A Practical Guide to Weak Instruments

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Abstract We provide a simple survey of the weak IV literature, aimed at giving practical advice to applied researchers. It is well-known that 2SLS has poor properties if instruments are exogenous but weak. We clarify these properties, explain weak instrument tests, and examine how behavior of 2SLS estimates and test statistics depend on instrument strength. A common standard for acceptably strong instruments is a first-stage F of 10, which renders two-tailed t-test size distortion modest. However, we show that 2SLS standard errors tend to be artificially small in samples where the estimate is most contaminated by the OLS bias. This means the t-test has inflated power to detect false positive effects when the OLS bias is positive. Surprisingly, this problem persists even if the first-stage F is in the thousands. Robust tests like Anderson-Rubin perform better, and should be used in lieu of the t-test even with strong instruments. In many realistic settings a first-stage F well above 10 may be necessary to give high confidence that 2SLS will outperform OLS. For example, in the archetypal application of estimating returns to education, we argue one needs F of at least 50.

JEL: *C12, C26, C36*

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1. INTRODUCTION

The past 30 years have seen an explosion of applied work using instrumental variable (IV) methods to deal with endogeneity problems. But since Bound et al. (1995), applied economists have become acutely aware that 2SLS estimators have poor properties when instruments are exogenous but "weak" – meaning they are weakly correlated with the endogenous variable. In particular, if instruments are only marginally significant in the first stage of 2SLS, it is now well understood that both the estimates and their standard errors can be very misleading – even in very large samples.

Our first goal is to explain the literature on weak instruments in a way accessible to applied researchers. We explain weak instrument tests, weak instrument robust inference and alternatives to 2SLS. Our main contribution is to highlight important problems with 2SLS that weak instrument tests gloss over. The behavior of t-tests is highly problematic, even in large samples where instruments are "strong" by conventional standards. This leads us to advocate a higher standard of instrument strength, and wider use of robust tests, in applications of IV. Robust tests like Anderson and Rubin (1949) should be widely adopted in lieu of the t-test, even if instruments are strong.

Concern with the poor properties of 2SLS in the weak instrument context led Staiger and Stock (1997) to advocate a higher standard of instrument relevance in the first stage of 2SLS. That is, to be confident the estimator is well-behaved, we should require instrument significance at a much *higher* level than 5% in the first stage. They find if the first-stage F is greater than 10 – corresponding to a t of 3.16 ($p = .0008$) in the one endogenous variable, one instrument case – then 2SLS two-tailed t-tests will reject a true null $H_0 : \beta = 0$ at a rate not "too far" from the correct 5% rate.

This $F > 10$ advice has been widely adopted in practice and presented in textbooks. For example, Stock and Watson (2015, p.490) say: "One simple rule of thumb is that you do need not to worry about weak instruments if the first stage F-statistic exceeds 10."

Later, Stock and Yogo (2005) derived thresholds for first-stage sample $\hat{F}$ based on the maximal size distortion in t-tests one is willing to tolerate. They find $\hat{F} > 16.4$ ensures (with 95% confidence) that a two-tailed 5% t-test will reject a true null H_0:β=0 at rate no higher than 10%. Recently, Lee et al. (2020) show one needs a much higher standard of $\hat{F} > 104.7$ to insure t-tests reject at a rate no higher that the correct 5% rate.

Unfortunately, these results are not well understood by applied researchers. They are derived using non-standard asymptotic theory, or complex small sample approximations, that are unfamiliar to most applied economists. This makes sorting through the diverse advice on acceptable first stage F levels a daunting task. We seek to explain weak instrument tests in a way that is accessible to applied researchers familiar with basic statistics. To make this possible, we focus primarily on the case of a single endogenous variable and a single instrument, as is very common in applied practice – see Andrews et al. (2019).

Since Stock and Yogo (2005), the weak instrument literature has been heavily focused on assessing size distortions in 2SLS t-tests.[1] We argue this has caused the literature to gloss over *other* important problematic properties of 2SLS that persist even when instruments are "strong" according to Stock-Yogo tests, and even in large samples.

In particular, 2SLS suffers from two key problems even when first-stage $\hat{F}$ is in the 10 to 20 range typically deemed acceptable. First, the estimator has very low power. Second, a strong association exists between 2SLS estimates and their standard errors: *the 2SLS estimator suffers from a very unfortunate tendency to generate artificially low standard errors precisely when it generates estimates most contaminated by endogeneity.*

Moreover, the association between 2SLS estimates and standard errors that we identify persists even if instruments are very strong (and even if samples are large). It has two important consequences: 2SLS estimates shifted towards OLS will appear spuriously precise, so the t-test has inflated power to judge such estimates significant. Conversely, 2SLS t-tests have little power to detect a true β opposite in sign to the OLS bias. This power asymmetry renders the t-test unreliable even if instruments are quite strong.

The practical import of this power asymmetry is serious: In an archetypal application of IV, one seeks to test if a policy intervention has a positive effect on an outcome, but a confound arises because those who receive the intervention tend to be positively selected on unobservables. In such a context, even if instruments are moderately strong by conventional standards, the 2SLS t-test will have spuriously inflated power to find false positive effects, and little power to detect true negative effects.

If the first-stage $\hat{F}$ meets the 105 threshold suggested by Lee et al. (2020) then 2SLS does exhibit nice properties in terms of both two-tailed t-test size and power. But the association between 2SLS estimates and their standard errors still has an important influence that distorts t-test results. 2SLS t-tests are much more likely to reject the null in samples where the 2SLS estimate is shifted in the direction of the OLS bias, and this problem persists unless until the first-stage F is in the thousands.

We go on to show that the weak instrument robust test of Anderson and Rubin (1949) greatly alleviates the power asymmetry problem, so it is far more reliable than the t-test. The AR test should be widely adopted in lieu of the t-test, even if instruments are strong.

[1]Stock and Yogo (2005) also considered bias relative to OLS. But this criterion can only be assessed given over-identification of degree 2, which is less common in practice than exact identification.

We also provide an empirical application to estimating the effect of anticipated income changes on consumption. This clearly shows the superiority of the AR test over the t-test. The AR test not only has correct size but also substantially better power. It seems clear the AR test should be much more widely adopted in applied work.

Applied researchers expect 2SLS to give more reliable results than OLS. So it is interesting to assess their relative performance. We show first-stage $\hat{F}$ must be well above conventional thesholds like 10 to give high confidence 2SLS will outperform OLS, unless endogeneity is very severe. So unless a higher standard can be met, one would be well-advised to seek better instruments or consider alternative approaches.

We then examine performance of the main alternative estimators to 2SLS: the Fuller (1977) estimator, JIVE and the unbiased estimator of Andrews and Armstrong (2017). We find the Fuller and unbiased estimators do offer improvements, but their performance cannot be judged adequate unless the first-stage $\hat{F}$ threshold is well above 10.

Finally, we examine the over-identified case. More instruments increase efficiency but make 2SLS t-tests more misleading, as the covariance between 2SLS estimates and their standard errors gets stronger. This makes robust inference even more important. The CLR test of Moreira (2003) outperforms the AR test in the over-identified case.

In summary, we find 2SLS performs very poorly when the first-stage $\hat{F}$ is toward the low end of the 10+ range deemed acceptable by current practice. We argue that a higher threshold should be adopted. Even then, it is essential to use robust tests like Anderson and Rubin (1949) unless first-stage $\hat{F}$ is in the thousands.

2. SOME BACKGROUND ON THE WEAK INSTRUMENT PROBLEM

To clarify the weak instrument problem we focus mainly on the case of one endogenous variable and one instrument, while assuming *iid* sampling. Issues of heteroskedastcity and multiple instruments are discussed later. We also abstract from exogenous covariates, as their inclusion complicates notation without changing anything of substance. Consider a structural equation where outcome y for person i is regressed on endogenous variable x:

$$y_i = x_i\beta + u_i, \text{ where } cov(x_i, u_i) \neq 0$$

The first-stage regression of x on the exogenous instrument z is:

$$x_i = z_i\pi + e_i, \text{ where } cov(z, u) = 0,\ cov(z, e) = 0,\ \pi \neq 0.$$

The regressor x is endogenous if $cov(e, u) \neq 0$, and the instrument z is valid if $cov(z, u) = 0$ and $\pi \neq 0$. It will often be convenient to assume $\pi > 0$, which can always be achieved by normalizing z. It is useful to decompose the error term e in the first stage into parts that are correlated and uncorrelated with the error u in the structural equation:

$$e_i = \rho u_i + \eta_i \quad \text{where} \quad cov\,(\eta, u) = 0,\ \ cov\,(z, \eta) = 0 \tag{1}$$

Here ρ controls the severity of the endogeneity problem, and x is exogenous if ρ=0.

The 2SLS estimator of β takes the following form, where ˆ denotes a sample value:

$$\hat{\beta}_{2SLS} = \frac{\sum_{i=1}^{n} z_i y_i}{\sum_{i=1}^{n} z_i x_i} = \beta + \frac{\sum_{i=1}^{n} z_i u_i}{\sum_{i=1}^{n} z_i x_i} = \beta + \frac{\widehat{cov}\,(z, u)}{\widehat{cov}\,(z, x)} \tag{2}$$

Clearly 2SLS is consistent: As $N \to \infty$ the sample covariance $\widehat{cov}(z, u)$ converges to its true value $cov(z, u) = 0$, and $\widehat{cov}(z, x)$ converges to $\pi\sigma_z^2 > 0$. So $\hat{\beta}_{2SLS}$ converges to the true β. But this result is not very useful in practice: Here we are interested in properties of 2SLS in finite samples – including, as we will see, very large finite samples.

We can substitute (1) into (2), and write $\hat{\beta}_{2SLS} - \beta$ in the instructive form:

$$\hat{\beta}_{2SLS} - \beta = \frac{\widehat{cov}\,(z, u)}{\pi \widehat{var}\,(z) + \widehat{cov}\,(z, e)} = \frac{\widehat{cov}\,(z, u)}{\pi \widehat{var}\,(z) + \widehat{cov}\,(z, \eta) + \rho \widehat{cov}\,(z, u)} \tag{3}$$

As a point of comparison, recall that the analogous expression for OLS is simply $\hat{\beta}_{OLS} - \beta = \widehat{cov}\,(x, u) / \widehat{var}(x)$, where the denominator only depends on observed covariates. This renders it far easier to analyze. In particular, under standard assumptions, the bias in OLS is simply $\rho var(u)/var(x)$, so OLS is unbiased in finite samples if $\rho = 0$.

Contrary to the simplicity of OLS, the $\widehat{cov}\,(z, \eta)$ and $\rho \widehat{cov}\,(z, u)$ terms sitting in the denominator of (3) render the finite sample properties of 2SLS quite complicated. In fact, as we explain below, 2SLS has odd finite sample properties that create serious problems if instruments are "weak," but that become irrelevant if instruments are "strong." We focus on the case of a perfectly exogenous instrument z with population covariance $cov(z, u)$=0, abstracting from problems created by violations of instrument exogeneity. Importantly, even small violations create severe asymptotic bias if instruments are weak, but this issue is discussed extensively elsewhere (see Wooldridge 5e 2012 p 521).

A crucial point is that in finite samples even a perfect instrument has some sample covariance with the error in the structural equation. Hence $\widehat{cov}(z, u)$ will be always be non-zero, even if the instrument is in fact exogenous. Similarly, $\widehat{cov}(z, \eta)$ will depart from zero in finite samples. As a result, the strength of the relationship between the instrument z and the endogenous variable x fluctuates from sample to sample, being stronger in samples where $\widehat{cov}(z, u)$ and $\widehat{cov}(z, \eta)$ are the same sign as π.

The fact that the sample covariances $\widehat{cov}(z, u)$ and $\widehat{cov}(z, \eta)$ appear in the denominator of (3) has unpleasant consequences for the finite sample behavior of the 2SLS estimator, even in large samples. We can learn a lot about these properties by carefully studying equation (3).[2] Now we list some key properties and provide a simple intuition for each:

First, the mean and variance of the 2SLS estimator do not exist: As $\widehat{cov}(z, u)$ and $\widehat{cov}(z, \eta)$ are both random variables, there is nothing to prevent finite sample realizations where $\widehat{cov}(z, x) = \pi \widehat{var}(z) + \widehat{cov}(z, \eta) + \rho \widehat{cov}(z, u) \approx 0$, causing $\hat{\beta}_{2SlS}$ to explode. Of course, this means the variance of $\hat{\beta}_{2SLS} - \beta$ doesn't exist either.

Second, the distribution of $\hat{\beta}_{2SLS} - \beta$ will exhibit skewness and fat tails (i.e., it is non-normal), rendering conventional t-tests non-normal and hence unreliable as a guide to inference. To see why, assume $\rho > 0$, so the endogeneity bias is positive, and that $\pi > 0$, so z has a positive *population* covariance with x. And to keep things simple, let us assume $\widehat{cov}(z, x) > 0$ so at least the *sample* covariance of z and x is the same sign as the population covariance - although this is not guaranteed.[3] Given these assumptions, the denominator of (3) is positive, so the *sign* of $\hat{\beta}_{2SLS} - \beta$ is determined by the sign of $\widehat{cov}(z, u)$ in the numerator. But the *magnitude* of $\hat{\beta}_{2SLS} - \beta$ is amplified (attenuated) when

[2] Research on finite sample properties of 2SLS with weak instruments relies on non-standard asymptotics – the local-to-zero asymptotics of Staiger-Stock (1997) or many-instrument asymptotics of Bekker (1994) – or on complex small sample theory; see Phillips (1983) or Rothenberg (1984). But it is surprising how much can be learned just by studying equation (3) using basic statistics. That is our approach here.

[3] We may also draw samples with $\widehat{cov}\,(z, x) \approx 0$. This generates extreme positive or negative outliers for $\hat{\beta}_{2SLS}$, depending on the sign of $\widehat{cov}\,(z, u)$. Also, if $\widehat{cov}\,(z, u)$ and/or $\widehat{cov}\,(z, \eta)$ go negative enough to drive $\widehat{cov}\,(z, x)$ negative, the sign of $\hat{\beta}_{2SLS} - \beta$ flips, so the distribution of $\hat{\beta}_{2SLS} - \beta$ can be bi-modal.

$\widehat{cov}(z,u) < 0$ (>0), which shrinks (inflates) the denominator. Thus, positive estimates of β are reined in while negative estimates are inflated, and the distribution of $\hat{\beta}_{2SLS} - \beta$ is skewed to the left. [We'll see this clearly in Section 4.]

Third, a large positive realization of $\rho\widehat{cov}(z,u)$ generates both an estimate shifted towards OLS and a low standard error - as it causes a large $\widehat{cov}(z,x)$. *Thus 2SLS has the unfortunate property that it gives artificially low standard errors in samples where $\hat{\beta}_{2SLS}$ is most shifted towards OLS.* As we'll see below, this association between 2SLS estimates and their standard errors has very important consequences that appear to have been largely neglected, or at least not adequately explored, in the prior literature.

Fourth, the median of $\hat{\beta}_{2SLS}$ is biased in the direction of OLS if the instrument is "weak." Recall that OLS is biased in a positive direction if $\rho > 0$. To see that 2SLS is biased in the same direction, consider the extreme case where the instrument is completely irrelevant, $\pi = 0$, so that $x_i = \eta_i + \rho u_i$. Then from (3) we have:

$$\hat{\beta}_{2SLS} - \beta = \frac{\widehat{cov}(z,u)}{\widehat{cov}(z,x)} = \frac{\widehat{cov}(z,u)}{\rho\widehat{cov}(z,u) + \widehat{cov}(z,\eta)} \tag{4}$$

Note that if $\rho > 0$ then $\hat{\beta}_{2SLS} - \beta$ is the ratio of two mean zero random variables that are positively correlated. The positive correlation causes the median of this ratio to be positive, simply because the numerator and denominator are more likely than not to have the same sign. Thus, the median of $\hat{\beta}_{2SLS}$ is biased in the same direction as OLS.

Furthermore, the two random variables that determine $\hat{\beta}_{2SLS} - \beta$ in (4) both have normal distributions in large samples. Marsaglia (2006) shows such a ratio has a Cauchy distribution shifted right by $\rho Var(u)/Var(x)$, which is exactly the OLS endogeneity bias. Thus, when $\pi = 0$, we see that the median bias of 2SLS is exactly equal to the OLS bias.[4]

Among these properties, the bias of the median 2SLS estimate toward OLS has received substantial attention in the applied literature since Bound et al. (1995). The non-existence of moments and non-normal shape of the $\hat{\beta}_{2SLS}$ distribution have received substantial attention from theorists (Richardson 1968, Sawa 1969, Phillips 1983, Rothenberg 1984), and Nelson and Startz (1990) and Mikusheva (2013) provide nice expositions. But, we argue, the problems created by the association between 2SLS estimates and their standard errors have received too little attention. We will explore that issue in detail below.

As we will now explain, the four problematic finite-sample properties of 2SLS remain relevant even in large samples if instruments are weak, but they vanish if instruments are sufficiently strong. All four properties arise from the perverse influence of the sample covariance $\widehat{cov}(z,e) = \widehat{cov}(z,\eta) + \rho\widehat{cov}(z,u)$ on the denominator of (3), so it is natural to assume they will vanish in a samples large enough so that $\widehat{cov}(z,e) \approx 0$. In fact, this describes the view of applied researchers prior to Bound et al. (1995).

The error in this logic is that, in a huge sample, it is also true that z may appear to be a significant predictor of x (at the 5% level) even if the true value of π is very small. In fact, as the sample size gets larger, the value of π that is likely to render z significant at the 5% level in the first-stage of 2SLS gets small exactly as fast as $\widehat{cov}(z,e)$ gets small. As a result, if z is only significant at the 5% level (and not better), then $\widehat{cov}(z,e)$ remains non-negligible relative to $\pi\widehat{var}(z)$. Hence the perverse influence of the $\widehat{cov}(z,e)$ term on the denominator $\pi\widehat{var}(z) + \widehat{cov}(z,e)$ remains important regardless of sample size.

[4] The Cauchy has fat tails, and its mean and variance do not exist. The 2SLS estimator inherits these properties, so the distribution of $\hat{\beta}_{2SLS} - \beta$ departs seriously from normality, rendering t-tests misleading.

Thus, large samples alone are not adequate for 2SLS to have nice properties. If our instrument is significant at ***only*** the 5% level in a large sample, it is not adequate to make the problems with 2SLS vanish. Given a "weak" instrument the problematic finite-sample properties of 2SLS remain relevant even in very large samples. What we need is an instrument that is "strong," meaning it meets a higher standard of instrument relevance – see Stock et al. (2002). To explain intuitively what is meant by "strong," we start by comparing the population and sample correlations between z and x:

$$corr(z,x) = \frac{\pi Var(z)+cov(z,e)}{\sigma_z \sigma_x} = \frac{\pi Var(z)}{\sigma_z \sigma_x}, \quad \widehat{corr}(z,x) = \frac{\pi \widehat{var}(z)+\widehat{cov}(z,e)}{\hat{\sigma}_z \hat{\sigma}_x}$$

Notice how the sample correlation between z and x is driven *both* by $\pi\widehat{var}(z)$, which reflects a true relationship between z and x, and also by $\widehat{cov}(z,e)$, which reflects *spurious* correlation between z and x in finite samples. An intuitive notion of a "strong" instrument is that $\pi var(z)$ should be large enough that we are confident the sample correlation between x and z mostly reflects their true relationship, not spurious correlation that arises because $\widehat{corr}(z,e) \neq 0$ in finite samples. That is, we want $\pi var(z)$ to be large enough that we can be confident that $|\pi\widehat{var}(z)| \gg |\widehat{cov}(z,e)|$.

It is simple to see why the strange finite sample properties of 2SLS that we discussed earlier vanish if $|\pi\widehat{var}(z)| \gg |\widehat{cov}(z,e)|$. In that case, the $\pi\widehat{var}(z)$ term in the denominator of (3) dominates the $\widehat{cov}(z,e)$ term, so realizations of $\widehat{cov}(z,x)$ that are near zero become extremely unlikely. This renders non-existence of the 2SLS estimator's mean and variance a mere academic curiosity. Furthermore, if the $\widehat{cov}(z,e)$ term is negligible, (3) reduces to just $\hat{\beta}_{2SLS} - \beta \approx \widehat{cov}(z,u)/\pi\widehat{var}(z)$, which is much simpler to deal with (as it resembles the expression for OLS). Under a fixed instrument assumption, the asymptotic distribution of $\hat{\beta}_{2SLS}$ is approximately normal and centered on β. So 2SLS is "approximately" unbiased, and normality is a decent approximation to its sampling distribution.

But how can we be confident that $|\pi\widehat{var}(z)| \gg |\widehat{cov}(z,e)|$ when π and $\widehat{cov}(z,e)$ are unobserved? First, note that we can rewrite this expression as:

$$|\pi| \cdot \widehat{var}(z) \gg \hat{\sigma}_z \hat{\sigma}_e \cdot |\widehat{corr}(z,e)| \rightarrow \frac{|\pi|\hat{\sigma}_z}{\hat{\sigma}_e} \gg |\widehat{corr}(z,e)|$$

If the instrument z is valid, $corr(z,e) = 0$, so $\widehat{corr}(z,e)$ converges to zero at a $\sqrt{N}$ rate, and $|\widehat{corr}(z,e)|$ is bounded in probability by $\frac{k}{\sqrt{N}}$ for a positive constant $k>0$. Thus:

$$\frac{|\pi|\hat{\sigma}_z}{\hat{\sigma}_e} \gg \frac{k}{\sqrt{N}}$$

Finally, substituting our consistent first-stage estimate $\hat{\pi}$ for the unobserved π, and squaring both sides, we obtain:

$$\sqrt{N}\frac{|\hat{\pi}|\hat{\sigma}_z}{\hat{\sigma}_e} \gg k \rightarrow N\frac{\hat{\pi}^2\hat{\sigma}_z^2}{\hat{\sigma}_e^2} \gg k^2$$

Recall that the sample F statistic for significance of z in the first stage is $\hat{F} = N\hat{\pi}^2\hat{\sigma}_z^2/\hat{\sigma}_e^2$. Thus our intuitive notion of wanting confidence that $|\pi\widehat{var}(z)| \gg |\widehat{cov}(z,e)|$ corresponds to a desire to have a first-stage F-statistic that is "big" in some sense.[5] The weak in-

[5]Because $F = NR^2/(1-R^2)$, a key insight is that properties of 2SLS do not depend on N or first-stage R^2 *per se*, but only how they combine to form F. So a large sample size alone is not sufficient to ensure 2SLS has an approximately normal sampling distribution. As Mikusheva (2013) explains, the convergence of $\sqrt{N}(\hat{\beta}_{2SLS} - \beta)$ to normality as $N \rightarrow \infty$ is very slow when the first-stage R^2 is small.

strument testing literature asks just how "big" the first-stage F needs to be for 2SLS to have nice properties. We explore this literature in the next section.

3. A SIMPLE GUIDE TO WEAK INSTRUMENT TESTS

Having explained the intuition behind weak instrument tests, we now examine them in more detail. Consider a model with a single endogenous variable x and a single exogenous instrument z. We focus on this simple case as it clarifies the key ideas, and it is the most common in applied practice. We let π determine the strength of the instrument, while $\rho \in [0,1]$ controls the extent of the endogeneity problem:

$$\begin{gathered} y_i = \beta x_i + u_i \\ x_i = \pi z_i + e_i \quad \text{where} \quad e_i = \rho u_i + \sqrt{1-\rho^2}\eta_i \\ u_i \sim iid\, N(0,1), \eta_i \sim iid\, N(0,1), z_i \sim iid\, N(0,1) \end{gathered} \tag{5}$$

This *iid* normal setup is not as restrictive as it appears, as Andrews et al. (2019) show that for any heteroskedastic DGP, there exists a homoskedastic DGP yielding equivalent behavior of 2SLS estimates and test statistics. Furthermore, any exogenous covariates can be partialed out of y and x without changing anything of substance.

Say we estimate the first-stage equation for x by OLS, and obtain $\hat{\pi}$ and $\hat{\sigma}_e^2$. We can test if z is a significant predictor of x using a standard F-test, given by $\hat{F} = N\hat{\pi}^2\hat{\sigma}_z^2/\hat{\sigma}_e^2$. For example, if N=1000 we conclude z is significant at the 5% level if $\hat{F} > 3.85$. This corresponds to a t-test of $t > 1.96$ (as F is the square of t in this case).

Prior to Bound et al. (1995), passing such an F-test would have been considered sufficient evidence to conclude one's instrument was relevant, and to proceed with 2SLS. But Bound et al. drew attention to situations where instruments are significant at conventional levels in the first stage of 2SLS, despite having a small or "weak" correlation with the endogenous variable. For instance, a quantitatively small correlation may be highly statistically significant in very large samples. In such cases median 2SLS estimates are biased towards OLS, and the 2SLS t-test is highly non-normal.

In an important paper, Staiger and Stock (1997) show that for an instrument to be "strong" enough for 2SLS to have acceptable finite sample properties it must meet a higher standard than 5% significance in the first stage. They formalize our statement in Section 2 that we want the first-stage F statistic to be large enough that we are confident that $|\pi\widehat{Var}(z)| \gg |\widehat{cov}(z,e)|$, which in turn implies 2SLS will have nice properties.

To understand the Staiger-Stock approach, we define the "concentration parameter" C, which measures strength of the instrument in first stage. It is closely related to the first-stage F-statistic. In particular, C is the true value of the F statistic that we could construct if we observed the unknown π and σ_e^2 that we estimate in the first stage. The finite sample properties of 2SLS only depend on N through C:

$$C = \text{"}true\text{"}\, F = N\frac{Var(z\pi)}{\sigma_e^2} = N\frac{\pi^2\sigma_z^2}{\sigma_e^2} = N\pi^2 \text{ and } \hat{F} = N\hat{\pi}^2\hat{\sigma}_z^2/\hat{\sigma}_e^2$$

The sample F-statistic is an estimate of C. Just as we showed for F, if C gets large we can be confident that $|\pi\widehat{Var}(z)| \gg |\widehat{cov}(z,e)|$, and the problems with the 2SLS estimator vanish. So if C is "large" in some sense the instruments are "strong." But how large does C need to be for 2SLS to have nice properties? And how large does the sample $\hat{F}$ need to be to give confidence that the unobserved C meets that threshold?

To address these questions, Stock and Yogo (2005) focus on a particular property of 2SLS, the size of a 5%-level two-tailed t-test. This is the rate of rejecting H_0:β =0 when it is in fact true. If a test has correct size it should reject at a 5% rate. But given the non-normality of the 2SLS estimator, the t-test size may depart substantially from 5%.

Formally, Stock-Yogo derive a formula for power of the t-test in terms of C, ρ and true β that we present in Appendix A. Evaluating power at β=0 gives the size of the test. Size depends on ρ, so Stock-Yogo focus on the *maximal* size distortion, which occurs when $\rho = \pm 1$. The integral in (A3) can be evaluated numerically at different levels of C.

For example, suppose you find it acceptable that a 2SLS two-tailed t-test rejects the null H_0:β=0 at the 5% level no more than 15% of the time. In other words, you are willing to tolerate a maximal size distortion of 10%. Numerical evaluation of (A3) reveals you need C=1.82. Suppose instead you want a maximal size distortion of just 5% (i.e., your 5% t-test rejects H_0:β=0 no more than 10% of the time). Then you need C=5.78. Finally, Lee et al. (2020) show one needs C=141.6 to bring maximal size distortion down to zero.

Thus, by requiring C to be large enough, we can render the maximal size distortion as small as desired. But we can't observe C, so we must rely on sample $\hat{F}$ as an estimate. Unfortunately, because C equals $Var(z\pi)/\sigma_e^2$ times a factor of N, sample $\hat{F}$ is not a very accurate estimate of C, and it doesn't get more accurate as sample size increases.

In particular, *regardless* of sample size, the sample $\hat{F}$ is a draw from a non-central F with non-centrality parameter C. Hence, to be confident (at the 95% level) that C is at least 1.82, we need $\hat{F}$ to be at least 8.96. If we want to be confident (at 95%) that C is at least 5.78, we need to have $\hat{F}$ of at least 16.38. In general, to be confident the concentration parameter C is at least c, we need a first-stage $\hat{F}$ well above c.

Table 1 lists various levels of C, the levels of π required to achieve them if N=1000, and the first-stage F-test critical value for a 5% test that C attains the desired level. For example, to be 95% confident that C is at least 2.3, we need $\hat{F} > 10$, which corresponds to the popular Staiger-Stock rule of thumb for acceptable instrument strength.

Table 1. First-Stage F Critical Values Required to Achieve Different Objectives

Concentration Parameter ("True First-Stage F")	Value of π	F critical value to reject C<c at 5%	Rejection rate for a 5% t-test of H_0:β=0
1.82	0.0427	8.96	15%
2.30	0.0480	10.00	SS Rule of Thumb
3.84	0.0620	13.00	–
5.78	0.0760	16.38	10%
10.00	0.1000	23.10	–
73.75	0.2716	104.70	5%

Table 1 also shows, in some key cases, the (maximal) 5% two-sided t-test size achieved by that level of C. For example, if C=5.78 (which we test for using $F_{.05}$=16.38) a 2SLS 5% two-tailed t-test rejects a true null hypothesis at no more than a 10% rate.[6] The

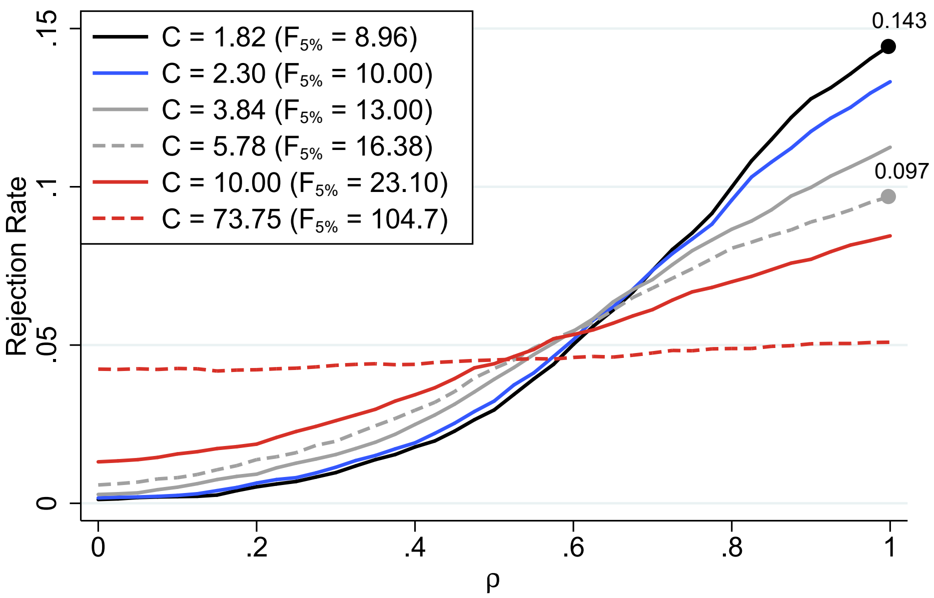
[6]We include C=10 as Staiger and Stock (1997) and Angrist and Pischke (2008) derive formulas indicating the bias of 2SLS relative to OLS is roughly $1/[1+C]$ if the first moment exists. So C=10 makes relative bias only 10%. This result is only useful with 2 or more instruments, so the mean of the 2SLS estimator exists. But we find it interesting to examine behavior of the 2SLS median in the C=10 case.

t-test size distortion depends only on C, not on N and π *per se*, so how we set N and π in Table 1 is somewhat arbitrary (i.e., the π levels are specific to N=1000). Finally, we include $F_{.05}$=104.7 in the last row of Table 1, as Lee et al. (2020) show that $\hat{F}$ >104.7 insures the maximal size of the 5% level t-test is no greater than 5%.[7]

Next, to gain a better understanding of how weak instrument tests work in practice, we implemented a simple simulation experiment to assess how well 2SLS estimates perform under each scenario in Table 1. We simulate data from the model in equation (5), assuming $\beta = 0$, varying the degree of endogeneity as captured by ρ in small increments from 0 to 1. We set the parameter π to each alternative level listed in Table 1, in order to vary the strength of the instrument. We generate artificial data sets of size N=1,000 for each combination of (π, ρ). As we've noted, it is the level of C, not N or π *per se*, that drives the properties of 2SLS. We report results from 10,000 Monte Carlo replications.

Figure 1 reports the rejection rate of a two-tailed t-test test of H_0: β=0 at the 5% level, based on the 2SLS parameter and standard error estimates, for each combination of C and ρ. The label shows both C and the associated F-test 5% critical level.

Figure 1. Rejection rate of $H_0 : \beta = 0$ using a 5% Two-tailed t-test



To understand Figure 1, it is important to recall that Stock and Yogo (2005) calculate worst-case (maximal) rejection rates over all values of ρ. As we see in Figure 1, the worst case corresponds to ρ near 1, so the endogeneity problem is very severe. The agreement between the results in Figure 1 and the Stock-Yogo analysis is striking. For C=1.82 they predict a worst-case rejection rate of 15%, while our simulations show 14.3%. And for C=5.78 they predict a worst-case rejection rate of 10%, while we obtain 9.7%.

But Figure 1 shows a focus on $\rho = 1$ is not innocuous, as rejection rates vary substantially with ρ. In the next section we discuss the implications of this phenomenon.

[7] This is based on a slightly different mode of analysis from Stock-Yogo. Here the maximum of equation (A3) is taken over C and ρ. We discuss their approach in Section C.

4. PROBLEMS WITH 2SLS HIDDEN BY WEAK INSTRUMENT TESTS

An obvious limitation of the Stock-Yogo analysis is that the size distortion in the 2SLS t-test when ρ=1 tells us little about rejection rates at lower levels of ρ. As we see in Figure 1, the size distortion in the t-test is sharply increasing in ρ even in cases that easily pass standard tests to rule out weak instruments, such as C=10 (F_{05}=23.1). Only in the very strong instrument case of C=74 (F_{05}=105) is size roughly invariant to ρ.

The dependence of 2SLS t-test size on the nuisance parameters ρ and C reflects the fact that it is not a "pivotal statistic." It is only asymptotically pivotal as C grows large. In contrast, the OLS t-test is pivotal, as its distribution is purely a function of the data.

Henceforth, we will refer to instruments as "weak" if they fall in the gray area between roughly C=2.3 and C=74 where (i) they pass conventional weak IV tests, but (ii) the distribution of the t-statistic (and size of the t-test) is still highly dependent on ρ.

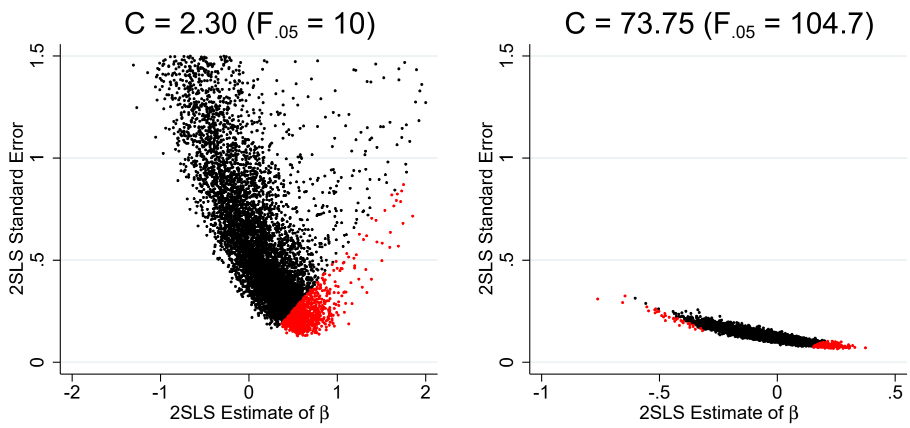
The results in Figure 1 raise serious concerns about the behavior of 2SLS t-tests when instrument are "weak" in this expanded sense. To see why, we need to understand why rejection rates are strongly increasing in ρ in such cases. There are two reasons:

First, when ρ=0 the 2SLS estimator has very low power. Estimates are roughly centered at zero, standard errors are very large, and it is very rare to reject H$_0$: β=0.

Second, if ρ>0, then, in samples where realized $\widehat{cov}(z,u)$ is high, 2SLS tends to generate both high estimates of β and (artificially) low standard errors. Thus, 2SLS obtains spurious power from the finite sample correlation between z and u. This is a very unfortunate property, as ***2SLS estimates appear to be spuriously more precise in samples where they are most shifted in the direction of the OLS endogeneity bias***. This is what causes the t-test to reject H$_0$:β=0 more frequently as ρ increases.

Figure 2 shows the association between 2SLS estimates and their estimated standard errors is quantitatively important. It plots $se(\hat{\beta}_{2SLS})$ against $\hat{\beta}_{2SLS}$. The left panel shows the case of ρ=0.8 and C=2.30 ($F_{.05}$=10). A strong negative association is very evident; in fact, the Spearman r_s is -0.576 and Kendall's τ is -0.511. The magnitudes involved are also substantial – 2SLS estimates that are most shifted toward the OLS bias appear to be *much* more precisely estimated. Of course this precision is spurious.

Figure 2. Standard Error of $\hat{\beta}_{2SLS}$ plotted against $\hat{\beta}_{2SLS}$ itself ($\rho = 0.80$)



Note: Runs with standard error >1.5 not shown. Red dots indicate $H_0:\beta=0$ rejected at 5% level.

The red dots in Figure 2 indicate cases where $\hat{\beta}_{2SLS}$ differs significantly from zero according to a two-tailed 5% t-test. In the C=2.30 ($F_{.05}$=10) case, the hypothesis H$_0$: $\beta = 0$ is rejected at a 10% rate. Due to the negative association between the 2SLS estimates and their standard errors, all rejections occur when $\hat{\beta}_{2SLS}$>0, and none when $\hat{\beta}_{2SLS}$<0. Only the estimates most shifted towards the OLS bias are ever judged significant.

The right panel of Figure 2 shows the case of ρ=0.8 and C=74 ($F_{.05}$=105). When the instrument is this strong the negative association between $se(\hat{\beta}_{2SLS})$ and $\hat{\beta}_{2SLS}$ persists. In fact, Spearman's r_s is -0.92 and Kendall's τ is -0.75, showing this is not just a weak instrument phenomenon. 2SLS now has a rejection rate near the correct 5% rate (4.87%). But 93% of those rejections occur when $\hat{\beta}_{2SLS} > 0$. A one-tailed 2.5% test of H$_0$: $\beta \leq 0$ rejects at a 4.54% rate. The asymmetry in positive vs. negative rejections is a direct consequence of the negative association between 2SLS estimates and standard errors.

Table 2 gives a broader view of this phenomenon by showing Spearman's r_s between $se(\hat{\beta}_{2SLS})$ and $\hat{\beta}_{2SLS}$ for different levels of C and ρ. Clearly, the negative association is not specific to the examples in Figure 2. Table 2 shows how this relationship gets stronger as ρ increases. This drives the pattern of rejection rates increasing with ρ in Figure 1.

Table 2. Spearman Rank Correlations (r_s) between $se(\beta_{2SLS})$ and $\hat{\beta}_{2SLS}$

Concentration Parameter	$F_{5\%}$	Spearman Correlations (r_s) $\rho = 0.2$	$\rho = 0.5$	$\rho = 0.8$
1.82	8.9	-0.113	-0.291	-0.492
2.3	10	-0.133	-0.359	-0.576
5.78	16.38	-0.267	-0.612	-0.875
73.75	104.7	-0.350	-0.720	-0.917

We now study power of the 2SLS t-test in more detail by simulating probabilities of rejecting H$_0$:β=0 when it is false. We consider alternative versions of model (5) where the true β is 0.30 or -0.30. Importantly, these would be quantitatively large but plausible values in typical empirical applications, as they imply a one standard deviation change in x induces an 0.25 standard deviation change in y. The results are reported in Table 3.

Table 3. Power of 2SLS t-Test – Frequency of Rejecting H$_0$: $\beta = 0$ (%)

Concentration Parameter	$F_{5\%}$	$\beta = 0.3$ $\rho = 0$	$\rho = 0.5$	$\rho = 1$	$\beta = -0.3$ $\rho = 0$	$\rho = 0.5$	$\rho = 1$
1.82	8.96	1.8	11.7	25.5	1.7	0.1	4.2
2.30	10.00	2.4	13.0	25.1	2.2	0.2	3.2
3.84	13.00	4.4	15.9	25.1	4.2	0.3	1.7
5.78	16.38	7.2	18.8	26.3	7.2	0.5	0.8
10.00	23.10	13.4	23.7	28.9	13.3	2.3	0.2
73.75	104.7	71.4	67.8	65.1	71.9	78.0	89.1

Note: The table reports the probability of rejecting the false null hypothesis H_0: $\beta = 0$.

A striking result in Table 3 is that the 2SLS t-test has almost no power to detect a sizeable true negative effect when the OLS bias is positive, unless instrument strength is far above conventional thresholds. For example, in the C=2.3 ($F_{.05}$=10) case widely considered an acceptable threshold for a strong instrument, the probability of rejecting the false null H_0: β=0 is only 0.2% when the true β is -0.3 and ρ = 0.50. Increasing instrument strength substantially to C=10 ($F_{.05}$=23.1) only increases power to 2.3%.

The negative association between 2SLS estimates and standard errors that arises when the OLS bias is positive drives this result. This is evident from the geometry of Figure 2. If β=-0.30 the cloud of points in Figure 2 is shifted left. This shift moves the most precisely estimated $\hat{\beta}$'s closer to zero, so they are rarely significant.

Another clear pattern in Table 3 is that power of the 2SLS t-test is asymmetric when the OLS bias is positive ($\rho > 0$). For example, if C=10 and ρ=0.5 the probability of rejecting the false null H_0: β=0 is 23.7% when β = 0.30 compared to only 2.3% when β=-0.30. This asymmetry also arises from the geometry of of Figure 2. If β=0.30 the cloud of points in Figure 2 is shifted right. This clearly generates more significant results, in contrast to the leftward shift (β=-0.30) that generates fewer.

Appendix A presents an expanded analysis of 2SLS t-test power curves. This shows the power asymmetry we have described is quite a general phenomenon. As a consequence, it is difficult for a 2SLS t-test to detect plausibly sized true negative effects when the OLS bias is positive. This pattern is reversed if the OLS bias is negative.

Finally, focusing on all the "weak" instrument cases in Table 3 – by which we mean all cases except C=74 – we see that the power of the 2SLS t-test is very low when ρ=0. But if true β is positive then power increases with the degree of endogeneity ρ. This is because, as ρ increases, the t-test derives more spurious power from the finite sample correlation between the instrument z and the structural error u.[8]

4.1. Median Bias of 2SLS

A potential alternative explanation for the pattern in Figure 1 is that bias in the median 2SLS estimate increases as the degree of endogeneity (ρ) increases. In principle, this could cause the rate of rejecting H_0: $\beta = 0$ to increase with ρ. But we can rule this out. For all values of C we consider, the instruments are strong enough that median bias in 2SLS is negligible, or at least modest, regardless of the degree of endogeneity.

Appendix E Figure E4 illustrates this point. If C=10 or better, 2SLS estimates are essentially median unbiased. Figure A4 plots the median 2SLS bias *relative* to the OLS bias. Relative bias is modest in all cases. For example, when C=2.30 ($F_{.05}$=10) the 2SLS median bias is less than 15% of the OLS bias unless ρ is very small. Thus, if median bias were one's only concern, a first-stage $\hat{F}$ of 10 would be quite sufficient.

4.2. Summary

The Stock-Yogo test assesses worst-case size distortions in two-tailed 2SLS t-tests. If an instrument passes these tests, it implies that size distortions are modest. But this conceals serious power problems that afflict the t-test unless instrument strength is far above conventional thresholds. *It is an unfortunate property of the 2SLS estimator that it tends to generate standard errors that are too low precisely when it also generates estimates shifted in the direction of the OLS bias.* One important consequence is the t-test has little power to detect plausibly large true negative effects when the OLS bias is positive. 2SLS is approximately median unbiased, but this property is not very useful if only estimates shifted in the direction of the OLS bias are likely to be significant.

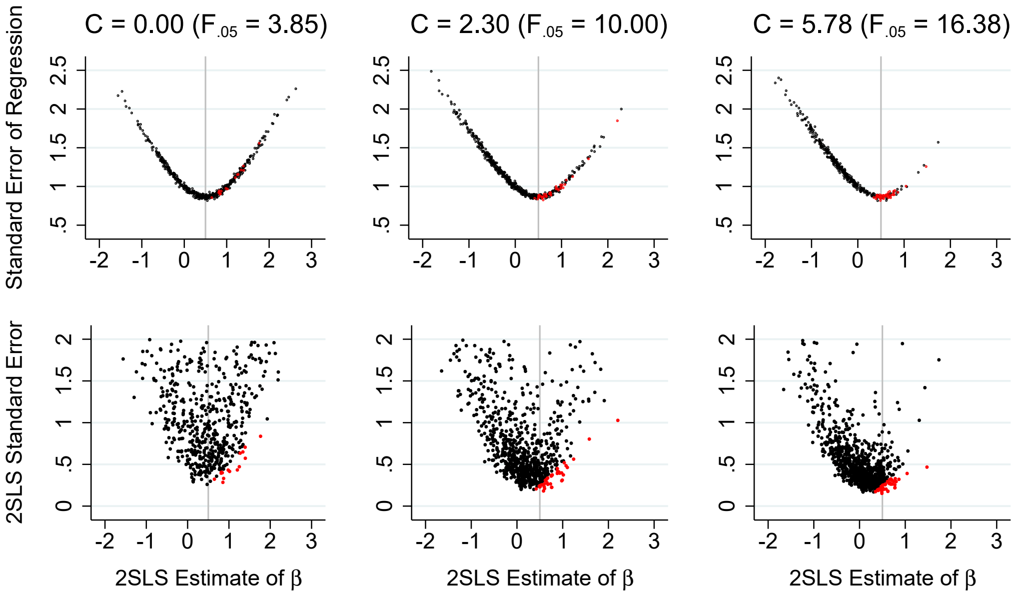
[8]Positive realizations of $\rho\widehat{cov}(z,u)$ increase $\widehat{cov}(z,x)$, the sample covariance between the instrument and the endogenous variable. This, in turn, generates a spurious reduction in the 2SLS standard error.

5. INSIGHTS FROM FINITE SAMPLE THEORY

Results from finite sample theory can help us understand the strong dependence between 2SLS estimates and their standard errors. Phillips (1989) derives two key properties of 2SLS in the unidentified case where the instrument is irrelevant. First, the 2SLS estimator converges in distribution to a scale mixture of normals centered on $E(\hat{\beta}_{OLS})$. Second, the 2SLS variance estimator ($\hat{\sigma}^2$) converges in distribution to a quadratic function of $\hat{\beta}_{2SLS}$, with a minimum at $E(\hat{\beta}_{OLS})=\rho Var(u)/Var(x)$.

These properties are shown in Figure 3, obtained by applying 2SLS to 1000 datasets of size N=1000 from the DGP in eqn. (5). We set β=0 and ρ=0.50 so $E(\hat{\beta}_{OLS})$=0.50. The left panel shows results in a completely unidentified model (C=0,π=0). As we see in the upper left, the standard error of regression ($\hat{\sigma}$) is indeed minimized at $\hat{\beta}_{2SLS}$=0.5. Of course, $\hat{\sigma}$ is a key driver of the standard error of $\hat{\beta}_{2SLS}$. This causes $\hat{\beta}_{2SLS}$ to appear (spuriously) more precise in the vicinity of the OLS bias, as we see in the lower left panel that plots $se(\beta_{2SLS})$ against $\hat{\beta}_{2SLS}$. Note how this looks similar to Figure 2.

Figure 3. The Dependence Between 2SLS Estimates and Standard Errors



Note: Runs with standard error > 2 not shown. Red dots indicate $H_0 : \beta = 0$ rejected at 5% level.

A point that is little appreciated (at least by applied researchers) is that this property of 2SLS in the unidentified case has a major influence on the behavior of 2SLS estimates and standard errors in strongly identified models. For this reason, Phillips (1989) calls this the "leading case." To illustrate, the middle and right panels of Figure 3 report results for identified models where C= 2.3 or 5.78. As the strength of identification increases, the 2SLS estimates shift left; they move away from the OLS bias and start to become median unbiased. Strikingly however, the quadratic relationship between $\hat{\sigma}^2$ and $\hat{\beta}_{2SLS}$ is unaffected. Stronger identification merely shifts most of the 2SLS estimates into the left side of the quadratic curve, but it does not affect the curve itself.

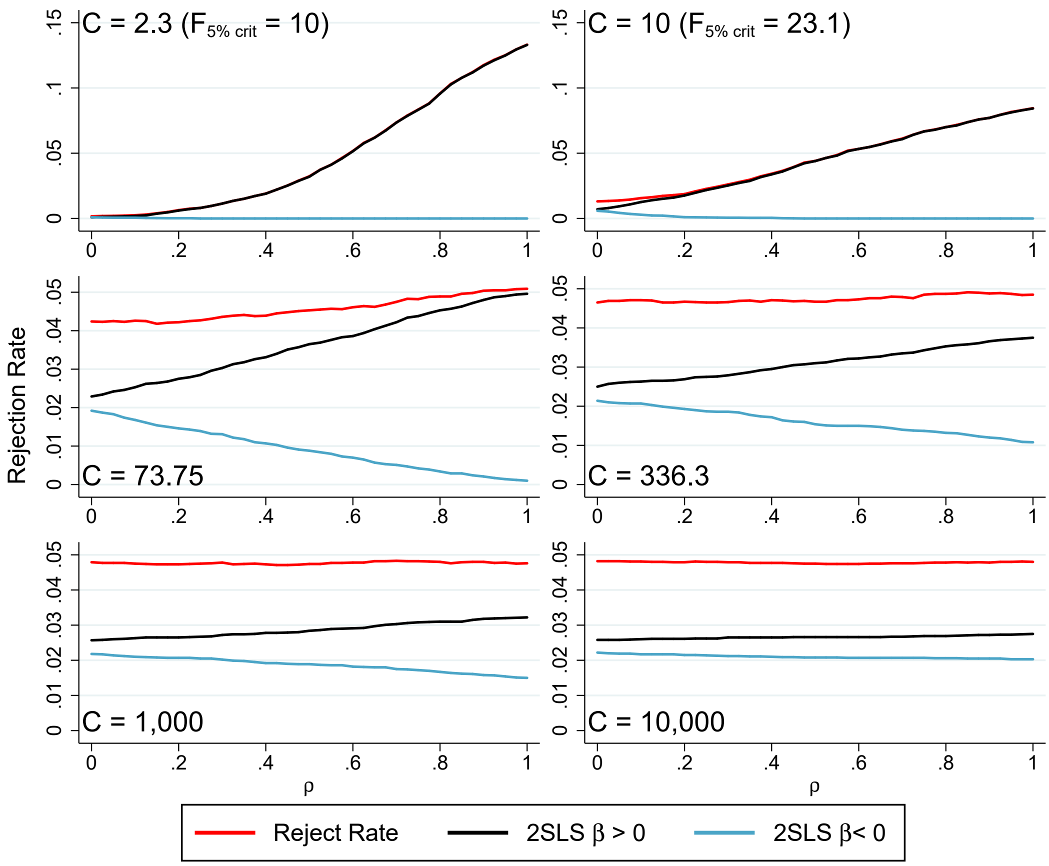
Thus, even in strongly identified models, there exists a strong association between the 2SLS standard error of regression ($\hat{\sigma}$) and the 2SLS estimate, such that ($\hat{\sigma}$) is minimized when estimates are in the vicinity of the OLS bias. This causes 2SLS estimates that are shifted towards the OLS bias to appear spuriously precise.

6. ONE-TAILED *T*-TESTS

An important consequence of the association between 2SLS estimates and their standard errors is that size distortions in one-tailed tests are much greater than distortions in two-tailed tests. This is shown in Figure 4. The red lines show rejection rates of two-tailed 5% t-tests of $\mathrm{H}_0 : \beta = 0$ for different levels of C and ρ. The black/blue lines show how frequently these rejections occur at positive/negative values of $\hat{\beta}_{2SLS}$. This is equivalent to plotting rejection rates of one-tailed 2.5% t-tests of H_0: $\beta \leq 0$ and H_0: $\beta \geq 0$.

In the case of C=2.30 ($F_{.05}$=10) that corresponds to the Staiger-Stock rule of thumb for acceptably strong instruments, the rejection rate of **both** the 5% two-tailed test and the 2.5% one-tailed test against H_0: $\beta \leq 0$ increase from 0% to to 14.5% as ρ increases from 0 to 1. But the one-tailed test against the null of H_0: $\beta \geq 0$ **never** rejects.

Figure 4. Rejection Rates of One and Two-Tailed Tests



We have seen that in the strong instrument case of C=74 ($F_{.05}$=105) there is negligible size distortion in two-tailed t-tests. Indeed, in Figure 4 we see the rejection rate of a 5% two-tailed test increases only modestly from 4.1% if $\rho = 0$ to 5% when $\rho = 1$.

But the size distortions in one-tailed t-tests remain substantial: The rejection rate of a 2.5% one-tailed test against H$_0$: $\beta \leq 0$ *increases* from 2.2% to 5% as ρ increases from 0 to 1. Conversely, the rejection rate of a 2.5% one-tailed test against H$_0$: $\beta \geq 0$ *declines* from 1.9% when $\rho = 0$ down to essentially zero when $\rho = 1$. This asymmetry is a direct consequence of the negative association between $se(\hat{\beta}_{2SLS})$ and $\hat{\beta}_{2SLS}$, which imparts positive (negative) 2SLS estimates of β with spuriously high (low) precision.

How strong do instruments need to be for one-tailed t-tests to have correct size? As we see in Figure 4, even if the concentration parameter C is increased to 336.3, which corresponds to a first-stage $F_{.05}$=400, the asymmetry in rejection rates for one-tailed tests remains substantial. Strikingly, we find C must be increased to roughly 10,000 to eliminate size distortions in one-tailed 2SLS t-tests. Only then are rejection rates of one-tailed tests insensitive to the level of ρ.

The asymmetry in rejection rates of one-tailed t-tests is of great practical importance. Applied researchers almost always use two-tailed tests because symmetry makes one-tailed tests redundant (e.g., a 5% two tailed-test is equivalent to a 2.5% one-tailed test). But as we see, this is false for 2SLS, even with very strong instruments.

7. THE ANDERSON-RUBIN TEST

The usual suggestion of the weak IV literature is to avoid the t-test, and instead use "robust" tests that have correct size even if instruments are weak. In the single instrument case the unambiguous choice is the Anderson and Rubin (1949) test. The AR test is simply the F-test from the reduced-form regression of y on z, which is $y = z\beta\pi + (\beta e + u) = z\xi + v$ where $\xi = \beta\pi$. It judges $\hat{\beta}_{2SLS}$ to be significant if z is a significant predictor of y in the reduced form regression. Why does this work? Given a valid instrument z, which must satisfy $\pi \neq 0$ and $cov(z, v) = 0$, a test of the null hypothesis $H_0 : \xi = 0$ provides an alternative way to test $H_0 : \beta = 0$. Equivalently, the AR test is the F-test from the regression of y on $z\hat{\pi}$, where $\hat{\pi}$ is the first stage estimate of π.

The AR test has correct size, regardless of instrument strength or the level of ρ, as it is simply an F-test from an OLS regression (i.e., it is a pivotal statistic). But AR has other highly desirable properties as well. The superior power properties of the AR test relative to the t-test are illustrated in Figure 5. It presents analytical power curves for both tests, for the model in (5), obtained as described in Appendix A. We set the level of instrument strength to C=10, which is well above conventional weak instrument thresholds (i.e., to have 95% confidence that C is at least 10 one needs a first-stage $\hat{F} > 23.1$). The left and right panels show results for $\rho = 0.50$ and 0.80, corresponding to moderate and severe endogeneity problems, respectively. We adopt a 5% level for both tests.

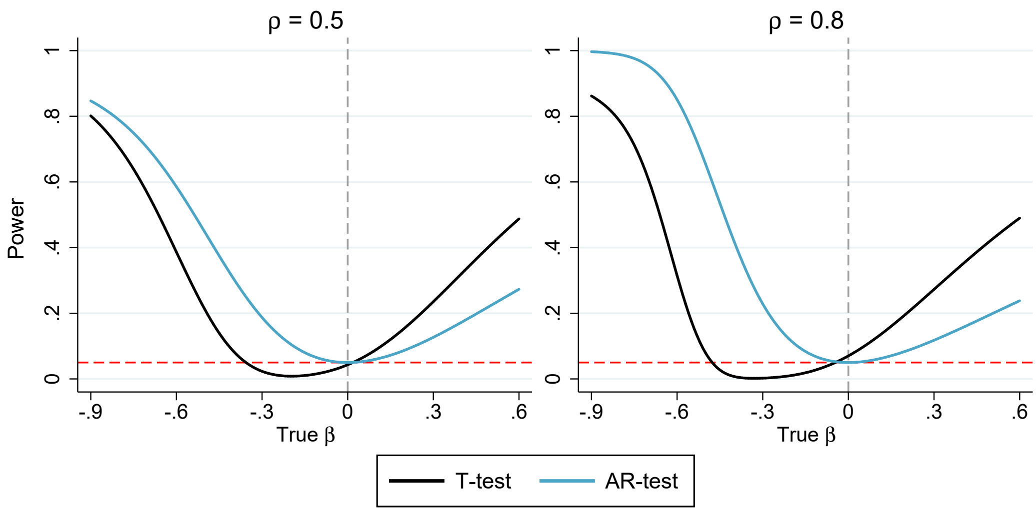
An unbiased statistical test has the desirable property that the probability of rejecting $H_0 : \beta = 0$ is minimized if the true β is in fact zero. We can see in Figure 5 that the AR test is unbiased. It also has correct size, as its power evaluated at $\beta = 0$ is exactly 5%.

In contrast, the t-test is biased. As we see in left panel of Figure 5, if $\rho = 0.50$ the power of the t-test is near zero when the true β is in the vicinity of -0.25. So the probability of rejecting $H_0 : \beta = 0$ is minimized when the true β is near -0.25 rather than at zero. And if $\rho = 0.80$ (right panel) the power of the t-test is near zero for true β in the -0.25 to -0.40 range. Recall that for the model in (5), β is approximately equal to the change in y (in standard deviations) induced by a one standard deviation change x. Effect sizes in this -0.25 to -0.40 std. dev. range are quite large in typical applications. Thus, these results show that the t-test has essentially no power to detect a wide range of substantively large true negative effects when the endogeneity bias afflicting OLS is positive.

This pattern is a direct consequence of the positive association between 2SLS estimates and standard errors that we discussed in Sections 4-6, which can cause the t-test to have very low power when the true value is opposite in sign to the OLS bias. In contrast, as we also see in Figure 5, the AR test has far better power to detect true negative effects. Crucially, Moreira (2009) shows that, in the single instrument case, the AR test is the uniformly most powerful unbiased test (for testing a point null hypothesis). This means it has better power than any other unbiased test, regardless of the true parameter value.

Figure 5 also shows that when the true β is positive the t-test has higher power than the AR test against H_0:β=0. But this property is not desirable: It reflects the facts that (i) the t-test is biased, and (ii) the 2SLS standard errors are spuriously small for estimates that are shifted in the direction of the OLS bias (positive).

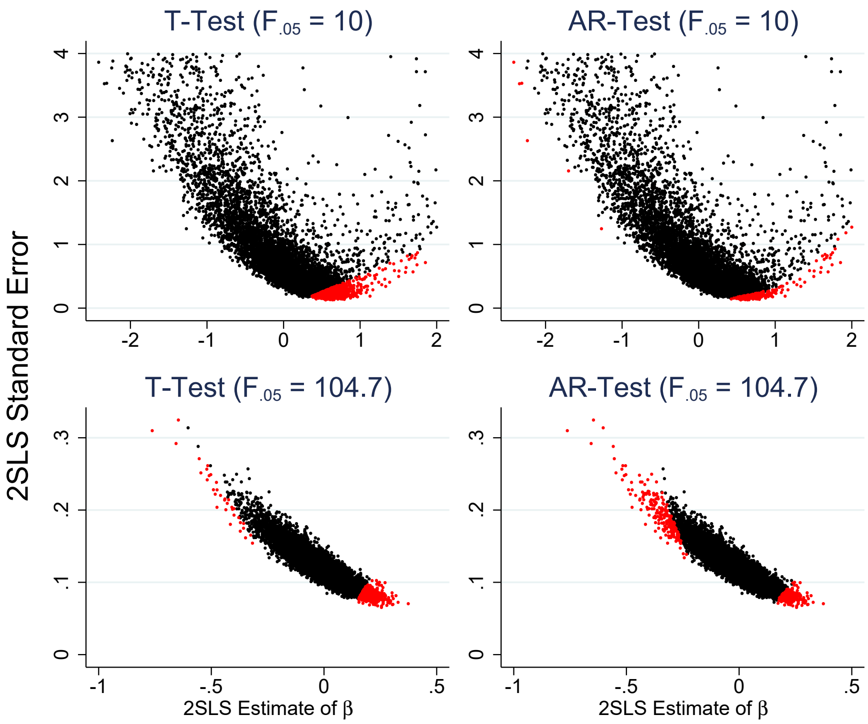
Figure 5. Power of the T-Test vs. AR-Test when $C = 10$



Another notable aspect of Figure 5 is the fact that the power of the two-tailed 5%-level t-test evaluated at $\beta = 0$ is roughly 5% when $\rho = 0.5$, and about 6% when $\rho = 0.8$. This illustrates the point of Angrist and Kolesár (2021) that two-tailed t-test size inflation is minor except in cases where endogeneity is extremely strong and/or the instrument is very weak. If this were one's only concern one might argue – as they do – for a sanguine view of the performance of the t-test. We argue instead that the truly concerning problems with the t-test are its bias and poor power properties, which the AR test avoids.

One thing Figure 5 does not reveal is the frequency of positive vs. negative rejections, as the power of a two-tailed test is defined as their sum. In Sections 4 to 6 we argued it is important to consider the sign of rejections. We consider this in Figure 6, which compares results from t-tests vs. AR tests, focusing on the case of ρ=0.80. The top panel considers the case of C=2.3 ($F_{.05}$=10) which corresponds to the Staiger-Stock rule of thumb for acceptably strong instruments. Here the 5% two-tailed t-test rejects H$_0$:β=0 at a 10% rate, and these all occur when $\hat{\beta}_{2SLS} > 0$, reflecting the severe power asymmetry of the t-test. The AR test rejects H$_0$:β=0 at a 4.8% rate, which only differs from the correct 5% rate due to sampling variation. However, in the top right of Figure 6, we see that 85% of those rejections occur when $\hat{\beta}_{2SLS} > 0$. So using AR does not avoid the asymmetry that most rejections occur at positive values, which is the direction of the OLS bias.

Figure 6. T-test vs. AR test rejections: $SE(\hat{\beta}_{2SLS})$ plotted against $\hat{\beta}_{2SLS}$ itself ($\rho = 0.80$)



Note: Runs with standard error > 4 are not shown. Red dots indicate $H_0 : \beta = 0$ is rejected at the 5% level. Results are for the 2SLS t-test (left panel) or the Anderson-Rubin test (right panel).

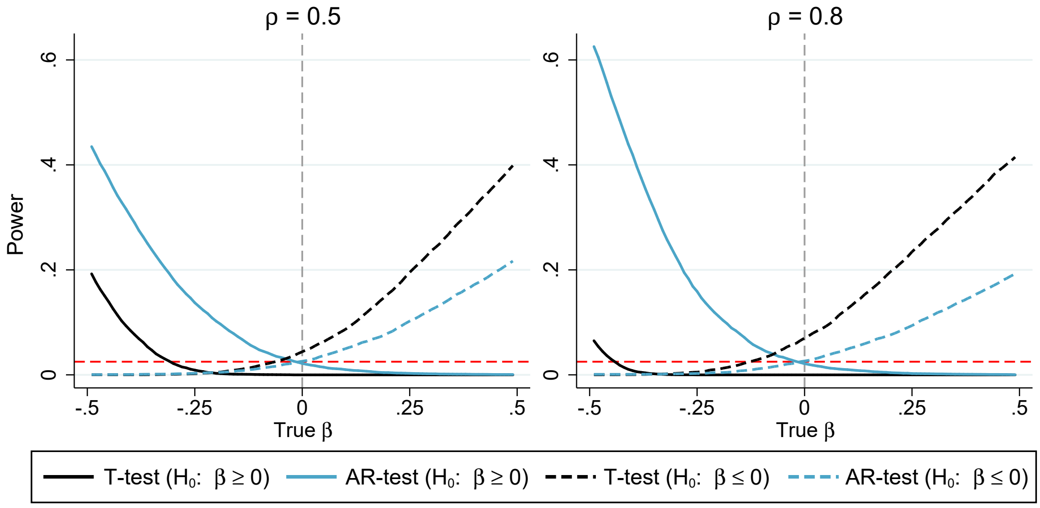
Thus, if instruments are weak, the AR test - like the t-test - is more likely to call 2SLS estimates significant if they are shifted in the direction of the OLS bias. The reason is again the strong positive association between $\rho\widehat{cov}(z, u)$ and $\hat{\beta}_{2SLS}$. A large value of $\rho\widehat{cov}(z, u)$ also generates a large value of the AR test. Thus, if $\rho > 0$ the AR test and $\hat{\beta}_{2SLS}$ have a positive association. Hence, the AR test is more likely to reject H$_0$: $\beta = 0$ if $\hat{\beta}_{2SLS} > 0$. Importantly, however, in contrast to the t-test, this problem with the AR test vanishes quickly as instrument strength increases - as we now show.

The bottom panel of Figure 6 reports results for the strong instrument case of $F_{.05}$=105. Here the size distortion in the two-tailed t-test is mostly eliminated. But, as the red shading shows, 93% of those rejections occur when $\hat{\beta}_{2SLS} > 0$. In contrast, the AR test exhibits a fairly even balance of positive (54%) vs. negative rejections. Thus, the AR test achieves this balance at a vastly smaller first-stage F than required for the t-test. This provides an additional reason to prefer the AR test to the t-test.

The AR test can be cast as a one-tailed test as follows: Reject H_0: $\beta \leq 0$ if the AR test is significant and $\hat{\beta}_{2SLS} > 0$. Conversely, reject H_0: $\beta \geq 0$ if the AR test is significant and $\hat{\beta}_{2SLS} < 0$. The AR test may not be the uniformly most powerful unbiased test against one sided alternatives. Figure 7 compares the power of one-sided 2.5% level AR and t-tests. We look at the case of C=10 and $\rho = 0.50$ or 0.80. If the size of these four tests were correct, then each should have a 2.5% rejection rate when the true $\beta = 0$.

As we have already seen, the size distortion in one-tailed t-tests is substantial. Consider the ρ=0.80 case: When $\beta = 0$ the 2.5% one-tailed test against H_0: $\beta \geq 0$ has power of 12% while that against H_0: $\beta \leq 0$ has zero power. In fact, the power of a one-tailed t-test to detect a true negative effect is essentially zero unless the effect size is at least -0.5. In contrast, the power of the one-tailed AR tests at $\beta = 0$ are much closer to 2.5% (i.e., 3% on the positive side and 2% on the negative side). The AR test has much greater power to detect true negative effects. For example, for a true $\beta = -0.4$ the one-tailed AR test against H_0: $\beta \geq 0$ has power of roughly 50% compared to near zero for the t-test.

Figure 7. Power of the One-Tailed t-Test vs. AR-Test when $C = 10$



The flip side of this pattern is that the one-tailed t-test has spuriously inflated power to detect positive effects in this context (where the OLS bias is positive). The 2.5% level t-test has power of 12% to reject H_0: $\beta \leq 0$ when the true β=0, while the size of the AR test is almost exactly 2.5%. As we move to higher positive values of the true β, the t-test power remains above that of the AR test. We emphasize this is not a desirable property, as it occurs because 2SLS standard errors are artificially small when the 2SLS estimate is shifted in the direction of the OLS bias, exaggerating the precision of positive estimates.

Summarizing the results of Sections 6 and 7, we have seen that the t-test causes 2SLS estimates to appear spuriously more precise when they are shifted in the direction of the OLS bias – even if instruments are strong – but the AR test is far less sensitive to this problem. Thus we make the following general recommendation: ***Based on its superior size and power properties, and to avoid over-rejecting the null when $\hat{\beta}_{2SLS}$ is shifted in the direction of the OLS bias, one should rely on the AR test rather than the t-test even when the first-stage F-statistic is in the thousands***.

We conclude this section with some important observations on the AR test. First, we note that Moreira (2009)'s optimal power result applies to *iid* settings. However, Moreira and Moreira (2019) extend it to settings with heteroskedasticity and clustering. In that case, one should implement the AR test using a heteroskedasticity and/or cluster robust F-test, as we illustrate in Section 8 and in our companion paper Keane and Neal (2021).

Second, the AR statistic is "pivotal," meaning its distribution under H_0: β=0 does not depend on the unknown ρ and π that govern the degree of endogeneity and strength of the instrument. This allows one to invert the AR test to form valid confidence intervals, as discussed in Anderson and Rubin (1949) and Dufour (2004), and illustrated in Section 8. In contrast, the distribution of the t-statistic is highly dependent on ρ and π, rendering confidence intervals suspect even in large finite samples with weak instruments.

If instruments are very weak the AR confidence interval can be unbounded. In particular, if the first-stage $\hat{F} < 3.84$ then a 95% confidence interval for β is unbounded. As Dufour (2004) notes, this is not a problem but rather an accurate reflection of uncertainty. If $\hat{F} < 3.84$ we do not have 95% confidence that the instrument is significant in the first stage, so we lack 95% confidence that the model is identified. It is an odd property of the t-test that it gives a bounded confidence interval in this case.

Third, with multiple instruments the AR test is no longer optimal. Instead, Moreira (2003) shows the conditional likelihood ratio (CLR) test is the uniformly most powerful unbiased test (the AR and CLR tests are equivalent in the single instrument case). Finlay and Magnusson (2009) provide a Stata command to calculate a heterskedastcity robust version of the CLR test, and to invert it to form confidence intervals. We compare the performance of t, AR and CLR tests in the over-identified case in Section 11.

8. EMPIRICAL EXAMPLE: THE "EXCESS" SENSITIVITY OF CONSUMPTION TO CURRENT INCOME

Here we present an empirical application that illustrates the ideas discussed in the previous sections: Estimating the elasticity of consumption with respect to anticipated income changes. As we will see, this application is characterized by a concentration parameter C that is just above 10. Thus conventional weak instrument testing thresholds are met, but as we will see, issues related to weak instruments are still relevant. It is interesting to examine the behavior of 2SLS hypothesis tests in this context.

As background, we note that simple versions of the permanent income hypothesis (PIH) imply the consumption elasticity should be zero. A positive value is often referred to as "excess sensitivity," which may be evidence of liquidity constraints. Note, however, that elaborations of the PIH to account for consumption/leisure substitution and/or consumer prudence (i.e., reluctance to borrow against uncertain future income) may also help to explain "excess sensitivity." Regardless, the elasticity of consumption with respect to anticipated income changes is of considerable interest.

To estimate the elasticity we run the regression:

$$\Delta lnC_{it} = \alpha + \beta \Delta lnY_{it} + \boldsymbol{\gamma} \mathbf{V}_{it} + \epsilon_{it} \tag{6}$$

where C_{it} is consumption of household i in period t, Y_{it} is household income, and $\mathbf{V}_{it}$ is a vector of control variables. This includes year dummies (to capture business cycle effects). Attanasio and Browning (1995) emphasize the importance of controlling for effects of household demographics on consumption, so we also include age of the household head, the change in age squared, and the change in number of children at home.

To estimate the effect of *anticipated* income changes we need to instrument for ΔlnY_{it} using a variable that is both known to consumers at time $t-1$ and predicts income growth. As Altonji and Siow (1987) pointed out, the instrument must also be uncorrelated with measurement error in income, ruling out using income at $t-1$. Fortunately, income is well approximated by an IMA(1,1) process, so ΔlnY_{it} is MA(2). Following Mork and Smith (1989), this means $lnY_{i,t-2}$ can be used as the instrument for ΔlnY_{it}, as it is known at t-1, predicts income growth, and is uncorrelated with error in measuring ΔlnY_{it} (if measurement error is serially uncorrelated). Following Mariger and Shaw (1993) we test if the MA income process is stable over our sample period, and cannot reject that it is.

We use data from the Panel Study of Income Dynamics (PSID), which has followed a sample of over 5,000 U.S. households and their descendants since 1968. The PSID became biannual in 1999. We use the most comprehensive consumption measure,[9] available from 2005 to 2019, giving us eight observations per household. The consumption and income variables refer to the survey year, so in estimating (6) we use changes over two year intervals. For income, we use total family income, which includes all taxable and transfer income for the head of household, spouse, and any other adults. The use of changes in log consumption and income accentuates measurement error, so as is typical in this literature we introduce a number of data screens to remove outliers.[10] This process left us with 643 households and seven observations per household, for a total sample size of $4,501$.

We report the results in Table 4. Estimating (6) by OLS we obtain a coefficient of 0.140 with a standard error of 0.017, indicating a positive covariance between consumption and income changes. But OLS does not estimate the elasticity with respect to anticipated income changes for two key reasons: First, observed income changes include both anticipated and unanticipated components, and the PIH predicts that unanticipated increases in income will increase consumption via an income effect, biasing the coefficient upward. Second, measurement error in income changes is likely to be substantial, biasing the coefficient downward. So the direction of bias is theoretically ambiguous.

We report the first stage 2SLS results in the second column of Table 4. As expected $lnY_{i,t-2}$ is a highly significant predictor of ΔlnY_{it}. A higher level of income at $t-2$ predicts an income decline from $t-1$ to t, as we expect given the MA(2) structure of ΔlnY_{it}. As we are now using actual data, rather than the *iid* normal data of our sampling experiments, we need to consider robust statistics. The heteroskedasticity robust partial F statistic is 10.28, so it is slightly above the commonly recommended threshold of 10.

The second stage 2SLS result is reported in the third column of Table 4. The estimated elasticity is 0.552, implying OLS is downward biased, and that current consumption is very sensitive to anticipated changes in current income. However, the heteroskedasticity robust standard error is 0.292, so the 2SLS t-test is not significant at the 5% level. In contrast, the last column presents reduced form results. The heteroskedasticity robust partial F statistic is 4.31, so the AR test indicates our elasticity estimate is significant at the 3.8% level. Inverting the AR test we obtain a 95% confidence interval of (0.03, 1.57) which excludes 0. Thus, the t-test and AR test disagree. This highlights the question of whether the t-test or AR test is more reliable.

[9]Total observed consumption is comprised of all food, housing, utilities, transport, education, childcare, healthcare, clothes, vacation, and recreation expenditure.

[10]We restrict the sample to households whose income was between $\$3,000$ and $\$1,000,000$ in every year. We also dropped households that report income or consumption changes of less than -70% or more than 300% between any two survey years. We impose a balanced panel by removing households with missing data in any survey year from 2005 to 2019 in an attempt to reduce noise.

Table 4. Elasticity Estimates - PSID

	OLS	2SLS 1^{st} Stage	2SLS 2^{nd} Stage	Reduced Form
Dependent Variable	ΔC_{it}	ΔY_{it}	ΔC_{it}	ΔC_{it}
ΔY_{it}	0.1398		0.5524	
	(0.0166)		(0.2920)	
	[0.0185]		[0.2024]	
ΔlnY_{t-2}		-0.0321		-0.0177
		(0.0100)		(0.0085)
		[0.0078]		[0.0062]
F-Stat (Hetero-σ Robust)		10.283		4.312
p-value		0.0014		0.0379
F-Stat (Cluster Robust)		16.965		8.182
R^2	0.0414	0.0256		0.0224

Note: Heteroskedasticity robust standard errors are in parentheses while standard errors clustered by individual are in square brackets. All regressions control for year effects, age, change in age^2 *and change in number of children.* $N = 4,501$

We now investigate the behavior of the AR test and the t-test in this data environment. We conduct the following experiment: Using our PSID sample of N=4,501 observations we can "bootstrap" a new artificial dataset by sampling 4,501 observations with replacement. We do this 10,000 times to form 10,000 artificial datasets. We then repeat the analysis of Table 4 on all 10,000 datasets, and summarize the results in Table 5:

Table 5. Results from Monte Carlo Bootstrap Samples

	OLS $\hat{\beta}$	OLS S.E.	2SLS $\hat{\beta}$	2SLS S.E.	F Stat. First Stage	Reduced Form $\hat{\beta}$	Reduced Form S.E.
Median	0.1395	0.0165	0.5502	0.2971	10.3122	-0.0177	0.0085
Mean	0.1395	0.0166	0.6135	2.7765	11.3651	-0.0177	0.0085
Std. Dev.	0.0164	0.0006	1.6630	156.3519	6.6763	0.0085	0.0003

Our method of constructing samples means the point estimates in Table 4 are the true values of the data generating process *in our simulation experiment*,[11] and the concentration parameter C of the DGP is 10.28. Thus, we are above conventional thresholds for an acceptably strong instrument. We begin by noting two features of Table 5:

First, the median OLS, 2SLS and reduced form estimates all agree closely with the point estimates reported in Table 4. The mean OLS and reduced form estimates also agree, while of course the sample mean of the 2SLS estimates does not (as the mean of the 2SLS estimator does not exist in the exactly identified case).

Second, the heteroskedasticity robust standard errors of the OLS and reduced form estimates agree with the empirical standard deviations of those estimates across the

[11] This because the variance-covariance matrix of (y, x, z) in the NLSY79 sample is the population variance-covariance matrix in the simulation experiment.

10,000 datasets, and also with the heteroskedasticity robust standard errors reported in Table 4. Thus, the asymptotic standard errors are a good guide to the actual sampling variation of the OLS and reduced from estimates. In contrast, the empirical standard deviation of the 2SLS estimates bears no resemblance to the 2SLS standard error, because the variance of 2SLS does not exist in the exactly identified case.[12]

Now we examine the behavior of the 2SLS standard error. In Figure 8 we plot $se(\hat{\beta}_{2SLS})$ against $\hat{\beta}_{2SLS}$ across the 10,000 samples. A strong *positive* association between 2SLS estimates and standard errors is evident, reversing the pattern in Figures 2 and 6. The reversal occurs because in this DGP the correlation ρ between the errors in the structural and reduced form equations is negative (-0.40). As a result, the mean OLS estimate of 0.14 is well below the true elasticity of $\beta = 0.55$. When the OLS bias is negative the association between 2SLS estimates and standard errors is positive. As we see in Fig. 8, the 2SLS standard errors imply the 2SLS estimates are much more precise when they are in the vicinity of the OLS bias than when they are near the true value of $\beta = 0.55$.

In the top panel of Figure 8 we assess the performance of the t-test. In the top left panel we shade in red cases where $\hat{\beta}_{2SLS}$ is significantly different from zero according to a two-tailed 5% level test. This occurs 39.7% of the time, which is the power level. In the right panel the red dots indicate cases where we reject the true null $\beta = 0.55$. This occurs in 3.58% of cases, so the size of the test is too small. This is consistent with Figure 1, looking at size in the case of C=10 ($F_{.05}$=23) and $\rho = 0.4$. More importantly, almost all rejections occur when $\hat{\beta}_{2SLS}$ is near zero, because the 2SLS standard errors are (spuriously) smaller when the estimate is shifted in the direction of the OLS bias.

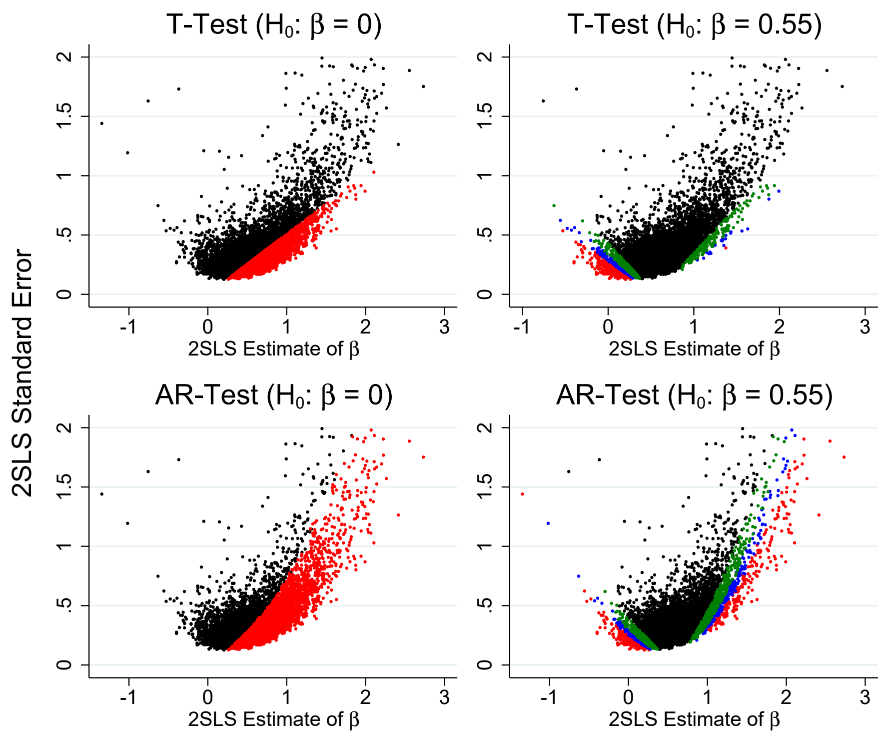
The bottom panel of Figure 8 assesses the performance of the AR test. In the bottom left panel we shade in red cases where $\hat{\beta}_{2SLS}$ is significant at the 5% level, which occurs 54.8% of the time. Thus the AR test exhibits substantially better power than the t-test (54.8% vs. 39.7%). In the right panel we consider AR tests of the true null $\beta = 0.55$. This is simply the (heteroskedasticity robust) partial F-test from a regression of $y - x\beta$ on the instrument and other exogenous variables. The red region again highlights rejections at the 5% level. This occurs in 4.69% of cases, so the size of the test is quite accurate. Moreover, those rejections are almost evenly distributed between cases where $\hat{\beta}_{2SLS}$ is above vs. below the true value of 0.55. Thus, the AR test largely solves the problem of asymmetry in test results that affects the 2SLS t-test.[13]

These results show that the AR test exhibits both substantially better power and more accurate size than the t-test in this data environment. Moreover, it does not suffer from the problem that estimates shifted in the direction of the OLS bias appear to be more precise. This illustrates that the problems with 2SLS t-tests and advantages of the AR test that we discussed in Sections 4–6 are not limited to the *iid* normal environment, but are also evident in a non-normal environment constructed from actual data.

[12]Table 4 also reports cluster-robust statistics that account for serial correlation. Given the negative serial correlation in residuals induced by the MA structure of consumption changes, this reduces the estimated standard errors. As a result, the cluster-robust 2SLS t-test indicates the elasticity estimate is significant. The cluster-robust standard error is appropriate for applied work in this case, given the panel structure of the data. However, in our simulation experiment we create artificial data by *iid* sampling with replacement from the 4,501 observations. This breaks the panel structure of the data, so the data structure in our sampling experiment is cross-sectional. Hence we focus on the heteroskedasticity robust statistics that ignore serial correlation, as these are what the sampling experiment will mimic.

[13]We also shade the 10% and 20% level rejections in blue and green. The AR test rejects at 9.85% and 19.9% rates, so size is accurate, and rejections are evenly distributed above/below the true value. The t-test, in contrast, only rejects at 7.1% and 13.9% rates, with 6.9% and 11.7% in the negative direction.

Figure 8. Standard Error of $\hat{\beta}_{2SLS}$ plotted against $\hat{\beta}_{2SLS}$ itself



Note: Runs with standard error > 1 are not shown. In the left panel, red dots indicate $H_0 : \beta = 0$ is rejected at the 5% level, while in the right panel red dots indicate $H_0 : \beta = 0.55$ is rejected at the 5% level. Blue and green indicate rejections at the 10% and 20% levels.

In general, the performance of the t-test deteriorates relative to the AR test as the endogeneity problem becomes more severe. In Keane and Neal (2021) we present an application to estimating labor supply elasticities. In that case $\rho = -0.70$, and the advantages of the AR test are much greater.

The AR test is widely recommended by theorists. For example, Andrews et al. (2019) state: "In just-identified models ... the AR test has (weakly) higher power than any other size-α unbiased test... Since AR confidence sets are robust to weak identification and are efficient in the just-identified case, there is a strong case for using these procedures..."

Despite its clear advantages, the AR test has been largely neglected by applied researchers. In fact, as far as we know, it has never been adopted in the large literature on estimating the elasticity of consumption with respect to anticipated income changes. In our application, given that the first-stage F statistic is only slightly above 10, conventional wisdom says we are in a borderline case where weak instruments may or may not be a concern. Clearly the AR test should be viewed as more reliable than the t-test in this context. Our experiment illustrates just how superior the AR test is in practice.

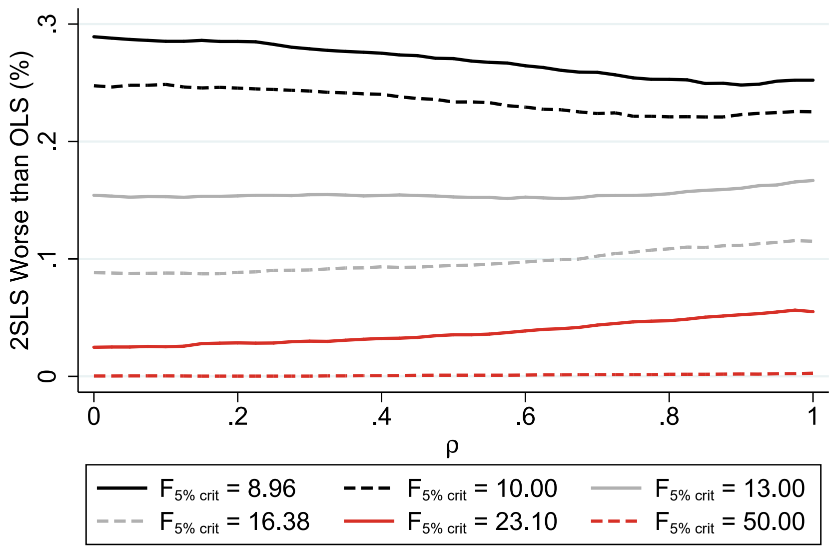
A limitation of the AR test is that it is not most powerful in over-identified settings, where the conditional likelihood ratio (CLR) test of Moreira (2003) may be preferable. The two tests are equivalent in the just-identified case. We compare the CLR and AR tests in Section 11 which covers the over-identified case.

9. PERFORMANCE OF 2SLS RELATIVE TO OLS

Conventional weak instrument tests ask if instruments are strong enough for 2SLS to have "nice" properties, such as modest size distortions in t-tests. But in practice applied researchers expect 2SLS to deliver more reliable results than OLS. So it is interesting to ask: "How strong must instruments be to give high confidence that 2SLS will generate more reliable results than OLS?" If instruments fail to meet this minimal standard, one would be well-advised to find better instruments or consider alternative approaches.

We start to explore this question by asking how often 2SLS estimation errors exceed the worst-case OLS bias. This occurs if x is *perfectly* correlated with the error in the outcome equation ($\rho = 1$ and π=0). The bias is then 1.0. In Figure 9 we report the frequency of $|\hat{\beta}_{2SLS}| > 1$ for the DGP in (5). We see 2SLS estimation errors of this magnitude are common if the first-stage F is toward the low end of conventional "strong" instrument thresholds. For example, if C=2.30 ($F_{.05}$=10), which corresponds to the Staiger-Stock rule of thumb, the risk of such extraordinary large outliers is a rather remarkable 22% to 25%. Interestingly, however, if we consider a moderately strong instrument case of C=29.4 ($F_{.05}$=50) such large outliers are virtually impossible.

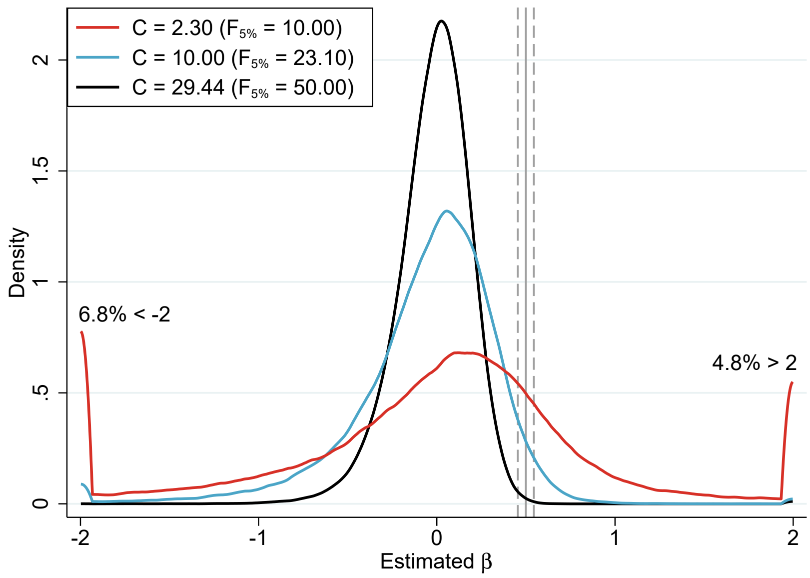
Figure 9. Probability of 2SLS Estimation Error Exceeding Worst-case OLS Bias.



Note: We plot the proportion of times that $|\hat{\beta}_{2SLS}| > 1$, the worst-case bias of OLS.

Figure 10 compares the density of 2SLS estimates in the cases of C=2.30 ($F_{.05}$=10), C=10 ($F_{.05}$=23.1) and C=29.4 ($F_{.05}$=50). In the first case the distribution is highly non-normal, with fat tails, high frequency of extreme outliers and left skewness very apparent. The distribution is still highly non-normal in the C=10 case. Only in the C=29.4 ($F_{.05}$=50) case does normality appear to be a decent approximation to the sampling distribution of the 2SLS estimator. Figure 10 also shows the mean OLS estimate (at 0.50) and the 95% confidence interval around that estimate. Careful inspection of the figure reveals that, due to their high dispersion, the 2SLS estimates are frequently further from the true value (β=0) than the OLS estimates. This is especially true in the C=2.3 and C=10 cases, but is much less common in the C=29.4 ($F_{.05}$=50) case.

Figure 10. Kernel Density of 2SLS Estimates when $\rho = 0.5$



Note: This figure plots the kernel densities of 2SLS estimates censored at $+-2$ *with various values of* C*. 100,000 simulations and a bandwidth of* 0.03 *was used. The grey line shows the mean OLS estimate, while the grey dotted lines represent the 95% confidence interval.*

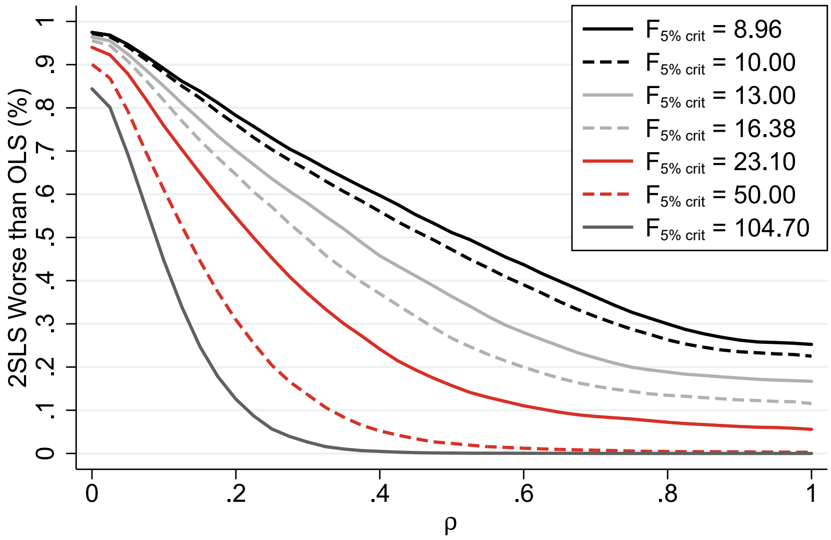
Next in Figure 11, we report the fraction of simulated datasets where 2SLS performs worse than OLS, meaning the 2SLS estimate of β is further from the truth than the OLS estimate. As expected, at low levels of endogeneity ($\rho \approx 0$) OLS is almost always better than 2SLS, as there is little bias and OLS is more efficient. What is surprising is the high frequency with which OLS outperforms 2SLS at much higher values of ρ. Take the case of C=2.30 ($F_{.05}$=10), which corresponds to the Staiger-Stock rule of thumb. The value of ρ has to approach 0.50 before the probability that 2SLS outperforms OLS passes 50%.

The results in Figure 11 are hard to assess without a prior on reasonable values of the unknown parameter ρ, which determines the extent of the endogeneity problem. This is not exceptional: As we discussed in Section 4, Stock-Yogo tests implicitly assume ρ is near one, so the endogeneity problem is very severe. This is because they evaluate the *worst-case* performance of 2SLS hypothesis tests, which occurs when ρ is near one.

In many applications one can put a reasonable prior on ρ, and assess the performance of 2SLS relative to OLS for different levels of instrument strength in that scenario. For example, consider the archetypal application of IV to estimating a regression of log wages on education. Using PSID data from 2015 we calculate a correlation between education and log earnings of 0.45.[14] Thus, if education has no true effect on earnings, and the only

[14] We use data on 30-54 year old household heads, and we partial out effects of age and age^2. The wage is constructed as labor income/hours. We screen on hours $\in [400, 4160]$, income $\in [\$3000, \$235884]$, wage>\$2.70 per hour, and valid data on education and labor income. This gives N=3,634.

Figure 11. Probability of 2SLS Performing Worse than OLS



Note: We plot the proportion of Monte Carlo replications where $|\hat{\beta}_{2SLS} - \beta| > |\hat{\beta}_{OLS} - \beta|$.

reason it is correlated with earnings is endogeneity - i.e., it is perfectly correlated with the latent ability endowment - then the highest possible value of ρ is 0.45. So in such an application a uniform prior on $\rho \in [0, 0.45]$ may be reasonable.

Applied researchers may find a prior on ρ unfamiliar, so it is worth noting that plausible values of ρ can be backed out empirically given any hypothesized value of β. For $\beta = \beta^p$, the implied value of ρ is simply the correlation of the residuals from (i) the regression of $y - x\beta^p$ on z and (ii) the first stage regression of x on z.[15] For example, if $\beta^p = 0$ this is the correlation of the reduced form errors, and if $\beta^p = \beta_{OLS}$ this is zero. Thus, a prior that $\beta \in (0, \beta_{OLS})$, which may be natural in many applications where one suspects positive selection into treatment, would correspond to a prior that ρ lies between zero and the correlation of the reduced form residuals. This is how we motivate the uniform prior on $\rho \in [0, 0.45]$ in the example of wages and education.

Table 6 shows the probability that 2SLS will outperform OLS under different scenarios for the concentration parameter C (and the associated first-stage F), and different priors on ρ. For example, if C=2.30 ($F_{.05}$=10), which corresponds to the Staiger-Stock rule of thumb for strong instruments, and given a uniform prior $\rho \in [0, 0.45]$, the probability that 2SLS outperforms OLS is only 26%. Alternatively, a researcher who thinks education is highly (but not completely) endogenous, might have a uniform prior of $\rho \in [0.35, 0.45]$. Even in that case, the probability that 2SLS outperforms OLS is only 41%.

Clearly then, in an application to estimating the effect of education on earnings, one should require a substantially higher level of instrument strength than the $\hat{F}$=10 threshold suggests. For instance, in the case of C=29.4 ($F_{.05}$=50), a uniform prior on $\rho \in [0, 0.45]$ implies a 65% chance that 2SLS will outperform OLS. Given a uniform

[15] Of course also including any exogenous control variables present in the application.

prior on $\rho \in [0.35, 0.45]$ this increases 95%. Thus, in the archetypal application of IV to estimating the return to education, if one believes ability bias is severe, one needs an $\hat{F}$ in the vicinity of at least 50 to have high confidence that 2SLS will outperform OLS.

In other contexts the endogeneity problem may plausibly be more severe. For example, consider a case where a uniform prior of $\rho \in [0.5, 1.0]$ is plausible. Then, if C=2.30 ($F_{.05}$=10), which corresponds to the Staiger-Stock rule of thumb for strong instruments, the probability that 2SLS outperforms OLS is 69%. Contrast this with the case of a uniform prior $\rho \in [0, 0.45]$, where this same level of instrument strength only gives a 47% chance that 2SLS will outperform OLS. Thus, the level of instrument strength required to have confidence that 2SLS is likely to outperform OLS depends heavily on one's prior about ρ. Applied researchers should consider this when assessing instrument strength.

In summary, the results of this section show instruments should be much stronger than standard thresholds, like the popular $\hat{F} > 10$, to give confidence that 2SLS results are likely to be superior to OLS, in the sense that $|\hat{\beta}_{2SLS} - \beta| < |\hat{\beta}_{OLS} - \beta|$. It is difficult to give any general rule of thumb for acceptable instrument strength by the latter metric, as the probability that 2SLS outperforms OLS is strongly increasing in the degree of endogeneity (ρ). We suggest that researchers assess the level of instrument strength required to have reasonable confidence that 2SLS will outperform OLS in any particular application, based on reasonable priors on the severity of the endogeneity problem (ρ).

Despite the difficulty of devising a general rule of thumb, a strong case can be made that applied researchers should adopt a higher threshold of instrument strength in the vicinity of $\hat{F} > 50$. As we have seen, this threshold renders extreme outlier 2SLS estimates very unlikely, and it makes 2SLS likely to outperform OLS even at moderate levels of endogeneity (ρ). If such a threshold cannot be met, it is advisable to seek stronger instruments, or pursue alternative strategies, such as OLS combined with a serious attempt to control for omitted variables. We reiterate that robust tests should be used in lieu of 2SLS t-tests regardless of $\hat{F}$.

Table 6. Probability of 2SLS Outperforming OLS (%)

Concentration Parameter	$F_{5\%Crit}$	Uniform Prior for ρ: 0 to 1	0 to 0.45	0.35 to 0.45	0.5 to 1
1.82	8.96	45	24	41	65
2.30	10	48	26	45	69
3.84	13	56	32	55	77
5.78	16.38	62	38	64	84
10.00	23.1	70	47	77	91
29.44	50	83	65	95	99
73.75	104.7	89	76	100	100

Note: We report the frequency of $|\hat{\beta}_{2SLS} - \beta| < |\hat{\beta}_{OLS} - \beta|$ across Monte Carlo replications, averaged across all possible values of ρ under a uniform prior that ρ falls in the indicated range.

Finally, we note that our whole discussion of properties of 2SLS has been centered on the *iid* normal case, in order to focus on key issues. This is not as restrictive as it may appear, as for any heteroskedastic DGP, there exists a homoskedastic DGP yielding equivalent behavior for 2SLS estimates and test statistics - see Andrews et al. (2019).

But in assessing acceptable first-stage F statistics in practice it is important to consider the impact of heteroskedasticity. In the *general* case of multiple instruments, as Andrews et al. (2019) note, it is inappropriate to use either a conventional or heteroskedasticity robust F-test to gauge instrument strength in non-homoskedastic settings. They suggest using the Olea and Pflueger (2013) effective first-stage F-statistic. However, as they point out, in the single instrument just-identified case, this reduces to the conventional robust F, and also coincides with the Kleibergen and Paap (2006) Wald statistic.

10. IS THERE A BETTER ALTERNATIVE TO 2SLS?

We have found that 2SLS performs very poorly in the C=2.3 ($F_{.05}$=10) case often viewed as a benchmark for acceptably strong instruments. It exhibits poor size and power properties, and in many plausible cases it is likely to underperform OLS, in that $\hat{\beta}_{2SLS}$ is likely to be further from the true value than $\hat{\beta}_{OLS}$. In this Section we consider three alternatives to 2SLS and ask whether they perform better when instruments are weak.

2SLS can be interpreted as IV using $z_i\hat{\pi}$ as the instrument for x_i, where $\hat{\pi}$ is obtained from OLS regression of x on z. Obviously $\hat{\pi}$ tends to be greater in samples where $\widehat{cov}(z, e)$ is greater, and this has an unfortunate consequence: For an individual observation i we have that $cov(z_i\hat{\pi}, e_i) > 0$, because a *ceteris paribus* increase in $z_i e_i$ drives up $\hat{\pi}$. If $\rho > 0$ this means $cov(z_i\hat{\pi}, u_i) > 0$, so the instrument is positively correlated with the structural error, which biases the 2SLS median towards OLS.[16]

Phillips and Hale (1977) noted this phenomenon, and suggested an alternative IV estimator using $z_i\hat{\pi}_{-i}$ as the instrument for x_i, where $\hat{\pi}_{-i}$ is obtained from OLS regression of x on z *excluding* observation i. This approach, later called "jackknife IV" (JIVE), breaks the correlation between $z_i\hat{\pi}$ and u_i. We report results using JIVE in Figure 12.

In the case of C=2.30 ($F_{.05}$=10) the JIVE estimator causes us to reject H$_0$: $\beta = 0$ via a two-tailed 5% t-test a striking 29% of the time, and *all* the rejections are positive. In Sections 4-6 we emphasized the problem that 2SLS is much more likely to judge estimates significant if they are shifted in the direct of the OLS bias. Here we see that JIVE makes this problem much worse. The negative association between $se(\hat{\beta}_{JIVE})$ and $\hat{\beta}_{JIVE}$ imparts positive $\hat{\beta}_{JIVE}$ estimates with spuriously high precision.

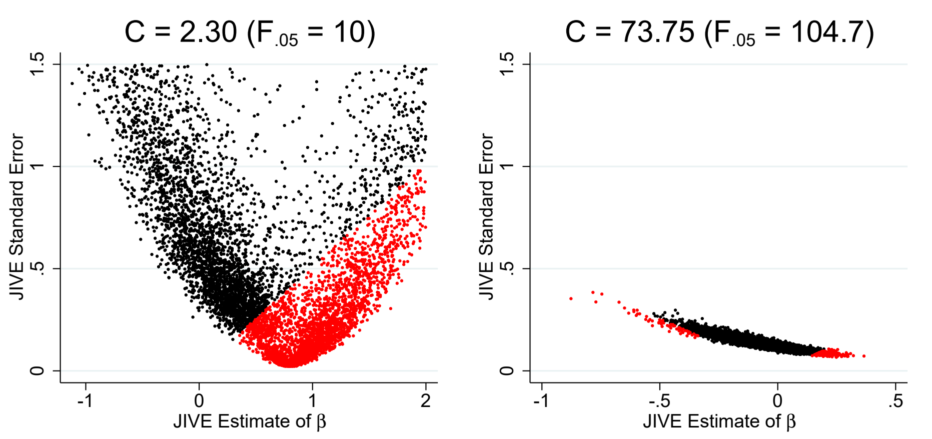
JIVE performs worse than 2SLS because the alternative instrument $z_i\hat{\pi}_{-i}$ has a smaller correlation with x than $z_i\hat{\pi}$, making the weak instrument problem worse. This has especially dire consequences if the instrument z is weak to begin with.[17] In the right panel of Figure 12 we see that in the relatively strong instrument case of C=73.75 ($F_{.05}$=104.7) JIVE does somewhat better (i.e, 3.84% rejections of which 83% are positive), but this is not comforting for an estimator designed for use with weak instruments.

The "k-class" estimators modify 2SLS by implementing IV using $kz_i\hat{\pi} + (1-k)x_i$ as the instrument for x_i. Obviously 2SLS uses $k = 1$. One important alternative to 2SLS is the Fuller (1977) estimator that uses $k = 1 - 1/N$ (in the one instrument case), thus leaving in a small part of x_i. This "stabilizes" the estimator. Hence, in contrast to 2SLS, the mean and variance of Fuller's estimator exist.

[16] The covariance of $z_i\hat{\pi}$ and u_i is of order $1/N$, as the influence of observation i on $\hat{\pi}$ vanishes as N grows large, but in finite samples it contributes to bias in the 2SLS median. Similarly, if instruments are strong in the sense discussed in Section 2, so we can be confident that $|\pi\widehat{Var}(z)| \gg |\widehat{cov}(z, e)|$, then the influence of any particular e_i on $\hat{\pi}$ becomes negligible.

[17] In fact, in the runs in the left panel of Figure 12, $\widehat{cov}(z, x)$ is always positive, as we would hope given that $cov(z, x) > 0$ in the population. But $\widehat{cov}(z\hat{\pi}_{-i}, x)$ has an incorrect negative sign 30% of the time!

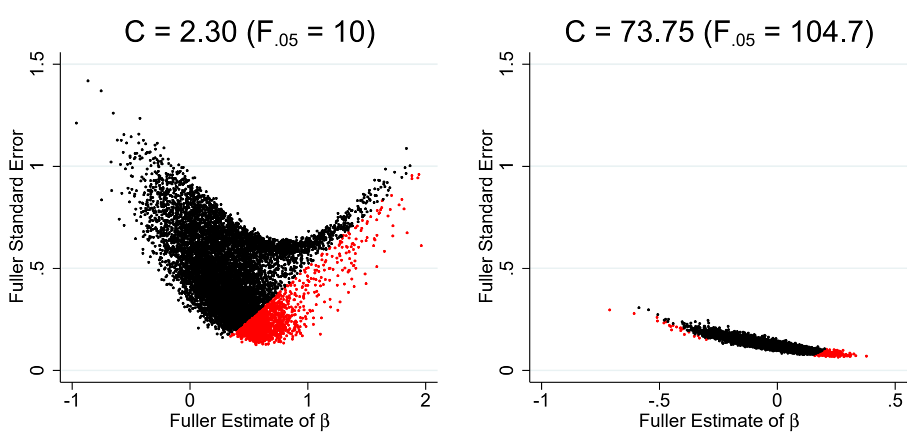
Figure 12. Standard Error of $\hat{\beta}_{JIVE}$ plotted against $\hat{\beta}_{JIVE}$ itself ($\rho = 0.80$)



Note: Runs with standard error > 1.5 not shown. Red dots indicate $H_0 : \beta = 0$ rejected at 5% level.

We report results using the Fuller estimator in Figure 13. First, consider the case of $\rho = 0.80$ and C=2.30 ($F_{.05}$=10). Comparison with Figure 2 shows that Fuller estimates and standard errors are substantially less dispersed than 2SLS. Fuller causes us to reject H_0: $\beta = 0$ via a two-tailed 5% t-test 15.4% of the time, compared to 10% for 2SLS, so its size distortion is greater. Just as with 2SLS, *all* the rejections occur when $\hat{\beta}_{Full}$ is positive: the negative covariance between $se(\hat{\beta}_{Full})$ and $\hat{\beta}_{Full}$ imparts spuriously high precision to Fuller estimates that are most shifted in the direction of the OLS bias.

Figure 13. Standard Error of $\hat{\beta}_{Full}$ plotted against $\hat{\beta}_{Full}$ itself ($\rho = 0.80$)



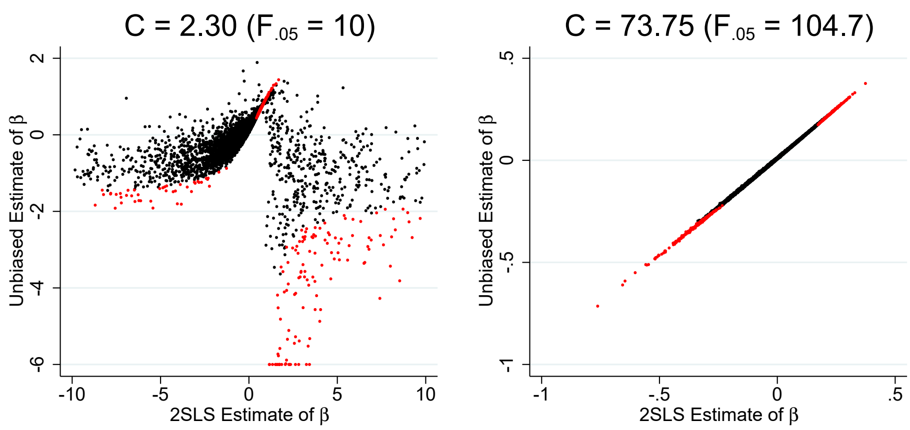
Note: Runs with standard error > 1.5 not shown. Red dots indicate $H_0 : \beta = 0$ rejected at 5% level.

In the right panel Figure 13 we report results for the relatively strong instrument case of C=73.75 ($F_{.05}$=104.7). Comparison with the right panel of Figure 2 reveals that 2SLS and Fuller estimates are very similar in this case.[18]

The third and final alternative we consider is the unbiased estimator of β proposed in Andrews and Armstrong (2017). To understand their approach, consider the reduced form regression of y on z, $y = z\beta\pi + (\beta e + u) = z\xi + v$ where $\xi = \beta\pi$. Obviously $\beta = \xi/\pi$. By analogy, the 2SLS estimator can be calculated by taking the ratio $\hat{\beta}_{2SLS} = \hat{\xi}/\hat{\pi}$ where $\hat{\pi}$ is the first-stage estimate of π. The problematic properties of 2SLS that arise when instruments are weak (see Section 2) may be understood as arising because $\hat{\pi}$ appears in the denominator of this ratio. Estimates of ratios have poor properties if the denominator is noisy, including the fact that $1/\hat{\pi}$ is not an unbiased estimator of $1/\pi$.

The Andrews-Armstrong idea is that an unbiased estimator of β can be obtained if one can construct an unbiased estimator of $1/\pi$. Their approach requires the researcher to be certain that $\pi > 0$, which is plausible in many applications. In that case, an unbiased estimate of $1/\pi$ can be constructed by taking $1/\hat{\pi}^*$, where $\hat{\pi}^* = \sigma_2\phi(\hat{\pi}/\sigma_2)/(1-\Phi(\hat{\pi}/\sigma_2))$. Here σ_2 is the standard deviation of $\hat{\pi}$, and ϕ and Φ are the standard normal density and cdf. Given $\hat{\pi}^*$ it is simple to construct an unbiased estimator that we denote $\hat{\beta}_U$.[19]

Figure 14. Andrews-Armstrong $\hat{\beta}_U$ plotted against $\hat{\beta}_{2SLS}$ ($\rho = 0.80$)



Note: Runs where $\hat{\beta}_U < -6$ were censored to -6. Red dots indicate $H_0 : \beta = 0$ is rejected at the 5% level using the Anderson-Rubin statistic.

It is important to understand how the first-stage estimate $\hat{\pi}$ is modified by this transformation. Note $\phi/(1-\Phi)$ is the inverse Mills ratio, so $\hat{\pi}^* = E(x|x > \hat{\pi})$ where $x \sim N(0, \sigma_2^2)$. Thus $\hat{\pi}^*$ is positive by construction, and always larger than $\hat{\pi}$. If $\hat{\pi}$ is negative, then $\hat{\pi}^*$ is a small positive number. As $\hat{\pi}$ grows large $\hat{\pi}^*$ approaches $\hat{\pi}$ from above.

[18] The Fuller estimator rejects at a 5.43% rate, but 96% of the rejections are when $\hat{\beta}_{Full} > 0$. So just as with 2SLS, severe one-tailed t-test size distortions persist even with quite strong instruments.

[19] The unbiased estimator of β is simply $\hat{\beta}_U = (\hat{\delta}/\hat{\pi}^*)+(\sigma_{12}/\sigma_2^2)$, where $\hat{\delta}$ is defined as $\hat{\xi}-(\sigma_{12}/\sigma_2^2)\hat{\pi}$. This works because $E(\hat{\xi}|\hat{\pi}) = \beta\pi + (\sigma_{12}/\sigma_2^2)(\hat{\pi} - \pi)$ where σ_{12} is the covariance between $\hat{\pi}$ and $\hat{\xi}$. Therefore $E(\hat{\delta}|\hat{\pi}) = \beta\pi - (\sigma_{12}/\sigma_2^2)\pi$, which is independent of $\hat{\pi}$. Hence we can write $E(\hat{\delta}/\hat{\pi}^*) = \beta - (\sigma_{12}/\sigma_2^2)$, from which it follows that $E\hat{\beta}_U = \beta$. To obtain a feasible estimator replace σ_{12} and σ_2 with their estimates.

We report results using $\hat{\beta}_U$ in Figure 14, where we plot the $\hat{\beta}_U$ against $\hat{\beta}_{2SLS}$. The red dots indicate estimates that are significant at the 5% level according to the Anderson-Rubin test. In the strong instrument case in the right panel, $\hat{\beta}_U$ and $\hat{\beta}_{2SLS}$ are nearly identical. The AR test rejects H_0: $\beta = 0$ at close to the correct 5% rate, and there is an fairly even balance between rejections at positive vs. negative estimates of β.

The weak instrument case in the left panel is more interesting. Of course the AR test rejects H_0: $\beta = 0$ at close to the correct 5% rate. But for 2SLS the rejections are highly asymmetric, as 85% occur when $\hat{\beta}_{2SLS} > 0$. So using AR does not avoid the asymmetry that most rejections occur at positive values, which is the direction of the OLS bias. As we see in the left panel of Figure 14, the unbiased estimator solves this problem, as it generates only 54% positive rejections. It achieves this in an interesting way: Specifically, it flips a large fraction of the significant 2SLS estimates from positive to negative. This occurs in cases where $\hat{\pi}$ is negative, so that $\hat{\pi}^*$ is positive (consistent with the prior).

Finally, Table 7 repeats the analysis of Section 9, by asking how often the alternative estimators outperform OLS, in the sense that $|\hat{\beta} - \beta| < |\hat{\beta}_{OLS} - \beta|$. In the $F_{.05} = 50$ case the Fuller and Unbiased estimators perform about the same as 2SLS, with JIVE slightly worse, so at this level of instrument strength there is little to be gained by using alternatives to 2SLS. When instruments are weaker ($F_{.05} = 10$ or 23) a clear ranking is evident with Fuller doing best, followed by Unbiased, then 2SLS and then JIVE. For instance, given a uniform prior $\rho \in [0, 0.45]$, which we have argued is plausible in the classic application of estimating returns to education, the probability that 2SLS outperforms OLS is only 26% when $F_{.05} = 10$. For Fuller the figure is 40% and for Unbiased it is 32%. So while these alternatives outperform 2SLS, their performance can hardly be considered acceptable in any absolute sense when instruments are weak.

Table 7. Probability of Estimators Outperforming OLS (%)

Estimator	Prior Expectation of ρ: 0 to 1	0 to 0.45	0.35 to 0.45	0.5 to 1
$C = 2.30$, $F_{5\%Crit} = 10.00$				
2SLS	48	26	45	69
JIVE	32	22	33	41
Fuller	65	40	65	87
Unbiased	60	32	56	84
$C = 10.00$, $F_{5\%Crit} = 23.10$				
2SLS	70	47	77	91
JIVE	57	38	62	74
Fuller	76	51	84	98
Unbiased	75	50	82	96
$C = 29.44$, $F_{5\%Crit} = 50.00$				
2SLS	80	60	92	98
JIVE	76	56	86	93
Fuller	82	62	94	99
Unbiased	82	62	94	99

Note: We report the frequency of $|\hat{\beta} - \beta| < |\hat{\beta}_{OLS} - \beta|$ across Monte Carlo replications, averaged across all possible values of ρ under a uniform prior that ρ falls in the indicated range.

The story changes, however, if the endogeneity problem is more severe. Given a uniform prior $\rho \in [0.5, 1.0]$ the probability that 2SLS outperforms OLS is only 69% when $F_{.05} = 10$. For Fuller the figure is 87% and for Unbiased it is 84%. So these alternatives do offer substantially improved performance over 2SLS when endogeneity is severe.

In summary, these results reinforce our earlier conclusion that a first-stage F of at least 50 is required to give reasonable confidence that any of the IV estimators will outperform OLS at moderate levels of ρ. But these estimators do offer improvements in cases where instruments are weaker and endogeneity is severe. All of these alternative estimators should be used in conjunction with robust tests like Anderson-Rubin.

We have focused on the one instrument case, but the performance of 2SLS tends to deteriorate with multiple instruments. We will see this clearly in Section 11 that covers the over-identified case. The absolute performance of the alternatives (LIML, Fuller, JIVE, Unbiased) will also deteriorate with multiple instruments, but less so. Hence, even higher thresholds of instrument relevance are desirable with multiple instruments.

11. THE CASE OF MULTIPLE INSTRUMENTS

Finally, we consider the over-identified case with one endogenous variable and multiple instruments. In the interest of space we focus on the case of three instruments, the number required for the mean and variance of the 2SLS estimator to exist.

With K instruments the definition of the concentration parameter C is unchanged. But "true F" is now C/K and the first-stage sample $\hat{F}$ is $(N/K)\hat{Var}(z\pi)/\hat{\sigma}_e^2$ which has a non-central $F(K, N-1; C/K)$ distribution. Table 8 lists, in the $K = 3$ case, several different levels of C, the associated true F, and the 5% critical value of the $F(3, \infty; C/K)$ distribution to which we compare $\hat{F}$ to test if "true F" exceeds that value.

We continue to work with the model in equation (5), and we focus on the simple case where the three instruments are independently distributed $N(0, 1)$, and the π coefficients on the three instruments are equal (so each is equally strong). Table 8 then lists the value of π required to attain each level of the concentration parameter when N=1000.

Table 8. First Stage F Critical Values Required to Achieve Different Objectives

Concentration Parameter	True First-Stage F Statistic	Value of π	F critical value to reject C<c at 5%	Goal Achieved
6.90	2.30	0.0480	6.93	Matching π
13.01	4.34	0.0659	10.00	SS Rule of Thumb
40.91	13.64	0.1168	22.30	Size = 10%
110.55	36.85	0.1920	50.00	
360.26	120.09	0.3465	142.50	Size = 5%

Note: The instrument vector z is significant at the 5% level in the first stage if F>2.60.

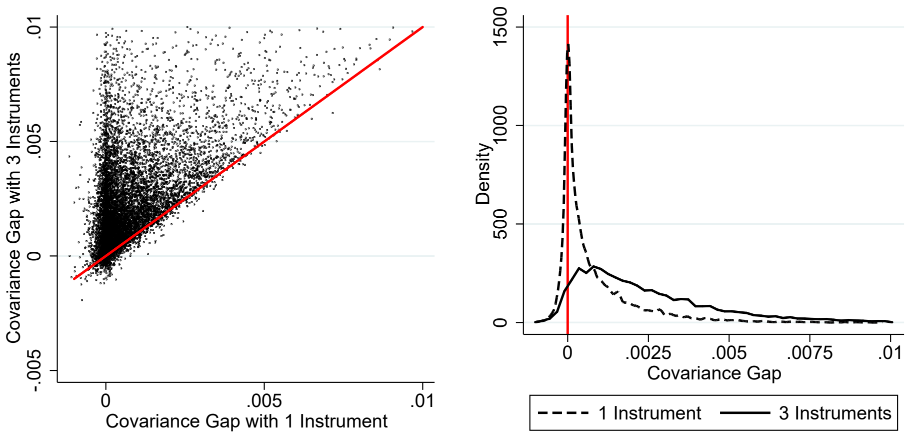
Recall from Table 1 that in the one instrument case π=0.048 gives a concentration parameter and "true F" of 2.3, and an associated 5% critical value of 10 for the $F(1, \infty; 2.3)$ distribution. Suppose now we have three equally strong instruments. As we see in Table 8 this triples C to 6.9, leaves "true F" unchanged at 2.3, and the 5% critical value of the $F(3, \infty; 2.3)$ distribution to test that "true F" is at least 2.3 is now 6.93.

One might think that three equally strong independent instruments are better than one, but it is actually a mixed bag. Recall 2SLS is IV using $z\hat{\pi}$ as the instrument for x, where $\hat{\pi}$ is obtained from OLS regression of x on z. Many problematic properties of 2SLS arise because the sample covariance of the feasible instrument $z\hat{\pi}$ with the structural error u tends to be greater than that of the optimal instrument $z\pi$, by virtue of how

OLS forms $\hat{\pi}$. And, unfortunately, the use of multiple instruments in the first stage of 2SLS tends to worsen the problem. This is because an instrument that happens to have a high sample covariance $\hat{cov}(z, e)$ with the first-stage error e will get a larger coefficient $\hat{\pi}$ in the first stage. This drives up the sample covariance $\hat{cov}(z\hat{\pi}, e)$. And if e and u are correlated (i.e., if we have endogeneity) this also drives up the magnitude of $\hat{cov}(z\hat{\pi}, u)$.

Figure 15 illustrates this problem. We first define the "covariance gap" as the difference $\hat{cov}(z\hat{\pi}, u) - \hat{cov}(z\pi, u)$. Using 10,000 artificial data sets generated from model (5) with $\rho = 0.5$ and N=1000, we calculate this covariance gap for the cases of 3 vs. 1 instrument. The left panel of Figure 13 shows how the covariance gap almost always increases, and often substantially, in the three instrument case. The right panel shows that the density of the covariance gap shifts sharply to the right. Thus, using three instruments greatly increases the problem of sample covariance between the instruments and the structural error, which will tend to bias the 2SLS estimate towards OLS.

Figure 15. Instrument Endogeneity in Samples with One vs. Three Instruments



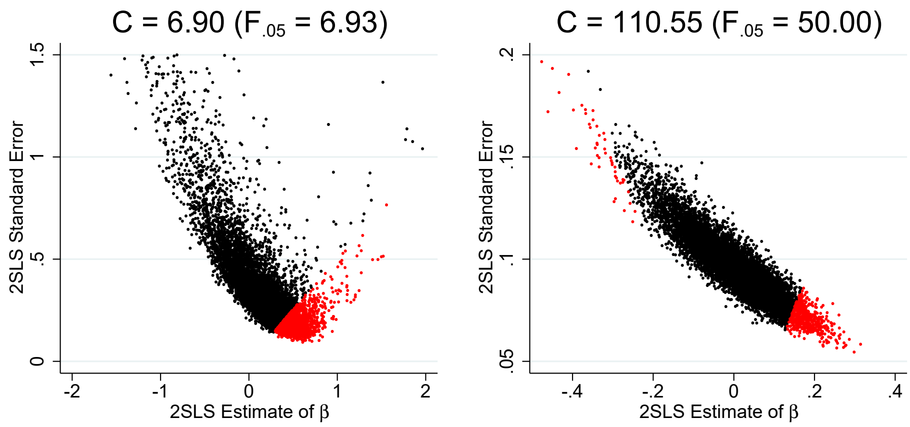
Note: We construct the "covariance gap" $\hat{cov}(z\hat{\pi}, u) - \hat{cov}(z\pi, u)$ for cases of 1 and 3 instruments with $\pi = 0.048$. We plot their joint distribution (left), and their marginal densities (right).

As a consequence of increased sample covariance $\hat{cov}(z\hat{\pi}, u)$, the association between 2SLS estimates and their standard errors gets even stronger in the multiple instrument case. This is shown in Figure 16. The left panel is comparable in all respects to the left panel of Figure 2, except now we add two equally strong independent instruments so K=3. This causes the Spearman r_s to increase in magnitude from -.576 to -.781. As the variance of the 2SLS estimator now exists, we can report a Pearson correlation of -0.547.

By comparing the left panels of Figures 2 and 16 we can see how adding two instruments affects size of the 2SLS t-test. The red dots again indicate cases where $\hat{\beta}_{2SLS}$ differs significantly from zero according to a two-tailed 5% t-test. With one instrument the size of the test was 10%, but now it is 19.9%, so the size distortion increases dramatically. As before, all rejections occur when $\hat{\beta}_{2SLS} > 0$. Due to covariance between 2SLS estimates and standard errors, only estimates shifted towards the OLS bias are ever significant.

The right panel of Figure 16 reports results for a strong instrument case of C=110.6 ($F_{.05}$=50). Here the the Spearman r_s between the 2SLS estimates and their standard errors is -.906, while the Pearson correlation is -.915. This illustrates our point from

Figure 16. $se(\hat{\beta}_{2SLS})$ plotted against $\hat{\beta}_{2SLS}$ with Three Instruments ($\rho = 0.80$)



Note: Runs with std. error > 1.5 *not shown. Red dots indicate* $H_0 : \beta = 0$ *rejected at 5% level.*

Section 5 that this strong association persists in strongly identified models. This, in turn, explains why 2SLS t-tests are unreliable even with strong identification. The 5% rejection rate of the two-tailed t-test is now 6%, so the size distortion is mostly eliminated. But fully 92% of those rejections occur when $\hat{\beta}_{2SLS} > 0$, so the asymmetry is still severe.

Figure 17 plots how size of two-tailed 5% t-tests of H_0:$\beta = 0$ depend on C and ρ. It is interesting to compare the case of C=2.3 in Figure 1 with C=6.9 in Figure 17. This corresponds to adding two new independent instruments of equal strength. Notice how in the 3 instrument case the rejection rate rises much more steeply with ρ, and it peaks at almost 30%, compared to only 13% in the one instrument case. Adding instruments worsens t-test size distortions by increasing the sample covariance between $z\hat{\pi}$ and u.

To achieve the Staiger-Stock rule of thumb ($F_{.05}$=10) in the three instrument case we need to increase π from 0.0480 to 0.0659, so we need three independent instruments that are each individually stronger than what we would require of a single instrument to achieve the same goal. Even then, as we see in Figure 17, the maximum rejection rate increases to almost 20%, compared to 13% in the single instrument case.

Stock and Yogo (2005) show that C=40.9 ($F_{.05}$=22.3) achieves a maximum rejection rate of 10% in the three instrument case. Figure 17 shows this is accurate. This corresponds to a π of 0.1168 on each of the three instruments in the first stage (see Table 8). But as we saw in Table 1, the same objective could be achieved using a single instrument with π=0.0760. Thus, if size distortion in two-tailed t-tests is one's primary concern, it is hard to justify using multiple instruments. This is consistent with Angrist and Pischke (2008)'s advice that applied researchers should choose their one best instrument.

A similar point emerges if we look at median bias. The left panel of Figure 18 shows how median bias varies with C and ρ in the three instrument case. For comparison, the red line shows the case of one instrument with $C = 2.3$ ($F_{.05}$=10). This can be compared to the blue line, which is the case where we add two additional equally strong independent instruments. Clearly this makes the median bias unambiguously worse.

It would be a mistake, however, to focus exclusively on median bias and size distortions of t-tests in assessing the performance of 2SLS. Efficiency and power considerations are

Figure 17. Rejection rate of $H_0 : \beta = 0$ using a 5% Two-tailed t-test (3 Instruments)

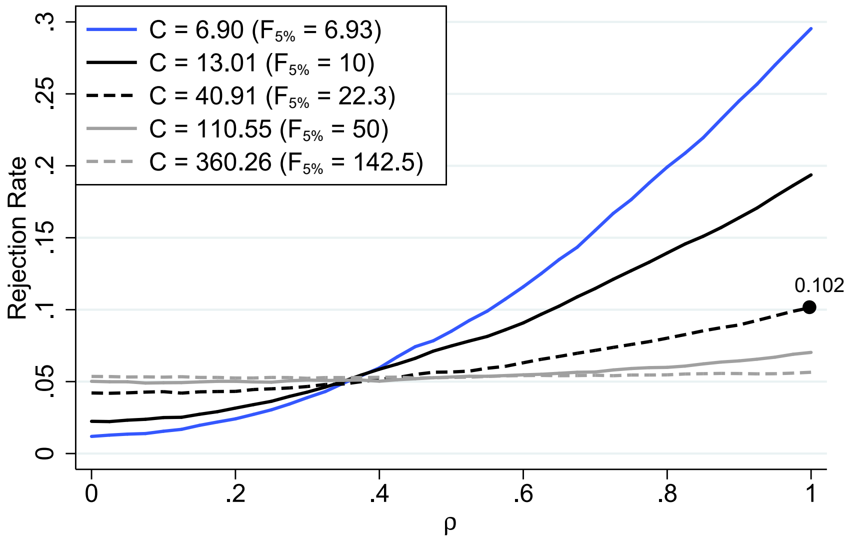
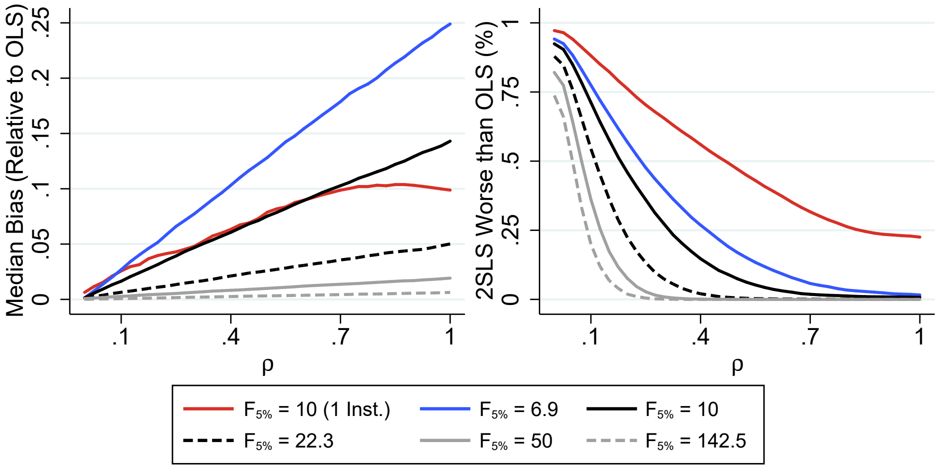


Figure 18. Performance of 2SLS Relative to OLS



Note: We plot median bias and proportion of Monte Carlo runs where $|\hat{\beta}_{2SLS} - \beta| > |\hat{\beta}_{OLS} - \beta|$.

also important, and robust tests are available. To explore efficiency, the right panel of Figure 18 shows how the probability of 2SLS performing worse than OLS varies with C ($F_{.05}$) and ρ in the three instrument case. We plot the proportion of simulated datasets where $|\hat{\beta}_{2SLS} - \beta| > |\hat{\beta}_{OLS} - \beta|$. For comparison, we also show (in red) the case of one instrument with $C = 2.3(F_{.05}{=}10)$. This may be usefully compared to the blue line, which is the case where we add two more equally strong independent instruments.

The addition of two equally strong independent instruments tremendously increases the probability that 2SLS will out-perform OLS. There is obviously a large efficiency gain from using the additional information. This gives a very different perspective on the potential efficacy of using multiple instruments.

These results naturally lead us to assess whether robust tests outperform the t-test in the multiple instrument case. For the three instrument case, Table 9 compares the t-test, the Anderson and Rubin (1949) test, Moreira (2003)'s conditional likelihood ratio test (CLR) and the Mills et al. (2014) ACT test.[20] We set ρ=0.80, and let the true β be 0, -0.3 or +0.3. Recall that $\beta = \pm 0.3$ correspond to fairly large effects, as they imply a one standard deviation change in x induces an 0.25 standard deviation change in y.

First consider the t-test. The top panel of Table 9 reports results when β=0. Clearly, the two-tailed t-test rejects the true null at far too high a rate in cases with C=6.9 ($F_{.05}$=6.93) or C=13 ($F_{.05}$=10). The size distortion in one-tailed t-tests is even greater, as all rejections occur when $\hat{\beta}_{2SLS} > 0$. In the middle panel, when β=-0.30, we see the t-test has essentially no power to detect a substantial true negative effect in these cases. Stunningly, if C=6.9, the t-test rejects H_0:β=0 only 2.5% of the time, and <u>all</u> of those rejections happen when $\hat{\beta}_{2SLS} > 0$ – i.e., we conclude β is positive when it is actually negative! Lastly, consider the bottom panel, where β=0.30. In the C=6.9 case the t-test rejects H_0:β=0 at a 46% rate. But this high value is not a good sign – it arises because the 2SLS standard errors are spuriously precise when $\hat{\beta}_{2SLS} > 0$.

Next we consider the AR test. It performs much better than the t-test, but it does not behave as well as it did in the single instrument case. When the true β=0 (top panel) the AR test of course rejects H_0:β=0 at approximately the correct 5% rate (deviating only due to sampling variation). But in the C=6.9 ($F_{.05}$=6.93) and C=13 ($F_{.05}$=10) cases nearly all of those rejections occur when $\hat{\beta}_{2SLS} > 0$.[21] Thus, the AR test is more likely to judge 2SLS estimates significant when they are shifted in the direction of the OLS bias. This problem is much less severe than it is for the t-test, but it is still of concern.

With one instrument we found the power asymmetry in the AR test vanished very quickly as instrument strength increased, but in the three instrument case it vanishes more slowly. As we discussed in Section 7, the source of the power asymmetry in the AR test is the positive covariance between $\rho\widehat{cov}(z,u)$ and $\hat{\beta}_{2SLS}$. A large $\rho\widehat{cov}(z,u)$ also generates a large value of the AR test. So if $\rho > 0$, the AR test and $\hat{\beta}_{2SLS}$ have a positive covariance. Adding instruments increases this covariance, as we saw in Figure 15.

Even with strong instruments the AR test has better power to detect true negative effects than true positive effects. For instance, when C=111 ($F_{.05}$=50) the AR test rejects H_0:β=0 at a 93.6% rate when the true β is negative, but only a 54% rate when the true β is positive. This is another manifestation of the covariance phenomenon: Geometrically, if true beta is positive, $\hat{\beta}_{2SLS}$ values that lie between zero and the true β (against the direction of the OLS bias) will correspond to spuriously low AR test values, so they are less likely to be judged significantly greater than zero. These limitations of the AR test in the multiple instrument case lead us now to consider other robust tests.

Next we consider the CLR test. Its performance is clearly superior to AR. When the true β=0 (top panel) the CLR test of course rejects H_0:β=0 at approximately the correct 5% rate. In the C=6.9 ($F_{.05}$=6.93) case 73% of those rejections occur when $\hat{\beta}_{2SLS} > 0$, so the CLR test does exhibit power asymmetry. But this vanishes rather quickly as

[20] In the single endogenous variable exactly-identified case, the AR test is equivalent to the conditional likelihood ratio (CLR) test (Moreira 2003) and the Langrange multiplier (LM) test (Kleibergen 2002). In more general settings these tests differ. We explain the ACT test, which adjusts t-test critical values based on instrument strength and the reduced form error covariance, in Appendix B.

[21] The power asymmetry of the AR test is also quite apparent when the true β=-0.3 (middle panel). With C=6.9 AR rejects the false null H_0:β=0 at a 12% rate, but a substantial fraction of those rejections (39%) occur when $\hat{\beta}_{2SLS} > 0$. So we often conclude β is positive when it is actually negative.

Table 9. Rejection Rates and Power With Three Instruments ($\rho = 0.8$) (%)

C	6.90	13.01	40.91	110.55	360.26
$F_{5\%Crit}$	6.93	10.00	22.30	50.00	142.50
$\beta = 0$					
T-Statistic					
Total Rejection Rate	0.198	0.134	0.082	0.060	0.054
When $\hat{\beta} > 0$	0.198	0.134	0.082	0.055	0.040
AR Test					
Total Rejection Rate	0.051	0.051	0.051	0.051	0.051
When $\hat{\beta} > 0$	0.048	0.042	0.036	0.032	0.029
ACT Test					
Total Rejection Rate	0.051	0.051	0.051	0.051	0.050
When $\hat{\beta} > 0$	0.026	0.025	0.025	0.024	0.024
CLR Test					
Total Rejection Rate	0.050	0.049	0.049	0.048	0.049
When $\hat{\beta} > 0$	0.036	0.028	0.023	0.023	0.024
$\beta = -0.3$					
T-Statistic					
Total Rejection Rate	0.025	0.007	0.354	0.949	1.000
When $\hat{\beta} < 0$	0.000	0.001	0.354	0.949	1.000
AR Test					
Total Rejection Rate	0.120	0.189	0.521	0.936	1.000
When $\hat{\beta} < 0$	0.074	0.166	0.519	0.936	1.000
ACT Test					
Total Rejection Rate	0.116	0.229	0.659	0.978	1.000
When $\hat{\beta} < 0$	0.113	0.228	0.659	0.978	1.000
CLR Test					
Total Rejection Rate	0.162	0.268	0.683	0.980	1.000
When $\hat{\beta} < 0$	0.140	0.263	0.683	0.980	1.000
$\beta = 0.3$					
T-Statistic					
Total Rejection Rate	0.460	0.453	0.583	0.838	0.997
AR Test					
Total Rejection Rate	0.075	0.097	0.221	0.540	0.977
ACT Test					
Total Rejection Rate	0.075	0.109	0.311	0.695	0.992
CLR Test					
Total Rejection Rate	0.097	0.138	0.327	0.708	0.993

Note: The table reports the frequency of rejecting the null hypothesis H_0: $\beta = 0$.

instrument strength increases. In the C=13 ($F_{.05}$=10) case the proportion of positive rejections is already down to 57%. Like AR, the CLR test has more power to detect true negative effects than true positive effects, but the asymmetry is less severe. For example, In the case of C=41 ($F_{.05}$=22.3) the CLR test rejects H$_0$:β=0 at a 68.3% rate when the true β is negative, and a 32.7% rate when the true β is positive. These compare to rates of 52.1% and 22.1% for the AR test. Thus, the CLR test exhibits better power to detect both positive and negative departures from the null.

Finally, consider the ACT test. If β=0 it has (approxmately) the correct 5% size by construction. In contrast to the AR and CLR tests, it also has symmetric rejections on both sides of 0 even when instruments are weak. So if test size were one's only concern the ACT test would be recommended. However, as Table 9 reveals, when $\beta = \pm 0.3$ the ACT test has inferior power to the CLR test, particularly when instruments are weak.

We conclude that no case can be made for using 2SLS t-tests in the over-identified case. The AR test clearly outperforms the t-test. But both the CLR and ACT tests dominate the AR test in terms of power. Finlay and Magnusson (2009) provide a heteroskedasticity robust implementation of the CLR test in Stata, that will also invert the test to form confidence intervals. In Keane and Neal (2021) we compare the t, AR and CLR tests in an empirical application, finding results consistent with those described here.

12. CONCLUSION

We have examined the behavior of 2SLS given different levels of instrument strength, focusing primarily on the a basic *iid* normal environment. In that context, Staiger-Stock suggested the popular rule of thumb that the first-stage F should be at least 10 for 2SLS t-tests to give reliable results. And, in the case of a single instrument, Stock-Yogo showed that a first-stage F of 16.4 ensures maximal size distortion in two-tailed 2SLS t-tests is no more than 5%. However, we find 2SLS estimates and t-tests are very poorly behaved in environments characterized by first-stage F-statistics in the 10 to 16.4 range.

The problem is not the Stock-Yogo analysis itself, which is perfectly correct, but rather that its focus on maximal size distortions of two-tailed t-tests masks other problems. First, 2SLS has very low power for first-stage F in the 10 to 16.4 range deemed acceptable by conventional tests. Second, *the 2SLS estimator has the unfortunate property that it tends to generate standard errors that are artificially too low precisely when it generates estimates that are shifted most strongly in the direction of the OLS bias.* Consequently, nearly all significant 2SLS estimates are severely shifted towards OLS when instruments are weak. Surprisingly, this power asymmetry persists when instruments are very strong.

In fact, we find standard 2SLS t-tests have little power to detect true negative effects when the OLS bias is positive, even when instruments are quite "strong" by conventional standards. This is of great practical importance, as it means there is little chance of detecting negative program effects given positive selection on unobservables.

One consequence of the association between 2SLS estimates and their standard errors is that size distortions in one-tailed t-tests are far greater than in two-tailed tests. Very high levels of instrument strength are needed to reduce those size distortions to modest levels. For example, if the first-stage F meets the 104.7 threshold suggested by Lee et al. (2020), which eliminates maximal size distortion in two-tailed t-tests, the size distortions in one-tailed t-tests are still enormous. In fact, we find a first-stage F-threshold of about 10,000 is needed to eliminate size distortions in one-tailed 2SLS t-tests.

The literature seems to have overlooked the problem of power asymmetry in 2SLS t-tests. Applied researchers rarely use one-tailed tests as they expect two-tailed tests to be symmetric (so a two-tailed 5% test is equivalent to a one-tailed 2.5% test). But that is completely false with 2SLS: Even with moderately strong instruments almost all estimates judged significant by two-tailed 2SLS t-tests are shifted in the direction of the OLS bias, rather then symmetrically distributed around the true value.

The asymmetry in 2SLS t-tests is highly relevant for applied work. Consider the classic problem of estimating the effect of education on wages. The usual concern is that unmeasured ability biases the OLS education coefficient upward. Our results imply that if the OLS bias is indeed positive, then larger positive 2SLS estimates of the effect of education on wages will *spuriously* appear more precise. This will naturally bias researchers towards exaggerating the effect of education.

We find the Anderson-Rubin (AR) test is far less susceptible to this power asymmetry problem that the t-test. That is, when instruments are weak the AR statistic tends to be greater when $\hat{\beta}_{2SLS}$ is shifted in the direction of the OLS bias. So AR over-rejects H_0: $\beta = 0$ when $\hat{\beta}_{2SLS}$ is shifted towards $E(\hat{\beta}_{OLS})$. Fortunately, however, the problem with the AR test becomes negligible at a much lower first-stage F threshold than for the t-test. Thus, we advise using the AR test even if the first-stage F is in the thousands.

We present an application to estimating "excess sensitivity" of consumption to income using PSID data to assess relative performance of AR and t-tests in a realistic setting. In this example the first-stage F-statistic is modestly above the threshold of 10. We show that in this context the AR test is clearly superior to the t-test in terms of both power and size. As Andrews et al. (2019) note, the AR test is robust to weak instrument problems and has (weakly) greater power than any alternative test in just-identified models. Given the weight of the empirical and theoretical evidence, it is clear the AR test should be widely adopted in lieu of the t-test even when instruments are strong.

Going beyond the focus on test statistics, we argue that a limitation of most prior work on weak instruments is that the quality of 2SLS estimates is evaluated in isolation, asking how strong instruments must be for 2SLS itself to exhibit acceptable statistical properties. In practice applied researchers expect 2SLS to give better results than OLS. So an alternative is to ask "How strong must instruments be for 2SLS to give more reliable results than OLS?" Given commonly used thresholds for testing weak instruments, we find probabilities that 2SLS will perform worse than OLS are substantial. For example, given a first stage F of 10, and given a uniform prior on the degree of the endogeneity problem, we calculate a 52% probability that 2SLS will generate an estimate of β further from the truth than OLS. But if the first-stage F is 50 this figure drops to only 17%.

Given these results, we advise applied researchers to think seriously about reasonable priors on the extent of endogeneity before assessing first-stage F thresholds. We give practical guidance on how to do this. For example, in the classic example of estimating the effect of education on wages, we show that a first-stage F threshold of 50 or better is required to have high confidence that 2SLS will outperform OLS. In cases where such a threshold cannot be met, the use of OLS combined with a serious attempt to control for sources of endogeneity may be a superior research strategy to reliance on IV.

We also evaluate alternatives to 2SLS, including the Fuller and JIVE estimators and the Unbiased estimator of Andrews and Armstrong (2017). If first stage F is 50 then 2SLS, Fuller and Unbiased behave very similarly, while JIVE is inferior. If the first stage F is lower and the level of endogeneity is moderate then none of these estimators is likely to outperform OLS. The Fuller and Unbiased estimators offer significant improvements over 2SLS (and OLS) when endogeneity is severe and instruments are weak.

Finally, we consider the over-identified case with a single endogenous variable. General conclusions carry over from the exactly identified case. In fact, the use of multiple instruments increases the covariance between 2SLS estimates and their standard errors. This makes it more essential to use robust tests like AR, ACT and the conditional likelihood ratio (CLR) test in lieu of the t-test, even when instruments are strong. The CLR test dominates the others in terms of power, so it appears to be the preferred approach in over-identified case, although ACT has more accurate size when instruments are weak. No case can be made for using 2SLS t-tests in either exact or over-identified models.

In conclusion, we note that recent papers by Andrews et al. (2019) and Young (2020) have emphasized that 2SLS can suffer from low power and size distortions in environments with heteroskedastic and/or clustered errors, even if conventional F tests appear acceptable. We complement that work by showing how similar problems may arise even in *iid* normal settings when instruments are acceptably strong by conventional standards.

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APPENDICES (FOR ONLINE PUBLICATION)

A. ANALYTICAL POWER FUNCTIONS OF THE AR AND T-TESTS

Consider the following just-identified *iid*-normal linear IV model:

$$\begin{gathered} y_i = \beta x_i + u_i \\ x_i = \pi z_i + e_i \quad \text{where} \quad e_i = \rho u_i + \sqrt{1-\rho^2}\eta_i \\ u_i \sim iid\, N(0,1), \eta_i \sim iid\, N(0,1), z_i \sim iid\, N(0,1) \end{gathered} \tag{A1}$$

The power of both the AR and t-tests depends on three parameters: the true β, the degree of endogeneity ρ, and the population t-statistic on z in the first-stage regression, which we denote λ (= square root of population F). The power of the AR test (i.e., rate of rejecting H_0:β=0 as a function of the true β) is simply:

$$Power_{AR} = \Phi(\lambda D - z_{1-\alpha/2}) + \Phi(-z_{1-\alpha/2} - \lambda D) \tag{A2}$$

where Φ is the standard normal cdf, $D = \beta/\sqrt{Var(v)}$ where $v = \beta e + u$ is the reduced form error with $Var(v) = 1 + 2\rho\beta + \beta^2$, and $z_{1-\alpha/2}$, and $z_{1-\alpha/2}$ is the $1 - \alpha/2$ quantile of the standard normal distribution. Below we set $\alpha = 0.05$.

To obtain the power function of the of the t-test we follow the analysis in Stock and Yogo (2005), Lee et al. (2020) and Angrist and Kolesár (2021). The power of the two-tailed 2SLS t-test is given by the integral:

$$\begin{aligned} &Power_t = \\ &\int_{-\infty}^{\infty} \left(\mathbb{I}\{t^2 \geq (1-\rho_0^2)z_{1-\alpha/2}^2\} f(t, D, \lambda, \rho_0) + \mathbb{I}\{t^2 \geq z_{1-\alpha/2}^2\}\right) \phi(t-\lambda) dt \end{aligned} \tag{A3}$$

where ϕ is the standard normal density, ρ_0 is the correlation of the reduced form errors, given by $\rho_0 = corr(\beta e + u, e) = (\rho + \beta)/\sqrt{1 + 2\rho\beta + \beta^2}$, and:

$$f(t, D, \lambda, \rho_0) = \Phi\left(\frac{a_2 - \lambda D - \rho_0(t-\lambda)}{\sqrt{1-\rho_0^2}}\right) - \Phi\left(\frac{a_1 - \lambda D - \rho_0(t-\lambda)}{\sqrt{1-\rho_0^2}}\right), \tag{A4}$$

$$a_1 = \frac{\rho_0 z_{1-\alpha/2}^2 t - |t| z_{1-\alpha/2}\sqrt{t^2 - (1-\rho_0^2)z_{1-\alpha/2}^2}}{z_{1-\alpha/2}^2 - t^2},$$

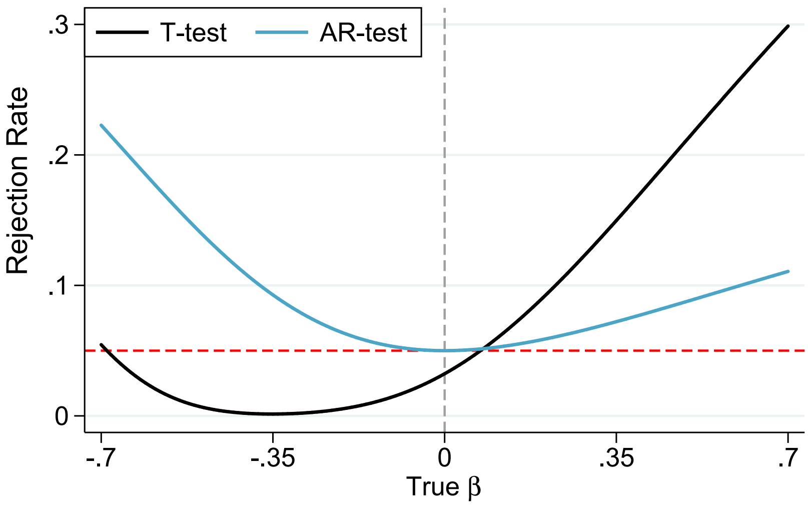
$$a_2 = \frac{\rho_0 z_{1-\alpha/2}^2 t + |t| z_{1-\alpha/2}\sqrt{t^2 - (1-\rho_0^2)z_{1-\alpha/2}^2}}{z_{1-\alpha/2}^2 - t^2}.$$

The integral in (A3) must be evaluated numerically. Notice that power of both tests depends on ρ, but its influence on t is much greater (as the simulations below illustrate).

A.1. Power of the AR vs t-test

We now present an example power comparison between the AR and t-test. Consider the case of C=2.3. To have 95% confidence that C is at least 2.3 we need $\hat{F} \geq 10$. This corresponds to the Staiger-Stock rule of thumb for an acceptably strong instrument, so this case is particularly interesting. We set $\beta = 0$ and $\rho = 0.5$, so the OLS bias is in the positive direction, $E(\hat{\beta}_{OLS}) = 0.5$. The results are shown in Figure A1.

Figure A1. Power of the T-Test vs. AR-Test when $F = 2.3$ ($\rho = 0.5$)



The severe power asymmetry of the t-test is evident in Figure A1. Because the OLS bias is positive ($\rho = 0.50$), the t-test has little power to detect a wide range of true negative β values. In fact, the t-test has power less than size for true effects in the 0 to -0.70 range, so it is uninformative over that range. Recall that β in equation (5) is roughly the std. dev. change in y induced by a one std. dev. change in x, so -0.70 would be a very large negative effect in most empirical applications. Thus, Figure A1 illustrates the t-test's poor power to detect a true β opposite in sign to the OLS bias, even if instrument strength satisfies the widely used first-stage $\hat{F}$ of 10 rule.

Figure A1 also illustrates the superior power properties of the AR test: First, the AR test is unbiased: It's power is appropriately minimized when the true β is 0 – in contrast to the t-test whose power is minimized when true β is roughly -0.35. Second, the power of the AR test is far superior when the true β is negative. For example, it reaches about 23% when true β is -0.70, compared to only 5% for the t-test. Notably, the power of both tests is rather uninspiring at this level of instrument strength, which is why we call for a higher standard of instrument strength in empirical practice – see Section 9.

Third, the AR test has correct size: It's power evaluated at β=0 is exactly 5%, compared to 3% for the the t-test. The weak IV literature tests for thresholds of instrument strength so that size *inflation* is not a problem.[22] It implicitly assumes it is acceptable if size is too small, as then the test is "conservative." But it seems odd to call the t-test conservative if it has no power to detect a large range of true negative effects.

Another notable feature of Figure A1 is how the t-test appears to have higher power than AR for positive values of β. This is the flip side of the power asymmetry problem: In samples where the 2SLS estimate is shifted in the direction of the OLS bias (here positive) the 2SLS standard error is spuriously small, which inflates t-test power. This is not a desirable property, as the standard error exaggerates the precision of the estimate in such cases. Again, it seems odd to call the t-test "conservative" when it has inflated power to detect positive effects and essentially no power to detect large negative effects.

[22]As we discussed in Sections 3-4, weak IV tests focus on worst-case size distortion. This occurs when endogeneity is severe, which leads to size inflation.

B. THE CONDITIONAL T-TEST APPROACH

It is well known that 2SLS t-tests generate misleading results when instruments are weak. Furthermore, as we saw in Sections 4-8, t-tests perform poorly even when instruments are quite strong by conventional standards. The AR test is a far less susceptible to these problems, making it much more reliable than the t-test in the single instrument case. In this section we explain another alternative to the conventional t-test that was proposed by Mills et al. (2014). This is known as the "conditional t-test" approach.

Critical values for a conventional t-test rely on the assumption that the test statistic is distributed $N(0,1)$ under the null. In fact the 2SLS t-test is highly non-normal and, due to the association between 2SLS estimates and standard errors, it is highly asymmetric, even when instruments are strong. The basic idea of a conditional t-test is to adjust the critical values of the 2SLS t-test to take this non-normality and asymmetry into account, thus obtaining a test with correct size – and perhaps better power properties as well.

As we saw in Figure 1, the size of 2SLS t-tests using standard critical values depends on F and ρ. However, it is possible to invert this relationship to find appropriate critical values *conditional* on measures of instrument strength and the error covariance structure such that the t-test has the correct size. As Mills et al. (2014) note however, there is no known closed form solution for this inversion, so these critical values must be obtained by simulating the conditional distribution of the 2SLS t-test. Fortunately this is a simple process, which we explain in detail in Appendix D.[23]

Table B1 shows summary statistics of critical values simulated using the DGP in (5) with ρ=0.80. For each level of C we report the median and standard deviation of the simulated critical values.[24] For example, if C=2.30 ($F_{.05}$=10), corresponding to the Staiger-Stock rule of thumb, median critical values for 2.5% left and right-tailed t-tests are -0.443 and 3.115, respectively. Using these critical values, one-tailed t-tests have *approximately* the correct 2.5% size – where the approximation arises because the critical values are simulated. The large deviation of these conditional t-test critical values from the usual values of $\pm$1.96 illustrates the extreme power asymmetry generated by the negative association between 2SLS estimates and standard errors.

As Mills et al. (2014) note, left and right-tail conditional critical values can be used in conjunction to form two-tailed conditional t-tests with approximately correct size. For example, we can combine the 2.5% left and right-tail critical values to form a two-tailed t-test with approximate size of 5%. Figure B2 presents a graphical illustration of how this asymmetric two-tailed conditional t-test (henceforth "ACT") works. The left panel shows results for 10,000 simulated datasets with C=2.30 and ρ=0.80. As before, we plot $SE(\hat{\beta}_{2SLS})$ against $\hat{\beta}_{2SLS}$. The red dots indicate cases where we reject the true null H_0:β=0 because the ratio of $\hat{\beta}_{2SLS}$ to the conditional critical value exceeds one. The ACT test achieves an overall 5.3% rejection rate, with a slight asymmetry of 2.8% negative and 2.6% positive rejections.

[23] We are grateful to Marcelo Moreira for providing Matlab code which does this calculation efficiently. But we have corrected some errors in that code, which has an important impact on results. As a consequence, the results presented in this section differ substantially from those reported in earlier drafts of this paper.

[24] The construction of Table B1 requires running a simulation within a simulation. For each dataset drawn from our DGP, we obtain estimates of instrument strength and the error covariance structure. Then, conditional on those estimates, we simulate the conditional distribution of the t-statistic using the algorithm in Appendix D. Hence, below each median critical value, we report in parenthesis the standard deviation of the critical values constructed under each true $C(F_{.05})$ scenario.

Table B1. Critical Values for Conditional One-Tailed t-tests, $\rho = 0.80$

	1%	2.5%	5%	95%	97.5%	99%
$C = 2.3$	-0.444	-0.443	-0.438	2.564	3.115	3.751
	(0.178)	(0.178)	(0.178)	(0.147)	(0.147)	(0.157)
$C = 10$	-0.925	-0.913	-0.884	2.236	2.762	3.393
	(0.178)	(0.167)	(0.146)	(0.102)	(0.128)	(0.157)
$C = 73.75$	-1.795	-1.585	-1.382	1.885	2.298	2.795
	(0.049)	(0.035)	(0.026)	(0.033)	(0.044)	(0.062)
$C = 336.3$	-2.084	-1.788	-1.524	1.760	2.123	2.554
	(0.032)	(0.024)	(0.019)	(0.025)	(0.032)	(0.046)
$C = 1,000$	-2.187	-1.861	-1.575	1.712	2.055	2.460
	(0.033)	(0.024)	(0.020)	(0.023)	(0.030)	(0.042)
$C = 10,000$	-2.283	-1.929	-1.622	1.666	1.990	2.369
	(0.036)	(0.026)	(0.021)	(0.022)	(0.028)	(0.038)

Note: The standard deviations in parentheses are across 10,000 simulations.

We have observed that standard 2SLS t-tests have little power to detect true negative effects when the OLS bias is positive, even when instruments are "strong" by conventional standards (e.g., $\hat{F} >10$). This is of great practical importance, as it means there is little chance of detecting negative program effects given positive selection on unobservables. We see here that the ACT addresses this problem by using a very "lenient" critical value in the left tail (in this case approximately -0.443 instead of -1.96).[25] Applied researchers may find it odd to adopt such a "weak" standard, but, given that association between 2SLS estimates and standard errors is an intrinsic property of the estimator, it is essential if one desires a "first do no harm" approach to policy evaluation. Conversely, the ACT adopts a very "strict" critical value of 3.115 for assessing positive effects.

The right panel of Fig. B2 presents results for the strong instrument case of C=74 ($F_{.05}$=105). Here the median left and right tail critical values used to form the 5% two-tailed ACT are -1.585 and 2.298. Notice that substantial asymmetry remains even at this high level of instrument strength. This is consistent with our observation in Section 6 that one-tailed t-tests using standard critical values only achieve approximate symmetry between left-tail and right-tail rejection rates if first-stage F is in the tens of thousands. The ACT achieves a correct overall 5% rejection rate, as well as symmetry with 2.5% negative and 2.5% positive rejections. Recall that for a conventional two-tailed t-test these figures were 0.3% and 4.5%, and for the AR test they were 2.3% and 2.7%, respectively.

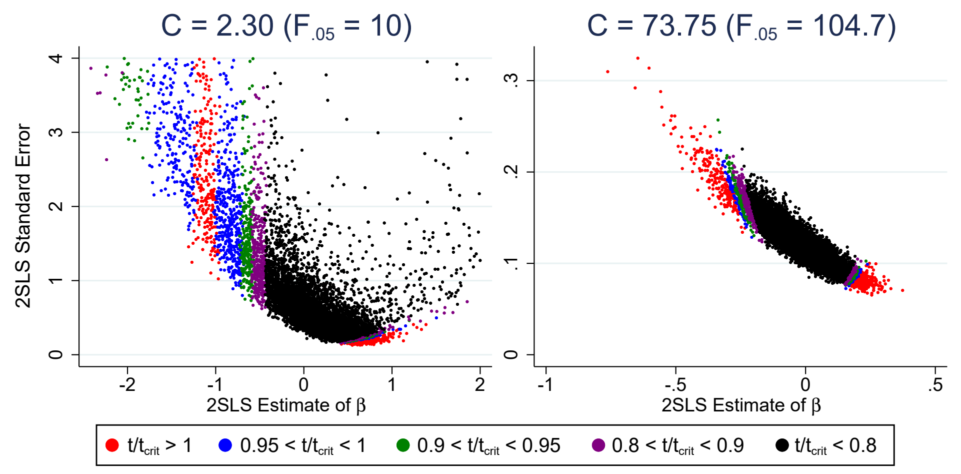
In Table B2 we examine power of the ACT test. We again consider two alternative true values, $\beta = 0.30$ or $\beta = -0.30$. These are quantitatively large values, as they imply a one standard deviation change in x induces an 0.25 standard deviation change in y.

Consider first the strong instrument case of C=74 ($F_{.05}$=105). Here the power of the ACT and AR tests are almost identical.[26] Both tests exhibit a clear power asymmetry: a 91% rejection rate when $\beta = -0.3$ but only a 53% to 54% rejection rate when $\beta = 0.3$.

[25] We emphasize that the conditional critical values differ across the 10,000 datsets, as each dataset has its own realization of $(\hat{F}, \hat{\rho})$, but the median critical values are -0.443 and 3.115.

[26] This may seem surprising, given that Andrews et al. (2007) found two-tailed conditional t-tests have very poor power. This is because – unlike the ACT test – the tests considered by Andrews et al. (2007) constrain the critical values to be symmetric around zero, which fails to deal with the power asymmetry problem we have emphasized. The same criticism applies to the tF test proposed by Lee et al. (2020) that we discuss in Appendix C.

Figure B2. The Mills et al. (2014) Asymmetric Conditional t-test ($\rho = 0.80$)



Note: Colors signify the ratio of the t-statistic to its critical value. Runs with s.e.> 4 not shown.

This asymmetry is not specific to this example: It is a general 2SLS property that follows from the positive association between $\rho\widehat{cov}(z, u)$, $\hat{\beta}_{2SLS}$ and the value of the AR test (as we discussed in Section 7). The moderately strong instrument case of C=29.4 ($F_{.05}$=50) is similar. As we see in Table B2, the two tests again have almost identical power, and both have more power to detect a true negative β (55% if $\beta = -0.3$ vs. 25% if $\beta = 0.3$).

Table B2. Power of the ACT and AR tests ($\rho = 0.8$) (%)

C	$F_{5\%}$	Conditional t-test (ACT)			AR Test		
		H_0:$\beta = 0$	$\beta > 0$	$\beta < 0$	H_0:$\beta = 0$	$\beta > 0$	$\beta < 0$
				$\beta = 0.3$			
2.30	10	7.3	5.6	1.7	6.6	6.5	0.1
5.78	16.38	9.4	8.1	1.3	8.4	8.3	0.2
29.44	50	25.5	25.5	0.1	25.2	25.2	0.0
73.75	104.7	53.7	53.7	0.0	53.4	53.4	0.0
				$\beta = -0.3$			
2.30	10	5.5	0.7	4.8	9.0	3.2	5.9
5.78	16.38	10.7	0.2	10.5	15.0	0.8	14.2
29.44	50	54.8	0.0	54.8	54.7	0.0	54.7
73.75	104.7	91.0	0.0	91.0	91.0	0.0	91.0

Note: The table reports the frequency of rejecting the false null hypothesis H_0: $\beta = 0$.

Thus, both the ACT and AR tests have good power to detect true effects that are opposite in direction to the OLS bias. If one adopts a "first do no harm" approach to policy evaluation, detection of true negative effects when selection into treatment is positive is a top priority. Thus the AR and ACT tests are clearly superior to the t-test, as the latter has little power to detect true negative effects in that context.

Next, we examine power of the ACT test in the case of C=2.30 ($F_{.05}$=10), often considered the standard for an acceptably strong instrument. In this case, power is very poor. The probability of rejecting $\mathrm{H}_0 : \beta = 0$ is only 7.3% when true $\beta = 0.3$, and only 5.5% when true $\beta = -0.3$. Moreover, these figures are inflated by the fact that a non-negligible proportion of these rejections occur when $\hat{\beta}_{2SLS}$ has the "wrong" sign. The AR test doesn't do any better. It has even lower power (only 6.6%) when true $\beta = 0.3$, and while it superficially seems to do a bit better (9%) when true $\beta = -0.3$, this is spuriously inflated by the fact that more than 1/3 of these rejections happen when $\hat{\beta}_{2SLS} > 0$.

The obvious conclusion is that in the C=2.30 ($F_{.05}$=10) case there is simply not much information in the data, and no choice of testing procedure will change that. This lack of power is concerning given the prevalence of the $\hat{F} > 10$ rule of thumb for acceptable instrument strength. Moving to the case of C=5.780 ($F_{.05}$=16.38) we see small improvements for both tests, as power attains levels of 8.4% to 15%, and "wrong sign" rejections become rare. But these power levels still seem uninspiring. The AR test has slightly better power to detect a true negative β than the ACT test – i.e., 15% vs. 10.7%.

Overall, we conclude that the ACT test is not a very useful alternative to the AR test in the single instrument case. It is much more difficult to implement and yields very similar results. We revisit this question in Section 11 on the over-identified case.

C. THE *T*F-TEST

Lee et al. (2020) propose a way to eliminate the maximal size distortion of the two-tailed 2SLS t-test by conditioning its critical values on the first-stage $\hat{F}$. They call this the tF-test. It is closely related to the ACT test we discussed in Section B, which conditions one-tailed t-test critical values on $\hat{F}$ and $\hat{\rho}$. The difference is that tF-test critical values are symmetric about zero, and worst-case values are assumed for both ρ and C.

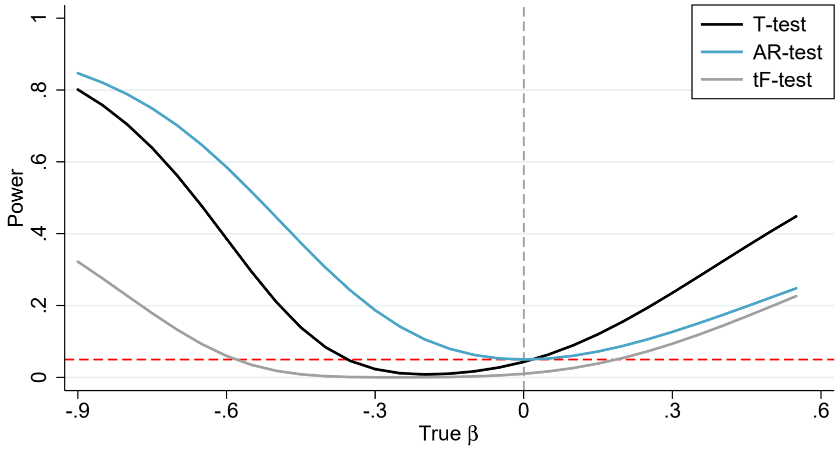
As we saw in Section 3 Figure 1, the size of the t-test is strongly increasing in ρ when instruments are weak, and the worst-case (maximal) size distortion occurs when $\rho = \pm 1$. Lee et al. (2020) show the worst case for C is $[\hat{F}/(\hat{F}^{1/2} + 1.96)]^2$. Using a procedure similar to that described in Appendix B, it is possible to simulate the distribution of the t-test conditional on $\hat{F}$, assuming ρ=1 and fixing C at the worst-case level.

Lee et al. (2020) show that a first-stage sample $\hat{F}$ of at least 104.7 is required to guarantee the size of a 5% level two-tailed t-test is no greater than 5% (i.e., worst-case size distortion is zero). Hence, if the first stage $\hat{F}$ is at least 104.7 the tF test uses the conventional ± 1.96 critical values to form a 5% test. At smaller values of $\hat{F}$ the t-test size is inflated. Hence the critical values must be scaled up to compensate. For example, if $\hat{F} = 10$ one must scale the 5% critical values up to ± 3.43 to reduce the maximum size of the t-test to exactly 5%. The smaller is the first-stage $\hat{F}$, the greater is the required scaling up of the critical values to eliminate size distortion.

When $\hat{F} < 3.84$ both the AR and tF 95% confidence intervals for β are unbounded. The reason is simple: If $\hat{F} < 3.84$ then the 95% confidence interval for the first-stage coefficient π includes the case of π=0. This means we do not have 95% confidence the model is identified. It would be logically inconsistent to place a 95% confidence interval on β when we don't have 95% confidence that the model is identified. Yet a 2SLS t-test based confidence interval does exactly that when $\hat{F} < 3.84$.

By construction tF-test critical values are always greater than or equal to conventional t-test critical values. So the power of the tF test is unambiguously less than that of the t-test. This can be observed in Figure C3, which compares the power curves of the tF, t and AR tests in the case of $C = 10$ and $\rho = 0.5$. The tF test has low power in general, and very little power to detect true negative effects when the OLS bias is positive.[27]

Figure C3. Power of the tF-test against the t-test and AR-test (C = 10, ρ = 0.5)



[27]Lee et al. (2020) provide a table of tF-test 5% critical values at selected values of $\hat{F}$. To construct power curves we require a smooth function that generates the critical values as a function of $\hat{F}$. We developed the following function that approximates the critical values to high accuracy (R^2=0.9995). Letting $f = \hat{F}$ and letting $c_{.05}(f)$ denote the critical for a 5% test we have:
$c_{.05}(f) = exp(2475f^{-2} - 5709ln(f)f^{-2} + 5253ln(f)^2f^{-2} - 2395ln(f)^3f^{-2} + 543ln(f)^4f^{-2} - 50ln(f)^5f^{-2} + 0.492)$ for all F on the interval $(3.84, 104.7)$ We replace the critical values in equation (A3) with the values obtained from this function to calculate the power curve of the tF-test.

D. SIMULATING THE DISTRIBUTION OF THE T-TEST

Following Mills et al. (2014), we begin by defining the covariance matrix of the reduced form errors Ω. The reduced-form equations are $y = z\beta\pi + (\beta e + u) = z\xi + v$ and $x = z\pi + e$. Thus we have $\Omega = \left(\begin{smallmatrix} \sigma_v^2 & \rho_0\sigma_v\sigma_e \\ \rho_0\sigma_v\sigma_e & \sigma_e^2 \end{smallmatrix}\right)$ where $\sigma_v^2 = Var(\beta e + u)$ and $\rho_0 = corr(\beta e + u, e) = corr(v, e)$. Mills et al. (2014) show that:

$$t_{2SLS} = \frac{\beta_{2SLS}}{\sigma_{2SLS}[c_{21}^2 t_{AR}^2 + 2c_{21}c_{22}t_{AR}t_{1'} + c_{22}^2 t_{1'}^2]^{-1/2}} \tag{D1}$$

where t_{AR} is the t-statistic from a regression of y_i on the instrument z_i (i.e. the "t-test version" of the AR test statistic),[28] $t_{1'}$ is the t-statistic of a regression of $x_i - \rho_0\frac{\sigma_e}{\sigma_v}y_i$ on z_i, and σ_{2SLS} is the standard error of the 2SLS regression. We also have $c_{11} = \sigma_v$, $c_{21} = \rho_0\sigma_e$, and $c_{22} = \sigma_e\sqrt{1-\rho_0^2}$. Furthermore, β_{2SLS} is given by:

$$\beta_{2SLS} = \frac{c_{11}c_{21}t_{AR}^2 + c_{11}c_{22}t_{AR}t_{1'}}{c_{21}^2 t_{AR}^2 + 2c_{21}c_{22}t_{AR}t_{1'} + c_{22}^2 t_{1'}^2} \tag{D2}$$

Importantly, the t-test version of the AR test t_{AR} and the modified first-stage t-statistic $t_{1'}$ are constructed to be independent. To see this, note that:

$$\begin{pmatrix} y_i \\ x_i - \rho_0\frac{\sigma_e}{\sigma_v}y_i \end{pmatrix} \sim N\left[\begin{pmatrix} z_i\pi\beta \\ z\pi(1-\rho_0\frac{\sigma_e}{\sigma_v}\beta) \end{pmatrix}, \begin{pmatrix} \sigma_v^2 & 0 \\ 0 & \sigma_e^2(1-\rho_0^2) \end{pmatrix}\right]$$

Thus y_i and $x_i - \rho_0\frac{\sigma_e}{\sigma_v}y_i$ are independent, which implies that t_{AR} and $t_{1'}$ are independent.[29] This allows us to simulate the distribution of t_{AR} while holding $t_{1'}$ fixed.

It is possible to use these equations to simulate the distribution of the 2SLS t-test under the null that $\beta_0 = 0$, and conditional on the strength of the instruments (captured by $t_{1'}$) and the covariance of the reduced form errors (Ω), using the following procedure:

1 Draw a simulated value of t_{AR} from the $N(0, 1)$ distribution holding $t_{1'}$ fixed.[30]
2 Calculate β_{2SLS} as above but using the simulated value of t_{AR}.
3 Re-estimate σ_{2SLS} using the value of β_{2SLS} from step 2.
4 Calculate t_{2SLS} using the values from the first to third step.
5 Repeat Steps 1-4 N times.
6 To obtain (simulated) critical values for α-level one-sided conditional t-tests, calculate the α and $1-\alpha$ percentiles of the N simulated values of t_{2SLS}.

The 2.5 and 97.5 percentiles from step 6 can be used as critical values for a 5%-level two-sided conditional t-test of the H_0:β=0. We call this an ACT test in the text.

The above procedure assumes Ω is known, but $\hat{\Omega}$ must be used in practice. This is not important in theory (or in practice given large samples), as the covariance structure of the reduced from errors can be consistently estimated (without knowing true β or ρ).

[28] Obviously the AR test is equivalent to the squared t-test for significance of the instrument z in the reduced form for y. We denote this by t_{AR} and refer to it as the "t-test version" of the AR test. It is obvious that t_{AR} is approximately standard normal (in large samples) regardless of the weakness of the instrument, as it is simply a t-test from an OLS regression.

[29] Note that t_{AR} is obtained from a projection of y_i on z_i, and $t_{1'}$ is obtained from a projection of $x_i - \rho_0\frac{\sigma_e}{\sigma_v}y_i$ on z_i. Since y_i and $x_i - \rho_0\frac{\sigma_e}{\sigma_v}y_i$ are independent, the independence of the two objects is preserved by these projections. For this reason, $t_{AR}|t_{1'} \sim t_{AR} \sim N(0, 1)$.

[30] In the case of multiple instruments ($k \geq 2$), one would draw t_{AR} from a $N(0, 1) + \sqrt{\chi_{k-1}}$ distribution.

E. SUPPLEMENTARY TABLES AND FIGURES

Table E1. Median Standard Error for $\hat{\beta}_{2SLS}$

Concentration Parameter ("True First-Stage F")	F critical value to reject C<c at 5%	Standard Error 2SLS
1.82	8.96	0.799
2.30	10.00	0.705
3.84	13.00	0.533
5.78	16.38	0.429
10.00	23.10	0.322
73.75	104.70	0.117

Note: The worst-case OLS bias is 1.0 when $\rho = 1$ and $\pi = 0$.

Figure E1. 2SLS Power Function, t-test of $H_0 : \beta = 0$ when true $\beta = 0.3$

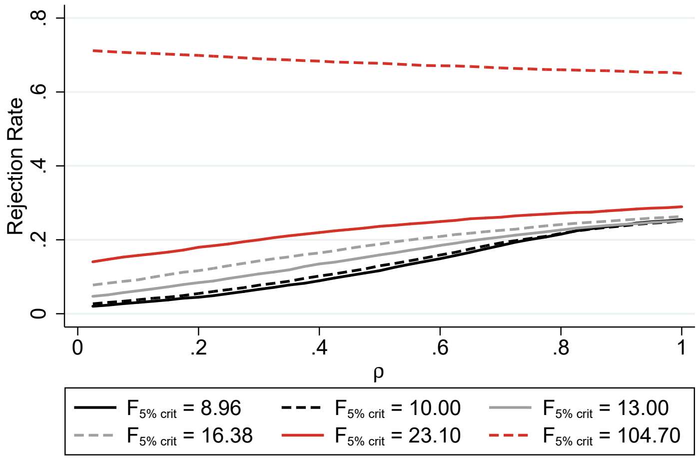


Figure E2. The Standard Error of Optimal IV Plotted Against $\hat{\beta}_{OPT}$ ($\rho = 0.80$)

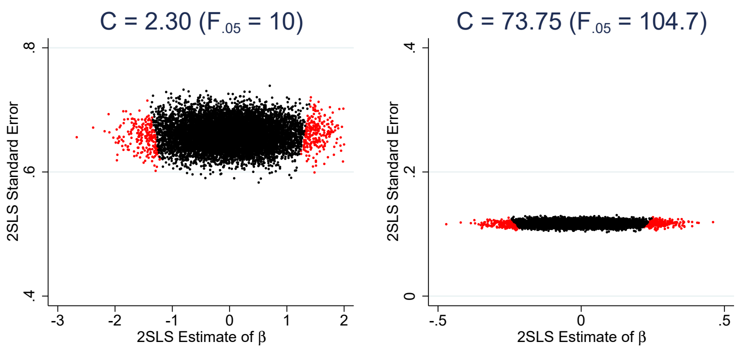
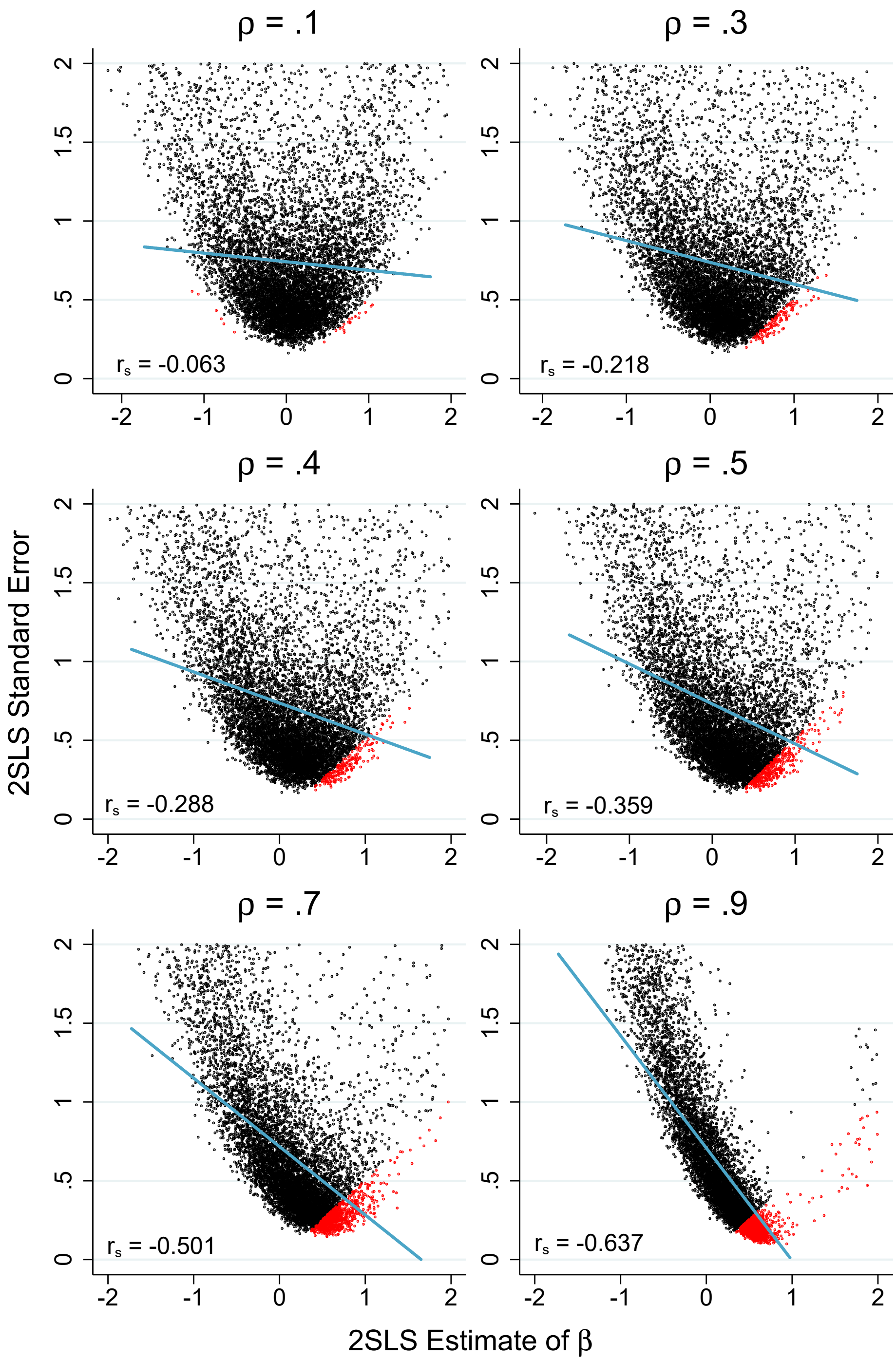
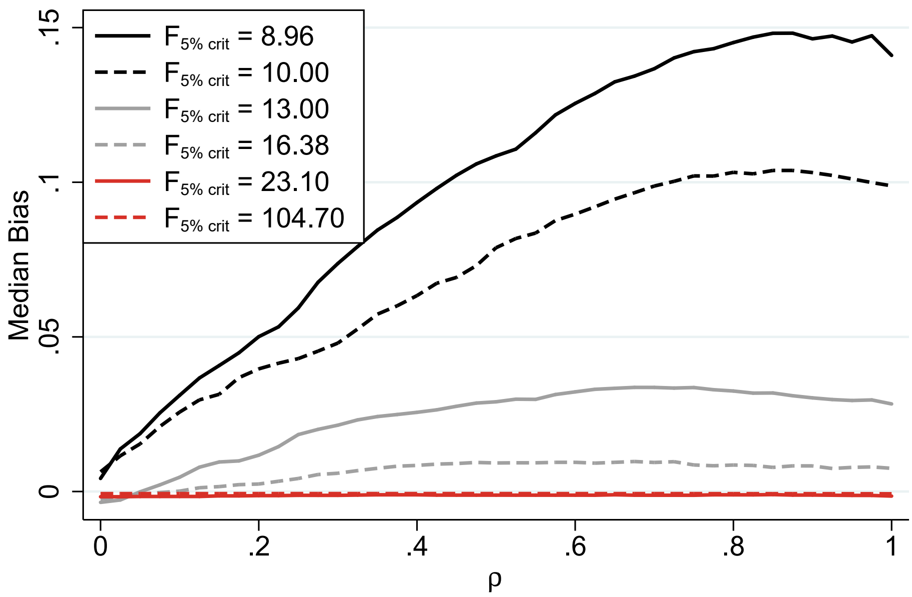


Figure E3. The Covariance of 2SLS Estimates and their Standard Errors



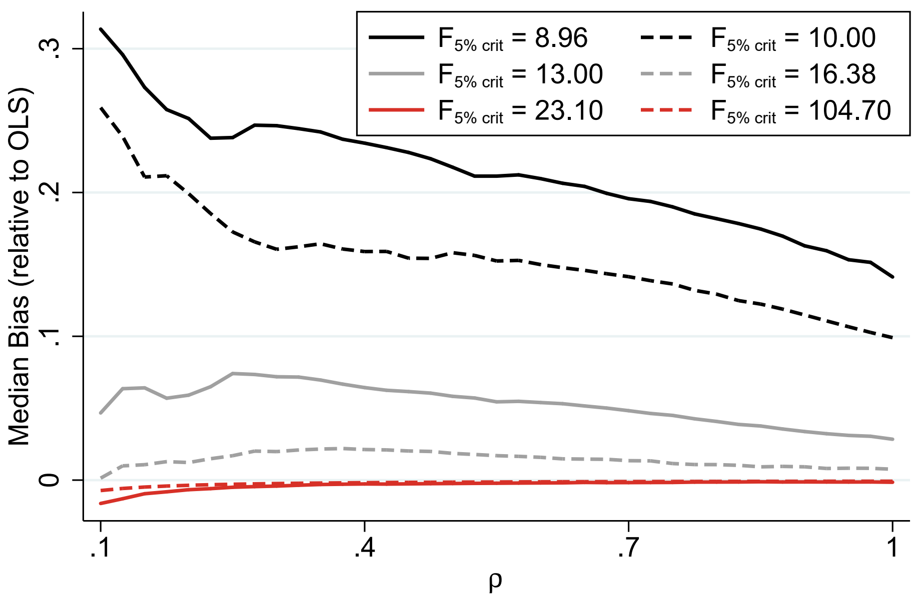
Note: This figure plots the Standard Error of $\hat{\beta}_{2SLS}$ against $\hat{\beta}_{2SLS}$ itself ($C = 2.3, F_{.05} = 10$). Red dots indicate $H_0 : \beta = 0$ rejected at 5% level. The OLS regression line for $SE(\hat{\beta}_{2SLS})$ against $\hat{\beta}_{2SLS}$ is presented in blue and excludes standard errors > 2, while the Spearman correlations r_s do not.

Figure E4. Median Bias of 2SLS by Instrument Strength and ρ



Note: We plot the median of the $\hat{\beta}_{2SLS}$ *estimates. The worst-case OLS bias is 1.0 when* $\rho = 1$ *and* $\pi = 0$.

Figure E5. Median Bias of 2SLS Relative to OLS Bias (%)



Note: We plot $median(\hat{\beta}_{2SLS})/median(\hat{\beta}_{OLS})$. *The worst-case OLS bias is 1.0 when* $\rho = 1$ *and* $\pi = 0$.