

# Valid $t$ -ratio Inference for IV<sup>1</sup>

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## Abstract

In the single IV model, current practice relies on the first-stage  $F$  exceeding some threshold (e.g., 10) as a criterion for trusting  $t$ -ratio inferences, even though this yields an anti-conservative test. We show that a true 5 percent test instead requires an  $F$  greater than 104.7. Maintaining 10 as a threshold requires replacing the critical value 1.96 with 3.43. We re-examine 57 AER papers and find that corrected inference causes half of the initially presumed statistically significant results to be insignificant. We introduce a more powerful test, the  $tF$  procedure, which provides  $F$ -dependent adjusted  $t$ -ratio critical values.

Keywords: Instrumental Variables, Weak Instruments,  $t$ -ratio, First-stage  $F$  statistic

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# 1 Introduction

Consider the single-variable instrumental variable (IV) model, with outcome  $Y$ , regressor of interest  $X$ , and instrument  $Z$ ,<sup>1</sup>

$$Y = \alpha + \beta X + u, \text{ where} \tag{1}$$
$$COV(u, Z) = 0.$$

When describing statistical inference procedures for these models, textbooks invariably recommend estimating  $\beta$  via the instrumental variable estimator  $\hat{\beta}_{IV} \equiv \frac{C\hat{O}V(Y, Z)}{C\hat{O}V(X, Z)}$  and associated standard error  $\hat{SE}(\hat{\beta}_{IV})$  and testing the null hypothesis that  $\beta = \beta_0$  using the  $t$ -ratio  $\frac{\hat{\beta}_{IV} - \beta_0}{\hat{SE}(\hat{\beta}_{IV})}$  with the usual critical value of 1.96 for a test at the 5 percent level of significance, or constructing 95 percent confidence intervals using the interval  $\hat{\beta}_{IV} \pm 1.96 \cdot \hat{SE}(\hat{\beta}_{IV})$ .<sup>2</sup> Most textbook treatments also note that these inference procedures give distorted Type I error (or coverage rates) when instruments are “weak,” and suggest using the first-stage  $F$  statistic<sup>3</sup> as a diagnostic, implying that one can reliably use the usual procedures if  $F$  exceeds a threshold, such as 10.<sup>4</sup>

Even though these  $t$ -ratio inference procedures are known to yield distortions in size and coverage rates, and even though alternatives (e.g., Anderson and Rubin (1949)) are known to have correct size/coverage – possessing attractive optimality properties while also being robust to arbitrarily weak instruments – applied

<sup>1</sup>It can be shown that all of our results apply to the single excluded instrument case more generally, allowing for other covariates and consistent variance estimators that accommodate departures from i.i.d. errors.

<sup>2</sup>Throughout the paper and tables and figures, for expositional purposes, we use “1.96<sup>2</sup>” as shorthand for  $(\Phi^{-1}(0.975))^2$ .

<sup>3</sup>That is,  $F = \left(\frac{\hat{\pi}}{\hat{SE}(\hat{\pi})}\right)^2$ , with  $\hat{\pi}$  and  $\hat{SE}(\hat{\pi})$  as the estimators and standard errors from a least squares regression of  $X$  on  $Z$ , i.e., the “first stage” regression.

<sup>4</sup>For example, Bound, Jaeger and Baker (1995) and Staiger and Stock (1997) advocate that applied researchers should report the values of the first-stage  $F$ -statistic by regressing the endogenous variable  $X$  on the instrument  $Z$ . Angrist and Pischke (2009) also provide guidance along these lines. For a recent review and discussion of the econometric literature, see the survey by Andrews, Stock and Sun (2019).

research, with rare exceptions, relies on  $t$ -ratio-based inference.<sup>5</sup> This continued practice is arguably based on a combination of a preference for analytical and computational convenience and the presumption that for practical purposes, the distortions in inference are small or negligible.

This paper theoretically and empirically assesses this presumption. Specifically, we derive expressions for the rejection probabilities for both the conventional  $t$ -ratio procedure and the common procedure of using a threshold for the first-stage  $F$  statistic to account for weak instruments. We use these expressions to precisely answer the following sets of questions: 1) Since it is known that being completely agnostic about the data-generating process will lead to  $t$ -ratio-based inference that will deliver incorrect size and confidence level (e.g., because of weak instruments), precisely which additional assumptions about the model in (1) can be imposed so that the conventional  $t$ -ratio procedure is valid? and 2) Since it is known that using the usual  $t$ -ratio procedure in conjunction with a modest threshold rule for  $F$  (e.g., 10) will yield Type I error that is too large, is there a threshold for  $F$  (or, alternatively, a higher critical value for  $t$ ) that would yield inferences with the intended size and confidence level? In other words, since it would be inaccurate to refer to these procedures as having the intended 5 percent Type I error, are there adjustments that can be made that would result in a true 5 percent test?

Our answers to these questions indicate that fixing these distortions (or specifying the assumptions needed to avoid the distortions) leads to a significant change to interpretation and practice. The IV  $t$ -ratio procedure is typically presented as asymptotically valid, applicable without needing to make any assumptions about model (1), other than  $COV(X, Z) \neq 0$ . But the results of Dufour (1997) show that the  $t$ -ratio procedure will lead to incorrect coverage in any (arbitrarily large) finite sample. We quantify this distortion, by showing that the usual 1.96 critical values for a 5 percent test *can* remain valid if one assumes that  $E[F]$  exceeds 143; strictly speaking, our calculations show that there exist data-generating processes with  $E[F] < 143$  that could lead to rejection probabilities (coverage probabilities)

<sup>5</sup>The test of Anderson and Rubin (1949) in the just-identified case has been shown to minimize Type II error among various classes of alternative tests. This is shown for homoskedastic errors, by Moreira (2002, 2009) and Andrews, Moreira and Stock (2006), and later generalized to cases for heteroskedastic, clustered, and/or autocorrelated errors, by Moreira and Moreira (2019).

greater than 0.05 (less than 0.95). Without knowing the true value of the nuisance parameter  $E[F]$ , the rejection probability can be arbitrarily close to 1.<sup>6</sup>

We also show that an alternative assumption can be used to justify the validity of the usual  $t$ -ratio procedure. This alternative assumption is agnostic about  $E[F]$  but instead limits the degree of endogeneity. Namely, it requires the correlation between the main equation and first-stage errors,  $\rho \equiv \text{Corr}(u, X - Z\pi)$ , be no greater than 0.565 in absolute value. Again, allowing the possibility of  $|\rho|$  being greater than 0.565 could potentially lead to the maximum distortion in type I error possible (e.g., rejecting with probability 1). These potential restrictions on  $E[F]$  or  $|\rho|$  appear to be significant departures from agnosticism about nuisance parameters.

Examining the more standard case, in which practitioners wish to remain agnostic about  $E[F]$  and  $\rho$ , we find that substantial changes to common usage of the first-stage  $F$  are required for inferences to be undistorted. As noted above, perhaps the most commonly employed rule of thumb for the first-stage  $F$  statistic is a threshold of 10: if  $F$  is beyond 10, then the usual critical values of 1.96 are typically used, with the understanding that there is a “small” amount of distortion to size/coverage. We show that it is in fact possible to adjust the threshold for  $F$  to be a finite value so that there is no distortion. However, these calculations show that the distortion-corrected threshold for  $F$  is far from 10, and is, in fact, 104.7.

An alternative approach is to maintain the commonly used threshold for  $F$  of 10, but then to adjust the critical values for  $t$  to achieve correct size and confidence level. Our calculations show that the required critical value in this case would be very large: 3.43. To put this adjusted critical value in perspective, consider the move from a 95 percent confidence interval to a 99 percent confidence interval—an exacting standard. This move only requires adjusting the critical value by about 31 percent, i.e., from 1.96 to 2.57. Our results show that using a threshold of 10 for  $F$  requires adjusting the critical value by about 74 percent, i.e., from 1.96 to 3.43, for the  $t$ -test to have correct size/coverage. In other words, using 3.43 as a critical value for  $t$ -ratio-based inference is even more stringent than using a 1 percent test and in fact is the critical value for a 0.06 percent test.

<sup>6</sup>This “worst-case scenario” occurs when  $E[F]$  approaches 1 and the correlation between  $u$  and  $X - Z\pi$  is 1 (or -1).

An important fact that has been recognized in the econometrics literature but possibly under-appreciated in applied research is that the validity of a decision rule that uses a single critical value for  $t$  and a single threshold for  $F$  requires the commitment to automatically accept the null hypothesis – *no matter the realized value of  $t$*  – if  $F$  does not exceed the threshold (e.g., 10 or 104.7). This amounts to confidence intervals that are dependent on  $F$ : If  $F > 104.7$  (or 10), then use  $\hat{\beta}_{IV} \pm 1.96 \cdot \hat{SE}(\hat{\beta})$  (or  $\hat{\beta}_{IV} \pm 3.43 \cdot \hat{SE}(\hat{\beta})$ ); otherwise, the confidence interval for  $\beta$  is the *entire real line*.

We consider the practical implications of these findings for applied research by examining all studies recently published in the *American Economic Review* (*AER*) that utilize a single-instrument specification. All of these papers use the usual  $t$ -ratio-based 2SLS inference outlined above, but only 2-3 percent of the specifications report the test of Anderson and Rubin (1949), despite the clear implication from the econometrics literature that this test should be part of best applied econometric practice. Surprisingly, for more than a quarter of the specifications, one cannot infer the associated first-stage  $F$  statistic from the published tables. For this group of specifications, their conclusions about statistical significance at the 5 percent level could remain unaltered were they to use our results and qualify their analysis by making one of the above two assumptions about the nuisance parameters: either  $E[F] > 142.6$  or  $|\rho| < 0.565$ .

For the *AER* specifications for which an  $F$  statistic can be derived from the published tables, the median is 42.0, with 25th and 75th percentiles at 12.4 and 299.5, respectively. 58 percent of the specifications satisfy both  $F > 10$  and the rejection rule  $|t| > 1.96$ , which is conventionally used to determine “statistical significance.” While Staiger and Stock (1997) notes the size distortion in such a procedure, conventional wisdom in the applied literature appears to treat such a procedure as having approximately 5 percent significance level. We re-examine the specifications with  $F > 10$  and  $t^2 > 1.96^2$  and find that using either of the size-corrected procedures described above to *actually* achieve 5 percent significance causes at least half of the specifications to become statistically insignificant, leading us to conclude that these calculations are of real importance for the field and that “ $F > 10$ ” is not a reliable rule for practical use if authors want to maintain a significance level of 5

percent.

As Andrews, Stock and Sun (2019) have noted, an important limitation to adopting a single threshold for  $F$  is the loss of informativeness of the data when the first-stage  $F$  is below the threshold. Even more worrisome is the possibility that researchers may selectively drop the specification because the  $F$  does not meet the threshold, a decision which, in repeated samples, distorts the size of the procedure even further, as those authors note. Therefore, to accommodate occurrences of  $F$  that are below 104.7, we use our theoretical results to construct a function  $c(F)$  and a procedure – which we call “ $tF$ ” – such that under the null hypothesis,  $\Pr [t^2 > c(F)] \leq 0.05$ , under any values of the nuisance parameters  $E[F]$  and  $\rho$ . Another motivation for providing this  $tF$  procedure is to aid in interpreting the potentially hundreds of studies that have already been published that did not use procedures with correct size, such as  $AR$ . Given the prohibitive cost of re-analyzing those studies, the  $tF$  procedure allows one to use already-published  $t$  and  $F$  statistics to reinterpret the results, conducting valid inference.

In contrast to the two “single critical value/threshold” procedures which suggest only half of published results are statistically significant at the conventional 5 percent level, the  $tF$  procedure allows us to conclude that almost four-fifths are statistically significant.

The paper is organized as follows. Section 2 uses recent papers published in the *AER* to characterize current inferential practices for the single-instrument IV model; these patterns motivate our areas of emphasis in the theoretical discussion. Deferring details and the more in-depth theoretical discussion to Section 4, Section 3 states the main theoretical results and illustrates the consequences of those results for the studies in our sample. Section 4 more formally derives the theoretical results, and Section 5 concludes. Lastly, we should re-emphasize that the findings and results of this paper, including specific numerical thresholds, are **not** reliant on i.i.d. or homoskedastic errors. Departures from i.i.d. errors, such as two-way clustering or auto-correlation, are easily accommodated as long as a corresponding consistent robust variance estimator is also employed.

## 2 Inference for IV: Current Practice

This section documents current practice for the single instrumental variable model, as reflected by recent research published in the *American Economic Review*. Our sample frame consists of all *AER* papers published between 2013 and 2019, excluding proceedings papers and comments, yielding 757 articles, of which 124 included instrumental variable regressions. Of these 124 studies, 57 employed single instrumental variable (just-identified) regressions. Consistent with the conclusion of Andrews, Stock and Sun (2019), this confirms that the just-identified case is an important and prevalent one from an applied perspective.

From these papers, we transcribed the coefficients, standard errors, and other statistics associated with each *IV* regression specification. Each observation in our final dataset is a “specification,” where a single specification is defined as a unique combination of 1) outcome, 2) endogenous regressor, 3) instrument, and 4) combination of covariates. The dataset contains 1310 specifications from 57 studies; among those studies, the average number of specifications was 22.98, with a median of 9, with 25th and 75th percentiles of 4 and 21, respectively. Since the purpose of our dataset is to fully characterize specifications that are reported in published studies, our coverage of studies will be broader than that of Andrews, Stock and Sun (2019), who compared *AR*-based and *t*-ratio-based inference, by obtaining the original microdata from the smaller subset of studies for which this was possible.

Each specification was placed into one of four categories, as shown in Table 1, according to the types of regressions for which coefficients and standard errors were reported: the coefficients and standard errors from 1) only the 2SLS, 2) the 2SLS and first-stage regression, 3) the 2SLS and the reduced-form regression of the outcome on the instrument, and 4) the 2SLS, the first stage, and the reduced form. In addition, we identified whether for each specification, the first-stage *F* statistic was explicitly reported, as indicated by the first two columns in Table 1.

For each configuration, Table 1 reports the number of specifications, as well as proportions (parentheses) and weighted proportions (brackets), where the weight for each specification is the inverse of the total number of specifications reported from its study. Henceforth, unless otherwise specified, when we refer to propor-

Table 1: Current Practice Implementing IV estimation, Published Papers from AER

Combinations of regressions reported	First Stage F-statistic?		Total
	No	Yes	
Two-Stage Least Squares	434 (0.331) [0.231]	132 (0.101) [0.095]	566 (0.432) [0.325]
Two-Stage Least Squares and First Stage	247 (0.189) [0.218]	274 (0.209) [0.157]	521 (0.398) [0.375]
Two-Stage Least Squares and Reduced Form	16 (0.012) [0.056]	7 (0.005) [0.038]	23 (0.018) [0.094]
Two-Stage Least Squares, First Stage, and Reduced Form	132 (0.101) [0.118]	68 (0.052) [0.088]	200 (0.153) [0.206]
Total	829 (0.633) [0.623]	481 (0.367) [0.377]	1,310 (1) [1]

N=1310. Drawn from 56 published papers. Each observation represents a unique combination of outcome, regressor, instrument, and covariates. Unweighted proportions are in parentheses, and weighted proportions are in brackets, where the weights are proportional to the inverse of the number of specifications in the associated paper.

tions, we refer to the weighted proportions, since we wish to implicitly give each study equal weight in the summary statistics that we report.

Table 1 shows that the modal practice among all combinations is for 2SLS coefficients to be reported without explicitly reporting the first-stage  $F$  statistic, representing about a quarter of the specifications. The second most common practice is to report both the 2SLS and the first-stage coefficients without reporting the  $F$  statistic, but it should be clear that the  $F$  statistic can be derived from squaring the ratio of the first-stage coefficient to its associated estimated standard error. The least common reporting combination was the 2SLS and the reduced form, while reporting the first-stage  $F$  (3.8 percent).

In the foregoing analysis, in order to maximize the number of specifications for which we have a first-stage  $F$  statistic, we first use the first-stage  $F$  statistic as com-

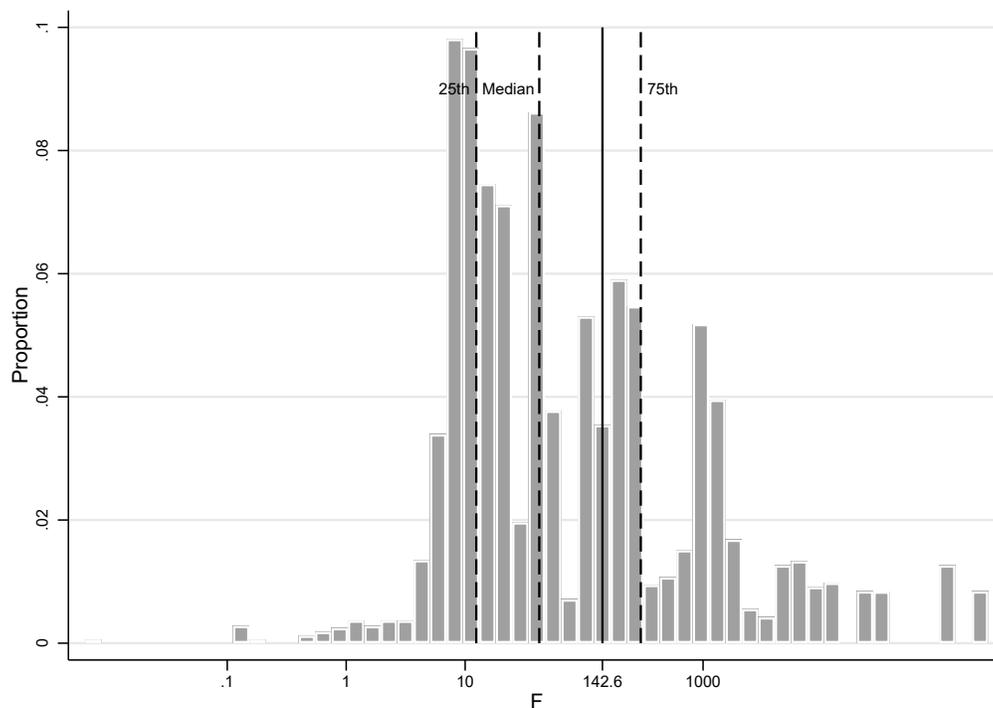
puted from the reported first-stage coefficients and standard errors, but whenever this is not possible we use the reported  $F$  statistic.<sup>7</sup>

Figure 1 displays the histogram of the  $F$  statistics in our sample on a logarithmic scale. The weighted 25th percentile, median, and 75th percentiles are 12.41, 41.99, and 299.48, respectively. Thus, most of the reported first-stage  $F$  statistics in these studies do pass commonly cited thresholds such as 10. More detail on these specifications is provided in Table 2a, which is a two-way frequency table for whether or not  $t^2$  exceeds  $1.96^2$  and whether or not  $F$  exceeds 10. Overall, the table indicates that for about 58 percent of the specifications, the estimated 2SLS coefficient would be “statistically significant” under the usual practice of using a critical value of 1.96 and would also loosely reject the hypothesis of “weak instruments.” We recognize that the null hypothesis of  $\beta = 0$  may not always be the hypothesis of interest across all the studies, and furthermore, in our data collection, we did not make any judgments as to the extent to which any particular regression specification was crucial for the conclusions of the article; note that in many cases, the 2SLS specification was used for a “placebo” analysis where insignificant results are consistent with the identification strategy of the paper. Below, our purpose is not to determine whether any particular study’s overall conclusions are unwarranted when using the corrections below. Instead, we are seeking to identify broad patterns across all studies to assess how much of a difference these corrected procedures would have made in the aggregate.

We conclude this section with the observation that  $AR$  test statistics or  $AR$  confidence regions are reported for less than 3 percent of the specifications, despite the fact that the econometric literature has provided clear guidance that reporting  $AR$  is part of applied econometric best practice. It is this stark difference between theory and practice that motivates our focus. We surmise that practitioners elect to use the  $t$ -ratio (supplemented with the use of the first-stage  $F$  statistic) over the  $AR$  statistic not because they believe it has superior properties, compared to  $AR$ -based infer-

<sup>7</sup>We find that among studies in which both the reported and computed  $F$  statistic are available, about 67 percent of the time the two numbers are within 5 percent of one another. For those specifications in which the reported  $F$  is the only  $F$  statistic available, there are some situations where it is not entirely clear whether the  $F$  statistic is the first-stage  $F$ ; there is a possibility that they are  $F$  statistics for testing other hypotheses.

Figure 1: Distribution of First-stage F-statistics



N=859 specifications. Scale is logarithmic. All specifications use the derived  $F$  statistic, and when not possible, the reported  $F$  statistic. Proportions are weighted; see notes to Table 1. Dashed lines correspond to the 25<sup>th</sup> (12.41), 50<sup>th</sup> (41.99), and 75<sup>th</sup> (299.48) percentiles of the distribution.

ence, but rather because it is presumed that any inferential approximation errors associated with the conventional  $t$ -ratio are minimal or acceptable.

We are also motivated by the fact that there are likely hundreds of other studies that have used the single-instrument  $IV$  model. Even though it can be argued that these studies *should have* used  $AR$ , if our sample is any indication, it may well be that most did not, and it could be prohibitively costly to replicate those hundreds of studies. For this reason, we take the reported statistics as given, and seek to specify precisely which assumptions previous researchers should have made to justify the inferences they made, or to reinterpret the meaning of their reported  $t$  and  $F$  statistics through the lens of a procedure that delivers the intended (e.g., 5 percent) level of significance.

Table 2a:  $t^2$  and First-stage F Statistics, Conventional Critical Value, Rule of Thumb Threshold of 10

	F<10	F≥10	Total
$t^2 \geq 1.96^2$	46 (0.054) [0.106]	375 (0.437) [0.577]	421 (0.49) [0.683]
$t^2 < 1.96^2$	49 (0.057) [0.068]	389 (0.453) [0.249]	438 (0.51) [0.317]
Total	95 (0.111) [0.174]	764 (0.889) [0.826]	859 (1) [1]

N=859. Unweighted proportions are in parentheses, and weighted proportions are in brackets. See notes to Table 1.

### 3 Valid $t$ -based Inference: Theoretical Results and Empirical Implications

This section states our main theoretical findings and defers more detailed discussion of derivations and how our findings connect to the existing econometric literature to Section 4. In order to make the theory readily accessible to applied researchers, we state our findings with minimal formalism, also deferring details and nuances of the results to Section 4. Whenever possible, we illustrate the practical implications of these results on the sample of studies described in Section 2. We focus on tests at the 5 percent level of significance and the corresponding 95 percent confidence interval because this is a commonly-reported standard used in applied research. However, we also report selected findings for 1 percent tests and 99 percent confidence intervals. It will be clear in Section 4 that our formulas can be used to analyze other levels of significance or confidence levels.

We begin by stating which restrictions on the data-generating process – over and above the textbook assumptions  $COV(Z, u) = 0$  and  $COV(Z, X) \neq 0$  – are sufficient so that  $t$ -ratio-based inference procedures have approximately correct size and coverage in (arbitrarily large) finite samples. We then focus on the common practice of

using the first-stage  $F$  for the purposes of making inferences about  $\beta$ . Specifically, the results of Stock and Yogo (2005) provide a numerical threshold (e.g., 10) for the purposes of making inferences about the strength of the instrument  $Z$ , defining instrument “weakness” according to the particular level of distortion – the degree of over-rejection beyond the desired Type I error. Here, we take this now-widespread notion of a single threshold for  $F$  as given, and explicitly incorporate that threshold into an inference procedure on the parameter of interest  $\beta$ . In particular, we seek procedures that have zero distortion.

Finally, motivated by the findings below that these simple adjustments to common procedures greatly alter the width of the confidence intervals, we maintain the notion of incorporating the first-stage  $F$  statistic for inference on  $\beta$ , and propose an extension to gain improvements in power.

### **3.1 Sufficient and Necessary Assumptions for Valid Inference: *t*-ratio only**

As shown in Table 1, about one-quarter of the specifications reported in our sample of published *AER* papers do not report enough information to compute the first-stage  $F$ .

From Dufour (1997), we know that any finite critical value for the  $t$ -ratio will lead to over-rejection for certain values of the model’s nuisance parameters. So, we start by seeking specific restrictions on the nuisance parameters that will allow standard  $t$ -ratio inference to achieve correct size and confidence level. There are two key (generally unknown) nuisance parameters to consider,  $E[F]$  where  $F$  is the first-stage  $F$ -statistic, and  $\rho \equiv \text{Corr}(u, v)$  where  $u \equiv y - \alpha - x\beta$  and  $v \equiv X - Z\pi$ .

Next we explore precisely what restrictions would be sufficient or necessary so that using the usual critical value of 1.96 would result in correct size (and coverage rates for confidence intervals). To gain some intuition for potential restrictions on  $E[F]$ , note that when instruments are weak (corresponding to small values of  $E[F]$ ), the size of a conventional  $t$ -test with critical value 1.96 can be arbitrarily close to one. On the other hand, when the instruments are especially strong (large values of  $E[F]$ ), the size of the conventional  $t$ -test with critical value 1.96 will be

arbitrarily close to its nominal size of 5 percent. Perhaps surprisingly, the change in the size of the conventional  $t$ -test with critical value 1.96 as we move from weak to strong instruments is not monotonically decreasing, which leads to the following characterization.

**Result 1a:** *In addition to the IV model in (1), consider the restriction that  $E[F] \geq \bar{F}$ . The smallest value of  $\bar{F}$  such that  $\Pr[t^2 > 1.96^2] \leq .05$  is 142.6.*

This means that in the absence of the first-stage  $F$  statistic, if researchers wish to claim that their use of the  $t$ -ratio or confidence intervals using  $1.96 \cdot SE(\hat{\beta}_{IV})$  delivers correct size and coverage, they could assume (without evidence) that the true mean of  $F$  from their data is greater than 142.6. The flip side of this statement is that if the truth is that  $E[F] < 142.6$ , there is potential to reject the null at a rate higher than the desired nominal rate. In the extreme, the probability of rejection can be arbitrarily close to 1.<sup>8</sup>

As shown in Figure 1, of the specifications for which the  $F$  statistic is available, most are below 142.6, which indicates that it might be tenuous to assume  $E[F] > 142.6$  for those studies that do *not* report the first-stage  $F$ .<sup>9</sup> At a minimum, there is every indication that such an assumption could be quite restrictive in practice.

Next we consider restrictions on the other key nuisance parameter,  $\rho$ . The following result presents an alternative to Result 1a, but focused on  $\rho$  instead of  $E[F]$ .

**Result 1b:** *In addition to the IV model in (1), consider the restriction that  $|\rho| < \bar{\rho}$ . The largest value of  $\bar{\rho}$  such that  $\Pr[t^2 > 1.96^2] \leq .05$  is 0.565.*

In words, Result 1b says that if a researcher is willing to assume that the degree of endogeneity is not too large, one can remain agnostic about  $E[F]$  (and even allow for non-identification, i.e.,  $E[F] = 1$ ), and still correctly make the claim that the usual  $t$ -ratio procedure under the null hypothesis rejects no more than 5 percent of the time.

**Remark.** The above conditions are sufficient for valid inference, and they are conditions based on constant thresholds. However, in principle there are combina-

<sup>8</sup>As noted, the probability of rejection is 1 when the degree of endogeneity is maximized (i.e.,  $|\rho| = 1$ ) and the instrument is completely uncorrelated with  $X$  (i.e.,  $E[F] = 1$ ). When these conditions are nearly true, then the rejection probability is nearly 1.

<sup>9</sup>To be clear, Figure 1 of course does not furnish a *proof* regarding any population concept, including  $E[F]$ , and the studies that do and do not report  $F$  are not necessarily similar.

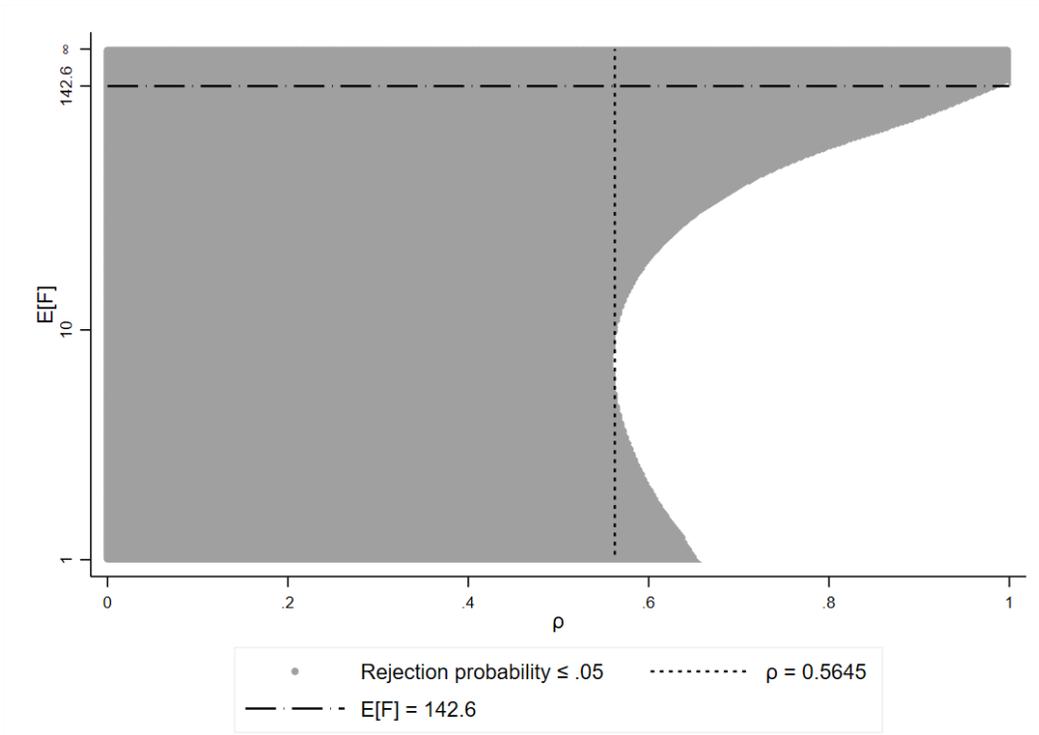
tions of  $\rho, E[F]$  under which  $\Pr[t^2 > 1.96^2] \leq .05$ , even if either one of the nuisance parameters does not fulfill the restrictions in Results 1a or 1b. The full set of combinations of values is depicted in Figure 2a; this figure is constructed using the derivations described in Section 4. If the  $t$ -ratio procedure were valid, the entire region would be shaded. Hence, the figure illustrates in a precise way the inferential limitations of the conventional  $t$ -ratio alone: applied researchers may well consider assumptions about  $\rho$  or  $E[F]$  to be unpalatable, perhaps undermining the original appeal of the instrumental variable strategy (which is typically intended to allow one to be agnostic about  $\rho$ ) in the first place. Figure 2a also shows immediately why hard threshold rules such as  $E[F] > 142.6$  or  $|\rho| < 0.565$  work to restore size/coverage in the IV model in (1). That is, all of the region to the left of the vertical line superimposed at  $|\rho| = 0.565$  is shaded, and all of the region above the horizontal line superimposed at  $E[F] = 142.6$  is shaded.

In light of these results that show over-rejection for a nontrivial region of the nuisance parameter space, it is tempting to conclude that a simple and practical approach to avoiding these problems is to adopt a “higher standard” of statistical significance. That is, one could use the procedure with the conventional 1 percent level critical value 2.58, and confidence intervals based on  $\pm 2.58 \cdot \hat{SE}(\hat{\beta}_{IV})$ . The next result shows that this approach does not, in fact, solve the size/coverage distortions discussed above. Moreover, a restriction of the parameter space for  $E[F]$  no longer works at the 1 percent level.

**Result 1c:** *For the 1 percent level of significance, there exists no  $\bar{F}$  such that  $\Pr[t^2 > 2.58^2] \leq 0.01$  for all  $E[F] \geq \bar{F}$ , and the largest  $\bar{\rho}$  such that  $\Pr[t^2 > 2.58^2] \leq 0.01$  for all  $|\rho| \leq \bar{\rho}$  is 0.43. The full set of values of  $|\rho|, E[F]$  for which  $\Pr[t^2 > 2.58^2] \leq 0.01$  is illustrated in Figure 2b.*

In Figure 2b, the shaded region is entirely contained within that of Figure 2a, indicating that the adoption of the 1 percent significance level requires *stronger* assumptions about the nuisance parameters for valid inference. In this sense, applied researchers should consider the use of the conventional critical values to be even more dubious at the 1 percent than at the 5 percent level.

Figure 2a: Combinations of  $E[F]$ ,  $\rho$  for  $Pr[t^2 > 1.96^2] \leq 0.05$



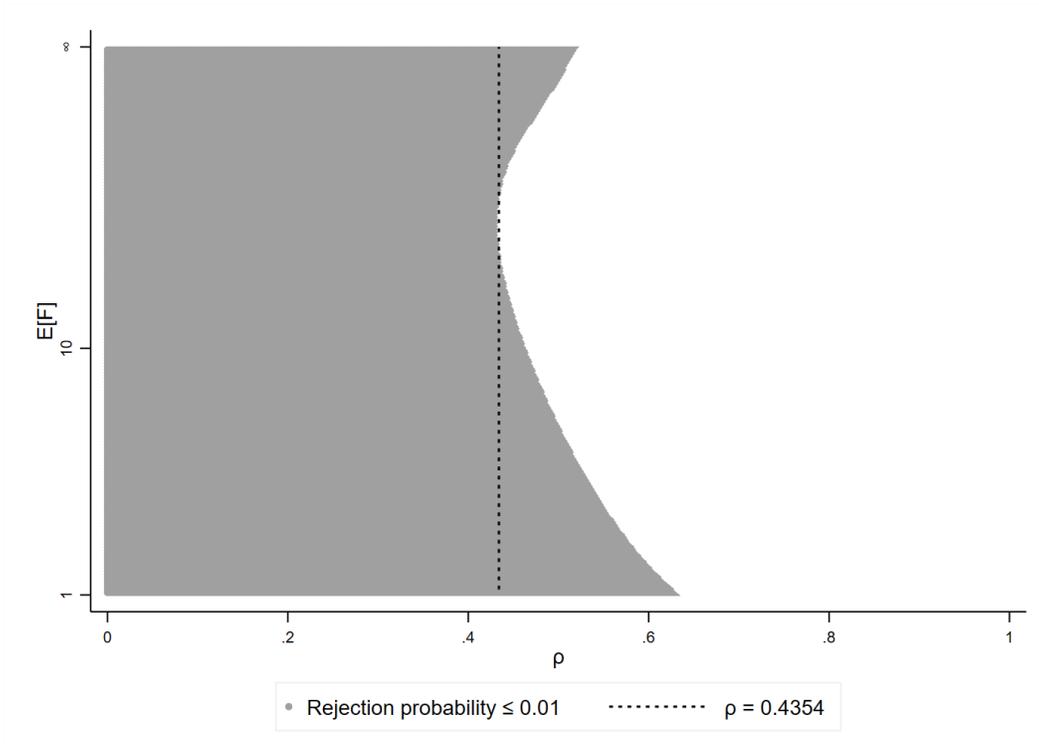
Vertical axis scale uses the transformation  $\frac{E[F]}{1 + \frac{E[F]}{10}}$ . Shaded region represents all combinations of  $E[F]$  and  $\rho$  such that the rejection probability is less than or equal to 0.05. Dashed line is the maximum  $\rho$  such that the region to the left is shaded. Horizontal dashed line (at 142.6) is the minimum  $E[F]$  such that the region above is shaded. The rejection probabilities for  $\rho < 0$  mirror those for  $\rho > 0$ .

### 3.2 $t$ -ratio-based Inference on $\beta$ Using Thresholds for $F$

We now turn to inference on  $\beta$  for studies where we observe or can infer the first-stage  $F$  statistic from the published tables. As is apparent from Table 1, this occurs for about  $(100 - 23.1 - 5.6 =) 71$  percent of the specifications in our sample.

It is now common practice for researchers to use the first-stage  $F$  statistic to assess “instrument strength.” The idea is that if  $F$  is sufficiently large, then the size or coverage distortions caused by using the usual 1.96 critical values can be expected to be “small.” Stock and Yogo (2005) make this concept precise by categorizing situations by the magnitude of  $E[F]$ , associating the degree of distortion with the threshold that separates “weak” and “strong” instruments. As an example, Stock

Figure 2b: Combinations of  $E[F]$ ,  $\rho$  for  $Pr[t^2 > 2.58^2] \leq 0.01$



Vertical axis scale uses the transformation  $\frac{E[F]}{10 + \frac{E[F]}{10}}$ . Shaded region represents all combinations of  $E[F], \rho$  such that the rejection probability is less than or equal to 0.01. Dashed line is the maximum  $\rho$  such that the region to the left is shaded.

and Yogo (2005) provide a critical value for the  $F$  statistic for testing the null hypothesis of a “weak instrument” at the 5 percent level of significance, where “weak instrument” could be defined as an instrument with  $E[F] \leq \bar{F}$ , so that if  $E[F] > \bar{F}$ , the test would over-reject by no more than 5 percent, so that the overall “worst case” rejection rate is 10 percent. In the case of a single instrument, the tables in Stock and Yogo (2005) indicate that the critical value for the first-stage  $F$  would be 16.38.

Stock and Yogo (2005) are careful to make the distinction between testing a null hypothesis about the weakness of an instrument, and the explicit use of the  $F$  statistic in making a decision about the hypothesized value of  $\beta$  (i.e. accept or reject). Stock and Yogo (2005) do not explicitly state what to conclude about  $\beta$  if  $F < 16.38$ , and in a recent survey of the literature, Andrews, Stock and Sun (2019)

demonstrate clearly that if the threshold for the first-stage  $F$  is used as a “screen” (where the study is abandoned if the  $F$  is not sufficiently large), size distortions will be exacerbated.

It appears that while these nuances are clear to those familiar with the theoretical literature, it is less clear that applied research has internalized these distinctions. In practice, applied research may be loosely interpreting the  $IV$   $t$ -ratio using 1.96 critical values as producing a 5 percent test “approximately” as long as  $F$ , for example, is greater than 10.<sup>10</sup>

We thus begin with a benchmark, reporting the actual significance level of the inference procedure commonly used in current practice.

**Result 2a:**  $\Pr [\{t^2 > 1.96^2\} \cap \{F > 10\}] \leq 0.113$  *for all values of  $\rho, E[F]$ . This implies that confidence intervals are  $\hat{\beta}_{IV} \pm 1.96 \cdot \hat{SE}(\hat{\beta}_{IV})$  when  $F \geq 10$  and  $(-\infty, \infty)$  when  $F < 10$ , and should be interpreted as 88.7 percent confidence intervals.*

Although the commitment to automatically accept the null hypothesis (or, equivalently, using the entire real line as the confidence region) when  $F < 10$  will seem unpalatable to practitioners, this is a necessary consequence of adopting a single threshold for  $F$  and a single critical value,  $1.96^2$  for  $t^2$ . Other rules for dealing with  $F < 10$  will necessarily only raise the size of the test procedure even more. For example, if one “throws away the data” if  $F < 10$ , then we obtain even more distortion of size, as illustrated through simulation by Andrews, Stock and Sun (2019). On the other hand, if one uses a finite critical value for  $t^2$  whenever  $F < 10$ , then maximum rejection probabilities will naturally be even greater than 0.113. Another possibility is to use the Anderson-Rubin test when  $F < 10$ . This procedure is considered in Result 2d below.

Since in applied research, it is common to report 95 percent confidence intervals, we turn to the question of what threshold for  $F$  would be high enough to ensure that its use in inference would deliver the correct size of 0.05.

**Result 2b:**  $\Pr [\{t^2 > 1.96^2\} \cap \{F \geq 104.7\}] \leq 0.05$  *for all values of  $\rho, E[F]$ .*

<sup>10</sup>The rule of thumb of 10 for the  $F$  statistic has a somewhat different motivation for Stock and Yogo (2005) from what seems to be perceived in applied research. The origin of the threshold 10 is related to controlling the bias of 2SLS estimators relative to the bias in the OLS.

Table 2b: Impact of Corrected Threshold for F and Critical Value for  $t^2$

	10<F<104.7	F≥104.7	Total
$t^2 \geq 3.43^2$	88 (0.235) [0.235]	75 (0.2) [0.162]	163 (0.435) [0.397]
$1.96^2 < t^2 < 3.43^2$	132 (0.352) [0.247]	80 (0.213) [0.355]	212 (0.565) [0.603]
Total	220 (0.587) [0.482]	155 (0.413) [0.518]	375 (1) [1]

N=375. Specifications are a subset of specifications from Table 2a. Unweighted proportions are in parentheses, and weighted proportions are in brackets. See notes to Table 1.

Therefore, investigators who wish to conduct inference at the 5 percent level using the usual  $1.96^2$  critical value and a single threshold for the  $F$  statistic will need to use a threshold of 104.7. Once again, using 104.7 as this threshold requires an infinite critical value for  $t^2$  when  $F < 104.7$ . In our sample of studies, this valid procedure has a dramatic impact on the conclusion of statistical significance, as shown in Table 2b. Of the studies that would typically be considered (erroneously) “statistically significant” at the 5 percent level, when the correct threshold in Result 2b is applied, about half become insignificant. From a practical perspective, there are two ways of viewing these results. On the one hand, there are studies that utilize instruments that result in  $F$  statistics that turn out to be greater than 104.7. For these studies, the conclusion about statistical significance does not change. On the other hand, this procedure also requires inflating the confidence intervals for those specifications with  $F < 104.7$  to be the entire real line, making all the results below the threshold statistically insignificant.

**Remark.** Given Result 1c, it is not surprising that a result analogous to Result 2b is not available at the 1 percent level. In particular, in the online appendix we show that there exists no finite threshold for  $F$  that delivers correct size for a 1 percent test using the usual critical values of  $\pm 2.575$ . More generally, we find that no such threshold for  $F$  exists for any  $\alpha$  for which  $q_{1-\alpha} > 4$  where  $q_{1-\alpha}$  is

the  $(1 - \alpha)$ th quantile of a  $\chi^2(1)$  distribution. So, there exists no threshold for  $F$  that will yield correct size when combined with a conventional  $t$ -ratio test for any  $\alpha < .0455$ .

There is another way to adjust the procedure so that it can be interpreted as a 5 percent test: raising the critical value for  $t^2$ .

**Result 2c:**  $\Pr[\{t^2 > 3.43^2\} \cap \{F > 10\}] \leq 0.05$  for all values of  $\rho, E[F]$ .

The threshold of 10 here is a rule of thumb introduced by Staiger and Stock (1997), and we focus on it because of its common appearance in textbooks and published research. The impact of this alternative rule is also illustrated in Table 2b. This time, while the rule does not render any of the studies insignificant by virtue of the  $F$  statistic, it instead renders 60 percent of the studies insignificant by virtue of the  $t^2$  statistic not exceeding  $3.43^2$ . Another way of interpreting this adjustment is that if one maintains the threshold of 10 for  $F$ , to obtain confidence intervals with 95 percent coverage, one must accept intervals that are larger by a factor of  $\frac{3.43}{1.96} \approx 1.74$ .

Finally, we address one other possible use of a single threshold for the  $F$  statistic. It might seem intuitive to construct a rule so that one uses a procedure that has correct size (e.g. Anderson and Rubin (1949)) if  $F$  is less than some  $\bar{F}$ , but uses the usual  $t$ -ratio procedure with  $\pm 1.96$  critical values when  $F$  exceeds  $\bar{F}$ . This idea is discussed in Andrews, Stock and Sun (2019).

**Result 2d:** Let  $AR$  be the statistic of Anderson and Rubin (1949). There exists no finite threshold  $\bar{F}$  such that  $\Pr[\{t^2 > 1.96^2\} \cap \{F \geq \bar{F}\}] + \Pr[\{AR > 1.96^2\} \cap \{F < \bar{F}\}] \leq 0.05$  for all values of  $\rho, E[F]$ .

This is an impossibility result that says that this “hybrid” test procedure cannot achieve the intended size of 0.05.

### 3.3 Using the $t$ and $F$ statistics: the “ $tF$ ” test procedure

In light of the findings above, we now propose a logical extension to the notion of using the first-stage  $F$ , as developed by both Staiger and Stock (1997) and Stock and Yogo (2005). Henceforth, we call this the “ $tF$ ” test procedure, which uses the usual  $t$  and first-stage  $F$  statistics, and rejects the null hypothesis if and only if

$t^2 > c(F)$ , where we graphically depict  $c(F)$  here and precisely define the function in Section 4.

There are three motivations for this procedure. First, in light of our discussion above, the use of hard threshold rules that are commonly used in empirical research poses a practical conundrum. In order for the standard  $t$ -ratio inference procedure to have correct size, researchers must pre-commit to infinitely wide confidence intervals if they observe  $F < 104.7$ . But in practice, we suspect that researchers would abandon the use of the instrument if they found this to be true, rather than report an interval of  $(-\infty, \infty)$ . Unfortunately, this practice in repeated samples will tend to truncate specifications in which  $F < 104.7$  and lead to the sort of distortion discussed by Andrews, Stock and Sun (2019).

Second, as can be seen by comparing the procedures in Results 2b and 2c, there is a trade-off between a threshold for  $F$  and the necessary critical value for  $t^2$  that would control rejection probabilities under all values of the nuisance parameters. Thus, it is intuitive to consider a logical extension – a frontier represented by a decreasing function  $c(F)$  in  $F$  for  $t^2$ , which can yield improvements in power compared to the single threshold rules of common practice.

Finally, in our full sample where the statistic is available, the  $F$  exceeds 104.7 about 43 percent of the time, which leaves 57 percent that do not meet the threshold. What are we to make of those studies? One could argue that these studies *should have* used the procedure of Anderson and Rubin (1949) in the first place, given its optimality properties (see e.g. Moreira (2002), Moreira (2003), Andrews, Moreira and Stock (2006), Moreira and Moreira (2019)). In addition, there may well be hundreds of studies – beyond our sample of *AER* papers – that also did not use Anderson and Rubin (1949), and instead reported the statistics  $t$  and  $F$ . It is likely to be quite costly or prohibitive to revisit these studies to recompute *AR* tests and confidence regions to obtain valid inference. Therefore, there is a practical payoff to having the option of re-assessing statistical significance and re-computing true 95 percent confidence intervals using only the already-reported  $t$  and  $F$  statistics in published studies, but doing so in a way that improves upon confidence intervals of  $(-\infty, \infty)$  when  $F < 104.7$ . The next result states that such an improvement is possible.

**Result 3:** *Let  $c(F)$  be defined by the equations in Subsection 4.3. Then  $\Pr [t^2 > c(F)] \leq 0.05$  for all  $\rho, E[F]$ .  $c(F)$  is graphically illustrated in Figure 3, with selected numerical values shown in Table 3.*

The critical value function  $c(F)$  is simply the logical extension and smooth generalization of the decision rules stated in Results 2a, 2b, and 2c. It is worth noting that while the function  $c(F)$  is an “ $F$ -dependent critical value for  $t^2$ ,” the probability statement in Result 3 is unconditional and reflects the joint distribution of  $t^2$  and  $F$ .<sup>11</sup>

Table 3 provides the values of  $\sqrt{c(F)}$  for values of  $\sqrt{F}$  from 2.0 to 9.9 for tests at the 5 percent level. Many of the entries are quite different from the usual 1.96 critical value.<sup>12</sup> For example, if one observes an  $F$  statistic of 6.25, then the table under the entry  $\sqrt{F} = 2.5$  shows a critical value of 4.92, so that a valid 95 percent confidence interval is  $\hat{\beta}_{IV} \pm 4.92 \cdot \hat{SE}(\hat{\beta}_{IV})$ . As another example, an  $F$  statistic of 49 would imply that the critical value (for  $\sqrt{F} = 7$ ) would be 2.16, leading to approximately a 10 percent wider confidence interval than is conventionally reported.

For an assessment of how this function performs in practice compared to the rules described in Results 2b and 2c, Figure 3 plots all of the specifications from Table 2a in  $t^2, F$  space (using the one-to-one transformations  $\frac{t^2}{1 + \frac{1.96^2}{10}}$  and  $\frac{F}{1 + \frac{F}{10}}$  for the vertical and horizontal scales). The size of each circle is proportional to the weights used in all of our tables. The figure provides a visualization of the consequences of adopting the decision rules of Results 2b and 2c. In the former case, it results in designating all of the specifications in the  $t^2 > 1.96^2, 10 < F < 104.7$  region (48 percent) as statistically insignificant. In the latter case, valid inference requires designating the specifications in the  $1.96^2 < t^2 < 3.43^2, F > 10$  region (60 percent) as statistically insignificant. Meanwhile, the use of the  $c(F)$  critical value function leads to a noticeable but considerably smaller proportion of specifications that would be rendered insignificant (21 percent).

Table 3 can be used directly for applied research. It is both convenient and commonly observed in published research to report the coefficient and standard error

<sup>11</sup>In other words, we are not referring to  $\Pr [t^2 > c(F_0) | F = F_0]$ .

<sup>12</sup>We have rounded all of our computed numbers to two decimal places, always rounding up, to produce slightly conservative critical values.

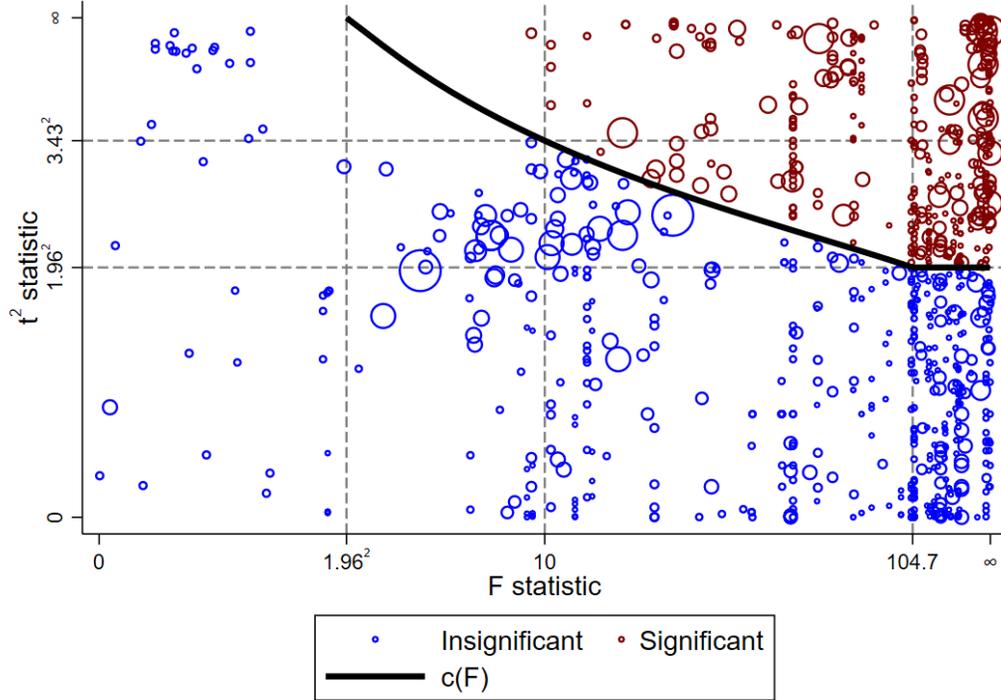
Table 3: Critical Value Table for  $c(F)$

		$x$								
		$1$	$2$	$3$	$4$	$5$	$6$	$7$	$8$	$9$
$y$	$0$	$\infty$	18.66	3.65	2.80	2.46	2.27	2.16	2.08	2.02
	$1$	$\infty$	9.74	3.51	2.75	2.43	2.26	2.15	2.07	2.01
	$2$	$\infty$	7.37	3.39	2.71	2.41	2.24	2.14	2.06	2.01
	$3$	$\infty$	6.18	3.29	2.67	2.39	2.23	2.13	2.06	2.00
	$4$	$\infty$	5.43	3.19	2.63	2.37	2.22	2.12	2.05	2.00
	$5$	$\infty$	4.92	3.11	2.60	2.35	2.21	2.11	2.04	1.99
	$6$	$\infty$	4.54	3.03	2.57	2.33	2.20	2.10	2.04	1.99
	$7$	$\infty$	4.25	2.97	2.54	2.32	2.19	2.10	2.03	1.99
	$8$	$\infty$	4.01	2.91	2.51	2.30	2.17	2.09	2.03	1.98
	$9$	$\infty$	3.82	2.85	2.48	2.29	2.16	2.08	2.02	1.98

This table contains critical values for  $|t|$  at the 5 percent significance level. The critical value associated with a given  $F$  statistic is contained in the cell, with the column corresponding to the integer part of  $\sqrt{F}$ , denoted  $x$ , and the row corresponding to the decimal part of  $\sqrt{F}$ , denoted  $y$ .

as a way to compactly provide sufficient information to conduct a test of a null hypothesis or to report a confidence interval. Table 3 facilitates reporting a corrected standard error – which could be reported as the " $tF$  0.05 standard error." As an example, if an individual computed a point estimate of 3.2, with a (conventionally computed) standard error of 1.5, and the first-stage  $F$  statistic of 9, then the entry under 3.0 for  $\sqrt{F}$  is 3.65, which means that the usual standard error would be inflated by a factor of  $\frac{3.65}{1.96}$ , yielding a " $tF$  0.05 standard error" of  $1.5 \times 1.862 \approx 2.79$ . The convexity of these critical values with respect to  $F$  suggests that one could reasonably use linear interpolation for values in between the values of  $\sqrt{F}$  reported in the table, knowing that the resulting interpolated values would be slightly conservative, relative to the true value. A different calculation (which can be obtained via the formulas we describe below) would be needed for a " $tF$  0.01 standard error" or for other levels of significance.

Figure 3: Statistical significance for AER studies:  $c(F)$  and single threshold/critical-value rules



$N=859$  specifications. Vertical scale is  $\frac{t^2}{1+\frac{t^2}{1.96^2}}$  and horizontal scale is  $\frac{F}{1+\frac{F}{10}}$ . Size of circle is proportional to the weight described in Table 1. Solid line is the  $c(F)$  function at the 0.05 level. Red and blue circles are those that are statistically significant and insignificant, respectively, according to  $c(F)$ .

## 4 Derivations of Results and Discussion

This section provides details on how we arrive at the conclusions presented in Section 3. The online appendix contains more detail on the formal results and general characterizations of the procedures considered.

We begin with a statement of the structural and first-stage equations including

additional covariates:

$$\begin{aligned} Y &= X\beta + W\gamma + u \\ X &= Z\pi + W\xi + v \end{aligned}$$

where  $W$  denotes the additional covariates which can include a one corresponding to an intercept term. Without loss of generality, we assume orthogonality between  $Z$  and  $W$ .<sup>13</sup>

Next we define our key statistics:

$$\begin{aligned} t &\equiv \frac{\hat{\beta}_{IV} - \beta}{\sqrt{\hat{V}_N(\hat{\beta}_{IV})}} \\ t_{AR} &\equiv \frac{\hat{\pi}(\hat{\beta}_{IV} - \beta)}{\sqrt{\hat{V}_N(\hat{\pi}(\hat{\beta}_{IV} - \beta))}} = \frac{\hat{\pi}(\hat{\beta}_{IV} - \beta)}{\sqrt{\hat{V}_N(\hat{\pi}\hat{\beta}) - 2\beta\widehat{COV}_N(\hat{\pi}\hat{\beta}, \hat{\pi}) + \beta^2\hat{V}_N(\hat{\pi})}} \\ f &\equiv \frac{\hat{\pi}}{\sqrt{\hat{V}_N(\hat{\pi})}} \\ \hat{\rho} &\equiv \frac{\widehat{COV}(Z(Y - X\beta), Z(X - Z\hat{\pi}))}{\sqrt{\widehat{VAR}(Z(Y - X\beta)) \cdot \widehat{VAR}(Z(X - Z\hat{\pi}))}} \\ \rho &\equiv \frac{COV(Z(Y - X\beta), Z(X - Z\pi))}{\sqrt{VAR(Z(Y - X\beta)) \cdot VAR(Z(X - Z\pi))}} \end{aligned}$$

$\hat{\beta}_{IV}$  is the instrumental variable estimator, and  $\beta$  is the parameter of interest.  $\hat{V}_N(\hat{\beta}_{IV})$  represents the estimated variance of  $\hat{\beta}_{IV}$ , which can be a consistent robust variance estimator to deal with departures from i.i.d. errors, including one- or two-way clustering (e.g. see Cameron, Gelbach and Miller (2011)). Then  $t$  is the usual  $t$ -ratio.  $t_{AR}$  is a “ $t$ -ratio form” of the statistic of Anderson and Rubin (1949), denoted  $AR$ ; that is,  $t_{AR}^2 = AR$ .<sup>14</sup> The denominators of  $t_{AR}$  and  $AR$  both depend on  $\beta$ . Our analysis proceeds under the null where the true value  $\beta$  coincides with its hy-

<sup>13</sup>Orthogonality can always be achieved by setting  $Z$  to be the residual of a regression of the instrument on the covariates  $W$ .

<sup>14</sup>Feir, Lemieux and Marmar (2016), in the context of fuzzy regression discontinuity designs, note that the  $AR$  statistic has a form that resembles the  $t$ -ratio-squared statistic, but with a different variance estimator.

pothesized value.  $\hat{V}_N(\hat{\pi}\hat{\beta})$ ,  $\widehat{COV}_N(\hat{\pi}\hat{\beta}, \hat{\pi})$ , and  $\hat{V}_N(\hat{\pi})$  denote the elements of the estimated variance-covariance matrix of the least squares coefficients  $\hat{\beta}_{IV}\hat{\pi} = \widehat{\beta}_{IV}\widehat{\pi}$  (the “reduced form” coefficient) and  $\hat{\pi}$  (the first-stage coefficient). Again, this notation allows for the use of a consistent robust variance-covariance estimators to reflect non-i.i.d. reduced form and first-stage errors.  $f$  is the  $t$ -ratio (for the null hypothesis that  $\pi = 0$ ) for the first-stage coefficient, and its square is equal to the  $F$  statistic.  $\hat{\rho}$  is the empirical correlation between  $Z(Y - X\beta)$  and  $Z(X - Z\hat{\pi})$ , and finally  $\rho$  is the unknown population correlation between  $Z(Y - X\beta)$  and  $Z(X - Z\pi)$ . If we were to consider variance formulas appropriate for homoskedastic models, we would simply remove the “ $Z$ ” in the definitions of  $\hat{\rho}$  and  $\rho$ , and  $\rho$  would have the interpretation of the population correlation between  $u$  and the first-stage residual  $(X - Z\pi)$ . The formulation here accommodates general consistent robust variance estimators in forming  $\hat{\rho}$ .

The results from Section 3 rely on two relations. First, there is a numerical equivalence between the conventional  $t$ -ratio and the three quantities  $t_{AR}$ ,  $f$ , and  $\hat{\rho}$ :<sup>15</sup>

$$t^2 = \frac{t_{AR}^2}{1 - 2\hat{\rho}\frac{t_{AR}}{f} + \frac{t_{AR}^2}{f^2}}. \quad (2)$$

The result is exact, and can be proven through straightforward algebraic manipulation using the definitions introduced at the beginning of this section.<sup>16</sup> The equivalence in (2) illustrates the incongruency between the claims of valid inference of procedures based on  $AR$  and  $t^2$ , both of which are presumed to be approximately distributed as  $\chi^2(1)$ . As is apparent in equation (2), the two statistics converge as  $f$  increases. However, if  $f$  has a non-degenerate distribution, then these two statistics will not generally be simultaneously distributed  $\chi^2(1)$ . Since  $t_{AR}$  is by definition a linear combination of a pair of least squares coefficients,  $AR = t_{AR}^2$  is generally accepted as the one that is well-approximated by a  $\chi^2(1)$  distribution. The question then is how distorted the conventional  $t$ -ratio procedure is.

<sup>15</sup>See online appendix for derivation of (2).

<sup>16</sup>Note, however, that it is important to use the “signed” versions of  $t_{AR}$  and  $f$  precisely as introduced in this section.

The second element that underpins our calculations follows from standard asymptotic arguments that imply that

(a)

$$\text{plim } \hat{\rho} = \rho; \tag{3}$$

and

(b) the vector  $(t_{AR}, f - f_0)'$  is well-approximated by a bivariate normal distribution:

$$N \left( \begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} 1 & \rho \\ \rho & 1 \end{pmatrix} \right) \tag{4}$$

where  $f_0 \equiv \frac{\pi}{\sqrt{\frac{AV(\hat{\pi})}{N}}}$  and  $AV(\hat{\pi})$  is the asymptotic variance of the first-stage coefficient estimator. This directly follows from the notion that the vector  $(\widehat{\pi\beta_{IV}}, \hat{\pi})$  is well-approximated by a bivariate normal since  $(t_{AR}, f - f_0)$  is a linear transformation of  $(\widehat{\pi\beta_{IV}}, \hat{\pi})$ .<sup>17</sup> Since  $f$  is normal with unit variance and mean  $f_0$ , then  $F$  is distributed with non-central chi-squared distribution with noncentrality parameter  $f_0^2$  and mean  $E[F] = 1 + f_0^2$ . When performing inference, equation (4) will provide the approximating distribution “under the null.” Under the alternative,  $t_{AR}$  will have a non-zero mean.

Together, (2), (3) and (4) allow us to examine the distortion of the  $t$ -ratio and other related procedures by computing rejection probabilities that use  $t$  and  $F$ , for any given value of the nuisance parameters  $\rho$  and  $f_0$ . These calculations are straightforward by first using (2) to determine the rejection region in the  $(t_{AR}, f)$  space and then applying the approximations in (3) and (4) to obtain the corresponding probability.

<sup>17</sup>This approximation is implicitly justified when the parameter  $\pi$  is a sequence that shrinks to zero as sample size increases so that  $f_0$  converges to a nonzero constant, as in the weak IV asymptotics approach (Staiger and Stock, 1997) that is commonly employed in the theoretical literature.

#### 4.1 $t$ -ratio procedure without $F$ : $\Pr [t^2 > q_{1-\alpha}]$

The probability of rejection under the usual  $t$ -ratio procedure is

$$\Pr [t^2 > q_{1-\alpha}]$$

where  $q_{1-\alpha}$  is the  $(1 - \alpha)$ th quantile of a  $\chi^2(1)$  distribution, where  $\alpha$  is the desired level of significance.

Using the relations in (2) and (3), this probability can be expressed as

$$\Pr \left[ \frac{t_{AR}^2}{1 - 2\rho \frac{t_{AR}}{f} + \frac{t_{AR}^2}{f^2}} > q_{1-\alpha} \right].$$

This probability is completely characterized by the values  $\beta, \rho$ , and  $f_0$ , and the inequality represents a well-defined area in  $t_{AR}$ - $f$  space, where we know that  $t_{AR}, f$  are distributed as a bivariate normal. An example of this area (in the case of  $\rho = .8$ ) is depicted by the shaded region in Figure 4, Panel A; the regions naturally will depend on the value of  $\rho$ . The above probability can be expressed as

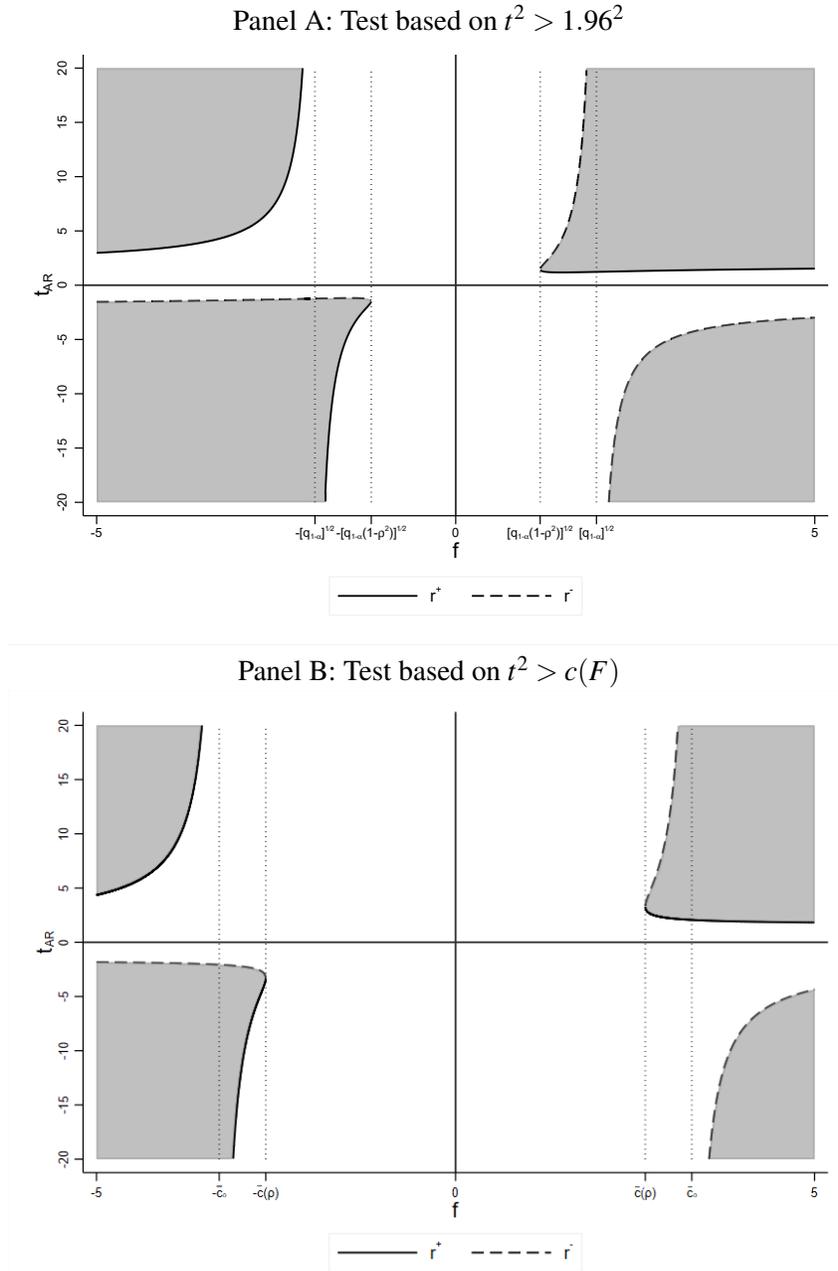
$$\begin{aligned} & \int_{\frac{\sqrt{q_{1-\alpha}}}{\sqrt{q_{1-\alpha}(1-\rho^2)}}}^{\sqrt{q_{1-\alpha}}} \int_{r^+(f)}^{r^-(f)} (\varphi_{f_0, \rho}(t_{AR}, f) + \varphi_{f_0, \rho}(-t_{AR}, -f)) dt_{AR} df \\ & + \int_{\frac{\sqrt{q_{1-\alpha}}}{\sqrt{q_{1-\alpha}(1-\rho^2)}}}^{\infty} \int_{\substack{(-\infty, r^-(f)] \\ \cup [r^+(f), \infty)}} (\varphi_{f_0, \rho}(t_{AR}, f) + \varphi_{f_0, \rho}(-t_{AR}, -f)) dt_{AR} df \end{aligned} \quad (5)$$

where

$$\begin{aligned} r^+(f) &= \frac{-\rho q_{1-\alpha} f + \sqrt{\rho^2 q_{1-\alpha}^2 f^2 + q_{1-\alpha} f^2 (f^2 - q_{1-\alpha})}}{f^2 - q_{1-\alpha}} \\ r^-(f) &= \frac{-\rho q_{1-\alpha} f - \sqrt{\rho^2 q_{1-\alpha}^2 f^2 + q_{1-\alpha} f^2 (f^2 - q_{1-\alpha})}}{f^2 - q_{1-\alpha}} \end{aligned}$$

and  $\varphi_{f_0, \rho}$  is the density of the bivariate normal distribution with mean  $(0, f_0)$ , unit variances with correlation  $\rho$  corresponding to  $(t_{AR}, f)$ . The expressions  $r^+(f)$  and  $r^-(f)$  follow from solving for  $t_{AR}$  since setting  $t^2$  equal to  $q_{1-\alpha}$  implicitly defines

Figure 4: Rejection regions in  $t_{AR}$ - $f$  space for tests at 5% level,  $\rho = .8$

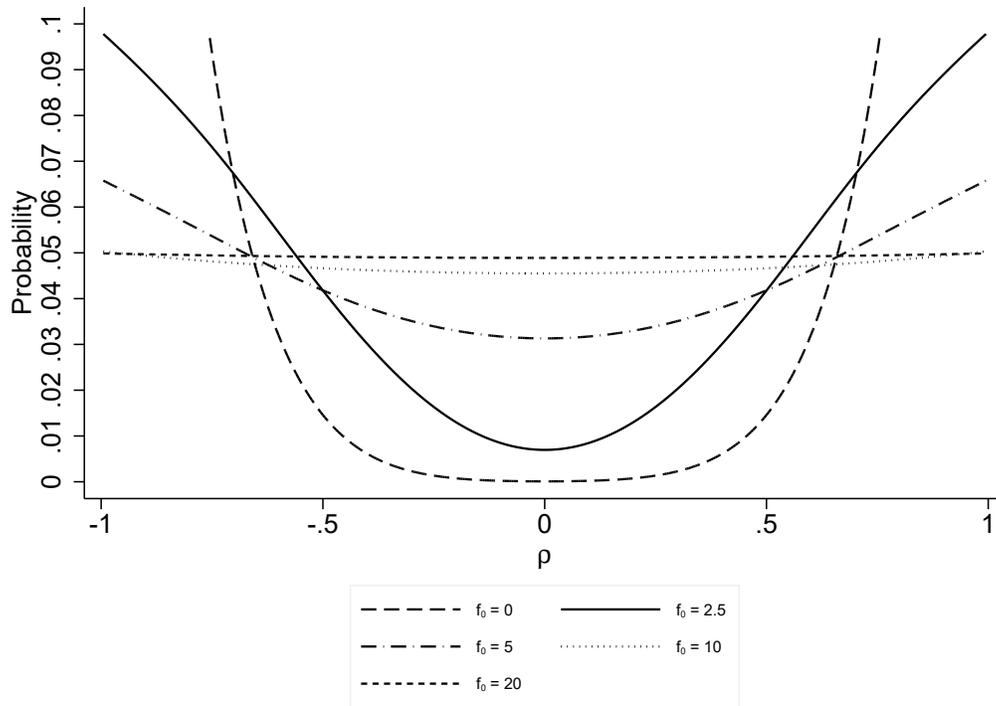


Shaded regions represent values of  $t_{AR}$  and  $f$  that correspond to values of  $t$  such that, for the 5 percent test,  $t^2 > 1.96^2$  in Panel A and  $t^2 > c(F)$  in Panel B.

a quadratic equation in  $t_{AR}$ . As Figure 4, Panel A illustrates, there is symmetry in the rejection regions around the origin, so we have used this symmetry to more compactly express the integral by having the two density terms for each line. Also, note that this formula still works for  $\rho = \pm 1$  when the joint distribution of  $(t_{AR}, f)$  concentrates on a 45 degree line shifted away from the origin (depending on the value of  $f_0$ ). Under a hypothesis for the value of  $\beta$ , the above expression for the probability of rejection can be computed up to an approximation error associated with numerical integration for any given parameters  $\rho, f_0$ .

Results 1a, 1b, and 1c can be derived from examining the surface of rejection probabilities as a function of the nuisance parameters  $f_0$  and  $\rho$ . Figure 5a illustrates some sections of this surface, plotting rejection rates against values of  $\rho$ , for selected values of  $f_0$ . It shows, as expected, that as  $f_0$  tends to zero, rejection rates exceed 0.05 with higher degrees of correlation  $\rho$ . As  $f_0$  rises, the rejection rates tend towards 0.05.

Figure 5a:  $Pr[t^2 > 1.96^2]$ , by  $\rho$  and  $f_0$



## 4.2 $t$ -ratio procedure with thresholds $\bar{c}$ and $\bar{F}$ :

$$\Pr [\{t^2 > \bar{c}\} \cap \{F > \bar{F}\}]$$

Rejection probabilities for  $t$ -ratio procedures with single thresholds for  $F$  and  $t$  can be derived in a parallel way. In essence, this amounts to using a critical value for the  $t$ -ratio (i.e.  $t^2 > \bar{c}$ ) if  $F$  exceeds a particular threshold (i.e.  $\bar{F}$ ), and accepting the null hypothesis otherwise. Specifically,

$$\begin{aligned} & \Pr [\{t^2 > \bar{c}\} \cap \{F > \bar{F}\}] & (6) \\ & = \int_{\sqrt{\bar{c}(1-\rho^2)} \vee (\sqrt{\bar{F}} \wedge \sqrt{\bar{c}})}^{\sqrt{\bar{c}}} \int_{r^+(f)}^{r^-(f)} (\varphi_{f_0, \rho}(t_{AR}, f) + \varphi_{f_0, \rho}(-t_{AR}, -f)) dt_{AR} df \\ & \quad + \int_{\sqrt{\bar{c}} \vee \sqrt{\bar{F}}}^{\infty} \int_{\substack{(-\infty, r^-(f)] \\ \cup [r^+(f), \infty)}} (\varphi_{f_0, \rho}(t_{AR}, f) + \varphi_{f_0, \rho}(-t_{AR}, -f)) dt_{AR} df \end{aligned}$$

This is a simple modification of expression (5) where the limits of integration for  $f$  are changed to reflect the threshold  $\bar{F}$ .<sup>18</sup>

Using this new expression, Result 2a is derived from finding the maximized rejection rate over all values of  $f_0$  and  $\rho$ , when  $\bar{c} = 1.96^2$  and  $\bar{F} = 10$ . Our inspection of this expression shows that for a wide range of values of  $f_0$ , rejection rates are maximized when  $\rho = 1$ . In the  $\rho = 1$  case,  $t_{AR} = f - f_0$  and this perfect correlation leads to the bivariate normal distribution for  $(t_{AR}, f)$  being characterized by a univariate normal distribution. Additionally, with  $\rho = 1$ , it suffices to examine the  $f_0 \geq 0$  case, so the rejection probability expression in (6) can be greatly simplified:

For  $f_0 \geq 0$ ,

$$\begin{aligned} & 1 - \Phi(\bar{r}_A \vee (\sqrt{\bar{F}} - f_0)) + \Phi(r_A \wedge (-\sqrt{\bar{F}} - f_0)) & (7) \\ & + \mathbf{1}\{f_0 > 4\sqrt{\bar{c}}, \sqrt{\bar{F}} - f_0 < \bar{r}_B\} \left[ \Phi(\bar{r}_B) - \Phi(r_B \vee (\sqrt{\bar{F}} - f_0)) \right] \end{aligned}$$

<sup>18</sup>We use the  $\vee$  and  $\wedge$  notation in the limits to denote the maximum and minimum of two arguments.

where  $\mathbf{1}\{\cdot\}$  denotes an indicator function and

$$\begin{aligned}\bar{r}_A &= \frac{-\rho f_0 + \sqrt{f_0^2 + 4|f_0|\sqrt{\bar{c}}}}{2}; & \bar{r}_A &= \frac{-\rho f_0 - \sqrt{f_0^2 + 4|f_0|\sqrt{\bar{c}}}}{2} \\ \bar{r}_B &= \frac{-\rho f_0 + \sqrt{f_0^2 - 4|f_0|\sqrt{\bar{c}}}}{2}; & \bar{r}_B &= \frac{-\rho f_0 - \sqrt{f_0^2 - 4|f_0|\sqrt{\bar{c}}}}{2}\end{aligned}$$

The rejection probability with  $\rho = 1$  and  $f_0 \geq 0$  in (7) can be analyzed as a function of  $f_0$ . It is straightforward to show that this rejection probability has a local maximum at  $f_0^* = \frac{\bar{F}}{\sqrt{\bar{F}} + \sqrt{\bar{c}}}$ . Plugging  $f_0 = f_0^*$  into (7) yields a maximum rejection probability of the form:

$$1 - \Phi\left(\frac{\sqrt{\bar{F}\bar{c}}}{\sqrt{\bar{F}} + \sqrt{\bar{c}}}\right) + \Phi\left(\frac{-\sqrt{\bar{F}\bar{c}} - 2\bar{F}}{\sqrt{\bar{F}} + \sqrt{\bar{c}}}\right) \quad (8)$$

With  $\bar{F} = 10$  and  $\bar{c} = 1.96^2$ , we can verify that this local maximum is a global maximum and yields a rejection rate of

$$1 - \Phi\left(\frac{1.96\sqrt{10}}{1.96 + \sqrt{10}}\right) + \Phi\left(\frac{-20 - 1.96\sqrt{10}}{1.96 + \sqrt{10}}\right) \approx 0.113$$

as stated in Result 2a.

While the fact that there is a distortion in the rejection rates is explicitly discussed by Stock and Yogo (2005) and is understood in the econometric literature, it is not clear that the implications of this nuance have been appreciated in applied work. To put it simply, it may well be that applied researchers used an 11.3 percent test, loosely thinking that they had a 5 percent test. At a surface level, it is understandable why this difference might have been considered "small." The difference between a two-tailed 10 percent and 5 percent test is reflected in the ratio  $\frac{1.96}{1.645}$ . However, as Results 2b and 2c show, in this case, it takes much more to go from a 11.3 to 5 percent test. It raises the necessary threshold for  $F$  from 10 to 104.7 (Result 2b), or it raises the necessary critical value from 1.96 to 3.43 (Result 2c).

Results 2b and 2c use the same equation:

$$.05 = 1 - \Phi\left(\frac{\sqrt{\bar{F}\bar{c}}}{\sqrt{\bar{F}} + \sqrt{\bar{c}}}\right) + \Phi\left(\frac{-\sqrt{\bar{F}\bar{c}} - 2\bar{F}}{\sqrt{\bar{F}} + \sqrt{\bar{c}}}\right)$$

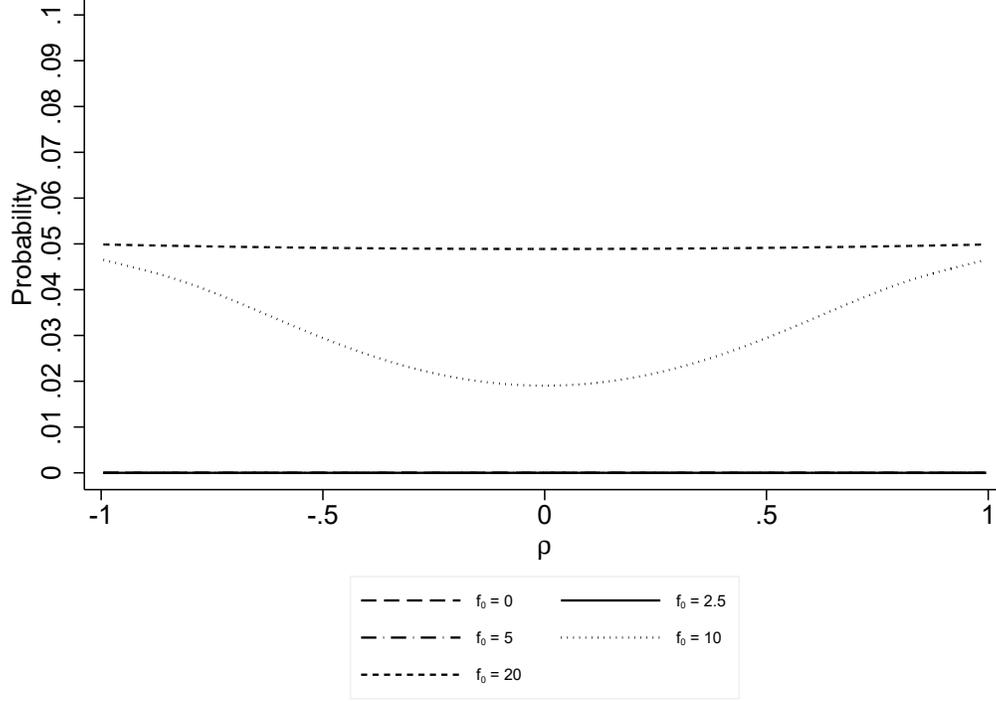
but instead of solving for the rejection rate using known  $\bar{c}$  and  $\bar{F}$ , we fix the rejection rate at 0.05, and solve for the unknown  $\bar{F}$  using  $\bar{c} = 1.96^2$  (Result 2b), or solve for the unknown  $\bar{c}$  using  $\bar{F} = 10$  (Result 2c). Since the rejection probability in (8) (the righthand side of the equation above) is monotonically decreasing in both  $\bar{F}$  and  $\bar{c}$ , it is straightforward to solve these equations numerically.

One can understand the trade-off between  $\bar{c}$  and  $\bar{F}$  in the following way. The actual size of these tests is a summation of the nominal, intended size, and a distortion. Result 2a says that when 10 is used as  $\bar{F}$ , then the distortion is the difference between 11.3 and 5 percent, i.e., 6.3 percent. Result 2b says that one can obtain a 5 percent test by bringing the amount of distortion to zero, via setting  $\bar{F}$  equal to 104.7. However, one could have achieved the same size by using the critical value of 3.43 associated with a 0.06 percent test along with an  $\bar{F}$  of 10. The distortion would be 4.94 percent, leading to a test at the 5 percent level (Result 2c).

Figure 5b provides rejection rates for the test with  $\bar{c} = 1.96^2$  and  $\bar{F} = 104.7$ , following the same layout as Figure 5a. It shows that for large values of  $f_0$  such as 20, as would be expected, the rejection rates are close to 0.05, for any value of  $\rho$ . But for  $f_0$  values of 5 or below, (i.e.,  $E[F] < 26$ ), the rejection probabilities are extremely low. This makes sense because at these values,  $F$  will almost certainly fall below 104.7.

Finally, we consider the procedure of using  $AR$  when  $F \leq \bar{F}$ , and the usual  $t$ -ratio procedure with a critical value of 1.96 when  $F > \bar{F}$ , as described in Result 2d. The expression for the rejection rate involves the addition of another term to (6) to

Figure 5b:  $Pr[\{t^2 > 1.96^2\} \cap \{F > 104.7\}]$ , by  $\rho$  and  $f_0$



account for the additional rejections from AR when  $F \leq \bar{F}$ :

$$\begin{aligned}
 & \Pr [(\{t^2 > \bar{c}\} \cap \{F > \bar{F}\}) \cup (\{t_{AR}^2 > \bar{c}\} \cap \{F \leq \bar{F}\})] \quad (9) \\
 &= \int_{\sqrt{\bar{c}(1-\rho^2)} \vee (\sqrt{\bar{F}} \wedge \sqrt{\bar{c}})}^{\sqrt{\bar{c}}} \int_{r^+(f)}^{r^-(f)} (\varphi_{f_0, \rho}(t_{AR}, f) + \varphi_{f_0, \rho}(-t_{AR}, -f)) dt_{AR} df \\
 &+ \int_{\sqrt{\bar{c}} \vee \sqrt{\bar{F}}}^{\infty} \int_{\substack{(-\infty, r^-(f)] \\ \cup [r^+(f), \infty)}} (\varphi_{f_0, \rho}(t_{AR}, f) + \varphi_{f_0, \rho}(-t_{AR}, -f)) dt_{AR} df \\
 &+ \int_0^{\sqrt{\bar{F}}} \int_{\substack{(-\infty, -\sqrt{\bar{c}}] \\ \cup [\sqrt{\bar{c}}, \infty)}} (\varphi_{f_0, \rho}(t_{AR}, f) + \varphi_{f_0, \rho}(-t_{AR}, -f)) dt_{AR} df
 \end{aligned}$$

with  $\bar{c}$  set to  $1.96^2$ . The additional term in this expression ensures that the rejection

probability in (9) is larger than the analogous rejection probability in (6) regardless of the value of  $\bar{F}$ . Therefore the question is whether there exists an  $\bar{F}$  greater than 104.7 that would control size. Inspection of this function through direct computation reveals that this rejection rate does not fall below 0.05 as  $\bar{F}$  increases, and we demonstrate this analytically in the Appendix. Intuitively, as we increase  $\bar{F}$  under the conventional threshold rule,  $\Pr[\{t^2 > 1.96^2\} \cap \{F > \bar{F}\}]$  must fall, while  $\Pr[\{AR > 1.96^2\} \cap \{F < \bar{F}\}]$  must increase. In fact, plugging  $\bar{c} = 1.96^2$  into the expression in (8) shows the value of  $\Pr[\{t^2 > 1.96^2\} \cap \{F > \bar{F}\}]$  when  $f_0 = f_0^* = \frac{\sqrt{\bar{F}}}{\sqrt{\bar{F}+1.96}}$  and  $\rho = 1$ . Evaluating  $\Pr[\{AR > 1.96^2\} \cap \{F < \bar{F}\}]$  at the same values of  $f_0$  and  $\rho$  yields:

$$\Phi(-1.96) - \Phi\left(\frac{-2\bar{F} - 1.96\sqrt{\bar{F}}}{\sqrt{\bar{F} + 1.96}}\right) \quad (10)$$

for  $\bar{F} \geq \frac{1.96^2}{2}$ . Adding the expressions in (8) and (10) gives the rejection probability  $\Pr[\{t^2 > 1.96^2, F > \bar{F}\} \cup \{AR > 1.96^2, F < \bar{F}\}]$  when  $f_0 = f_0^* = \frac{\sqrt{\bar{F}}}{\sqrt{\bar{F}+1.96}}$  and  $\rho = 1$  for  $\bar{F} \geq \frac{1.96^2}{2}$ :

$$1 - \Phi\left(\frac{1.96\sqrt{\bar{F}}}{\sqrt{\bar{F} + 1.96}}\right) + \Phi(-1.96) > 1 - \Phi(1.96) + \Phi(-1.96) = 0.05$$

This argument shows concretely that combining rejection rules  $\{t^2 > 1.96^2, F > \bar{F}\}$  and  $\{AR > 1.96^2, F < \bar{F}\}$  yields a size greater than 0.05 for all  $\bar{F}$  as claimed in Result 2d.

### 4.3 $tF$ procedure: $\Pr[t^2 > c(F)]$

Now consider a generalization of the above threshold decision rules: reject if and only if  $t^2 > c(F)$  where  $c(F)$  is a critical value function. For any well-defined function  $k(F)$ , it is possible to use the inequality

$$\frac{t_{AR}^2}{1 - 2\rho \frac{t_{AR}}{f} - \frac{t_{AR}^2}{f^2}} \leq k(F)$$

to identify the acceptance region in  $t_{AR}$ - $F$  space and use the bivariate normality of  $t_{AR}, f$  to compute the acceptance probability for any given  $\rho, f_0$ .

We seek a particular  $k(F)$ , call it  $c(F)$ , that controls the rejection rates to be no greater than 0.05 under all values of  $f_0, \rho$ .

Below, we provide the outline of the derivation, and refer to the appendix for further details. Our approach is as follows:

1. Construct a function  $\tilde{c}(\sqrt{F})$  such that  $\Pr[t^2 > \tilde{c}(\sqrt{F})] = 0.05$  for all  $f_0$  focusing on the “worst case”/extreme case of  $|\rho| = 1$ . Then define  $c(F) = 1 [F < \tilde{F}] \cdot \tilde{c}(\sqrt{F}) + 1 [F \geq \tilde{F}] \cdot 1.96^2$ , where  $\tilde{F} = \min\{F | \tilde{c}(\sqrt{F}) = 1.96^2\}$ .
2. Verify through our formulas that for fixed values of  $f_0$  the acceptance probability for the rule  $t^2 \leq c(F)$  is larger when  $|\rho| < 1$ .

We begin by recognizing that when  $\rho = 1$ ,  $t_{AR} = f - f_0$ , so that (2) and (3) lead to

$$t^2 = \frac{(f - f_0)^2}{1 - 2\frac{(f-f_0)}{f} + \frac{(f-f_0)^2}{f^2}} \quad (11)$$

$$= \frac{f^2 (f - f_0)^2}{f_0^2}. \quad (12)$$

We immediately recognize that  $t^2$  is a fourth-order polynomial in  $f$ .

Online Appendix Figure 1 gives a graphical depiction of this fourth-order polynomial for different values of  $f_0$ . Our intermediate objective is to solve for the function  $\tilde{c}(\sqrt{F}) = \tilde{c}(|f|)$  such that for *any given*  $f_0$ , the probability that the parts of the polynomial curves whose  $t^2$  values exceed  $\tilde{c}(|f|)$  is exactly equal to 0.05. Doing this amounts to characterizing the points in which any polynomial curve intersects  $\tilde{c}(|f|)$ , because the  $f$ -coordinates of those points of intersection, along with the chosen  $f_0$  are sufficient to compute the acceptance/rejection probability because  $f$  is normal with mean  $f_0$ .

The details of computing this  $\tilde{c}(|f|)$  function are a bit tedious and mechanical, and therefore are relegated to the appendix. The true  $\tilde{c}(|f|)$  is not equal to a constant critical value at  $1.96^2$  after some threshold. Indeed, given that the ingredients for its computation are essentially a fourth-order polynomial as well as the c.d.f. of a standard normal, it would be somewhat surprising if the resulting function were a constant in any region of  $f$ . It is true that  $\lim_{f \rightarrow \infty} (\tilde{c}(|f|)) = 1.96^2$ . We consider it

more practical, and in the spirit of extending the hard threshold rules examined in the previous section, to connect  $\bar{c}(|f|)$  to such a constant critical value and single  $F$  threshold rule, so we set  $c(F) = 1 [F < \tilde{F}] \cdot \bar{c}(\sqrt{F}) + 1 [F \geq \tilde{F}] \cdot 1.96^2$ , where  $\tilde{F} = \min \{F | \bar{c}(\sqrt{F}) = 1.96^2\} = 104.7$ . In this way, if practitioners observe an  $F$  greater than 104.7, they can use the usual critical value  $1.96^2$  for  $t^2$ , and otherwise use the  $F$ -dependent critical values reported in Table 3.

It is important to note that  $\tilde{F}$  is strictly greater than the threshold  $\bar{F}$  referenced in Result 2b. For the latter quantity, by construction there exists an  $f_0$  (with  $\rho=1$ ) such that the rejection probability is exactly equal to 0.05. Since  $\bar{c}(\sqrt{F})$  is constructed so that the rejection probability is equal to 0.05 for any  $f_0$  (with  $\rho=1$ ), the value of  $F$  at which  $\bar{c}(\sqrt{F})$  is equal to  $1.96^2$  cannot be equal to (or less than)  $\bar{F}$ . In practice, however, the numerical difference between these two quantities is extremely small; this leads to both quantities being rounded to the same number, 104.7.

Having constructed  $c(F)$ , we are in a position to examine the case of  $|\rho| < 1$  with our formulas for the rejection probabilities. Specifically, using the function  $c(f^2)$ , with some modification of (5) we obtain the probability of rejection as

$$\int_{\bar{c}(\rho)}^{\bar{c}_0} \int_{r^+(f)}^{r^-(f)} (\varphi_{f_0, \rho}(t_{AR}, f) + \varphi_{f_0, \rho}(-t_{AR}, -f)) dt_{AR} df \quad (13)$$

$$+ \int_{\bar{c}_0}^{\infty} \int_{\substack{(-\infty, r^-(f)] \\ \cup [r^+(f), \infty)}} (\varphi_{f_0, \rho}(t_{AR}, f) + \varphi_{f_0, \rho}(-t_{AR}, -f)) dt_{AR} df$$

where

$$r^+(f) = \frac{-\rho c(f^2) f + \sqrt{\rho^2 c(f^2)^2 f^2 + c(f^2) f^2 (f^2 - c(f^2))}}{f^2 - c(f^2)}$$

$$r^-(f) = \frac{-\rho c(f^2) f - \sqrt{\rho^2 c(f^2)^2 f^2 + c(f^2) f^2 (f^2 - c(f^2))}}{f^2 - c(f^2)}$$

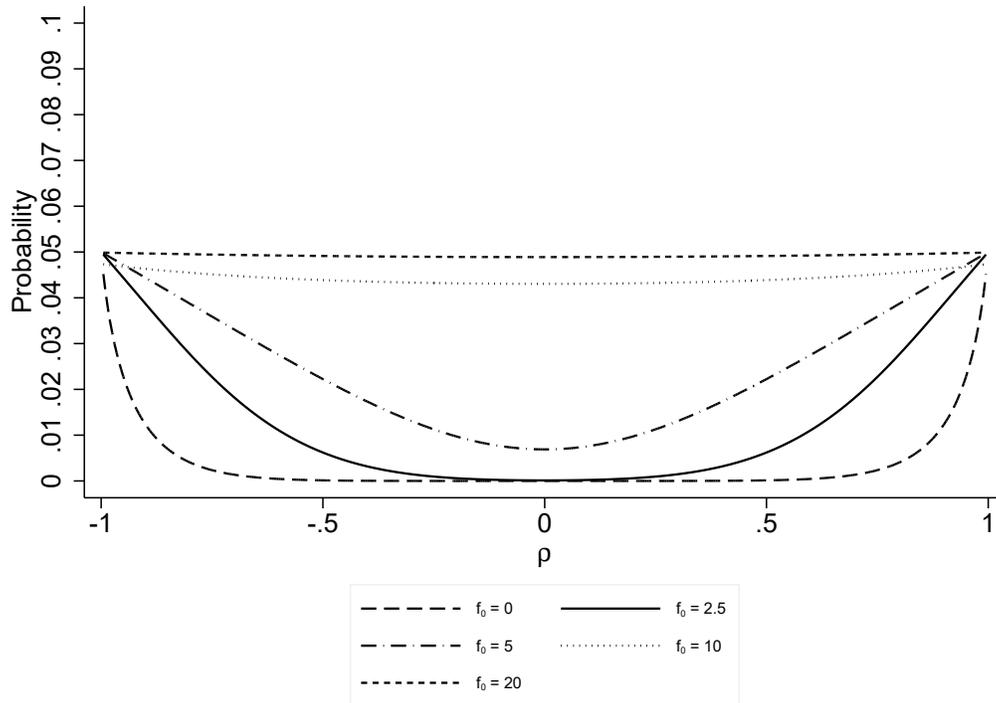
and where  $\bar{c}_0$  is the positive value of  $f$  that satisfies  $c(f^2) = f^2$ , and  $\bar{c}(\rho)$  is the positive value of  $f$  that satisfies  $c(f^2) (1 - \rho^2) = f^2$ .

The expressions in (13) are parallel to those in (5), with two modifications:

1) the critical value function  $c(F)$  replaces  $q_{1-\alpha}$  in the definitions of  $r^+(f)$  and  $r^-(f)$ , and 2) the limits of integration must change accordingly to accommodate the altered vertical asymptotes in the integral. As an analogy to Panel A, Panel B in Figure 4 illustrates the rejection regions using  $c(F)$ , again for the example of  $\rho = 0.8$ .

With these expressions in hand, we can numerically compute the rejection probability  $\Pr[t^2 > c(F)]$  for the entire nuisance parameter space. Figure 5c is analogous to Figures 5a and 5b, and it shows that indeed, for any given  $f_0$ , the rejection probabilities are lower for  $|\rho|$  smaller than 1. An important difference between Figure 5c and 5b is that with Figure 5b, when  $f_0$  is low (0, 2.5, or 5) the rejection probabilities are nearly zero, but lowering the threshold for  $F$  would cause the test to over-reject at higher levels of  $f_0$ . From Dufour (1997), we know that the critical value function must be unbounded for some  $F$  to achieve correct size. For our  $c(F)$ , the value is infinite for  $F < 1.96^2$ .

Figure 5c:  $\Pr[t^2 > c(F)]$ , by  $\rho$  and  $f_0$



## 5 Conclusion and Extensions

For several decades now the inference procedure of Anderson and Rubin (1949) has been available to researchers for instrumental variable models. At its core, the procedure is straightforward: in order to test the null hypothesis that  $\beta = \beta_0$ , one can examine the (appropriately normalized) statistic  $\hat{\pi}\hat{\beta}_{IV} - \beta_0\hat{\pi}$ . This statistic should be normal since the estimators for the reduced-form coefficient  $\hat{\pi}\hat{\beta}_{IV}$  and the first-stage coefficient  $\hat{\pi}$  are, by the central limit theorem, jointly normal. Indeed, Anderson and Rubin (1949) point out that in the normal homoskedastic model with nonstochastic  $Z$ , their statistic is exactly distributed as  $F(1, N - k)$ . Moreira (2002, 2009), Andrews, Moreira and Stock (2006), and Moreira and Moreira (2019) have characterized the sense in which  $AR$  is optimal in this single excluded instrument case.

Yet even today, perhaps partly due to its prominent role in textbook treatments of instrumental variables,  $t$ -ratio-based inference, commonly accompanied by the use of the “first-stage  $F$ ” statistic, is the predominant choice of applied researchers. This widespread use may also be due to its analytical convenience, the aesthetic of reporting confidence intervals centered around the IV estimate, and the reality that its implementation is already built into the base routines of popular statistical packages. Finally, its continued use is not motivated by the belief that it is “better” than  $AR$ , but instead by the belief that its use is “not bad,” especially when supplemented with the diagnostic statistic of the “first-stage  $F$ .” From the existing theoretical literature, it is no surprise that the use of thresholds like 10 or 16.38 lead to some distortion in inference on  $\beta$ , and that the use of those procedures amounts to adopting a lower standard for statistical significance.

Our paper asks what adjustments are necessary to obtain zero distortion. That is, we consider the practical implications of applying a consistent standard for the validity of inference. For example, since 95 percent confidence intervals are commonly reported, we ask what must be assumed or done for  $t$ -ratio- and  $F$ -based test procedures to have 5 percent significance. It should be clear that our approach can be applied to consider other levels of significance.

Our derivations show that requiring zero distortion in inference has large implications for practice. For example, in order to obtain valid inference armed only

with the  $t$ -ratio, one must rule out large portions of the nuisance parameter space. Examples of assumptions that could be made ( $E[F] > 142.6$  or  $|\rho| < .565$ ) that would guarantee valid inference in finite samples only serve to highlight the limitations of the  $t$ -ratio, since researchers typically would like to remain agnostic about such nuisance parameters.

When using the first-stage  $F$  statistic, while Stock and Yogo (2005) are precise about the purpose and limitations of the first-stage  $F$ , applied researchers have perhaps implicitly adopted an over-simplified and loose interpretation: “if the  $F$  statistic exceeds 10, reasonably ignore any inference problems associated with  $IV$ .” Our paper shows that when we apply the “95 percent confidence” standard to  $IV$ , it would require raising the threshold for  $F$  from 10 to 104.7. Alternatively, one could maintain the hard threshold for  $F$  to be 10, but this would require raising the conventional critical values of  $\pm 1.96$  to  $\pm 3.43$ . Perhaps unsurprisingly, when we apply these corrections to re-evaluate published papers in the *AER*, a substantial proportion (about half) of specifications that would have been reported at the 5 percent level of significance are no longer statistically significant. Furthermore, as Dufour (1997) has noted, given the possibility of non-identification, *any* procedure with correct coverage must generate completely uninformative (unbounded) confidence intervals with positive probability. In this case, the use of a single threshold for  $F$  (and a single critical value for the  $t$ -ratio) requires a commitment to unbounded confidence intervals whenever  $F$  does not exceed the specified threshold.

Motivated by the desirability of a more powerful procedure – but one that can be used to re-assess previous studies, virtually all of which (in our sample of *AER* papers) elected not to use  $AR$ -based inference – we develop the  $tF$  procedure, which simply extends the critical values for the  $t$ -ratio, as a function of  $F$ , when  $F < 104.7$ , in a way to control rejection rates to the desired 5 percent level. The table of critical values is provided in Section 3.

We conclude noting some related issues that we believe are worthy of deeper investigation. While the  $tF$  procedure has clear power advantages over the single  $F$ -based thresholds, there is a question of how any of these procedures perform compared to  $AR$ .  $AR$  is known to be optimal among tests with certain properties, one of which is unbiasedness and neither the commonly used  $F$ -based threshold rules

of Result 2a, 2b, or 2c, nor the  $tF$  procedure of Result 3 are unbiased tests.

Finally, the scope of our study was limited to the common case of the single instrument IV model, but it would be natural to expect the same kinds of issues to be at play with the over-identified model. In ongoing work, we are exploring these same issues within the context of over-identified models.

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# Appendix: For Online Publication

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