Online Appendix

A Frequently Asked Questions

The novelty of equivalence testing in economics often generates easily-addressable misunderstandings about equivalence testing when applied to economic research. This appendix leverages extensive peer review feedback to answer frequently asked questions about the equivalence testing framework and the empirics in this paper. I thank the numerous peer reviewers who clarified uncertainties that readers may also share.

A Null Results in Economics

A.1 You state that when researchers are trying to show a null result, imprecision is 'good' because it makes statistically insignificant results more likely. But given how strong null result penalties are in economics, is there any evidence that economists actually prefer more imprecise estimates?

Unfortunately, yes. Dreber, Johannesson, & Yang (2024) show systematic evidence of 'reverse p-hacking' for placebo tests in economics publications, where null results are of interest. They find that rejection rates for placebo tests that have a nominal size of 5% are significantly above 5%, suggesting that when null results are of interest, economists selectively report imprecise results more frequently.

A.2 How exactly are economists using null results right now? Are they mostly for robustness tests or do they make up a main conclusion of the paper?

Most null claims made in the abstracts of articles from top economics journals are main claims, rather than claims about secondary analyses. Table 2 shows that across 279 null claims identified in the abstracts of articles published in Top 5 economics

journals, 162 (58%) are headline claims of the articles. Null claims about mechanisms, robustness checks, and subgroup analyses each only constitute between 13-15% of that sample.

A.3 Don't most economists who fail to find statistically significant results speak with nuance about their results to prevent readers from misinterpreting statistically insignificant results as null results?

Not in the most prominent communications of their results. Table 2 shows that nearly 70% of null claims made in the abstracts of articles published in Top 5 economics journals are 'unqualified', in the sense that they do not qualify themselves with references to effect sizes, statistical significance, or a lack of evidence. The same holds for 64% of 'main' claims made in abstracts, and 72-83% of claims regarding secondary analyses.

A.4 Abadie (2020) finds that statistically insignificant results can often be more informative than statistically insignificant results, even if estimates are not particularly precise. How do you reconcile this with your position that null results in the current economics literature are often uncredible due to low precision and power?

It is important to distinguish between a conclusion's *informativeness* and its *certainty*, as Abadie (2020) focuses on informativeness whereas I focus on certainty. Abadie (2020) defines informativeness as the extent to which one's prior distribution over some relationship δ shifts after learning new information about δ (namely, standard NHST conclusions concerning δ). The basic intuition behind Abadie (2020) is that if the prior probability that $\hat{\delta}$ is statistically significantly different from zero is high, then learning that $\hat{\delta}$ is not statistically significantly different from zero is surprising, and thus should considerably shift a Bayesian's prior distribution concerning δ to a new posterior. Abadie (2020) shows that even if an estimate $\hat{\delta}$ is not precisely estimated, so long as the prior probability that $\hat{\delta}$ will be statistically significantly different from zero

exceeds 50%, a statistically insignificant $\hat{\delta}$ will always shift the prior distribution of δ more than a statistically significant $\hat{\delta}$. Abadie's (2020) results imply that a statistically insignificant result can greatly shift beliefs about the underlying relationship even if that insignificant result is not particularly certain or robust, so long as the researcher's prior belief that the result will be statistically significant is strong enough.

My positions and Abadie (2020) do not contradict one another, and are in fact complementary. My primary concern is not with how much a conclusion causes people to change their beliefs; it is instead about how robust and certain the conclusion is. Abadie's (2020) findings demonstrate that it is more dangerous than previously thought to interpret an underpowered statistically insignificant result as a null result, because this can have large effects on people's priors even if the null result is not particularly robust or certain. Additionally, Abadie (2020) shows that *ceteris paribus*, a statistically insignificant result is more informative when the estimate is better-powered, as this increases the prior probability that the estimate will be statistically significantly different from zero. In the same vein, an estimate can be more tightly bounded to zero using equivalence testing if it is better-powered. This implies that tightly-bounded null results under equivalence testing can be both robust and informative if the prior belief that the relationship of interest is meaningfully large is strong enough.

B Equivalence Testing

B.1 How do you actually determine the region of practical equivalence (ROPE) for a given estimate?

A flexible and generally credible approach for setting ROPEs is to survey researchers or other stakeholders for their judgments of what would constitute the smallest effect size(s) of interest for their relationship of interest. One can then take a median of responses to those surveys to set ROPE boundaries. A survey of this kind was fielded for this paper. As a template for future researchers, one can both see the survey and

download its original Qualtrics file at https://socialscienceprediction.org/s/602202. This survey is discussed in Section G. At stages in the research process where preregistration is still credible, I also propose cost-benefit analysis as a useful method for defining ROPEs. I discuss these approaches in further depth in Section 7.1.

B.2 If I have reason to believe that researchers or stakeholders will not have a clear idea of the effect sizes that are practically meaningful for my relationship of interest, then why is it credible to base my ROPEs off of their judgments?

ROPEs based on judgments of independent researchers or stakeholders are credible because they are independent of the researcher's data and team. This is useful because ignoring this independence condition can often lead to uncredible ROPEs. Ofori et al. (2023) show that in high-impact journals, the median superiority trial yields a risk difference estimate of 2.1%. However, in cardiovascular non-inferiority trials – where researchers are interested in null results and have discretion to set their own ROPE boundaries – the ROPE for risk differences is set larger than 2.1% in nearly 72% of trials. Setting ROPEs independently of the data and the research team can ensure that such uncredible ROPEs do not arise.

B.3 Regardless of whether ROPEs are set by the researcher or by an independent sample, aren't they still kind of subjective and arbitrary? Why not let theory and prior literature guide ROPE selection?

Even when ROPEs are decided by subjective judgment, this does not mean that they are arbitrary. Economists routinely make judgments about whether relationships are economically significant, and these judgments are typically based on a wide range of considerations, including theory, implicit cost-benefit considerations, and expectations of effect sizes from economists' experience with the prior literature. Equivalence testing simply encourages researchers to make and state concrete judgments about

what effect sizes matter for the relationships they study (or elicit them from others), and properly test whether those relationships are bounded beneath their smallest effect sizes of interest.

B.4 Practical significance for many economic relationships can often be determined *via* cost-benefit analysis; why not use it to set ROPEs?

Cost-benefit analysis can be used to set ROPEs. Given respective monetary valuations V_D and V_Y of unit increases in exposure D and outcome Y, Section 7.1.2 provides ROPEs that can be used to test whether the benefits of a unit change in D are significantly smaller than the associated costs. Unlike ROPEs determined through survey-elicited judgments, it is quite important that ROPEs set through cost-benefit analysis are pre-registered. This is because unlike ROPEs set by the judgments of independent parties, V_D and V_Y are typically set directly by the researcher themselves, and thus represent two researcher degrees of freedom that can be used to ROPE-hack.

B.5 What is an 'exact' equivalence test, and why is it different from other equivalence tests?

An 'exact' equivalence test is one whose critical values are based on the critical values for the t-distribution rather than the normal distribution, which accounts for the fact that this inference is conducted in a finite-sample setting. Exact equivalence tests are in this way contrasted with 'asymptotically approximate' equivalence tests, which are based on the critical values for the normal distribution. The tests are named in this way because as $df \to \infty$, the t-distribution converges to the normal distribution. Using exact rather than asymptotically approximate tests means that the critical values for significance are slightly higher, but for reasonable sample sizes, the differences are marginal. E.g., when $df = 100$, the 95% exact critical value for a one-sided test is 1.66 instead of 1.645; when $df = 1000$, that exact critical value decreases to 1.646.

B.6 When you speak of the 'error rate control' implied by the size of the equivalence test, what is the exact error whose rate is being controlled?

The error rate controlled by the size of an equivalence test is the probability that a researcher concludes that a relationship δ is significantly bounded within a ROPE $[\epsilon_-, \epsilon_+]$ when δ is in fact not located within ROPE. This error rate will naturally depend on the true value of δ. The rate at which one incorrectly concludes that δ is bounded within $[\epsilon_-, \epsilon_+]$ will be quite low if the true value of δ is far beneath ϵ_- or far above ϵ_+, whereas the risk of such incorrect equivalence conclusions is higher if δ is close to the ROPE bounds. The 'worst case scenario' for error control that one must consider is that where δ asymptotically approaches either ϵ_- from below or ϵ_+ from above. The error rate controlled by equivalence testing is that which arises under this worst case scenario: i.e., the expected probability that one incorrectly concludes that $\delta \in [\epsilon_-, \epsilon_+]$ when δ approaches ϵ_- from below or ϵ_+ from above. In practice, this means that an equivalence test with a 5% significance level maintains a size of $\alpha = 0.05$ when $\delta = \epsilon_-$ or $\delta = \epsilon_+$.

This error rate control can be verified through a simple simulation. Let N be a desired sample size, R be the number of simulation runs, $\Phi(\mu, s)$ be a normal distribution with mean μ and standard deviation s, and z_q be quantile q of the inverse cumulative density function for the standard normal distribution. For each simulation run:

1. Generate variable μ_- by randomly drawing N observations from the distribution $\Phi(\epsilon_-, 1)$ and obtain its sample mean and sample standard deviation, respectively $\hat{\mu}_-$ and $\hat{s}_-$. Compute the $(1 - \alpha)$ equivalence confidence interval as

$$\text{ECI}^-_{1-\alpha} = \hat{\mu}_- \pm \frac{z_{1-a}\hat{s}_-}{\sqrt{N}}.$$

2. Generate variable μ_+ by randomly drawing N observations from the distribution

$\Phi(\epsilon_+, 1)$ and obtain its sample mean and sample standard deviation, respectively $\hat{\mu}_+$ and $\hat{s}_+$. Compute the $(1-\alpha)$ equivalence confidence interval as

$$\text{ECI}^+_{1-\alpha} = \hat{\mu}_+ \pm \frac{z_{1-a}\hat{s}_+}{\sqrt{N}}.$$

As $R \to \infty$, at most $100 \times (1-\alpha)\%$ of the $\text{ECI}^-_{1-\alpha}$ will be entirely bounded in $[\epsilon_-, \epsilon_+]$, and at most $100 \times (1-\alpha)\%$ of the $\text{ECI}^+_{1-\alpha}$ will be entirely bounded in $[\epsilon_-, \epsilon_+]$. In other words, when the true parameter sits exactly on one of the ROPE boundaries, no more than 5% of the 95% ECIs of that parameter will be entirely bounded within the ROPE in expectation, and thus no more than 5% of estimates for μ will be significantly bounded within the ROPE in expectation. Of course, if the ROPE is held constant, this error rate will decline as μ_- grows more negative or as μ_+ grows more positive.

B.7 Suppose that the ROPE is $[\epsilon_-, \epsilon_+]$. An equivalence test will yield $p > \alpha$ by construction if $\hat{\delta} \leq \epsilon_-$ or $\hat{\delta} \geq \epsilon_+$. Doesn't this mean that equivalence tests are structurally underpowered?

No. In fact, this property actually demonstrates equivalence testing's error rate control. Consider that in standard NHST, it also is impossible to obtain $p < \alpha$ if a point estimate is exactly equal to zero. This does not mean that standard NHST is structurally underpowered; it simply means that standard NHST will categorically not reject the null hypothesis when the alternative hypothesis is trivially untrue. Equivalence testing exhibits the same property, as the alternative hypothesis – that δ is bounded inside the ROPE – is trivially untrue if point estimate $\hat{\delta}$ is not even located inside the ROPE.

B.8 Suppose that δ is located inside the ROPE. Does the size or power of an equivalence test depend on the true value of δ?

δ's location never controls the size of an equivalence test. This is because the size of the test controls error rates *under the null hypothesis*, which in the case of equivalence testing presumes that δ is outside of the ROPE. Even if δ is truly inside the ROPE, its location does not change the error control implied under the null hypothesis that δ sits beyond the ROPE bounds.

However, the power of an equivalence test always depends on δ's location – it is always easier to bound a relationship within a ROPE when the (estimated) relationship is in the center of the ROPE than it is when that (estimated) relationship is near the ROPE bounds. This property is not unique to equivalence testing. Consider that in standard NHST, it takes more power to conclude that a small estimate is significantly different from zero than it does to conclude this for a large estimate. In a similar vein, it takes more power to conclude that a large estimate is practically equal to zero than it does to conclude this for a small estimate.

B.9 What happens when an estimate is neither statistically significant under standard NHST nor significantly bounded within its ROPE under equivalence testing?

Such estimates yield 'inconclusive' results. An inconclusive result implies that the researcher does not have sufficient power and precision to say whether or not the relationship is practically negligible. I discuss the implications of inconclusive results in Section 7.2.

B.10 Why not just report confidence intervals and interpret what parameter values can be ruled out with reasonable confidence?

This approach is still available under equivalence testing – in fact, compared to the standard approach of just reporting and interpreting the $(1 - \alpha)$ confidence interval,

equivalence testing allows researchers to more tightly bound estimates to zero by using *equivalence* confidence intervals (ECIs). Section 4.2 explains that the smallest ROPE within which one can significantly bound an estimate under equivalence testing is the $(1-\alpha)$ ECI, which is equivalent to the $(1-2\alpha)$ confidence interval. Intuitively, this is because the $(1-\alpha)$ confidence interval inverts two-sided tests, whereas frequentist equivalence tests are based on one-sided tests (see Section 4.1). Consequently, at a significance level of $\alpha = 5\%$, a researcher who reports that they can only bound a relationship to within ≈ 1.96 standard errors of its point estimate is being too hard on themselves. Equivalence testing's properties imply that a researcher can significantly bound a relationship within ≈ 1.645 standard errors of its point estimate. I highlight this advantage of equivalence testing in Section 4.2.

B.11 Why not just use a power approach? If I can show that I'm well-powered to detect a given effect size, isn't a statistically insignificant result enough to evidence a credible null result?

Having 80% power to detect a given effect size is neither necessary nor sufficient to imply that a statistically insignificant result can be significantly bounded beneath that effect size. A higher power target can make the power approach sufficient to imply that a statistically insignificant result is practically null, but ironically, doing so actually renders the power approach less well-powered to detect null results than equivalence testing. For further details, see Section 4.3.

B.12 Why not use a Bayesian approach? Would a Bayesian approach preclude the need to set a ROPE?

A Bayesian approach does not preclude the need for ROPE-setting. Typical Bayesian equivalence testing methods include the Bayes factor approach and the HDI-ROPE approach (where HDI indicates the 'highest-density interval'; see Linde et al. 2023). The Bayes factor approach evaluates the multiplier by which the prior odds ratio

between the alternative hypothesis and the null hypothesis are multiplied to obtain the posterior odds ratio once the data is observed. In contrast, the HDI-ROPE approach computes the $(1 - \alpha)$ HDI for the posterior distribution of the relationship of interest, and assesses whether this HDI is entirely bounded within the ROPE. Both approaches require specifying a ROPE; in the Bayes factor case, this is because both the prior and posterior hypothesis odds ratios concern hypotheses regarding whether the relationship of interest is inside its ROPE.

I focus on frequentist approaches because applications of Bayesian equivalence testing must specify a prior, which are typically either set *ad hoc* or pinned to naïve reference priors (see Linde et al. 2023). The former approach creates an important researcher degree of freedom akin to ROPE-hacking, whereas the latter often makes unreasonable presumptions about the relative odds that relationships are or are not practically negligible.

However, the survey-based approach I advocate for ROPE elicitation in Section 7.1.1 actually provides a comprehensive approach for simultaneously eliciting credible ROPEs and credible priors. A ROPE elicitation survey can simultaneously ask researchers or stakeholders both what they predict a given relationship will be and what effect sizes would be practically meaningful for that relationship. Responses to the latter question can be used to build the ROPE (e.g., by taking the median ROPE bounds provided by respondents), whereas responses to the former question can be used to set the prior (e.g., by taking the proportion of predictions of the relationship that fall inside the ROPE). As with independently-elicited ROPEs, this eliminates the researcher degree of freedom involved in researchers choosing the priors for their hypotheses and ensures that such priors are set independently of the data and the research team. This Bayesian application of such elicitation surveys was actually an intended application of the Social Science Prediction Platform (DellaVigna, Pope, & Vivalt 2019).

C The Paper's Empirics

C.1 You select the estimates for your replication sample specifically because at least one estimate per claim is not statistically significantly different from zero. Doesn't this sample selection affect your estimates of equivalence testing failure rates?

Though the selection of estimates for the replication sample will naturally affect equivalence testing failure rates, there is no better sample to obtain. Consider two counterfactuals. The first would be to include all the positive claims from these papers. This would naturally increase equivalence testing failure rates, but the resulting rates would be both upward-biased and fundamentally uninteresting (because no one is concerned about whether positive results are robust to equivalence testing). The second would be broadening the sample of null claims to those which are defended entirely by estimates that are statistically significantly different from zero, but which have been judged by the authors to be precisely bounded beneath some practically negligible effect size. This option simply does not exist, as in top economics journals, this kind of inference is virtually never conducted; null claims are nearly always defended by at least one statistically insignificant result. Though Online Appendix B documents that the initial sample of 287 potentially relevant articles reduces to 158 articles after discarding papers whose null claims are not defended by at least one statistically insignificant estimate, almost all of these cases emerge because the claim is not defended by *any* estimate that could be summarized by a single parameter. Many such results were either theoretical or graphical, and thus could not be tested for equivalence. My review revealed virtually no cases where researchers defended null claims with equivalence testing or Bayesian inference.

C.2 You select the estimates for your replication sample specifically because at least one estimate per claim is not statistically significantly different from zero. Don't you need to adjust your critical values due to this selection?

No, this is not necessary. In fact, the equivalence tests conducted in my paper are an application of Campbell & Gustafson's (2018) 'conditional equivalence testing' procedure. As discussed in Section 7.2, this framework first tests whether a relationship is statistically significantly different from zero. Then if and only if that test yields insignificant results, an equivalence test is performed. Campbell & Gustafson (2018) show empirically that this procedure produces similar results to inferences with Bayes factors under inverse variance priors.

C.3 Your standardized effect sizes σ and r are functions of the estimate itself and, in the case of σ, descriptive statistics of the variables that produce the regression estimate. Doesn't this raise an issue because the benchmark ROPEs you use to bound estimates are themselves a function of the estimate?

No, this is not a concern for either effect size measure. To see why, it's useful to review what each measures and how they're constructed.

- σ is a measure of *size*, scaled by the standard deviation of the outcome and, if the exposure is not binary, the standard deviation of the exposure:

$$\sigma = \begin{cases} \frac{\hat{\delta}}{\sigma_Y} \text{ if } D \text{ is binary} \\ \frac{\hat{\delta}\sigma_D}{\sigma_Y} \text{ otherwise} \end{cases} \qquad s = \begin{cases} \frac{\text{SE}(\hat{\delta})}{\sigma_Y} \text{ if } D \text{ is binary} \\ \frac{\text{SE}(\hat{\delta})\sigma_D}{\sigma_Y} \text{ otherwise} \end{cases} .$$

  The rescaling of the estimate is only based on the variance of the individual variables in the regression, *not* the covariance between them.

- r is a measure of *fit*, as is the case with correlation coefficients in general. Here

the estimate is rescaled by its precision and the degrees of freedom:

$$r = \frac{t_{\text{NHST}}}{\sqrt{t_{\text{NHST}}^2 + df}} \qquad \text{SE}(r) = \frac{1 - r^2}{\sqrt{df}}.$$

The rescaling here is more driven by the size of the point estimate directly, as this size enters t_{NHST}. However, this does not pose an issue for determining the size of ROPEs for partial correlation coefficients because r is a measure of fit, not size.

Virtually any attempt to compare estimates across or even within papers will require some form of standardization, as the size of the relationship of interest for any estimate will depend on the exact relationship being estimated. This is a fundamental problem of meta-analysis, and any meaningful rescaling of an estimate must necessarily be a function of the estimate. The specific standardizations employed here convert the estimates I obtain in my re-estimations into units that are widely-used in social science meta-analyses. After unit conversion, the partial correlation ROPE of $[-0.1, 0.1]$ and the standardized coefficient ROPE of $[-0.2, 0.2]$ still exogenously originate from disciplinary thresholds for small effect sizes.

C.4 Why do you notate standardized coefficients with σ when this symbol is typically used to denote standard deviations?

This is actually the precise reason σ is used. As discussed in Section 5.1, standardized coefficients can be interpreted as 'standard deviation effects'. For binary exposure variables, σ represents the number of outcome standard deviations associated with a one-point increase in the exposure variable. For nonbinary exposure variables, σ represents the number of outcome standard deviations associated with a one-standard deviation increase in the exposure variable.

C.5 What exactly is an 'equivalence testing failure rate' (ETFR)? What makes claim-level and article-level ETFRs different from one another?

The ETFR is the average partition-level proportion of estimates that cannot be significantly bounded within a given ROPE at a 5% significance level, and thus 'fail' the equivalence test at that ROPE. The claim-level ETFR is the average claim-level proportion of estimates defending a given claim that cannot be significantly bounded within a given ROPE. Likewise, the article-level ETFR is the average article-level proportion of estimates defending null claims in a given article that cannot be significantly bounded within a given ROPE. Section 5.2 provides an illustrative example, which is visualized for convenience in Online Appendix Figure A1. More detailed mathematical definitions for ETFRs are provided in Online Appendix H.

C.6 You say that high equivalence testing failure rates are evidence of high Type II error rates in economics. Are you saying that all the estimates which 'fail' your equivalence tests are actually measuring meaningfully large relationships rather than null relationships?

No, an estimate 'failing' an equivalence test does not imply that the underlying relationship is actually large. An unfortunate consequence of standard null hypothesis significance testing is that it drives researchers to binarize results as either 'significant' or 'insignificant'. However, equivalence testing reveals that there is a third categorization: results can be *inconclusive*. When an estimate is not significantly different from zero nor significantly bounded within its ROPE, this yields an inconclusive result, as the researcher does not have sufficient power or precision to say whether or not the estimate is meaningfully large. ETFRs essentially measure how often estimates defending null claims in economics in fact yield inconclusive results. High ETFRs are thus a measure of the robustness of null claims in economics to equivalence testing, and in the same way that the non-robustness of a statistically significant result likely

signals Type I error, the non-robustness of a statistically insignificant result likely signals Type II error. I explain this point in Section 5.2, and elaborate on inconclusive results in Section 7.2.

C.7 How do you justify the benchmark ROPEs you choose when computing your main results, beyond the fact that they're convenient thresholds?

There are two key justifications for the benchmark ROPEs, both of which are articulated in Section 5.1. The first is that they are based on Cohen's (1988) small effect size thresholds for standardized coefficients and regression coefficients, and are thus thresholds that are typically considered 'small' in the social sciences. The second is that these 'small' effect size benchmarks are actually quite large for economics. In Online Appendix E, I perform a benchmarking exercise where I estimate standardized coefficients and partial correlation coefficients for the main result in ten recent, highly-cited economics papers that advertise significant, plausibly large effects. I find that the median standardized coefficient size is 0.206σ, and that the median partial correlation coefficient is $0.16r$. Based on over 22,000 estimates that inform economic meta-analyses, Doucouliagos (2011) also finds that a partial correlation of 0.1 is larger than more than a quarter of all published effect sizes in economics. In other words, a standard coefficient of 0.2 and a partial correlation of 0.1 are rather lenient ROPE lengths in economics. High equivalence testing failure rates for these ROPEs thus likely imply that many null results in economics would not be robust to equivalence testing if more tailored smallest effect sizes of interest were used as ROPE lengths.

C.8 Does the robustness of equivalence testing to null results depend on whether the null claim is a main finding of the paper?

No, equivalence testing failure rates are high both for estimates defending 'main' claims and for those defending null claims about secondary analyses. Online Appendix

Table A8 shows that equivalence testing failure rates remain significantly bounded above median SSPP acceptability thresholds both for estimates defending main claims and for those defending secondary claims such as mechanism analyses, robustness checks, or subgroup analyses.

C.9 Are high equivalence testing failure rates concentrated in null claims that don't appropriately qualify their statistically insignificant results with nuance?

No, equivalence testing failure rates are high both for null claims that are, and that are not, appropriately qualified. Online Appendix Table A7 shows that equivalence testing failure rates are significantly above median SSPP acceptability thresholds both for estimates defending qualified null claims and for those defending unqualified null claims.

C.10 What would equivalence testing failure rates look like if you set different ROPEs instead of your standardized coefficient ROPE of $[-0.2, 0.2]$ and your partial correlation ROPE of $[-0.1, 0.1]$?

In Section 6.3, I show that equivalence testing failure rates remain quite high regardless of what ROPE length is used. I do this by plotting 'failure curves', which show the distribution of equivalence testing failure rates as ROPE lengths are allowed to become longer. Results in this section imply that one must be willing to tolerate obscenely large ROPEs to obtain acceptable equivalence testing failure rates for null results in economics.

C.11 When discussing mechanisms of high equivalence testing failure rates, you say that ECI half-widths stochastically dominate estimate magnitudes. But isn't the size of a relationship positively correlated with the noisiness of that relationship?

Estimate magnitudes and ECI half-widths are positively correlated. However, even if estimate magnitudes and ECI half-widths positively covary, Figure 8 shows that ECI half-widths consistently remain larger than estimate magnitudes as the two measures move together towards the top of their respective distributions, implying that ECI half-widths typically contribute more to the ECI outer bound than effect sizes. Table 3 also shows that ECI outer bounds are more elastic with respect to ECI half-widths than they are with respect to estimate magnitudes. Because elasticities are scale-invariant, this confirms that my finding that ECI half-widths are a more dominant determinant of ECI outer bounds than estimate magnitudes is not driven by the scale of the standardized effect size.

B Systematic Review Process

My initial sample consists of all articles registered in Web of Science as published from 2015 onwards in a Top 5 economics journal (specifically *American Economic Review*, *Econometrica*, *Journal of Political Economy*, *Quarterly Journal of Economics*, and *Review of Economic Studies*). I obtained bibliographic information on this set of 3732 articles from Web of Science on 28 July 2023. This bibliographic information was then loaded into ASReview, an interface that employs machine learning and text classification to assist with managing systematic literature reviews by sorting abstracts from most to least relevant (van de Schoot et al. 2021). I then manually reviewed the abstracts, classifying them as relevant if the abstract makes some claim that a phenomenon or relationship is either negligible or nonexistent. After reviewing 2987 abstracts, 50 consecutive abstracts were assessed to be irrelevant, and thus the remaining 745 articles are discarded as irrelevant based on ASReview's relevance probability ranking.[1] The abstract reviews yield 603 potentially relevant records, at which point all articles published prior to 2020 were discarded, ensuring that the sample reflects only the most recent practice in the economics literature and has the highest probability of re-estimability while still keeping the number of (attempted) reproductions down to a feasible level.[2] 287 potentially relevant articles published from 2020-2023 arise from this first phase of the systematic search.

I then examine the abstracts of each of these 287 potentially relevant articles, isolating every null claim made in each abstract and discarding an article if, upon further inspection, its abstract does not in fact make an identifiable null claim. This step produces 556 null claims across 285 articles. For each of these null claims, I attempt to locate the estimate(s) used to support that claim within the article. I

[1] This is an intended feature of ASReview – the probability ranking permits early cessation of the review process with a strong reassurance that the most relevant articles still remain in the sample (van de Schoot et al. 2021).

[2] The additional articles from 2015-2019 help ensure the quality of the relevance probability ranking, and thus the irrelevance of discarded articles.

discard a claim if it is not defended by at least one statistically insignificant estimate, otherwise storing the main estimate(s) being used to defend that claim. An estimate can be statistically insignificant either if an estimate's Type I error rate measure (e.g., a p-value) is reported to be above 0.05 or, equivalently, if an estimate's 95% confidence interval intersects zero. I discard articles if no null claims remain after this discarding process. This step yields my intermediate sample of 2346 estimates across 279 claims in 158 articles. Thereafter, I attempt to reproduce every estimate in the intermediate sample. Estimates are discarded when data is not available for reproduction or the reproduction is not conformable to my final analysis. After such discarding, my final sample consists of 876 estimates across 135 null claims in 81 articles.

C Final Sample

All publications included in the final sample are cited in these references. All publications in the final sample also are part of the intermediate sample. These references also cite repositories wherein the data for the final sample's articles are stored, when applicable. Data for articles without a separate repository is linked to the publisher's online version of the article itself. Bagues & Campa (2020), which is in the final sample, makes use of data from Casas-Arce & Saiz (2015), which is not in the final sample. Historical datasets in Bureau of Labor Statistics (2022) are cited at the direction of Gertler, Huckfeldt, & Trigari (2020).

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D Intermediate Sample

The following publications are included in the intermediate sample, but are not included in the final sample.

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E Effect Size Benchmarking

Table A1 shows the values of σ and r for a selected sample of ten highly-cited and recent results from the economics literature that represent plausibly large effects. I term this the 'benchmarking sample'. All articles in this sample have publicly available replication repositories and are published between 2015-2020. I isolate one main claim of each article and the primary estimate used to defend this claim. The benchmarking sample thus consists of ten articles, each with one claim and one estimate defending that claim. Online Appendix F provides citations for all articles in the benchmarking sample, along with associated replication repositories (when applicable).

Two features of Table A1 are worth noting. First, though σ and r are quite positively correlated and always share the same sign, they do not necessarily monotonically correspond. Second, though the estimates in this benchmarking sample are all statistically significant under the standard NHST framework, their effect sizes are also quite small in general.

Article	Setting	Outcome Variable	Exposure Variable	Initial p-Value	σ	r	Location
Acemoglu & Restrepo (2020)	Difference-in-differences analysis of U.S. commuting zones, 1990-2007	Employment rates (continuous)	Industrial robot exposure (continuous)	0.000	-0.206	-0.16	Table 7, Panel A, US exposure to robots, Model 3
Acemoglu et al. (2019)	Difference-in-differences analysis of countries, 1960-2010	Short-run log GDP levels (continuous)	Democratization (binary)	0.001	0.005	0.255	Table 2, Democracy, Model 3
Berman et al. (2017)	African 0.5×0.5 longitude-latitude cells with mineral mines, 1997-2010	Conflict incidence (binary)	Log price of main mineral (continuous)	0.012	0.521	0.007	Table 2, ln price x mines > 0, Model 1
Deschênes, Greenstone, & Shapiro (2017)	Difference-in-differences analysis of U.S. counties, 2001-2007	Nitrogen dioxide emissions (continuous)	Nitrogen dioxide cap-and-trade participation (binary)	0.000	-0.134	-0.468	Table 2, Panel A, NOx, Model 3
Haushofer & Shapiro (2016)	Experiment with low-income Kenyan households, 2011-2013	Non-durable consumption (continuous)	Unconditional cash transfer (binary)	0.000	0.376	0.195	Table V, Non-durable expenditure, Model 1
Benhassine et al. (2015)	Experiment with families of Moroccan primary school-aged students, 2008-2010	School attendance (binary)	Educational cash transfer to fathers (binary)	0.000	0.18	0.252	Table 5, Panel A, Attending school by end of year 2, among those 6-15 at baseline, Impact of LCT to fathers
Bloom et al. (2015)	Field experiment with Chinese workers, 2010-2011	Attrition (binary)	Voluntarily working from home (binary)	0.002	-0.397	-0.196	Table VIII, Treatment, Model 1
Duflo, Dupas, & Kremer (2015)	Experiment with Kenyan primary school-aged girls, 2003-2010	Reaching eighth grade (binary)	Education subsidy (binary)	0.023	0.1	0.125	Table 3, Panel A, Stand-alone education subsidy, Model 1
Hanushek et al. (2015)	OECD adult workers, 2011-2012	Log hourly wages (continuous)	Numeracy skills (continuous)	0.000	0.091	0.316	Table 5, Numeracy, Model 1
Oswald, Proto, & Sgroi (2015)	UK students, piece-rate laboratory task	Productivity (continuous)	Happiness (continuous)	0.018	0.753	0.244	Table 2, Change in happiness, Model 4

Note: Effect sizes and initial standard NHST p-values of each estimate are reported. Each original estimate can be found in its respective article at the specified location. Some articles' results are reproduced using data from repositories (Hanushek 2016; Benhassine et al. 2019; Berman et al. 2019; Deschênes, Greenstone, & Shapiro 2019; Duflo, Dupas, & Kremer 2019), whereas others are reproduced using files linked to the publisher's online webpage for the article.

Table A1: Effect Size Benchmarking

F Benchmarking Sample

All articles and associated replication repositories (when applicable) of the benchmarking sample are provided here.

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G SSPP Data

The SSPP survey was posted publicly to the SSPP website, and any interested respondent was free to take the survey. The survey was also publicly disseminated on Twitter/X by the SSPP. 58 of the 62 survey respondents (93.5%) are members of the SSPP's Superforecaster Panel, which is a sample of researchers that are pre-selected by SSPP and are paid a semi-annual flat rate for completing a sufficient proportion of the surveys that are posted to the SSPP website each month. The remaining four respondents are not part of the Superforecaster Panel, and are not incentivized to take the survey.

My SSPP sample is relatively young, with the median respondent being 32.5 years of age (mean = 34.6, SD = 10.8). Though much of the sample has ample experience with making predictions for social science research questions by virtue of being part of the Superforecaster Panel, my sample rates their five-point Likert confidence in their predictions at a median of 2.5 (mean = 2.4, SD = 1). This is sensible, as only nine respondents (14.5%) report conducting prior research on the topics discussed in my survey. The sample is male-dominated, with 53 respondents (85.5%) reporting a masculine gender identity. The SSPP sample also predominantly originates from WEIRD countries (Henrich, Heine, & Norenzayan 2010) – 42 respondents (67.7%) spent the majority of their time prior to starting university education in OECD member states, and 48 respondents (77.4%) have spent the majority of their time since starting university education in OECD member states.

H Equivalence Testing Failure Rate Computation

Let j be an individual partition, and let i index an individual estimate. j represents an individual claim when calculating claim-level ETFRs, whereas j represents an entire article when calculating article-level ETFRs. Each estimate i belongs to exactly one partition j. Because all ETFRs in this paper are calculated for symmetric ROPEs, it is sufficient to define ETFR $R(\epsilon, \tau)$ as a function of ROPE length $\epsilon > 0$ and effect size measure $\tau \in \{\sigma, r\}$. Further, because the ECI approach described in Definition 4.3 yields identical results to the TOST procedure described in Definition 4.2, I approach ETFR calculation by defining the exact 95% ECI's outer bound $\text{ECIOB}_{i,j}(\tau)$ for each effect size measure τ of every estimate i. Let M_j represent the number of estimates i belonging to partition j, and let M be the total number of partitions j. One can then calculate the ETFR as

$$R(\epsilon, \tau) = \sum_{j=1}^{M} \sum_{i=1}^{M_j} \frac{\mathbb{1}\left[|\text{ECIOB}_{i,j}(\tau)| > \epsilon\right]}{M_j M}. \tag{A1}$$

I also calculate claim-level ETFRs that apply an inverse weighting approach, ensuring that each article receives the same weight in the sample. Let $W_{j,k}$ be equal to 1 divided by the number of claims that belong to claim j's article, and let k be an individual article. Then the inverse-weighted claim-level ETFR can be written as

$$R_{\text{Wgt.}}(\epsilon, \tau) = \frac{1}{\sum_{j=1}^{M} W_{j,k}} \sum_{j=1}^{M} W_{j,k} \sum_{i=1}^{M_{j,k}} \frac{\mathbb{1}\left[|\text{ECIOB}_{i,j,k}(\tau)| > \epsilon\right]}{M_{j,k}}, \tag{A2}$$

where $M_{j,k}$ is now the number of estimates belonging to claim j in article k, and M is now the total number of articles.

I measure precision using standard errors of the mean for the unweighted ETFRs in Equation A1 and standard errors of the weighted mean for the weighted ETFRs

in Equation A2. The standard error of the mean for an ETFR is

$$\text{SE}\left[R(\epsilon,\tau)\right] = \frac{\text{SD}\left[R(\epsilon,\tau)\right]}{\sqrt{M}}, \tag{A3}$$

where $\text{SD}\left[R(\epsilon,\tau)\right]$ is just the within-sample standard deviation of $R(\epsilon,\tau)$. Let the ETFR for claim j in article k be defined as

$$R_{j,k}(\epsilon,\tau) = \sum_{i=1}^{M_{j,k}} \frac{\mathbb{1}\left[|\text{ECIOB}_{i,j,k}(\tau)| > \epsilon\right]}{M_{j,k}}.$$

Though Gatz & Smith (1995) note that there is no universally-agreed definition for the standard error of the weighted mean, they find that one formulation produces closer estimates to the bootstrap than other competing formulas. In this setting, the square of that optimal formula can be written as

$$\begin{aligned}\left(\text{SE}\left[R_{\text{Wgt.}}(\cdot)\right]\right)^2 = \frac{M}{(1-M)\,M^2}\Bigg[&\sum_{j=1}^{M}\left\{\left[W_{j,k}R_{j,k}(\cdot) - \overline{W}_{j,k}R_{\text{Wgt.}}(\cdot)\right]^2\right\} - \\ &2R_{\text{Wgt.}}(\cdot)\sum_{j=1}^{M}\left\{\left(W_{j,k} - \overline{W}_{j,k}\right)\left[W_{j,k}R_{j,k}(\cdot) - \overline{W}_{j,k}R_{\text{Wgt.}}(\cdot)\right]\right\} + \\ &\left[R_{\text{Wgt.}}(\cdot)\right]^2\sum_{j=1}^{M}\left\{\left[W_{j,k} - \overline{W}_{j,k}\right]^2\right\}\Bigg],\end{aligned}$$

where $\overline{W}_{j,k}$ is the mean inverse weight $W_{j,k}$ across all claims and M is the total number of articles. The results in Section 6.2 show that this standard error derivation corresponds quite closely with simple standard errors for unweighted ETFRs as derived in Equation A3.

I Online Appendix Tables and Figures

	(1)	(2)	(3)	(4)	(5)	(6)
γ_r	-0.046	·	-0.02	0.002	0.214	0.228
	(0.016)	(·)	(0.017)	(0.02)	(0.023)	(0.028)
Type	Judgment	Judgment	Judgment	Judgment	Prediction	Prediction
Rate	Type I Error	Type II Error	TOST/ECI Failure	TOST/ECI Failure	TOST/ECI Failure	TOST/ECI Failure
Effect Size Measure			σ	r	σ	r

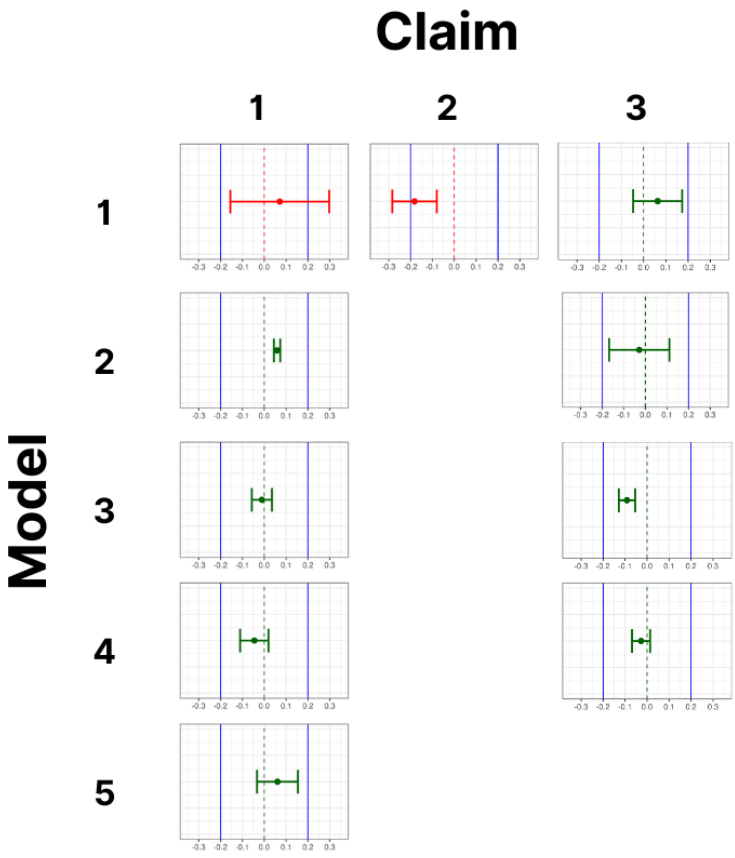
Note: This table provides the numerical estimates displayed in Figure 4.

Table A2: Within-Researcher Estimates of Differences in Predictions/Judgments

	(1)	(2)	(3)	(4)	(5)	(6)
Equivalence Testing Failure Rate	0.361	0.385	0.379	0.633	0.609	0.617
	(0.035)	(0.041)	(0.044)	(0.038)	(0.044)	(0.048)
Effect Size Measure	σ	σ	σ	r	r	r
SSPP Tolerance	0.1065	0.1065	0.1065	0.1295	0.1295	0.1295
Aggregation Level	Claim	Claim	Article	Claim	Claim	Article
Inverse Weighting		x			x	

Note: This table provides the numerical estimates displayed in Figure 6. The σ ROPE is $[-0.2, 0.2]$, and the r ROPE is $[-0.1, 0.1]$. SSPP tolerance indicates the median SSPP respondent's tolerance for ETFRs for the given ROPE (see Section 5.3).

Table A3: Main Equivalence Testing Failure Rate Estimates



	1	2	3
# models not significantly bounded within the ROPE	1	1	0
Total # models defending the claim	5	1	4
Failure rate	20%	100%	0%
Average claim-level failure rate	(20% + 100% + 0%) / 3 = **40%**		

Figure A1: Visualization of Equivalence Testing Failure Rate Computation

J Robustness Checks

This appendix reports extended robustness checks on the main results in Section 6.2.

	Estimates	Claims	Articles	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Initially Insignificant Estimates	790	134	80	0.34 (0.035)	0.36 (0.041)	0.353 (0.044)	0.618 (0.039)	0.587 (0.045)	0.594 (0.05)
Panel B: Initially Significant Estimates	86	32	26	0.576 (0.087)	0.625 (0.088)	0.622 (0.091)	0.719 (0.081)	0.756 (0.078)	0.756 (0.084)
Effect Size Measure				σ	σ	σ	r	r	r
SSPP Tolerance				0.1065	0.1065	0.1065	0.1295	0.1295	0.1295
Aggregation Level				Claim	Claim	Article	Claim	Claim	Article
Inverse Weighting					x			x	

Note: Estimates with initial standard NHST p-values ≥ 0.05 (before conformability changes, if applicable) are removed from the sample in Panel A, and constitute the entire sample in Panel B. The σ ROPE is $[-0.2, 0.2]$, and the r ROPE is $[-0.1, 0.1]$. SSPP tolerance indicates the median SSPP respondent's tolerance for ETFRs for the given ROPE (see Section 5.3).

Table A4: ETFR Robustness – Initial Estimate Significance

	Estimates	Claims	Articles	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: CYCD Removed	675	105	63	0.342 (0.04)	0.362 (0.046)	0.356 (0.049)	0.62 (0.044)	0.617 (0.049)	0.628 (0.054)
Panel B: CYBD Removed	563	91	59	0.36 (0.045)	0.37 (0.049)	0.369 (0.054)	0.621 (0.047)	0.558 (0.053)	0.562 (0.058)
Panel C: BYCD Removed	563	124	74	0.398 (0.038)	0.417 (0.043)	0.409 (0.047)	0.651 (0.04)	0.631 (0.046)	0.64 (0.051)
Panel D: BYBD Removed	653	119	73	0.365 (0.038)	0.39 (0.043)	0.386 (0.046)	0.634 (0.04)	0.625 (0.046)	0.629 (0.052)
Effect Size Measure				σ	σ	σ	r	r	r
SSPP Tolerance				0.1065	0.1065	0.1065	0.1295	0.1295	0.1295
Aggregation Level				Claim	Claim	Article	Claim	Claim	Article
Inverse Weighting					x			x	

Note: Panels denote whether estimates corresponding to continuous/binary outcome/exposure variables (respectively) are removed from the sample. For example, 'CYBD removed' implies that estimates corresponding to a continuous outcome variable and a binary exposure variable are removed from the sample. The σ ROPE is $[-0.2, 0.2]$, and the r ROPE is $[-0.1, 0.1]$. SSPP tolerance indicates the median SSPP respondent's tolerance for ETFRs for the given ROPE (see Section 5.3).

Table A5: ETFR Robustness – Regressor Type Combination

	Estimates	Claims	Articles	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Reproducible Estimates Removed	803	123	74	0.388 (0.038)	0.406 (0.043)	0.399 (0.047)	0.618 (0.04)	0.607 (0.046)	0.615 (0.051)
Panel B: Non-Conformable Estimates Removed	807	130	77	0.358 (0.036)	0.37 (0.041)	0.365 (0.044)	0.65 (0.038)	0.626 (0.044)	0.636 (0.049)
Effect Size Measure				σ	σ	σ	r	r	r
SSPP Tolerance				0.1065	0.1065	0.1065	0.1295	0.1295	0.1295
Aggregation Level				Claim	Claim	Article	Claim	Claim	Article
Inverse Weighting					x			x	

Note: Estimates are non-reproducible if my best attempts to reproduce the exact published estimates using the article's replication repository do not succeed. Estimates are 'non-conformable' if the models that produce them require conformability modifications before inclusion in the final sample. The σ ROPE is $[-0.2, 0.2]$, and the r ROPE is $[-0.1, 0.1]$. SSPP tolerance indicates the median SSPP respondent's tolerance for ETFRs for the given ROPE (see Section 5.3).

Table A6: ETFR Robustness – Reproducibility/Conformability

	Models	Claims	Articles	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Unqualified Null Claims	599	88	58	0.356 (0.043)	0.384 (0.049)	0.379 (0.052)	0.671 (0.047)	0.66 (0.052)	0.664 (0.057)
Panel B: Qualified Null Claims	277	47	30	0.372 (0.063)	0.349 (0.068)	0.351 (0.074)	0.562 (0.064)	0.504 (0.072)	0.515 (0.077)
Effect Size Measure				σ	σ	σ	r	r	r
SSPP Tolerance				0.1065	0.1065	0.1065	0.1295	0.1295	0.1295
Aggregation Level				Claim	Claim	Article	Claim	Claim	Article
Inverse Weighting					x			x	

Note: Estimates defend 'qualified' or 'unqualified' claims based on the definition in Table 2. The σ ROPE is $[-0.2, 0.2]$, and the r ROPE is $[-0.1, 0.1]$. SSPP tolerance indicates the median SSPP respondent's tolerance for ETFRs for the given ROPE (see Section 5.3).

Table A7: ETFR Robustness – Qualification

	Models	Claims	Articles	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Main Null Claims	567	83	53	0.375 (0.044)	0.39 (0.051)	0.382 (0.053)	0.558 (0.048)	0.528 (0.056)	0.537 (0.06)
Panel B: Secondary Null Claims	309	52	33	0.34 (0.06)	0.369 (0.067)	0.367 (0.073)	0.753 (0.057)	0.745 (0.062)	0.753 (0.069)
Effect Size Measure				σ	σ	σ	r	r	r
SSPP Tolerance				0.1065	0.1065	0.1065	0.1295	0.1295	0.1295
Aggregation Level				Claim	Claim	Article	Claim	Claim	Article
Inverse Weighting					x			x	

Note: Estimates defend 'main' or 'secondary' claims based on the definition in Section 3, where 'secondary' claims include mechanism, robustness, and subgroup claims. The σ ROPE is $[-0.2, 0.2]$, and the r ROPE is $[-0.1, 0.1]$. SSPP tolerance indicates the median SSPP respondent's tolerance for ETFRs for the given ROPE (see Section 5.3).

Table A8: ETFR Robustness – Prominence

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