV estimates apply to an irrelevant subpopulation, even if the instruments themselves are valid.

References

Adelino, Manuel, Song Ma, and David Robinson. 2017. "Firm Age, Investment Opportunities, And Job Creation." *The Journal of Finance*, 72(3): 999–1038. 3, 16, 17, 23, 24, 25, 26, 27

Angrist, Joshua D., and Jörn-Steffen Pischke. 2009. *Mostly Harmless Econometrics: an Empiricist's Companion.* Princeton University Press. 2, 15

Angrist, Joshua D., and Jörn-Steffen Pischke. 2010. "The Credibility Revolution in Empirical Economics: How Better Research Design Is Taking the Con out of Econometrics." *Journal of Economic Perspectives*, 24(2): 3–30. 2

Angrist, Joshua D., Guido W. Imbens, and Donald B. Rubin. 1996. "Identification of Causal Effects Using Instrumental Variables." *Journal of the American Statistical Association*, 91(434): 444–455. 15

Bartik, Timothy J. 1991. *Who Benefits From State And Local Economic Development Policies? Books from Upjohn Press*, W.E. Upjohn Institute for Employment Research. 23

Berg, Tobias, Markus Reisinger, and Daniel Streitz. 2021. "Spillover Effects In Empirical Corporate Finance." *Journal of Financial Economics*, 142(3): 1109–1127. 3

Blandhol, Christine, John Bonney, Magne Mogstad, and Alexander Torgovitsky. 2022. "When Is TSLS Actually LATE?" *Working Paper.* 15

Borusyak, Kirill, Peter Hull, and Xavier Jaravel. 2018. "Quasi-Experimental Shift-Share Research Designs." National Bureau of Economic Research Working Paper 24997. 3

Card, David. 2001. "Estimating The Return To Schooling: Progress On Some Persistent Econometric Problems." *Econometrica*, 69(5): 1127–1160. 6

Card, David, and Alan B. Krueger. 1995. "Time-Series Minimum-Wage Studies: A Meta-Analysis." *The American Economic Review*, 85(2): 238–243. 4

Christensen, Garret, and Edward Miguel. 2018. "Transparency, Reproducibility, And The Credibility Of Economics Research." *Journal of Economic Literature*, 56(3): 920–80. 4

Cohn, Jonathan B., Zack Liu, and Malcolm I. Wardlaw. 2022. "Count (And Count-Like) Data In Finance." *Journal of Financial Economics*, 146(2): 529–551. 3

Davidoff, Thomas. 2016. "Supply Constraints Are Not Valid Instrumental Variables For Home Prices Because They Are Correlated With Many Demand Factors." *Critical Finance Review*, 5(2): 177–206. 20

DiTraglia, Francis J., and Camilo García-Jimeno. 2021. "A Framework For Eliciting, Incorporating, And Disciplining Identification Beliefs In Linear Models." *Journal of Business & Economic Statistics*, 39(4): 1038–1053. 3

Doucouliagos, Hristos, and T. D. Stanley. 2009. "Publication Selection Bias In Minimum-Wage Research? A Meta-Regression Analysis." *British Journal of Industrial Relations*, 47(2): 406–428. 4

Duranton, Gilles, Peter M. Morrow, and Matthew A. Turner. 2014. "Roads And Trade: Evidence From The US." *The Review of Economic Studies*, 81(2): 681–724. 3, 16, 17, 26, 27, 28

Durbin, J. 1954. "Errors In Variables." *Review of the International Statistical Institute*, 22(1/3): 23–32. 2

Erickson, Timothy, and Toni M. Whited. 2000. "Measurement Error and the Relationship between Investment and q." *The Journal of Political Economy*, 108(5): 1027–1057. 4

Erickson, Timothy, and Toni M. Whited. 2002. "Two-Step Gmm Estimation Of The Errors-In-Variables Model Using High-Order Moments." *Econometric Theory*, 18(3): 776–799. 4

Erickson, Timothy, and Toni M. Whited. 2012. "Treating Measurement Error In Tobin's Q." *The Review of Financial Studies*, 25(4): 1286–1329. 4

Erickson, Timothy, Colin Huan Jiang, and Toni M. Whited. 2014. "Minimum Distance Estimation Of The Errors-In-Variables Model Using Linear Cumulant Equations." *Journal of Econometrics*, 183(2): 211–221. 4, 10

Geary, R. C. 1943. “Relations Between Statistics: The General And The Sampling Problem When The Samples Are Large.” *Proceedings of the Royal Irish Academy. Section A: Mathematical and Physical Sciences*, 49: 177–196. 2

Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift. 2020. “Bartik Instruments: What, When, Why, And How.” *American Economic Review*, 110(8): 2586–2624. 3

Grieser, William D, and Charles J Hadlock. 2019. “Panel-Data Estimation In Finance: Testable Assumptions And Parameter (In) Consistency.” *Journal of Financial and Quantitative Analysis*, 54(1): 1–29. 3

Hausman, Jerry. 2001. “Mismeasured Variables In Econometric Analysis: Problems From The Right And Problems From The Left.” *The Journal of Economic Perspectives*, 15(4): 57–67. 8

Jiang, Wei. 2017. “Have Instrumental Variables Brought Us Closer To The Truth.” *The Review of Corporate Finance Studies*, 6(2): 127–140. 2, 6, 10, 15

Mian, Atif, and Amir Sufi. 2014. “What Explains The 2007–2009 Drop In Employment?” *Econometrica*, 82(6): 2197–2223. 3, 16, 17, 18, 19, 20, 21, 22, 23, 24, 25, 27

Mian, Atif, Kamalesh Rao, and Amir Sufi. 2013. “Household Balance Sheets, Consumption, And The Economic Slump*.” *The Quarterly Journal of Economics*, 128(4): 1687–1726. 19

Mogstad, Magne, Alexander Torgovitsky, and Christopher R. Walters. 2021. “The Causal Interpretation Of Two-Stage Least Squares With Multiple Instrumental Variables.” *American Economic Review*, 111(11): 0–3663. 15

Oster, Emily. 2019. “Unobservable Selection And Coefficient Stability: Theory And Evidence.” *Journal of Business & Economic Statistics*, 37(2): 187–204. 3

Pancost, N. Aaron, and Garrett Schaller. 2022. “Measuring Measurement Error.” Working paper, University of Texas at Austin McCombs School of Business. 2

Reiersøl, Olav. 1941. “Confluence Analysis By Means Of Lag Moments And Other Methods Of Confluence Analysis.” *Econometrica*, 9(1): 1–24. 2

Roberts, Colin J. 2005. “Issues In Meta-Regression Analysis: An Overview.” *Journal of Economic Surveys*, 19(3): 295–298. 4

Roberts, Michael R, and Toni M Whited. 2013. "Endogeneity in Empirical Corporate Finance." In *Handbook of the Economics of Finance*. Vol. 2, 493–572. Elsevier. 10

Saiz, Albert. 2010. "The Geographic Determinants Of Housing Supply." *The Quarterly Journal of Economics*, 125(3): 1253–1296. 18, 20

Stanley, T. D. 2008. "Meta-Regression Methods For Detecting And Estimating Empirical Effects In The Presence Of Publication Selection*." *Oxford Bulletin of Economics and Statistics*, 70(1): 103–127. 4

Stanley, T. D., and Hristos Doucouliagos. 2014. "Meta-Regression Approximations To Reduce Publication Selection Bias." *Research Synthesis Methods*, 5(1): 60–78. 4

Stanley, T. D., and Stephen B. Jarrell. 1989. "Meta-Regression Analysis: A Quantitative Method Of Literature Surveys." *Journal of Economic Surveys*, 3(2): 161–170. 4

Wald, Abraham. 1940. "The Fitting Of Straight Lines If Both Variables Are Subject To Error." *The Annals of Mathematical Statistics*, 11(3): 284–300. 2

Whited, Toni M. 2001. "Is It Inefficient Investment That Causes The Diversification Discount?" *The Journal of Finance*, 56(5): 1667–1691. 4

A Proofs

Proof of Proposition 1

We can relax equation (1) by only requiring it to hold in expectation:

$$\mathbb{E}\left[y_i|\chi_i\right] = \chi_i\beta + \mathbb{E}\left[u_i|\chi_i\right].$$

Following Assumptions 1(i)-1(iv), an OLS regression of y_i on x_i yields

$$\begin{aligned}
\beta^{\text{OLS}} &\equiv \frac{\text{cov}\left(y_i, x_i\right)}{\text{var}\left(x_i\right)} \\
&= \frac{\text{cov}\left(\chi_i\beta + u_i, \chi_i + \varepsilon_i\right)}{\text{var}\left(x_i\right)} \\
&= \frac{\text{var}\left(\chi_i\right)\beta + \text{cov}\left(\chi_i, u_i\right)}{\text{var}\left(x_i\right)} \\
&= \tau^2\beta + \gamma,
\end{aligned}$$

where $\tau^2 \equiv \frac{\text{var}(\chi_i)}{\text{var}(x_i)}$ and $\gamma \equiv \frac{\text{cov}(x_i,u_i)}{\text{var}(x_i)}$.

The corresponding IV estimator converges to

$$\begin{aligned}
\beta^{\text{IV}} &\equiv \frac{\text{cov}\left(y_i, z_i\right)}{\text{cov}\left(x_i, z_i\right)} \\
&= \frac{\text{cov}\left(\chi_i\beta + u_i, z_i\right)}{\text{cov}\left(\chi_i + \varepsilon_i, z_i\right)} \\
&= \frac{\text{cov}\left(\chi_i, z_i\right)}{\text{cov}\left(\chi_i, z_i\right)}\beta \\
&= \beta,
\end{aligned}$$

which follows from Assumptions 1(v) and 1(vi). □

Proof of Proposition 2

Given Assumptions 2(i)-2(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

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

where $\gamma_j \equiv \frac{\text{cov}_j\left(x_i, u_{ij}\right)}{\text{var}(x_i)}$ and the covariance operator $\text{cov}_j\left(\cdot\right)$ is relative to the joint distribution of $\left(\chi_i, z_i, u_{ij}, \varepsilon_i\right)$ across i for a given j.

For each j, the corresponding IV estimator converges to

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

which follows from Assumptions 2(v) and 2(vi).

Thus, under Assumption 2(vii), the slope of the meta-regression converges to

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

The intercept of the meta-regression converges to

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

□

Proof of Proposition 3

Following equation (10), we can write the variance of observed $\hat{\beta}_j^{\text{IV}}$ across j as

$$
\begin{aligned}
\text{var}\left(\hat{\beta}_j^{\text{IV}}\right) &= \text{var}\left(\beta_j\right) + \text{var}\left(\varsigma_j^{\text{IV}} \varepsilon_j^{\text{IV}}\right) \\
&= \text{var}\left(\beta_j\right) + \mathbb{E}\left[\left(\varsigma_j^{\text{IV}}\right)^2\right],
\end{aligned}
$$

where the first line follows from the mutual independence among $\left(\beta_j, \varepsilon_j^{\text{IV}}, \varsigma_j^{\text{IV}}\right)$, and the second follows from both mutual independence and the fact that $\varepsilon_j^{\text{IV}}$ is mean-zero with unit variance.

Consequently, the slope of the meta-regression converges to

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

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

The intercept of the meta-regression converges to

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

□

Statement and Proof of Proposition 3'

We now extend Assumption 2 and Proposition 3 to allow for a vector of perfectly measured control variables $\vec{w}_i$, which we assume includes an intercept. 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 $\vec{w}_i$.

Assumption 2'

(i) $\left(y_{ij}, x_i, z_i, \vec{w}_i\right)$ is a sequence of observable variables $\left(y_{ij}, x_i, z_i\right)$ and $1 \times W$ vectors $\vec{w}_i$ for $i = 1, 2, ..., n$.

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

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

$$y_{ij} = \chi_i \beta_j + \vec{w}_i \kappa_j + u_{ij}, \tag{23}$$
$$x_i = \chi_i + \varepsilon_i. \tag{24}$$

(iv) $\mathbb{E}[\chi_i \varepsilon_i] = \mathbb{E}_j[u_{ij}\varepsilon_i] = 0$ and $\mathbb{E}_j[u_{ij}\vec{w}_i] = \mathbb{E}[\varepsilon_i \vec{w}_i] = \vec{0}$ for each j.

(v) $\mathbb{E}_j[z_i u_{ij}] = \mathbb{E}[z_i \varepsilon_i] = 0$ for each j.

(vi) $\mathbb{E}[\dot{\chi}_i z_i] \neq 0$.

(vii) β_j and $\dot{\gamma}_j$ are independent, where $\dot{\gamma}_j \equiv \frac{\text{cov}_j(\dot{x}_i, u_{ij})}{\text{var}(\dot{x}_i)}$.

Note that Assumptions 2'(i)-2'(iv) imply that we can rewrite equations (23) and (24) as:

$$\dot{y}_{ij} = \dot{\chi}_i \beta_j + u_{ij},$$
$$\dot{x}_i = \dot{\chi}_i + \varepsilon_i,$$

where $\mathbb{E}[\dot{\chi}_i \varepsilon_i] = 0$ and $\dot{y}_{ij}$ is the residual from a regression of y_{ij} on $\vec{w}_i$ for a given j. For each j, we therefore have a familiar representation. Let β_j^{OLS} denote the OLS estimator from regressing y_{ij} on x_i after controlling for $\vec{w}_i$, and let β_j^{IV} denote the corresponding IV estimator using z_i as an instrument for x_i.

Proposition 3'. *Given Assumption 2', the meta-regression estimates converge to*

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

Proof. The proof is analogous to the proofs of Propositions 2 and 3, but in this case, each OLS coefficient is given by

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

while the corresponding IV coefficient converges to

$$\begin{aligned}\beta_j^{\mathrm{IV}} &\equiv \frac{\mathrm{cov}_j\left(\dot{y}_{ij}, z_i\right)}{\mathrm{cov}\left(\dot{x}_i, z_i\right)} \\ &= \frac{\mathrm{cov}_j\left(\dot{x}_i \beta_j + u_{ij}, z_i\right)}{\mathrm{cov}\left(\dot{x}_i, z_i\right)} \\ &= \dot{\tau}^2.\end{aligned}$$

The slope of the meta-regression estimator therefore converges to

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

The intercept of the meta-regression converges to

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

□

Proof of Proposition 4

Given Assumptions 3(i)-3(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

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

For each j, Assumption 3(v) implies that the corresponding IV estimator converges to

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

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

Thus, under Assumption 3(vi), the slope of the meta-regression converges to

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

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

The intercept of the meta-regression converges to

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

□

Proof of Corollary 4.1

The Cauchy-Schwarz inequality implies

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

where $r_{xz} \equiv \mathrm{corr}\left(x_i, z_i\right)$. By conditioning on the value of $\hat{b}$, we can write this inequality as

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

where $k_\phi \equiv \frac{\phi^2-\hat{\rho}^2}{\hat{b}}$. Since $\tau^2 \geq 0$, the above inequality is equivalent to (15).

□

Proof of Proposition 5

Given Assumptions 4(i)-4(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

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

For each j, Assumption 4(v) implies that the corresponding IV estimator converges to

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

Thus, under Assumption 4(vi), the slope of the meta-regression converges to

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

where $\psi_1 \equiv \frac{\mathrm{cov}\left(\beta_j^{\chi}, \beta_j^{z}\right)}{\mathrm{var}\left(\beta_j^{z}\right)}$ 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

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

□

Proof of Corollary 5.1

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 $\mathrm{cov}_j\left(\chi_i z_i, \beta_{ij}\right) = 0$, 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 (18) is equivalent to (14).

□

Proof of Corollary 5.2

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 5, the OLS estimator converges to

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

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

$$\begin{aligned}
\beta_j^{z} &\equiv \frac{\mathbb{E}_j\left[\chi_i z_i \beta_{ij}\right]}{\mathbb{E}\left[\chi_i z_i\right]} \\
&= \frac{\mathbb{P}\left(\pi_i=1\right) \mathbb{E}\left[\chi_i z_i | \pi_i=1\right] \beta_{j1} + \left(1-\mathbb{P}\left(\pi_i=1\right)\right) \mathbb{E}\left[\chi_i z_i | \pi_i=0\right] \beta_{j0}}{\mathbb{P}\left(\pi_i=1\right) \mathbb{E}\left[\chi_i z_i | \pi_i=1\right] + \left(1-\mathbb{P}\left(\pi_i=1\right)\right) \mathbb{E}\left[\chi_i z_i | \pi_i=0\right]} \\
&= \frac{\mathbb{P}\left(\pi_i=1\right) \operatorname{var}\left(z_i | \pi_1=1\right) \alpha \beta_{j1}}{\mathbb{P}\left(\pi_i=1\right) \operatorname{var}\left(z_i | \pi_1=1\right) \alpha} \\
&= \beta_{j1}.
\end{aligned}$$

Therefore, following Proposition 5, the corresponding IV estimator converges to

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

Finally, note that

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

and

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

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 5, the meta-regression estimator converges to

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

□