One Instrument to Rule Them All: 
The Bias and Coverage of Just-ID IV*

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Abstract
We argue that in microeconometric applications, just-identified instrumental variables (IV) estimates are virtually unbiased, and the usual inference strategies are likely adequate. Confidence interval undercoverage exceeds 5% only for endogeneity beyond that seen even when IV and OLS estimates differ by an order of magnitude. Three widely-cited applications are used to explain why endogeneity is likely low enough for IV estimates to be reliable. IV identification typically implies a first-stage sign restriction; most analysts probably screen their estimates accordingly. We show that screening on the estimated first stage sign halves median bias of conventional IV without reducing coverage.

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1 Introduction

The Bekker (1994) and Bound et al. (1995) critiques of the heavily over-identified two-stage least squares (2SLS) estimates reported in Angrist (1990) and Angrist and Krueger (1991, AK91) sparked a flood of interest in the finite-sample behavior of instrumental variables (IV) estimates. In the intervening three decades, attention to bias in 2SLS estimates with many weak instruments has become a staple of applied microeconometrics. The fact that the finite-sample distribution of 2SLS estimates is shifted towards the ordinary least squares (OLS) probability limit is especially worrying. IV is often motivated by fears that OLS estimates are compromised by omitted variable bias (OVB). The IV analyst hopes, therefore, that when IV estimates are close to OLS, this signals minimal OVB rather than substantial finite-sample bias in the IV estimates.

The heavily over-identified models that prompted 1990s IV critiques are a case of many weak instruments, roped together by 2SLS in an effort to estimate a single causal effect with acceptable precision. Strikingly, Bound et al. (1995) show that in the specifications reported in AK91 that interact quarter of birth dummies with 10 year-of-birth and 50 state-of-birth dummies to generate 180 instruments, replacing real quarter of birth dummies with fake dummies randomly drawn yields 2SLS estimates and standard errors much like those generated by the real thing. But most studies using IV (including Angrist (1990) and AK91) report just-identified IV (just-ID IV) estimates computed with a single instrument, along with estimates from over-identified models. Just-ID IV estimates are less obviously biased than the estimates generated by heavily over-identified models, and the empirical relevance of bias in just-ID IV applications remains a matter of debate. Our analysis comes in the wake of recent and renewed interest in the finite-sample properties of just-ID IV, an interest reflected in Andrews and Armstrong (2017), Lee et al. (2020), and Keane and Neal (2021), among others.

We argue here that in typical microeconometric applications of just-ID IV, conventional IV estimates and $t$-tests are unlikely to be compromised by failures of the usual asymptotic theory. Our analysis builds on the (approximate) finite-sample normality of reduced-form and first-stage estimators (in the argot of classical simultaneous equations models, these are both estimated “reduced forms”). This modeling framework parallels that in Andrews et al. (2019) and earlier theoretical investigations of weak instrument problems. The normality of reduced-form estimates is justified by conventional asymptotic reasoning, as well as by the local-to-zero asymptotic sequence used in Staiger and Stock (1997) and Stock and Yogo (2005), in which the first stage shrinks to zero at a rate inversely proportional to the square root of the sample size.

Our setup has only two free parameters: the correlation between structural and first-stage residuals (henceforth, “endogeneity”) and the population first-stage $F$-statistic. This fact lends itself to the construction of easily-interpreted rejection contours characterizing conventional second-stage $t$-tests and confidence interval coverage rates. We see, for example, that for endogeneity less than about 0.76, 95% confidence interval coverage is distorted by no more than 3% for any population $F$. An important insight here is that, even though bias increases when the first stage gets weaker, second-

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1This “fake instruments” simulation was originally suggested by Alan Krueger. Although not an empirical study, Bekker (1994) is likewise motivated by a heavily over-identified specification in Angrist (1990) that uses 73 draft lottery dummies plus interaction terms as instruments for Vietnam-era veteran status. This application is featured at the end of Bekker’s paper, and, originally, in an Amsterdam bar in 1992, where Paul Bekker first confronted Angrist with claims of bias.
stage precision falls. In contrast with the over-identified case, conventional just-ID IV standard errors reflect this, and confidence intervals widen accordingly. This keeps coverage high unless endogeneity is extraordinarily high.2

Few analysts can gauge endogeneity outside of a particular empirical context. We therefore use three applications to calibrate endogeneity: the AK91 study, the Angrist and Evans (1998, AE98) IV estimates using a dummy for same-sex siblings as an instrument for family size, and the Angrist and Lavy (1999, AL99) fuzzy regression discontinuity estimates of class size effects on student learning. These studies span a range of OVB scenarios, from modest (for most of the AK91 estimates), to substantial (in AE98, where OLS exceeds IV by about 50%), to dramatic (in AL99, where IV exceeds small, insignificant OLS estimates by an order of magnitude). Yet, the absolute value of estimated endogeneity is no more than 0.47 in these applications, and over 0.4 only for a single specification and sample. Although three examples do not make a theorem, we argue that the features of these studies that limit endogeneity are common to empirical strategies designed to estimate causal effects or to mitigate attenuation bias in models with measurement error.

Our theoretical case for a sanguine view of conventional just-ID IV builds on Andrews and Armstrong’s (2017) observation that a contemporary analyst pursuing a just-ID IV strategy likely has a first-stage direction in mind. In particular, Andrews and Armstrong (2017) show how to use a sign restriction on the population first stage to construct an estimator, denoted \( \hat{\beta}_U \), that is unbiased when the reduced-form estimates are normally distributed. Given a theoretical first-stage sign restriction, it’s tempting to apply this to first-stage estimates. We show, however, that conditional on the sign of the estimated first stage, \( \hat{\beta}_U \) is no longer unbiased: as it turns out, \( \hat{\beta}_U \) is unbiased by virtue of the fact that it averages two conditional estimators, each biased but in opposite directions.

This discouraging result raises the question whether and how we might use sign restrictions to mitigate the bias of IV in a manner consistent with empirical practice. Our answer to this question comes in the form of a novel theoretical result showing that the median bias of just-ID IV is roughly halved if we condition on the first-stage estimate having the expected sign (we focus on median bias because the conventional just-ID IV estimator has no moments). Analysts might justifiably worry that screening on the basis of first-stage estimates distorts inference.3 Perhaps surprisingly, we also show that pre-screening on the sign of the first stage is virtually costless: rejection contours for a sign-screened estimator differ little from those obtained without screening. The upshot is that sign-screening mitigates the already-modest bias of just-ID IV without degrading coverage. To the extent that such screening is a feature of modern empirical work, reported IV estimates reflect the impressively minimal bias characteristic of sign-screened IV.

The next section details the normal just-ID IV setup and derives an expression for endogeneity in terms of OLS OVB. Section 3 reviews the relationship between \( t \)-test rejection rates and the parameters that govern the normal model. This section also explains why endogeneity in applied

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2The spirit of this argument differs from that in Stock and Yogo (2005), which focuses on worst-case rejection rates over all possible endogeneity values. Lee et al. (2020) and Keane and Neal (2021), discussed further in Section 3 below, consider possible restrictions on endogeneity. But these studies stress the fact that the standard definition of coverage doesn’t involve restrictions on parameter values.

3Recent years have seen growing awareness of the bias induced by screening on the basis of a first-stage pre-test, such as the first-stage \( F \)-statistic. Hall et al. (1996) appear to be the first to note that pre-testing on the first-stage \( F \)-statistic for just-ID IV reduces confidence interval coverage; Andrews et al. (2019) draw on recent empirical scholarship to demonstrate the empirical relevance of pre-test bias in IV applications.
microeconometrics is unlikely to be high enough for conventional IV inference to mislead. Section 4 shows how and why conditioning on the sign of the estimated first stage strengthens the case for this view. Section 5 concludes. Proofs and details behind numerical calculations appear in the appendix.

2 Setup

We observe a sample of \( n \) units indexed by \( i \), with data on outcome variable, \( Y_i \), a scalar treatment variable, \( D_i \), a vector of covariates, \( X_i \), and a scalar instrument, \( Z_i \). Population regressions of outcome and treatment on the instrument and covariates define the reduced form and first stage. These are written as follows:

\[
Y_i = Z_i \delta + X_i' \psi_1 + u_i, \tag{1}
\]
\[
D_i = Z_i \pi + X_i' \psi_2 + v_i. \tag{2}
\]

The parameter of interest is \( \beta = \frac{\delta}{\pi} \), the ratio of the reduced-form and first-stage regression coefficients on \( Z_i \). Provided that the instrument, \( Z_i \), satisfies an exclusion restriction and is relevant (i.e. \( \pi \neq 0 \)), this parameter captures the causal effect of \( D_i \) on \( Y_i \). More generally, if treatment effects are heterogeneous and a monotonicity condition holds, \( \beta \) is a weighted average of individual causal effects (Angrist & Imbens, 1995; Imbens & Angrist, 1994). While treatment effect heterogeneity affects the interpretation of \( \beta \), heterogeneity has no bearing on the behavior of the estimators and inference procedures considered in our just-ID IV setting.

Let \( \hat{\delta} = \sum_{i=1}^{n} \hat{Z}_i Y_i / \sum_{i=1}^{n} \hat{Z}_i^2 \) and \( \hat{\pi} = \sum_{i=1}^{n} \hat{Z}_i D_i / \sum_{i=1}^{n} \hat{Z}_i^2 \) denote OLS estimates of \( \delta \) and \( \pi \), where \( \hat{Z}_i \) is the residual from a regression of \( Z_i \) on \( X_i \). Under mild regularity conditions that allow the errors \((u_i, v_i)\) to be non-normal, heteroskedastic, and serially or cluster-dependent, \((\hat{\delta}, \hat{\pi})\) is consistent and asymptotically normal as \( n \to \infty \), with an asymptotic covariance matrix that can be consistently estimated. Importantly, this holds regardless of the strength of the instrument. We therefore follow Andrews et al. (2019) and earlier analyses of weak instrument problems by assuming this large-sample approximation holds exactly. Specifically, we assume:

\[
\left( \frac{\hat{\delta}}{\hat{\pi}} \right) \sim \mathcal{N} \left( \left( \begin{array}{c} \pi \beta \\ \pi \end{array} \right), \Sigma = \left( \begin{array}{cc} \sigma_{\delta}^2 & \sigma_{\delta \pi} \\ \sigma_{\delta \pi} & \sigma_{\pi}^2 \end{array} \right) \right), \tag{3}
\]

with a known covariance matrix, \( \Sigma \). This distributional assumption is implied by the Staiger and Stock (1997) weak-instrument asymptotic sequence (see Andrews et al. (2019, Section 3.2) for additional discussion and references). Finite-sample results under eq. (3) can therefore be seen as asymptotic under the Staiger and Stock (1997) sequence.

Equation (3) is our only substantive restriction; this assumption allows us to focus on the weak instrument problem, separating this from other finite-sample problems, such as the effect of high-leverage observations on the quality of the normal approximation to the distribution of the OLS estimators \((\hat{\delta}, \hat{\pi})\) and the challenge of standard-error estimation with clustered data.\(^4\) With (3) as foundation, we

\(^4\)Young (2021) discusses these problems in an IV context.
derive finite-sample properties of the IV estimator:

$$\hat{\beta}_{IV} = \frac{\hat{\delta}}{\hat{\pi}},$$  \hspace{1cm} (4)$$

and the null rejection rate for the corresponding Wald test. The latter is based on the $t$-statistic centered at the parameter of interest, $\beta$, divided by the estimated IV standard error, $\hat{\sigma}_{IV}$:

$$t_{W} = \frac{\hat{\beta}_{IV} - \beta}{\hat{\sigma}_{IV}}; \quad \hat{\sigma}_{IV}^{2} = \frac{\sigma_{\delta}^{2} - 2\sigma_{\delta \pi} \hat{\beta}_{IV} + \sigma_{\pi}^{2} \hat{\beta}_{IV}^{2}}{\pi^{2}},$$  \hspace{1cm} (5)$$

where $\hat{\sigma}_{IV}^{2}$ estimates the asymptotic variance of $\hat{\beta}_{IV}$ under standard $n \to \infty$ asymptotics. The corresponding theoretical variance is $\sigma_{IV}^{2} = (\sigma_{\delta}^{2} - 2\sigma_{\delta \pi} \beta + \sigma_{\pi}^{2} \beta^{2})/\pi^{2}$. In a homoskedastic model with constant causal effects, this simplifies to the familiar formula

$$\sigma_{IV}^{2} = \frac{\sigma_{\delta}^{2}}{nE[Z_{i}^{2}]\pi^{2}},$$

where $\tilde{Z}_{i}$ is the residual from the population projection of $Z_{i}$ on $X_{i}$, and $\sigma_{\epsilon}^{2}$ is the variance of the residual in the structural equation,

$$Y_{i} = D_{i}\beta + X_{i}^{0}(\psi_{1} - \psi_{2}\beta) + \epsilon_{i},$$  \hspace{1cm} (6)$$

that motivates IV estimation in the classic linear set-up (the structural residual is $\epsilon_{i} = u_{i} - v_{i}\beta$).

The Anderson and Rubin (1949, AR) statistic for just-ID IV, which can be written:

$$t_{AR} = \frac{\delta - \pi \beta}{\sqrt{\sigma_{\delta}^{2} - 2\sigma_{\delta \pi} \beta + \sigma_{\pi}^{2} \beta^{2}}},$$  \hspace{1cm} (7)$$

is a useful complement to $t_{W}$. The AR statistic differs from $t_{W}$ in that it replaces $\hat{\beta}_{IV}$ with the null value of $\beta$ in the formula for $\hat{\sigma}_{IV}^{2}$: in the context of the just-ID IV model described by eq. (3), the square of $t_{AR}$ equals the Lagrange multiplier statistic testing whether $\frac{\delta}{\pi}$ equals $\beta$.\footnote{Since the moment restrictions in the IV model are linear, Proposition 3 in Newey and West (1987) implies that $t_{AR}^{2}$ is also the relevant likelihood ratio statistic. See Anderson and Rubin (1949) for the general form of the AR statistic in over-identified models with a fixed number of instruments and Mikusheva and Sun (2021) for an adaptation to models with many weak instruments.}

The AR test has size undistorted by weak instruments under the Staiger and Stock (1997) sequence. Moreover, in our just-ID IV context, an AR test is optimal among unbiased tests (Moreira, 2009). AR tests are also compelling by virtue of the fact that, when testing $\beta = 0$, $t_{AR}$ is the $t$-statistic for the associated reduced form. It’s hard to imagine a convincing case for statistical significance of an IV estimate when the associated reduced form is statistically indistinguishable from zero. We focus here on a conventional Wald-type $t$-statistic because the conventional test—and the associated confidence intervals—are ubiquitous in applied work.\footnote{Since Wald and Lagrange multiplier-type tests are first-order asymptotically equivalent, the $t_{W}$ vs $t_{AR}$ distinction is asymptotically negligible. In finite samples, inference based on these two statistics may differ. While rejection rates for the two tests cannot be ranked in general, confidence intervals derived by inverting $t_{AR}$ have infinite length when}
Given the assumption of a known covariance matrix for the first-stage and reduced-form estimates, both $t_W$ and $\hat{\sigma}_V^2$ depend on the data only through $(\hat{\delta}, \hat{\pi})$. These have distributions determined by the two unknown parameters, $\pi$ and $\beta$. Rather than $\pi$ and $\beta$, however, it is illuminating to characterize finite-sample behavior in terms of a pair of parameters that measures instrument strength and the degree of endogeneity (a reparametrization adopted in Staiger and Stock (1997) and Lee et al. (2020), among others). The first parameter, denoted $E[F]$, is defined as:

$$E[F] = \pi^2/\sigma_{\hat{\beta}}^2 + 1.$$  

Because $E[F]$ is the expectation of $F = \hat{\pi}^2/\sigma_{\hat{\beta}}^2$, the $F$-statistic testing $\pi = 0$, it’s sometimes called the population first-stage $F$-statistic, a term adopted here.

The second parameter is defined as:

$$\rho = \text{cor}(\hat{\delta} - \hat{\pi} \hat{\beta}, \hat{\pi}) = \frac{\sigma_{\hat{\pi}}}{\sqrt{\sigma_{\hat{\delta}}^2 - 2 \beta \sigma_{\hat{\delta}} \sigma_{\hat{\beta}} + \sigma_{\hat{\beta}}^2}} \times (\sigma_{\hat{\beta}}/\sigma_{\hat{\beta}}^2 - \beta).$$  

(8)

Under independent heteroskedastic errors, $\rho$ is also given by $\text{cor}(\hat{Z}_i \epsilon_i, \hat{Z}_i v_i)$. When, in addition, the errors $(u_i, v_i)$ are homoskedastic, $\rho = \text{cor}(\epsilon_i, v_i)$, where $\epsilon_i$ is the structural residual in (6). We therefore refer to $\rho$ as (the degree of) endogeneity.\footnote{This simplification is obtained using the fact that, under homoskedasticity, the variance of $v_i$ is $\sigma_v^2 = \sigma_e^2 - nE[Z_i^2]$ and the variance of $\epsilon_i$ is $\sigma_e^2 = (\sigma_e^2 - 2\beta \sigma_{\hat{\delta}} ^2 + \sigma_{\hat{\beta}}^2) - nE[Z_i^2]$, with $\text{cov}(v_i, \epsilon_i) = (\sigma_{\hat{\delta}} - \beta \sigma_e^2) - nE[Z_i^2]$. The homoskedastic formula for the variance of $\epsilon_i$ also leads yields the simplification of the formula for $\sigma_{\hat{\beta}}^2$ noted above.}

With weak instruments as well as homoskedastic error terms, $\rho$ is proportional to the bias of the OLS estimand. This can be seen by using the first-stage and reduced-form equations to write the OLS slope coefficient, $\hat{\beta}_{OLS}$, as follows:

$$\hat{\beta}_{OLS} = \frac{E[\hat{D}_i X_i]}{E[\hat{D}_i^2]} = \frac{E[\hat{Z}_i^2 \pi^2 \beta + E[u_i v_i]}{E[Z_i^2] \pi^2 + \sigma_e^2}

= R_p^2 \beta + \frac{E[u_i v_i]}{\sigma_v^2};

R_p^2 = \frac{E[\hat{Z}_i^2] \pi^2}{E[Z_i^2] \pi^2 + \sigma_e^2}.

(9)

where $\hat{D}_i$ is the residual from a population regression of $D_i$ on $X_i$, and $\sigma_v^2 = E[v_i^2]$. The weight multiplying $\beta$ in (9), denoted $R_p^2$, is the population partial $R^2$ generated by adding the instrument to the first-stage regression. When the instrument is weak, $R_p^2$ is close to zero, and (9) is approximately $E[u_i v_i]/\sigma_v^2$. The OLS estimand likewise converges to $E[u_i v_i]/\sigma_v^2$ in the Staiger and Stock (1997) weak-instrument sequence (which takes $\pi \rightarrow 0$). This in turn equals $\sigma_{\hat{\beta}}^2/\sigma_{\hat{\beta}}^2$ under homoskedasticity, so the second term on the right-hand side of (8),

$$\sigma_{\hat{\beta}}^2/\sigma_{\hat{\beta}}^2 - \beta = \hat{\beta}_{WOLS} - \beta,

(10)

is the weak-instrument OVB of OLS (where we’ve introduced the notation $\hat{\beta}_{WOLS}$ for $\sigma_{\hat{\beta}}^2/\sigma_{\hat{\beta}}^2$). Moreover, when $\pi = 0$, it follows from (3) that $\hat{\beta}_{WOLS} - \beta$ is the median bias of $\hat{\beta}_W$ with no independence
or heteroskedasticity assumptions on the errors in (1) and (2). Thus, $\rho$ also measures endogeneity in the sense that it’s proportional to the median bias of the IV estimator when the instrument is irrelevant.

3 Rejection Rates in Theory and Practice

We’re interested in the bias of $\hat{\beta}_{IV}$ and in $t$-test rejection rates when the null hypothesis is true. Beginning with the latter, the null rejection rate for a two-sided $t$-test with level $\alpha$ is the probability that the absolute value of a $t$-statistic, $|t_W|$, exceeds $z_{1-\alpha/2}$, the $1-\alpha/2$ quantile of a standard normal distribution. This is:

$$R_W = P_{E[F],\rho}(|t_W| > z_{1-\alpha/2}),$$

where $P_{E[F],\rho}$ is the distribution of $t_W$ parameterized by $E[F],\rho$. We evaluate $R_W$ by numerical integration, a computation detailed in Appendix A.2.

Summarizing the behavior of a conventional 5% nominal test, Panel (a) in Figure 1 depicts rejection rates for $t_W$ as a contour plot indexed by $\rho$ and $E[F]$. The figure shows that rejection rates greatly exceed the nominal 5% rate only if the instrument is weak (i.e., $E[F]$ is close to 1) and endogeneity is high. In particular, if $|\rho| < 0.76$, rejection rates are below 10%, regardless of the strength of the first stage. If $|\rho| < 0.565$, the nominal 5% test under-rejects (this cutoff is also noted in Lee et al. (2020)). A simple corollary, further substantiated below, is that the coverage of conventional nominal 95% confidence intervals for $\hat{\beta}_{IV}$ is likely to be satisfactory in most applications.

The modest over-rejection seen in Figure 1 is explained by a signal feature of just-ID IV: the bias of $\hat{\beta}_{IV}$ rises as the instrument grows weaker, but precision falls apace. The IV standard error reflects this lack of precision well enough that, unless endogeneity is egregious, inference is distorted little. This contrasts with over-identified 2SLS with many weak instruments (as in Bekker (1994) and Bound et al. (1995)), where, bias notwithstanding, the usual standard errors for 2SLS remain small enough for the $t$-statistic to be misleading.

Our conclusions here also contrast with those drawn in Stock and Yogo (2005) and Lee et al. (2020) regarding the reliability of inference based on a conventional just-ID IV $t$-statistic. Although Lee et al. (2020) present a similar plot, both studies emphasize worst-case rejection rates over $\rho$, for a given $E[F]$. As can be seen in our Figure 1, this worst-case rejection rate occurs at $|\rho| = 1$. In the same spirit, Keane and Neal (2021) highlights simulations showing that conventional just-ID IV $t$-tests can be misleading when endogeneity is very high. Sections 3.1 and 3.2 explain why we are not much concerned with high values of $\rho$.

Keane and Neal (2021) also observe that, since $\hat{\sigma}_{IV}$ and $\hat{\beta}_{IV}$ tend to be negatively correlated when $\rho$ is positive, most false rejections occur when $\hat{\beta}_{IV} > \beta$. This, they argue, militates so strongly against $t_W$ that conventional Wald tests are to be avoided even with a first-stage $F$ in the thousands. As we see it, the fact that tests based on $t_W$ have asymmetric power does not make conventional
frequentist inference unreliable. The conventional standard for reliability of inference is the accuracy of confidence interval coverage, gauged without reference to the particular value of the estimate. Our analysis adheres to this standard.\(^9\)

### 3.1 The Anatomy of Endogeneity

We put endogeneity in context using three applications. These are the AK91 study that launched the modern weak instruments literature, the AE98 estimates using a dummy for same-sex siblings (of first- and second-born children) as an instrument for family size, and the AL99 fuzzy regression discontinuity estimates of class size effects. The AE98 and AL99 first-stage \(t\)-statistics exceed those for AK91 and are arguably out of the zone where an instrument might be considered weak. With a first-stage \(t\)-statistic of almost 8, the AK91 quarter-of-birth instrument also seems strong enough. But all three studies can be used to calibrate endogeneity and to document contextual features that constrain it.

Table 1 reports key statistics for specifications drawn from each study (some estimates in the table differ slightly from those in the original). The first row in Panel A shows estimates of the economic returns to schooling in the AK91 sample of men born 1920-29. Here, OLS and IV estimates equal 0.080 and 0.072, respectively. These are close, so endogeneity is small in this case, with an estimated \(\rho\) of only 0.043. Schooling returns estimated in the second AK91 sample, consisting of men born 1930-39, exhibit more OVB. In this sample, the IV estimate of 0.105 surprisingly exceeds the OLS estimate of 0.072 (IV estimation of the returns to schooling is usually motivated by a concern that omitted ability controls causes OLS estimates to be too large). Endogeneity is correspondingly larger at \(\rho = -0.175\), but still well outside the danger zone depicted in Figure 1.\(^10\)

The AK91, AE98, and AL99 studies span a range of OVB scenarios, from modest in the first AK91 sample, to substantial in AE98 (where OLS magnitudes consistently exceed IV by at least 50%), to dramatic in AL99 (where IV exceeds small, insignificant OLS estimates, mostly by an order of magnitude, and sometimes with a sign flip). Yet, the magnitude of endogeneity exceeds 0.40 in only one specification, that for reading scores in the AL99 discontinuity sample (which consists of classes in schools with enrollment near the cutoff that determines class size). Just-ID IV inference in all three of these studies is therefore unlikely to be compromised by weak instruments.\(^11\)

Although the consistently moderate levels of endogeneity documented in Table 1 do not add up to a theorem, these applications have features in common with many IV-driven microeconometric investigations of causal effects. First, measured against endogenous variable standard deviations, first-
stage coefficient estimates, denoted here by $\hat{\pi}$, are small. In particular, the range of first-stage impact in eq. (2) runs from under 4% of the endogenous variable standard deviation in AK91, to about 12% of the endogenous variable standard deviation in AE98, with the AL99 first stage in-between.

Most importantly, endogeneity in research on causal effects is often capped by the modest size of the effects of interest. To make this point, it’s helpful to write $\rho$ as a function of OVB. Using eqs. (9) and (10), we can express $\rho$ under homoskedasticity as:

$$\rho = \frac{\sigma_v}{\sigma_\epsilon} (\beta_{WOLS} - \beta)$$

$$\approx \frac{\sigma_D}{\sigma_Y} (\beta_{OLS} - \beta).$$

We can use this expression to compute $\rho$ by replacing $\beta$ with $\hat{\beta}_{IV}$. The relevance of this representation of $\rho$ can be seen in the AE98 estimates of the effects a third child on weeks worked by women aged 21-35 in the 1980 Census. Here, the first-stage partial R-square ($R^2_p$) is close to zero, while the difference between the conventional OLS estimate and the corresponding IV estimate is 3.42. The term multiplying this, $\frac{\sigma_v}{\sigma_\epsilon}$, is well-approximated by the ratio of the endogenous variable standard deviation to the dependent variable standard deviation (denoted $\frac{\sigma_D}{\sigma_Y}$), a ratio of about 0.022. The product of these two terms gives 0.075, equal to the value of $\rho$ reported in the table for this sample.

Equation (11) suggests alternative bounds on endogeneity. For starters, in the AK91 scenario, it seems reasonable to assume that the (causal) economic returns to schooling are no more than double the OLS estimate and certainly positive. Under these restrictions, the descriptive statistics in Table 1, which approximate $\frac{\sigma_v}{\sigma_\epsilon}$ at around 5.2, suggest $|\rho|$ can be no more than about 0.41. Although substantial, this is well below the 0.76 threshold for coverage concerns. With $\beta$ bounded below by zero, large magnitudes of $\rho$ require $\beta$ to far exceed $\beta_{WOLS}$. Only when the causal effect of schooling is triple the OLS estimate (so that OLS is too small by 0.16) does the endogeneity danger zone become relevant.\textsuperscript{12}

Many microeconomic IV applications involve linear probability models in which causal effects are changes in probabilities. The AE98 estimates of the effect of the birth of a third child on female labor force participation in 1980, for example, range from roughly $-0.18$ for OLS to $-0.12$ for IV. Labor force participation rates for women with only two children run around 57%. Causal effects might therefore be as large as $-0.57$, but no larger, since probabilities can't be negative. In this case, $\frac{\sigma_v}{\sigma_\epsilon}$ is about 1 (again, using standard deviations in the data rather than residuals), so $\beta_{OLS} - \beta$ can be no larger than $-0.18 + 0.57 = 0.39$, thereby bounding $\rho$ at this value. This generous bound makes no use of the fact that selection bias is likely to make OLS estimates of family-size effects on female supply too large (in magnitude) rather than too small. Other applications with Bernoulli outcomes admit similar sorts of bounds.

A related argument, appropriate for models with continuous outcomes, shows endogeneity to be constrained by plausible values for causal effects measured in standard deviation units. This line of

\textsuperscript{12}Keane and Neal (2021) consider bounds on $\rho$ in the context of estimates of the economic returns to schooling motivated by the view that OLS returns should exceed causal effects. Although this seems defensible, it’s worth noting that the literature surveyed by Card (2001) reports many IV estimates in excess of the corresponding OLS estimates, a pattern first highlighted by Lang (1993).
reasoning is especially apt for education research where standard-deviation-denominated effect sizes are widely reported. The influential Tennessee STAR class size experiment analyzed in Krueger (1999), for instance, generated a reduction of 7 students per class, roughly one standard deviation of class size in the AL99 data. The STAR experiment yielded treatment effects of about 0.2σ, an impact typical of education interventions deemed to have been effective. At the same time, education researchers often view effect sizes as large as half a standard deviation in the outcome distribution as rare, if not implausible. Using the fact that $\frac{\sigma_v}{\sigma_\epsilon} \approx (1 - R_p^2)$ in the AL99 data, the scenario of a half-standard deviation effect size generated by a one-standard deviation reduction in class size implies $\frac{\sigma_v}{\sigma_\epsilon} \frac{\beta}{1 - R_p^2} = -0.5$ on the second line of eq. (11). At the same time, OLS estimates of class size effects in AL99 are mostly zero (as is often found in class size research; see e.g., Hanushek (1986)), so the magnitude of endogeneity is capped at 0.5.

Contributing to all three of these empirically-grounded arguments is the fact that endogeneity under homoskedasticity can be split into the difference between two R-squared-like terms:

$$\rho \approx \frac{\sigma_D}{\sigma_Y} (\beta_{OLS} - \beta) = \frac{\sigma_D}{\sigma_Y} \beta_{OLS} - \frac{\sigma_D}{\sigma_Y} \beta.$$ (12)

The square of the first term, $\left( \frac{\sigma_D}{\sigma_Y} \beta_{OLS} \right)^2$, is the variation in the dependent variable accounted for by $D_i$ in an analysis-of-variance for $Y_i$. In microeconometric applications, this term is mostly small, as is the causal analog that determines the square of the second term, $\left( \frac{\sigma_D}{\sigma_Y} \beta \right)^2$. This fact limits the magnitude of the difference between them. Consistent with this claim, the many IV estimates collated in Chernozhukov and Hansen (2008) likewise show modest endogeneity.

Finally, it’s worth noting that these bounds on $\rho$ are remarkably insensitive to the presumption of homoskedasticity. To see this, note that we can also bound $\rho$ by plugging our estimates of components of covariance matrix $\Sigma$, which allow for heteroskedasticity or clustering, into the definition of $\rho$ in eq. (8), and bounding the treatment effect $\beta$. Assuming (as above) that the returns to schooling lie between 0 and 0.16, effects of a third child on labor force participation lie between 0 and $-0.57$, and class size effects lie between 0 and $0.5 \sigma_Y / \sigma_D$, yields upper bounds on $|\rho|$ equal to 0.41, 0.36, and 0.57, respectively. These numbers are close to the corresponding homoskedasticity-based bounds.

### 3.2 When Measurement Error Motivates IV

In addition to IV for causal effects, a second major arena for microeconometric IV involves models with measurement error. Suppose the regression of interest is $Y_i = D_i^* \beta + X_i' \gamma + \eta_i$, where $\eta_i$ is a residual uncorrelated with $(D_i^*, X_i)$ by definition. The regressor $D_i^*$ is unobserved; we see only a noisy measure, $D_i = D_i^* + e_i$, where the measurement error, $e_i$, is assumed to be classical, that is uncorrelated with $(D_i^*, X_i, \eta_i)$. Replacing $D_i^*$ with $D_i$ yields the structural equation to be instrumented:

$$Y_i = D_i \beta + X_i' \gamma + (\eta_i - e_i \beta)$$

$$= D_i \beta + X_i' \gamma + \epsilon_i,$$

where $\epsilon_i = \eta_i - e_i \beta$ is the structural residual. Given an instrument correlated with $D_i^*$ and uncorrelated with $e_i$, the coefficients of interest are consistently estimated by IV. The first stage in this scenario
can be written as in (2), with first-stage residual, $v_i$.

To calibrate endogeneity in this model, note first that, given the classical measurement error assumption, $\text{cov}(v_i, \epsilon_i) = -\sigma_v^2 \beta$. Under homoskedasticity, endogeneity squared can therefore be written:

$$\rho^2 = \frac{\sigma_v^2 \beta^2}{\sigma_v^2 \sigma_e^2} = \frac{\sigma_v^2 \beta^2}{\sigma_v^2 (\sigma_v^2 + \beta^2 \sigma_e^2)} \leq \frac{\sigma_v^2}{\sigma_v^2} = \frac{1 - r}{1 - R_p^2}, \quad (13)$$

where $r = \sigma_D^2 \sigma_D^2$ denotes the reliability (or signal-to-noise ratio) of mismeasured $D_i$, after partialing out covariates.\(^{13}\) Although we can’t speak to reliability across all fields, labor economists have collected evidence on the reliability of key variables of interest. These include schooling, earnings, hours worked, and hourly wages. Schooling often appears on the right-hand side of wage equations, while earnings, hours, and hourly wages are used in various configurations to estimate labor supply elasticities. The Angrist and Krueger (1999) summary of reliability estimates suggests $r \approx 0.9$ for schooling and $r \approx 0.8$ for earnings, falling to about 0.65–0.75 for hours worked and hourly wages. The lower end of this range may be more relevant for wage reliability after partialing out covariates.\(^{14}\) With $r = 0.65$ as a reasonably conservative value, we’d need to see an $R_p^2$ equal to at least 0.4 for $\rho$ to reach 0.76. But $E[F] = \frac{n R_p^2}{1 - R_p^2} + 1$, so, at this level of first-stage fit, $E[F]$ is way nowhere near the trouble zone for any sample size that’s empirically relevant. This suggests that, unless reliability is unusually low, microeconometric measurement error can be expected to generate parameter combinations for which conventional IV inference is trouble-free.

4 Bias Under a Good Sign

Having made an empirical case for a sanguine view of just-ID IV, we add a novel analytical argument. This builds on the idea that IV identification strategies are most credible when a strong institutional or theoretical foundation explains the first stage. These foundations usually imply a sign for $\pi$. In the AK91 application, for example, the quarter-of-birth first stage arises from the fact that children born later in the year enter school younger, and are therefore constrained by compulsory attendance laws to stay in school longer than those born earlier. The AE98 same-sex instrument for family size is predicated on parents’ preference for mixed-sex sibships. The AL99 Maimonides Rule instrument for class size is derived from regulations that determine class size as a function of enrollment. In these and many other applied micro applications, institutions or preferences determine the sign of $\pi$.

Andrews and Armstrong (2017) use this insight to motivate an IV estimator that is unbiased under a first-stage sign restriction. Without loss of generality, assume $\pi > 0$, and let $\mu(x) = \frac{1 - \Phi(x)}{\phi(x)}$ denote the Mills’ ratio of a standard normal random variable, where $\phi(x)$ and $\Phi(x)$ are the standard normal density and cdf evaluated at $x$. Let $t_1 = \frac{\pi}{\sigma_x}$ denote the first-stage $t$-statistic, the square of which is the first-stage $F$-statistic. The Andrews-Armstrong unbiased IV estimator, denoted $\hat{\beta}_{U}$, exploits the

\(^{13}\)The first equality in (13) follows from the definition of correlation, the middle inequality uses the fact that $\sigma_v^2$ must be non-negative, and the last equality uses the definition of partial $R^2$ in eq. (9).

\(^{14}\)See Table 11 in Angrist and Krueger (1999). The reliability of variables other than wages is less sensitive to covariate adjustment.
fact that when first-stage estimates are normally distributed,
\[ E \left[ \frac{\mu(t_1)}{\sigma_{\hat{\beta}}} \right] = \frac{1}{\pi}. \]

In other words, \( \hat{\tau} \equiv \mu(t_1)/\sigma_{\hat{\beta}} \) is an unbiased estimator of the reciprocal of the first-stage coefficient, \( \pi \). Define
\[
\hat{\beta}_U \equiv \hat{\tau}(\hat{\delta} - \beta_{WOLS}) + \beta_{WOLS} = t_1\mu(t_1)\hat{\beta}_{IV} + (1 - t_1\mu(t_1))\beta_{WOLS}.
\]
(14)

As noted above, \( \beta_{WOLS} \) is the slope from a population regression of the estimated reduced form on the estimated first stage. \( \hat{\delta} - \beta_{WOLS} \) and \( \hat{\tau} \) are therefore uncorrelated, and, since \( E[\hat{\delta} - \beta_{WOLS} \hat{\tau}] = (\beta - \beta_{WOLS})\pi \), it follows that \( \hat{\beta}_U \) is unbiased for \( \beta \). Moreover, \( \hat{\beta}_U \) is a linear combination of conventional IV estimates and \( \beta_{WOLS} \) with coefficients, or weights, given by \( t_1\mu(t_1) \) and \( 1 - t_1\mu(t_1) \).

To interpret \( \hat{\beta}_U \), observe that \( t_1\mu(t_1) = \pi \hat{\tau} \), so that the weights in (14) reflect the extent to which \( \hat{\tau} \) differs from \( 1/\pi \). If \( t_1 > 0 \), these weights are bounded by a classic Mills’ ratio inequality (e.g. Feller, 1968, p. 175) which implies:
\[
0 \leq 1 - t_1\mu(t_1) \leq \frac{1}{t_1^2}. \quad (15)
\]

Thus, when the first stage is right-signed, the weights \( t_1\mu(t_1) \) in eq. (14) lie between 0 and 1, and we can interpret \( \hat{\beta}_U \) as shrinking the conventional IV estimate towards OLS. The amount of shrinkage is bounded by the reciprocal of \( t_1^2 \), that is, by \( 1/F \). When \( F = t_1^2 = 10 \), the unbiased estimator shrinks \( \hat{\beta}_{IV} \) at most 10% of the way towards \( \beta_{WOLS} \).

The shrinkage interpretation of \( \hat{\beta}_U \) seems surprising: since \( \hat{\beta}_{IV} \) is biased towards the OLS probability limit, shrinkage towards OLS increases bias. This counterintuitive fact is reconciled with the unbiasedness of \( \hat{\beta}_U \) by the following theorem:

**Theorem 1.** Consider the model in (3), and suppose that \( \pi > 0 \). Let \( \lambda = \pi / \sigma_{\hat{\beta}} = \sqrt{E[F]} - 1 \). Then the relative mean bias of \( \hat{\beta}_U \) conditional on \( t_1 > 0 \) can be written:
\[
\frac{E[\hat{\beta}_U - \beta | t_1 > 0]}{\beta_{WOLS} - \beta} = \sqrt{\frac{\pi}{2}} \frac{\phi(\lambda)}{\Phi(\lambda)},
\]
while, conditional on \( t_1 < 0 \), relative mean bias is:
\[
\frac{E[\hat{\beta}_U - \beta | t_1 < 0]}{\beta_{WOLS} - \beta} = -\sqrt{\frac{\pi}{2}} \frac{\phi(\lambda)}{1 - \Phi(\lambda)}.
\]

The estimator \( \hat{\beta}_U \) is therefore unbiased because it averages conditional positive bias when \( t_1 > 0 \) and conditional negative bias when \( t_1 < 0 \). As in Stock and Yogo (2005), the theorem scales mean bias by the weak-IV OVB of the OLS estimand, defined in eq. (10). This simplifies bias formulas, while the relationship between conditional and unconditional bias stands without this rescaling.\(^{15}\)

It is hard to imagine an analyst who is prepared to sign the population first stage while ignoring the sign of the estimated first stage. Such conditioning, however, strips \( \hat{\beta}_U \) of its appeal. In contrast with the deleterious effects of sign-screening on \( \hat{\beta}_{WOLS} \), the next section shows that first-stage sign-screening

\(^{15}\)Stock and Yogo (2005) focus on relative mean bias for 2SLS models with over-identifying restrictions.
has surprisingly salutary effects on the sampling distribution of conventional IV estimates.

4.1 Sign-Screened Bias and Coverage

Suppose we assume \( \pi \) is positive and report second-stage estimates only when first-stage estimates are positive as well (equivalently, when \( t_1 > 0 \)). In contrast with \( \hat{\beta}_{UI} \), sign-screening reduces the bias of \( \hat{\beta}_{IV} \) markedly. Moreover, screening on the estimated first-stage sign has no downside in terms of coverage: in contrast with procedures that screen on the magnitude of the first-stage \( F \)-statistic, screening on the sign of the corresponding \( t \)-statistic is shown here to have little effect on rejection rates for a conventional second-stage \( t \)-test.

Because the expectation of a just-ID IV estimator is undefined, our investigation of sign-screening looks at median bias (the expectation of 2SLS exists only for over-identified models). As in Theorem 1, we scale bias by the weak-IV OVB of the OLS estimand. We are interested in the difference between relative median bias computed unconditionally and conditional on \( \hat{\pi} > 0 \).

Surprisingly, Theorem 2 below shows that the worst-case relative bias obtains in the limit as \(|\rho| \to 0\) (this is not the same as relative bias when \( \rho = 0 \); with no endogeneity, both IV and OLS are unbiased, so that relative bias is discontinuous in \( \rho \)). The relationship between \( \rho \) and relative median bias therefore contrasts with that in Section 3, which shows higher endogeneity leading to worse coverage. This reversal reflects the fact that, although the bias of \( \hat{\beta}_{IV} \) increases with endogeneity, OVB increases faster. Because modest endogeneity is empirically relevant, median relative bias is reasonably characterized by the worst case over \( \rho \). This leads us to the following theoretical result:

**Theorem 2.** Consider the model in eq. (3), and suppose that \( \pi > 0 \). Let \( \lambda = \pi / \sigma_{\hat{\pi}} = \sqrt{E[F]} - 1 \).

Then, for a given \( \lambda \), unconditional relative median bias of \( \hat{\beta}_{IV} \) is characterized by

\[
\sup_{\rho} \left| \frac{\text{median}_{E[F] \rho}(\hat{\beta}_{IV} - \beta)}{\beta_{WOLS} - \beta} \right| = \frac{\phi(\lambda)}{\lambda \Phi(\lambda) - 1/2} + \phi(\lambda).
\]

Moreover, if \( \lambda \geq 0.84 \), the relative median bias of \( \hat{\beta}_{IV} \) conditional on \( \hat{\pi} > 0 \) satisfies

\[
\sup_{\rho} \left| \frac{\text{median}_{E[F] \rho}(\hat{\beta}_{IV} - \beta \mid \hat{\pi} > 0)}{\beta_{WOLS} - \beta} \right| = \frac{\phi(\lambda)}{\lambda \Phi(\lambda) + \phi(\lambda)}.
\]

Equivalently, these expressions give the limit of relative unconditional and conditional median bias as \(|\rho| \to 0\).

Figure 2 plots the two bias expressions in the theorem (see Appendix A.5 for computational details). We see that: (i) unconditional relative bias falls rapidly as first-stage strength increases; (ii) unless \( E[F] \) is exceedingly low, conditional median bias is only about half as large as the corresponding unconditional bias. This is a consequence of the fact that the ratio of conditional to unconditional median bias bounds is:

\[
1 - \frac{0.5 \lambda}{\lambda \Phi(\lambda) + \phi(\lambda)}.
\]

For \( \lambda \) greater than about 1.5, this is within 1 percentage point of 0.5, since the normal cdf is then close to one and the normal density close to zero. The shaded regions in Figure 2 delineate the range
of variation in relative median bias as a function of \( \rho \). The fact that the relative median bias varies little with \( \rho \) is in line with the classic result that, in overidentified models with homoskedastic errors, the relative mean bias of 2SLS depends only on \( E[F] \) (Richardson, 1968). Appendix A.5 also shows that the conditional median bias of \( \hat{\beta}_{IV} \) is always less than that of \( \hat{\beta}_U \), and at least 50% smaller once \( \lambda \geq 1.16 \). The appendix shows further that, replacing relative median bias bounds with the absolute value of conditional and unconditional median bias, it remains true that unless \( E[F] \) is exceedingly low, sign-screening halves median bias.

Screening on the sign of the first-stage estimate clearly mitigates bias. Since many analysts likely pursue a just-ID IV identification strategy only when the first-stage estimates are signed as expected, the inner line in Figure 2 may be a better guide to the bias in published IV estimates. But perhaps this sort of screening is costly. Econometricians have long warned that screening on the first-stage \( F \)-statistic risks a form of pretest bias and so may do more harm than good. Intuitively, when \( \pi \) is truly zero, large \( F \)-statistics signal realizations in which the in-sample correlation between instruments and structural errors is largest. Consequently, when instruments are weak, large \( F \)s come with especially misleading \( \hat{\beta}_{IV} \) realizations. Perhaps sign-screening runs a similar risk.

By way of evidence on this point, panel (b) of Figure 1 plots rejection contours for a conventional (second-stage) \( t \)-test conditional on \( \hat{\pi} > 0 \). That is, the figure plots contours for:

\[
R^c_W = P_{E[F],\rho}(|t_W| > z_{1-\alpha/2} | \hat{\pi} > 0).
\]

Comparison of the two panels in Figure 1 shows that screening on the first-stage sign affects rejection rates little. For instance, the endogeneity cutoff required to keep rejections rates below 10% is \( |\rho| \leq 0.75 \), rather than the unconditional 0.76. This result is explained by the fact that, when the instrument is very weak, screening has two effects. On one hand, the bias of \( \hat{\beta}_{IV} \) is reduced. At the same time, screening out wrong-signed first-stage estimates leads to an overestimate of first-stage strength, on average. These two effects are just about offsetting, so that the rejection contours in panel (a) of Figure 1 are much like those in panel (b).

What practical lesson should we draw from this? The careful analyst judges statistical evidence by standard errors rather than \( t \)-statistics, and hesitates to declare a finding conclusive based on \( \hat{\beta}_{IV} \) alone when the reduced form is statistically uninformative. Just-ID IV is a strong hand, but the reduced form, a regression coefficient with anodyne statistical properties, is the IV analyst’s ace in the hole. The good properties of the AR test statistic can be seen as justifying this view since the \( t \)-statistic for the reduced form and \( t_{AR} \) coincide for a zero null. At the same time, an empiricist hewing to conventional IV reporting strategy—showing reduced-form, first-stage, and IV estimates and the associated standard errors—can be reassured that, provided the first stage has the anticipated sign, the bias of just-ID IV is likely to be minimal and conventional confidence interval coverage adequate. Only when endogeneity exceeds a level that applied microeconomists rarely encounter is inference likely to be misleading.

\[16\] Andrews and Armstrong (2017) shows numerically that the unconditional median bias of \( \hat{\beta}_U \) is smaller than that of \( \hat{\beta}_{IV} \) when \( E[F] \) is small, while this bias ranking reverses for larger \( E[F] \). Andrews and Armstrong (2017) notes also that the median absolute deviation of \( \hat{\beta}_U \) is always smaller than that of \( \hat{\beta}_{IV} \). Our numerical calculations indicate that this fails to hold for all parameter values conditional on the estimated first stage sign.
In the context of the AK91, AE98, and AL99 studies, first-stage sign screening adds no action items to the empirical agenda. The first-stage estimates in these applications are robustly right-signed. The reduced forms, reported in detail in Table 1, are consistent with second-stage estimates in clearly showing that the instrument moves outcomes in a manner implied by plausible first-stage values. In applications with weaker instruments than these, an empirical strategy that begins by examining the first-stage sign would seem to have no downside. Claims of credible causal evidence requires more than this, however. In AK91, for instance, the quarter-of-birth story holds water because schooling can be seen to move sharply up and down with quarter of birth as predicted by compulsory attendance laws, across 30 birth cohorts in three data sets, and because graduate degree completion that should be changed little by compulsory attendance, indeed moves little with quarter of birth. This coherence is part of what gives the AK91 first stage its strength.

5 Summary and Conclusions

Assuming reduced-form and first-stage estimates are approximately normally distributed, null rejection rates for conventional t-tests in just-ID IV models are distorted little unless endogeneity is extraordinarily large. A corollary is that conventional IV standard errors are likely to yield confidence intervals with good coverage. Three widely-cited applications, two of which demonstrate considerable OVB in OLS estimates, are characterized by modest endogeneity and consequently fall well inside the low-distortion comfort zone. We’ve argued that these three examples should be seen as representative rather than idiosyncratic: the structure of much applied micro research naturally bounds endogeneity.

We’ve also introduced a new theoretical reason to be unconcerned with the bias of just-ID IV. This builds on the Andrews and Armstrong (2017) argument that in credible applications of just-ID IV, the analyst is rarely agnostic about the direction of the first stage. Unlike Andrews and Armstrong (2017), however, we impose the same sign restriction on the estimated as well as the theoretical first stage. Such conditioning roughly halves the median bias of the IV estimator. Moreover, in contrast with screening on the first-stage F, sign-screening generates no pretest bias. Since most analysts likely impose an estimated first-stage sign screen as a matter of course, the bias reduction sign-conditioning engenders is already reflected in published empirical work.

References


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Young, A. (2021). *Leverage, heteroskedasticity and instrumental variables in practical application* [Unpublished working paper, LSE]. https://personal.lse.ac.uk/YoungA/LeverageandIV.pdf
Figure 1: Contour plot of the rejection rate of conventional $t$-test with nominal level $\alpha = 0.05$ as function of $E[F]$ and $\rho$. Panel (a) plots the unconditional rejection rate $R_W$. Panel (b) plots the rejection rate $R^c_W$ conditional on $\hat{\pi} > 0$. See Appendix A.2 for computational details.
Figure 2: Bound on median bias of $\hat{\beta}_{IV}$ in units of OLS bias, i.e. as a fraction of $|\beta_{WOLS} - \beta|$. Dashed line: unconditional median bias. Solid line: median bias conditional on $\hat{\pi} > 0$. The yellow and blue shaded areas delineate the range of variation in unconditional and conditional median bias, respectively, over possible values of $\rho$. 
Table 1: Estimates and Endogeneity in Three IV Applications.

<table>
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<tr>
<th>Sample</th>
<th>Outcome</th>
<th>Treatment</th>
<th>Instrument</th>
<th>OLS</th>
<th>IV</th>
<th>Reduced-form</th>
<th>IV</th>
<th>Endogenity</th>
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<td>(8)</td>
<td>(9)</td>
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<td>(0.016)</td>
<td>(0.003)</td>
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<td>−0.006</td>
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<td>Same-sex</td>
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<td>(0.001)</td>
<td>(0.009)</td>
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<td>1990 Census, mothers aged 18–35 (n = 380,007)</td>
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<td>Same-sex</td>
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Notes: This table reports IV and OLS estimates replicating the AK91, AE98, and AL99 studies discussed in the text. For each study, the table reports IV and OLS estimates from multiple samples, as well as the corresponding first-stage estimates, $\hat{\pi}$, reduced-form estimates, $\hat{\delta}$, and estimates of the endogeneity parameter, $\hat{\rho}$. The endogeneity estimate denoted $\hat{\rho}^*$ is computed assuming homoskedasticity. Standard errors appear in parentheses. These are robust for AK91 and AE98, and clustered on school for AL99. Standard deviations for the outcome, treatment, and instrument are reported in columns 1–3 in square brackets.
Appendix A Derivations and Proofs

The appendix uses the notation \( \hat{\beta}_{IV} = (\hat{\beta}_{IV} - \beta)/|\beta_{WOLS} - \beta| \) and \( \hat{\beta}_U = (\hat{\beta}_{IV} - \beta)/|\beta_{WOLS} - \beta| \) to denote the IV and the unbiased estimator, after centering and scaling by the weak-IV OVB of OLS. Also, we let \( \lambda = \pi/\sigma = \sqrt{E[F]} - 1 \), and \( s = \rho/\sqrt{1 - \rho^2} \).

A.1 Estimating \( \rho \)

We estimate \( \rho \) (defined in eq. (8)) using first-stage and IV estimates, and the associated first-stage, reduced-form and IV standard errors. To see how this works, rewrite eq. (5) as:

\[
\sigma^2_{\bar{z}} = \frac{\sigma^2_\delta \hat{\beta}_{IV}^2 - \pi^2 \sigma^2_{\bar{z}} + \sigma^2_\delta}{2 \hat{\beta}_{IV}}. \tag{A.1}
\]

With this in hand for \( \sigma^2_{\bar{z}} \), endogeneity can be computed as the sample analog of eq. (8), replacing \( \beta \) with \( \hat{\beta}_{IV} \). The resulting endogeneity estimator is:

\[
\hat{\rho} = \frac{\sigma_{\bar{z}}}{|\pi|\sigma_{\bar{z}}} \times (\sigma^2_{\bar{z}}/\sigma^2_\delta - \hat{\beta}_{IV}).
\]

Under the normal model in eq. (3), this estimator depends on the data only through \( \hat{\beta}_{IV} \), with the derivative given by \( \partial \hat{\rho}/\partial \hat{\beta}_{IV} = \hat{\delta} - \pi^2 \sigma^2_{\bar{z}} + \sigma^2_\delta \cdot (\hat{\rho}^2 - 1)/|t_1| \). Hence, the delta-method standard error for \( \hat{\rho} \) is simply \( (1 - \hat{\rho}^2)/|t_1| \). Paralleling concerns with finite-sample coverage of the usual confidence interval for \( \beta \), we might worry that confidence intervals for \( \rho \) based on delta-method standard errors suffer from undercoverage if endogeneity is high and the instruments are weak. We therefore also compute confidence sets for \( \rho \) by inverting the AR statistic, which are immune to this concern. Since \( \rho \) is monotone decreasing in \( \beta \), if the AR confidence set is given by \([\beta_\ell, \beta_u]\), this leads to the confidence set for \( \rho \) given by \([g(\beta_u), g(\beta_\ell)]\), where \( g(\beta) = \frac{\sigma_{\bar{z}}}{\sqrt{\sigma^2_\delta - 2\sigma_{\bar{z}} \pi^2 \sigma^2_{\bar{z}}}} \times (\sigma^2_{\bar{z}}/\sigma^2_\delta - \beta), \) and by \([-1, g(\beta_u)] \cup [g(\beta_\ell, 1] \) if the AR confidence set takes the form \((-\infty, \beta_\ell] \cup [\beta_u, \infty) \). As noted in the text, intervals computed in this way are virtually indistinguishable from the those based on the delta-method standard errors reported in Table 1.

A.2 t-Test Rejection Rates

This section writes the rejection probabilities of the \( t \)-test as an integral indexed by \((E[F], \rho)\). Stock and Yogo (2005) use Monte Carlo methods to compute unconditional rejection probabilities in a similar setup. The calculation described here is much faster. More importantly, it allows us to easily compute both unconditional rejection rates and rejection rates conditional on sign-screening.

Using eq. (8), and the fact that \( \beta_{WOLS} - \beta \) and \( \rho \) have the same sign, we may write \( t_{AR} \) as

\[
t_{AR} = \frac{(\hat{\delta} - \hat{\pi} \beta)|\rho|}{\sigma_{\bar{z}}|\beta_{WOLS} - \beta|}. \tag{A.2}
\]
Consequently,

\[ \hat{\beta}_{IV} = \frac{\hat{\delta}_t - \hat{\beta}_W}{\sigma_{t_1}|\hat{\beta}_{WOLS} - \beta|} = \frac{t_{AR}}{|\rho| t_1}. \tag{A.3} \]

Thus,

\[ t_W = \frac{\text{sign}(t_1) t_{AR}}{\sqrt{\frac{\sigma_W^2}{\sigma_{WOLS}^2 - \beta^2} + \frac{\sigma_W^2}{\sigma_{WOLS}^2 + \beta^2} \rho^2 + \frac{t_{AR}^2}{t_1^2} - 2\rho t_{AR}/t_1}} = \frac{\text{sign}(t_1) t_{AR}}{\sqrt{1 + t_{AR}^2/t_1^2 - 2\rho t_{AR}/t_1}}. \tag{A.4} \]

where the first equality uses eq. (A.3) and the definition of \( \beta_{WOLS} \), and the second equality uses eq. (8). This expression for \( t_W \) implies that conditional on \( t_1 \), the rejection region \( \{|t_W| \geq z_{1-\alpha/2}\} \) is quadratic in \( t_{AR} \). Solving this quadratic inequality implies that the rejection region is given by

\[ t_{AR} \in \begin{cases} \emptyset & \text{if } t_1^2 \leq (1 - \rho^2)z_{1-\alpha/2}^2, \\ [a_1, a_2] & \text{if } (1 - \rho^2)z_{1-\alpha/2}^2 \leq t_1^2 \leq z_{1-\alpha/2}^2, \\ (-\infty, a_2) \cup (a_1, \infty) & \text{if } t_1^2 \geq z_{1-\alpha/2}^2 \end{cases}. \]

where

\[ a_1 = \frac{\rho z_{1-\alpha/2}^2 t_1 - |t_1|z_{1-\alpha/2}\sqrt{t_1^2 - (1 - \rho^2)z_{1-\alpha/2}^2}}{z_{1-\alpha/2}^2 - t_1^2}, \]

\[ a_2 = \frac{\rho z_{1-\alpha/2}^2 t_1 + |t_1|z_{1-\alpha/2}\sqrt{t_1^2 - (1 - \rho^2)z_{1-\alpha/2}^2}}{z_{1-\alpha/2}^2 - t_1^2}. \]

Note that \( \text{cor}(t_{AR}, t_1) = \rho \), so that

\[ P(t_{AR} \leq x \mid t_1) = \Phi((x - \rho(t_1 - \lambda))/\sqrt{1 - \rho^2}). \tag{A.5} \]

Thus, conditional on \( t_1 \), the rejection probability is given by

\[ P(|t_W| \geq z_{1-\alpha/2} \mid t_1) = (P(t_{AR} \leq a_2 \mid t_1) - P(t_{AR} \leq a_1 \mid t_1)) \mathbb{1}\{t_1^2 \geq z_{1-\alpha/2}^2(1 - \rho^2)\} \]

\[ + \mathbb{1}\{t_1^2 \geq z_{1-\alpha/2}^2\} \]

\[ = f(t_1; \lambda, \rho) \mathbb{1}\{t_1^2 \geq (1 - \rho^2)z_{1-\alpha/2}^2\} + \mathbb{1}\{t_1^2 \geq z_{1-\alpha/2}^2\}, \tag{A.6} \]

where

\[ f(t_1; \lambda, \rho) = \Phi\left(\frac{a_2 - \rho(t_1 - \lambda)}{\sqrt{1 - \rho^2}}\right) - \Phi\left(\frac{a_1 - \rho(t_1 - \lambda)}{\sqrt{1 - \rho^2}}\right). \]

Since \( t_1 \sim \mathcal{N}(\lambda, 1) \), the rejection probability conditional on \( t_1 \geq c \) is therefore given by

\[ P(|t_W| \geq z_{1-\alpha/2} \mid t_1 \geq c) = \int_c^\infty \left[ \mathbb{1}\{t_1^2 \geq (1 - \rho^2)z_{1-\alpha/2}^2\} f(t_1; \lambda, \rho) + \mathbb{1}\{t_1^2 \geq z_{1-\alpha/2}^2\} \right] \phi(t_1 - \lambda) dt_1. \]

The unconditional rejection probability \( R_W \) obtains by setting \( c = -\infty \). The rejection probability conditional on sign screening, \( R_W^c \), obtains by setting \( c = 0 \). The coverage contours in Figure 1 evaluate the above expression as a function of \((\rho, \lambda)\) by numerical integration.
A.3 Proof of Theorem 1

We may write

\[ \tilde{\beta}_U = t_1 \mu(t_1) \tilde{\beta}_{IV} + (1 - t_1 \mu(t_1)) \text{sign}(\rho) = \mu(t_1) \frac{t_{AR}}{\lambda} + (1 - t_1 \mu(t_1)) \text{sign}(\rho) \] (A.7)

where the first equality follows from eq. (14), and the fact that \( \beta_{WOLS} - \beta \) and \( \rho \) have the same sign, and the second equality applies eq. (A.3).

Since \( E[t_{AR} \mid t_1] = \rho(t_1 - \lambda) \), the relative bias conditional on \( t_1 \) is given by

\[ E[\tilde{\beta}_U \mid t_1] = \text{sign}(\rho) [1 - \lambda \mu(t_1)] . \]

By arguments analogous to those in the proof of Lemma 2.1 in Andrews and Armstrong (2017), we have

\[ E[\lambda \mu(t_1) \mid t_1 > 0] = \frac{1}{\Phi(\lambda)} \int_{t_0}^{\infty} \lambda - \Phi(t) \phi(t) dt = \frac{e^{-\lambda^2/2}}{\Phi(\lambda)} \int_{t_0}^{\infty} (1 - \Phi(t)) \cdot \lambda e^t dt \]

\[ = \frac{e^{-\lambda^2/2}}{\Phi(\lambda)} \left\{ [e^t(1 - \Phi(\lambda))]_{t_0}^{\infty} + \int_{t_0}^{\infty} \phi(t)e^t dt \right\} \]

\[ = \frac{1}{\Phi(\lambda)} \left[ -\frac{1}{2} e^{-\lambda^2/2} + \int_{t_0}^{\infty} \phi(t) dt \right] = -\frac{1}{2} \frac{e^{-\lambda^2/2}}{\Phi(\lambda)} + 1 , \]

where the first line uses the definition of the Mills’ ratio, the second line uses integration by parts, and the third follows by completing the square. It therefore follows that

\[ \frac{E[\tilde{\beta}_U - \beta \mid t_1 > 0]}{\beta_{WOLS} - \beta} = \frac{1}{2} \frac{e^{-\lambda^2/2}}{\Phi(\lambda)} = \sqrt{\frac{\pi}{2}} \frac{\phi(\lambda)}{\Phi(\lambda)} . \]

The second claim follows by an analogous argument.

A.4 Proof of Theorem 2

The distribution of \( \tilde{\beta}_{IV} \) conditional on \( t_1 \) can then be written as

\[ P(\tilde{\beta}_{IV} \leq x \mid t_1; s) = \begin{cases} 
P(t_{AR} \leq t_1 x \mid t_1) = \Phi(s \lambda - (1 - \text{sign}(s)x)t_1) & \text{if } t_1 \geq 0 , \\
P(t_{AR} \geq t_1 x \mid t_1) = \Phi(-s \lambda - (1 - \text{sign}(s)x)t_1) & \text{if } t_1 < 0 . 
\end{cases} \] (A.8)

where the first equality uses eq. (A.3), and the second equality follows from eq. (A.5). Observe that since \( P(\tilde{\beta}_{IV} \leq x \mid t_1; -s) = 1 - P(\tilde{\beta}_{IV} \leq -x \mid t_1; -s) \), the distribution is symmetric in \( s \). It therefore suffices to consider \( s > 0 \).

We first prove the claims concerning the distribution of \( \tilde{\beta}_{IV} \) conditional on \( t_1 > 0 \). By eq. (A.8),
this distribution is given by

\[ P(\tilde{\beta}_{IV} \leq x \mid t_1 > 0; s) = \frac{1}{\Phi(\lambda)} \int_{-\lambda}^{\infty} \Phi(s[\lambda - (1 - x)t_1]) \phi(z)dz. \quad (A.9) \]

Observe that the conditional median, denoted \( m_c = m_c(s) \), of \( \tilde{\beta}_{IV} \) is smaller than 1, since

\[ P(\tilde{\beta}_{IV} \leq 1 \mid t_1 > 0; s) = \frac{\Phi(s\lambda)}{\Phi(\lambda)} \int_{-\lambda}^{\infty} \phi(z)dz = \Phi(s\lambda) > 1/2. \quad (A.10) \]

Next, by the mean value theorem, for some \( \tilde{s} = \tilde{s}(x, s) \in [0, s] \),

\[ P(\tilde{\beta}_{IV} \leq x \mid t_1 > 0; s) = \Phi(0) + \frac{s}{\Phi(\lambda)} \int_{-\lambda}^{\infty} ((x - 1)z + x\lambda)\phi(s((x - 1)z + x\lambda)) \phi(z)dz \]

\[ = \frac{1}{2} + \frac{s}{s^2(1 - x)\Phi(\lambda)} \int_{-\infty}^{\tilde{s}\lambda} y\phi(y) \phi(a + by)dy, \]

where the second line uses the change of variables \( y = \tilde{s}x\lambda - \tilde{s}(1 - x)z \), and we let \( a = x\lambda/(1 - x) \), \( b = -\frac{1}{s(1 - x)} \). By line 111 of Table 1 in Owen (1980),

\[ \int x\phi(x)\phi(a + bx) = \frac{\phi(a/t)}{t^2} \left[ -\phi(tx + ab/t) - \frac{ab}{t} \Phi(tx + ab/t) \right], \quad t = \sqrt{1 + b^2}. \quad (A.11) \]

Applying this result to the preceding display then yields

\[ P(\tilde{\beta}_{IV} \leq x \mid t_1 > 0; s) = \frac{1}{2} + \frac{s}{s^2(1 - x)\Phi(\lambda)} \left[ -\phi(t\tilde{s}\lambda + ab/t) - \frac{ab}{\sqrt{1 + b^2}} \Phi(t\tilde{s}\lambda + ab/t) \right] \]

\[ = \frac{1}{2} + \frac{s}{\Phi(\lambda)} \frac{\phi(a/(1 - x))}{s^2(1 - x)^2 + 1} \left[ \frac{x}{1 - x} \frac{\lambda}{\tilde{g}(x, \tilde{s})} \Phi(\lambda g(x, \tilde{s})) - \phi(\lambda g(x, \tilde{s})) \right], \]

where \( g(x, \tilde{s}) = \frac{s^2|1 - x| + \text{sign}(1 - x)}{\sqrt{s^2(1 - x)^2 + 1}} \), and \( \tilde{g}(x, \tilde{s}) = \sqrt{s^2(1 - x)^2 + 1} \). When evaluated at \( x = m_c \), the expression in square brackets must equal zero by definition of the median. Therefore, \( m_c > 0 \), and since we also know from eq. (A.10) that \( m_c < 1 \), the conditional median must satisfy

\[ m_c = \frac{1}{\frac{\lambda}{\tilde{g}(m_c, \tilde{s}(m_c, s))} \frac{\Phi(\lambda \tilde{g}(m_c, \tilde{s}(m_c, s)))}{\Phi(\lambda \tilde{g}(m_c, \tilde{s}(m_c, s))) + 1}}. \quad (A.12) \]

We have

\[ \frac{\lambda}{\tilde{g}(\lambda \tilde{g})} \geq \frac{\lambda}{\tilde{g}(\lambda \tilde{g})}, \quad \text{and} \quad \frac{\lambda}{\tilde{g}(\lambda \tilde{g})} \geq \frac{\lambda}{\phi(\lambda \tilde{g})} \geq 0.84. \]

Here the first inequality follows because \( \Phi(x)/\phi(x) \) is increasing in \( x \), and \( \tilde{g} \geq \tilde{g} \), and the second inequality follows because \( \frac{\phi(x)}{x\phi(x)} \) is increasing for \( x \geq 0.84 \), and \( \tilde{g} \geq 1 \). Therefore,

\[ m_c \leq \frac{\phi(\lambda)}{\lambda \Phi(\lambda) + \phi(\lambda)} = \lim_{s \to 0} m_c(s), \]

where the equality follows since the right-hand side of eq. (A.12) converges to \( \frac{\phi(\lambda)}{\lambda \Phi(\lambda) + \phi(\lambda)} \) as \( s \to 0 \).
We now prove the claims concerning the unconditional distribution of \( \tilde{\beta}_{IV} \). From eq. (A.8), we obtain

\[
P(\tilde{\beta}_{IV} \leq x; s) = 1 - \Phi(\lambda) + \int_{-\infty}^{\lambda} \Phi(s(x \lambda - (1 - x)z)) \phi(z) dz - \int_{-\infty}^{-\lambda} \Phi(s(x \lambda - (1 - x)z)) \phi(z) dz. \tag{A.13}
\]

Observe that the median of \( \tilde{\beta}_{IV} \), denoted \( m_u = m_u(s) \) is smaller than 1, since

\[
P(\tilde{\beta}_{IV} \leq 1; s) = 1 - \Phi(\lambda) + \Phi(s\lambda) \left[ \int_{-\lambda}^{\lambda} \phi(z) dz - \int_{-\infty}^{-\lambda} \phi(z) dz \right] = 1 - \Phi(\lambda) + \Phi(s\lambda) \left[ \int_{-\lambda}^{\lambda} \phi(z) dz \right] = 1 - \Phi(\lambda) + [\Phi(\lambda) - 1] + [\Phi(\lambda) - 1] = \Phi(\lambda) > 1/2. \tag{A.14}
\]

By arguments as in the conditional case, for \( x < 1 \),

\[
P(\tilde{\beta}_{IV} \leq x; \rho) = \frac{1}{2} + \frac{s}{\bar{s}^2(1 - x)} \left[ \int_{-\lambda}^{\lambda} y \phi(y) \phi(a + by) dy - \int_{-\infty}^{\infty} y \phi(y) \phi(a + by) dy \right]
\]

\[
= \frac{1}{2} + \frac{s}{\bar{s}^2(1 - x)} \left[ -2\phi(t\tilde{s} + ab/t) - 2 \frac{ab}{t} \Phi(t\tilde{s} + ab/t) + \frac{ab}{t} \right]
\]

\[
= \frac{1}{2} + \frac{s}{\bar{s}^2(1 - x)} \left[ -2\phi(\lambda g(s, x)) + 2 \frac{x}{1 - x} \frac{\lambda}{g} \Phi(\lambda g(s, x)) - \frac{x}{1 - x} \frac{\lambda}{g} \right].
\]

Here the first line follows by the mean value theorem, where \( \tilde{s} = \tilde{s}(x, s) \in [0, s] \), the second line uses eq. (A.11), and the last line follows by algebra. When evaluated at \( x = m_u \), the expression in square brackets must equal zero by definition of the median. Therefore, \( m_u > 0 \), and it must satisfy

\[
m_u = \frac{1}{\frac{\lambda}{\bar{g}} \frac{\Phi(\lambda g) - 1/2}{\phi(\lambda g)} + 1}. \tag{A.15}
\]

Now,

\[
\frac{\lambda}{\bar{g}} \frac{\Phi(\lambda g) - 1/2}{\phi(\lambda g)} \geq \frac{\lambda}{\bar{g}} \frac{\Phi(\lambda g) - 1/2}{\phi(\lambda g)} \geq \frac{\lambda}{\bar{g}} \frac{\Phi(\lambda) - 1/2}{\phi(\lambda)}.
\]

Here the first inequality follows because \( \Phi(x)/\phi(x) \) is increasing in \( x \), and \( g \geq \bar{g} \), and the second inequality follows because \( \frac{\Phi(x) - 1/2}{x \phi(x)} \) is increasing for \( x > 0 \). As a result,

\[
m_u \leq \frac{\phi(\lambda)}{\lambda(\Phi(\lambda) - 1/2 + \phi(\lambda))} = \lim_{s \to 0} m_u(s),
\]

where the equality follows since the right-hand side of eq. (A.15) converges to \( \frac{\phi(\lambda)}{\lambda(\Phi(\lambda) - 1/2 + \phi(\lambda))} \) as \( s \to 0 \).
A.5 Median Bias Comparisons

To evaluate the relative median bias of \( \hat{\beta}_{IV} \) as a function of both \( E[F] \) and \( \rho \) conditional on \( t_1 \geq c \), we first evaluate

\[
P(\hat{\beta}_{IV} \leq x \mid t_1 \geq c; \rho, \lambda) = \frac{1}{\Phi(\lambda - c)} \int_{c-\lambda}^{\infty} f_{IV}(z; x, \lambda, s) \phi(z) dz
\]

(A.16)

by numerical integration. Here we use the formula \( f_{IV}(z; x, \rho, \lambda) = \Phi(s|\text{sign}(z + \lambda)\lambda - (1 - x)|z + \lambda|) \) from eq. (A.8) for the cdf conditional on \( z = t_1 - \lambda \). We then numerically solve for the median. For unconditional median, we set \( c = -\infty \), and for the median conditional on sign screening, we set \( c = 0 \).

The shaded regions in Figure 2 correspond to the range of the absolute value of the relative median bias as \( \rho \) varies between \(-1\) and \(1\).

To compare the relative median bias to that of \( \hat{\beta}_U \), it suffices to consider \( \rho > 0 \), since the distributions of \( \hat{\beta}_U \) and \( \hat{\beta}_{IV} \) are symmetric in \( \rho \). By eq. (A.7), it follows that for \( t_1 > 0 \),

\[
P(\hat{\beta}_U \leq x \mid t_1 > 0) = P(\hat{\beta}_{IV} \leq x - (1 - x) \frac{1 - t_1 \mu(t_1)}{t_1 \mu(t_1)} \mid t_1 > 0),
\]

which for \( x < 1 \) is smaller than \( P(\hat{\beta}_{IV} \leq x \mid t_1 > 0; s) \). Since the median of \( \hat{\beta}_{IV} \) conditional on \( t_1 > 0 \) is smaller than 1, it follows that the conditional median bias of \( \hat{\beta}_{IV} \) is always smaller than that of \( \hat{\beta}_U \).

To compare the relative magnitudes of the median biases, we compute the relative median bias of \( \hat{\beta}_U \) analogously to that of \( \hat{\beta}_{IV} \), except we replace \( f_{IV} \) in eq. (A.16) with \( f_U(z; x, \lambda, \rho) = \Phi(s|\lambda - (1 - \text{sign}(s)x)|\mu + z|) \) (it follows from eqs. (A.5) and (A.7) that this is the cdf \( \hat{\beta}_U \) conditional on \( z = t_1 - \lambda \)). We then compute the ratio \( \frac{\text{median}_{\lambda,\rho}(\hat{\beta}_U \mid t_1 > 0)}{\text{median}_{\lambda,\rho}(\hat{\beta}_{IV} \mid t_1 > 0)} \) of the median biases on a fine grid of values of \((\rho, \lambda)\). This ratio is greater than 2 if \( E[F] \geq 2 \), and greater than 3 if \( E[F] \geq 3 \), regardless of the value of \( \rho \). Likewise, comparison of the ratio of the conditional and unconditional median IV bias, \( \frac{\text{median}_{\lambda,\rho}(\hat{\beta}_{IV} \mid t_1 > 0)}{\text{median}_{\lambda,\rho}(\hat{\beta}_{IV})} = \frac{\text{median}_{\lambda,\rho}(\hat{\beta}_{IV} - \beta \mid t_1 > 0)}{\text{median}_{\lambda,\rho}(\hat{\beta}_{IV} - \beta)} \) shows that the ratio lies between 0.5 and 0.525 for \( \lambda \geq 1.5 \), regardless of the value of \( \rho \).