

# Why Are Replication Rates So Low?

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*This paper provides theoretical and empirical evidence for why replication rates are low in the social sciences. Theoretically, I show that we should expect replication rates to fall below their intended power targets owing to issues with common power calculations in replication studies, even in the case where original studies are unbiased and there is no  $p$ -hacking or treatment effect heterogeneity. Empirically, I find that the interaction of issues with common power calculations and low power in original studies can fully explain observed replication rates in experimental economics and social science, and two-thirds of the replication rate gap in psychology. (JEL C18, C53, C90)*

In a 2016 survey conducted by *Nature*, 90% of researchers across various fields agreed that the scientific community faces a ‘reproducibility crisis’ (Baker, 2016). Growing consensus has been supported by high-profile replication projects which find that the replication rate – i.e. the fraction of replications that are significant with the same sign as the original study – is just 36% in psychology, 61% in experimental economics, and 62% in experimental social science (Open Science Collaboration, 2015; Camerer, Dreber et al., 2016, 2018).

Understanding the underlying cause of low replication rates is important for researchers and reformers aiming to improve the credibility of published research. There is a large literature examining a wide range of explanations, including selective publication against null results (Franco, Malhotra and Simonovits, 2014; Open Science Collaboration, 2015; Camerer, Dreber et al., 2016, 2018);  $p$ -hacking and other questionable research practices (Ioannidis, 2005, 2008; Simonsohn, Nelson and Simmons, 2014; Brodeur et al., 2016; Brodeur, Cook and Heyes, 2020, 2022); and heterogeneity across original studies and replications in research design and experimental subjects (Higgins and Thompson, 2002; Cesario, 2014; Simons, 2014; Stanley, Carter and Doucouliagos, 2018; Bryan, Yeager and O’Brien, 2019).

In this article, the main theoretical result shows that we should expect the replication rate to fall short of its intended target, regardless of whether or not there is selective publication,

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\**This version:* March 14, 2023. Brown University. patrick.vu@brown.edu. I am especially grateful for the feedback, advice, and encouragement of Jonathan Roth. For helpful comments, suggestions and conversations, I thank Johannes Abeler, Daniel Björkegren, Pedro Dal Bó, Anna Dreber, Peter Hull, Toru Kitagawa, Soonwoo Kwon, and Jesse Shapiro, as well as seminar participants at Brown University, University College London and the AIMOS 2022 conference.

and even in ‘ideal’ conditions with no  $p$ -hacking or heterogeneity. This is because of issues with the common approach of setting power in replications. Let  $RP(x, \theta, \sigma_r)$  be the probability of successfully replicating a study with realized original effect size  $x$ , true effect  $\theta$ , and replication standard error  $\sigma_r$ . Replicators commonly set the replication standard error (or equivalently the replication sample size) so that  $RP(x, \theta, \sigma_r)$  equals a pre-specified power target if  $x = \theta$  (e.g. 90%). In practice, replication rates consistently fall below this target, which is commonly interpreted as an indicator that original effects are biased due to selective publication or  $p$ -hacking. However, this article highlights that under the common approach for setting replication power, the replication probability function,  $RP(\cdot)$ , is a non-linear function of the original estimate  $X$ . Thus, even if original estimates were unbiased, with  $\mathbb{E}[X] = \theta$ , Jensen’s Inequality implies that the expected replication rate does not in general equal the replication probability evaluated at the expectation:  $\mathbb{E}[RP(X, \theta, \sigma_r)] \neq RP(\theta, \theta, \sigma_r)$ . In the main theoretical result, I show that local concavity in the replication probability function implies that  $\mathbb{E}[RP(X, \theta, \sigma_r)] < RP(\theta, \theta, \sigma_r)$ , and thus estimated replication power is upwards biased. This means that stated replication rate targets in large-scale replication studies do not provide an attainable benchmark against which to judge replication rates observed in practice; even if original studies were unbiased, such targets are not in fact reachable in expectation.

Issues with common power calculations in replications are most severe when power in original studies is low. This is because the concavity of the replication probability function,  $RP(\cdot)$ , is more pronounced when original studies have low power. Given estimates of power between 20–40% in various empirical literatures (Button et al., 2013; Ioannidis, Stanley and Doucouliagos, 2017; Stanley, Carter and Doucouliagos, 2018; Arel-Bundock et al., 2022), this provides a strong theoretical basis for expecting low replication rates a priori, even in the absence of other issues such as selective publication or  $p$ -hacking.

To what extent can these theoretically insights explain the low replication rates actually observed in large-scale replication studies? Answering this empirical question requires knowledge of the underlying distribution of latent studies i.e. the distribution of true effects and standard errors for published and unpublished studies. To obtain this, I estimate the empirical model in Andrews and Kasy (2019), which estimates the latent distribution of studies by adjusting for publication bias. With these model estimates, I implement a novel approach for producing replication rate predictions in three large-scale replications (Open Science Collaboration, 2015; Camerer, Dreber et al., 2016, 2018). Importantly, estimation only uses data from original studies, and predictions are based on the power calculations that were actually implemented in replications. The model does not incorporate researcher manipulation or heterogeneous treatment effects. The empirical exercise asks, in effect, whether observed replication rates could have been predicted by issues with power alone, before the replication studies themselves were

actually undertaken.

The predicted replication rate is almost identical to observed replication rates in experimental economics (60% vs. 61%) and experimental social science (55% vs. 57%). This provides evidence that the interaction of replication power issues and low power in original studies is sufficient to explain observed replication rates in these fields. This is consistent with evidence of a low propensity of  $p$ -hacking in experimental settings, perhaps because of fewer researcher degrees of freedom compared to observational studies (Brodeur et al., 2016; Brodeur, Cook and Heyes, 2020; Imai et al., 2020). In psychology, the model predicts a replication rate of 55%. This is well below mean intended power of 92%, but still above the observed replication rate of 35%. Here, the model can account for around two-thirds of the observed replication rate gap. This discrepancy suggests that factors not included in the model – for example, heterogeneous treatment effects,  $p$ -hacking, or differences across subfields – may be important in psychology.

*Related Literature.*—This article contributes to the large metascience literature and the growing literature on predicting research outcomes (Ioannidis, 2005; Franco, Malhotra and Simonovits, 2014; Gelman and Carlin, 2014; Dreber et al., 2015; Maxwell, Lau and Howard, 2015; Anderson and Maxwell, 2017; Stanley, Carter and Doucouliagos, 2018; Miguel and Christensen, 2018; Altmejd et al., 2019; Amrhein, Trafimow and Greenland, 2019; DellaVigna, Otis and Vivaldi, 2020; Gordon et al., 2020; Frankel and Kasy, 2022; DellaVigna and Linos, 2022; Nosek et al., 2022). Andrews and Kasy (2019) and Kasy (2021) provide stylized examples showing that the replication rate can vary widely depending on the latent distribution of studies (i.e. the joint distribution of true effects and standard errors for published and unpublished studies). Theoretically, this article builds on this observation by establishing that the expected replication rate is bounded above by its nominal target owing to issues with common power calculations in replication studies. This result holds for any distribution of latent studies. Empirically, I provide evidence that among the profusion of explanations for low replication rates, issues with power calculations alone can adequately account for observed replication outcomes in experimental economics and social science.

## I. Theory

### A. Model of Large-Scale Replication Studies

We begin by introducing a model of how original studies are generated, published, and replicated. The model presented here is a special case of the model in Andrews and Kasy (2019), where we explicitly incorporate how replicators calculate standard errors.

Suppose a large-scale replication study is conducted on an empirical literature of interest

and we observe the estimated effect sizes and standard errors for original studies and their replications. Let upper case letters denote random variables, lower case letters realizations. Latent studies (published or unpublished) have a superscript \* and published studies have no superscript. The model has five steps:

1. **Draw a population parameter and standard error:** Draw a research question with population parameter ( $\Theta^*$ ) and standard error ( $\Sigma^*$ ):

$$(\Theta^*, \Sigma^*) \sim \mu_{\Theta, \Sigma}$$

where  $\mu_{\Theta, \Sigma}$  is the joint distribution of latent true effects and latent standard errors.

2. **Estimate the effect:** Draw an estimated effect from a normal distribution with parameters from Stage 1:

$$X^* | \Theta^*, \Sigma^* \sim N(\Theta^*, \Sigma^{*2})$$

3. **Publication selection:** Selective publication is modelled by the function  $p(\cdot)$ , which returns the probability of publication for any given  $t$ -ratio. Let  $D$  be a Bernoulli random variable equal to 1 if the study is published and 0 otherwise:

$$\mathbb{P}(D = 1 | X^*/\Sigma^*) = p\left(\frac{X^*}{\Sigma^*}\right) \quad (1)$$

4. **Replication selection:** Replications are sampled from published studies ( $X, \Sigma, \Theta$ ) (i.e. latent studies ( $X^*, \Sigma^*, \Theta^*$ ) conditional on publication ( $D = 1$ )). Replication selection is modelled by the function  $r(\cdot)$ , which returns the probability of being chosen for replication for any given  $t$ -ratio. Let  $R$  be a Bernoulli random variable equal to 1 if the study is chosen for replication and 0 otherwise:

$$\mathbb{P}(R = 1 | X/\Sigma) = r\left(\frac{X}{\Sigma}\right) \quad (2)$$

5. **Replication:** Replicators set the replication standard error  $\sigma_r(X, \beta^n)$  as a function of the original estimated effect  $X$  and intended statistical power  $1 - \beta^n$ , where  $\beta^n$  denotes nominal Type II error. A replication draw is made with:

$$X_r | X, \Theta, D = 1, R = 1 \sim N\left(\Theta, \sigma_r(X, \beta^n)^2\right)$$

We observe i.i.d draws of  $(X, \Sigma, X_r, \sigma_r(X, \beta^n))$  from the conditional distribution of

$(X^*, \Sigma^*, X_r, \sigma_r(X^*, \beta^n))$  given  $D = 1$  and  $R = 1$ . This model is equivalent to the one presented in [Andrews and Kasy \(2019\)](#), except instead of remaining agnostic about how  $\sigma_r$  is chosen, I impose an empirically relevant functional form for how replicators set  $\sigma_r$ , which is a key focus of the analysis.

In particular, I examine the statistical properties of the common method of setting the replication standard error to detect the original estimate  $X$  with a prespecified power target  $1 - \beta^n$ . This approach is implemented, for example, in [Open Science Collaboration \(2015\)](#) and [Camerer, Dreber et al. \(2016\)](#), and a survey of replications the psychology literature by [Anderson and Maxwell \(2017\)](#) shows that it is the most commonly implemented approach.

**DEFINITION 1** (Common power rule). *The common power rule to detect original effect size  $x$  with intended power  $1 - \beta^n$  sets the replication standard error to*

$$\sigma_r(x, \beta^n) = \frac{|x|}{1.96 - \Phi^{-1}(\beta^n)} \quad (3)$$

*This is equivalent to setting the replication sample size to  $N \times \left[ \frac{\sigma}{|x|} (1.96 - \Phi^{-1}(\beta^n)) \right]^2$ , where  $N$  and  $\sigma$  are the original study's sample size and standard deviation, respectively.*

The justification for the common power rule is that replication power equals its intended target of  $1 - \beta^n$  when  $x = \theta$ .<sup>1</sup> In practice, replication rates consistently fall below this benchmark, which is typically taken as evidence that original estimates are biased because of selective publication or  $p$ -hacking. While this argument has intuitive appeal, it does not account for the fact that replication power is a non-linear function of the random original estimate  $X$ ; thus, even if  $\mathbb{E}[X] = \theta$ , the replication probability evaluated at the expectation (which equals the intended target) will not, in general, be equal to the expected replication rate.

This argument is developed more detail in the following section, under a number of regularity conditions and assumptions imposed on the DGP. First, we impose the normalization that true effects are positive:<sup>2</sup>

**ASSUMPTION 1** (True effect normalization). *The support of  $\Theta$  is a subset of the non-negative real line.*

In step three of the model, we allow for fairly general forms of selective publication:

**ASSUMPTION 2** (Publication selection function). *Let  $p(t) \neq 0$  for all  $|t| \geq 1.96$ ,  $p(t)$  be weakly*

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<sup>1</sup>For a formal statement and proof, see Lemma B1.

<sup>2</sup>Large-scale replications include studies that examine different questions and outcomes. Normalizing true effects to be positive is justified because relative signs across studies are arbitrary.

increasing for all  $t \geq 1.96$ , and  $p(t) = p(-t)$  for all  $t \geq 1.96$ . Allow  $p(\cdot)$  to take any form when  $t \in (-1.96, 1.96)$ .

This allows for (weakly) higher publication probabilities for higher  $t$ -ratios (e.g. at common critical thresholds 1.64, 1.96, 2.58). It also allows for publication regimes with no selective publication at all (i.e.  $p(t) = 1$  for all  $t$ ). Finally, in step 4, which models the replication selection mechanism, we assume that the set of significant results chosen for replication is a random sample from published, significant results:

ASSUMPTION 3 (Replication selection function). *For all  $|t| \geq 1.96$ , let  $r(t) = c \in (0, 1]$  and allow  $r(\cdot)$  to take any form when  $t \in (-1.96, 1.96)$ .*

### B. Common Power Calculations and Low Replication Rates

This section defines the key concepts and then discusses the main result. First, we define the replication probability of a single study and then use this to define the expected replication probability over multiple studies.

DEFINITION 2 (Replication probability of a single study). *The replication probability of a published study  $(X, \Sigma, \Theta)$  chosen for replication ( $R = 1$ ) is*

$$RP(X, \Theta, \sigma_r(X, \beta^n)) = \mathbb{P}\left(\frac{|X_r|}{\sigma_r(X, \Sigma, \beta^n)} \geq 1.96, \text{sign}(X_r) = \text{sign}(X) \mid X, \Theta, \beta^n, R = 1\right) \quad (4)$$

This definition captures the dual requirement that the replication estimate is statistically significant and has the same sign as the original study.

DEFINITION 3 (Expected replication rate). *The expected replication probability is defined over published studies  $(X, \Sigma, \Theta)$  which are chosen for replication ( $R = 1$ ) and statistically significant ( $S_X = 1$ ). It is equal to*

$$\mathbb{E}\left[RP(X, \Theta, \sigma_r(X, \beta^n)) \mid R = 1, S_X = 1\right] \quad (5)$$

Substituting the common power rule in Definition 1 for the replication standard error gives the expected replication rate under the common power rule. Note that while insignificant results may be replicated, they are not included in the replication rate in Definition 3, in line with the main definition reported in most large-scale replication studies (Klein et al., 2014;

Open Science Collaboration, 2015; Camerer, Dreber et al., 2016, 2018; Klein et al., 2018).<sup>3</sup> With this, we can state the main theoretical result:

**PROPOSITION 1** (The common power rule implies the expected replication rate is below its target.) *Consider the model in I.A. Under assumptions 1, 2, and 3, if replication standard errors are set by the common power rule to detect original estimates with intended power  $1 - \beta^n \geq 0.8314$ , then*

$$\mathbb{E}\left[RP(X, \Theta, \sigma_r(X, \beta^n)) | R = 1, S_X = 1\right] < 1 - \beta^n \quad (6)$$

From a practical perspective, Proposition 1 means that replicators who set the replication sample size to detect original effect sizes should not expect the replication rate to reach its intended target, regardless of whether or not there is selective publication, and even under ‘ideal’ conditions with conditions with no researcher manipulation, replications with identical designs and comparable samples (i.e. no heterogeneity in true effects), no measurement error, random sampling in replication selection, and high-powered original studies. That the intended target is not in fact attainable in expectation underscores fundamental difficulties in interpreting replication rate gaps observed in large-scale replication studies.

Figure 1 provides intuition for this result. It plots the replication probability function in Definition 2 as a function of the original effect  $X$ , for a fixed  $\theta$  and assuming that the common power rule is applied with intended target  $1 - \beta^n = 0.9$ . It is evident that the replication probability is non-linear in  $X$ , which implies that  $\mathbb{E}[RP(X, \theta, \sigma_r)] \neq RP(\mathbb{E}[X], \theta, \sigma_r)$ , even if  $X$  is unbiased. If  $RP(\cdot)$  were globally concave, Proposition 1 would immediately follow from Jensen’s inequality. However, it is only locally concave around the true effect  $\theta$ . The proof of Proposition 1 shows that when  $1 - \beta^n > 0.8314$ , local concavity is sufficient to arrive at the same result for any distribution of latent studies.

The difference between the expected replication rate and its intended target is larger when power in original studies is low. This is because the concavity of  $RP(\cdot)$  is more pronounced when original studies have low power. As an illustration, Figure 2 plots the relationship between the expected replication rate and original power assuming replication power is set to 90%, close to mean reported replication power in Open Science Collaboration (2015) and Camerer, Dreber et al. (2016). To highlight the impact of original power, the relationship is derived assuming

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<sup>3</sup>Replication power calculations themselves are typically designed with this definition in mind. Complementary replication measures include: the relative effect size; whether the 95% confidence interval of the replication covers the original estimate; replication based on meta-analytic estimates; the 95% prediction interval approach (Patil, Peng and Leek, 2016); the ‘small telescopes’ approach (Simonsohn, 2015); and the one-sided default Bayes factor (Wagenmakers, Verhagen and Ly, 2016).

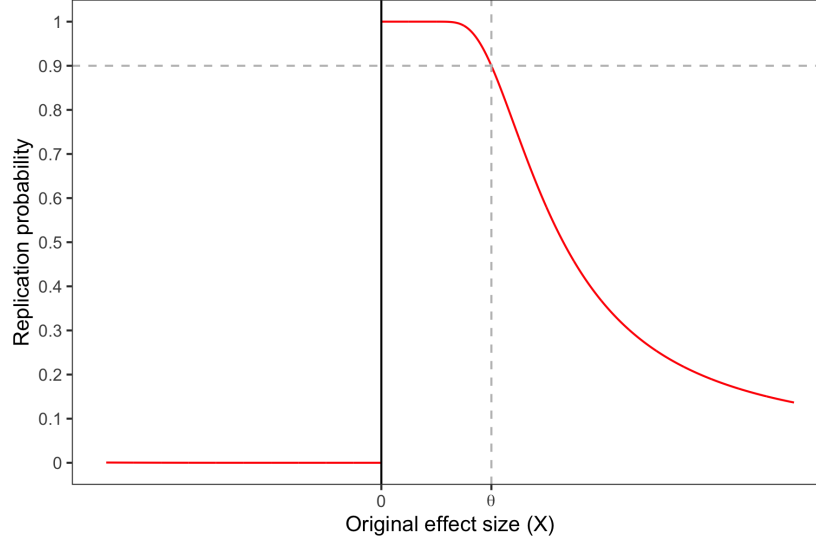


FIGURE 1. REPLICATION PROBABILITY FUNCTION CONDITIONAL ON  $\Theta$

*Notes:* Replication probability function in Definition 2 conditional on a fixed  $\theta$ . The replication standard error is calculated using the common power rule in Definition 1 to detect original effect sizes with 90% power (i.e.  $\sigma_r(X, \beta^n) = |X|/3.242$ ).

no  $p$ -hacking, no selective publication, and no heterogeneity (i.e. assuming exact replications). The plot shows that the expected replication rate is bounded below its intended target of 90%, in line with Proposition 1, and is especially low when original studies have low power. For instance, a randomly drawn unbiased, original study with 33% power has an expected replication probability of around 50%. With relatively low estimates of power across various empirical literatures, this provides strong theoretical grounds for expecting low replication rates in practice, even in the absence of issues with  $p$ -hacking or treatment effect heterogeneity. For intuition, note that the replication probability of a null effect is 0.025 (regardless of the how the replication standard error is chosen). Continuity implies that when original studies have true effects close to zero, and therefore have low statistical power, that replication probabilities will also be very low.

Two other factors affect the replication rate; however, empirically, their impact turns out to be relatively small. First, as can be seen in Figure 1, when original estimates are significant but with the ‘wrong’ sign, the probability of replication is very low (below 0.025) because it requires the highly unlikely event that the replication estimate also has the wrong sign and is statistically significant. Second, the replication rate induces upward bias in original estimates because it is, by definition, calculated on a selected sample of significant findings. Replication estimates will regress to the mean (Galton, 1886; Hotelling, 1933; Barnett, Van Der Pols and Dobson,



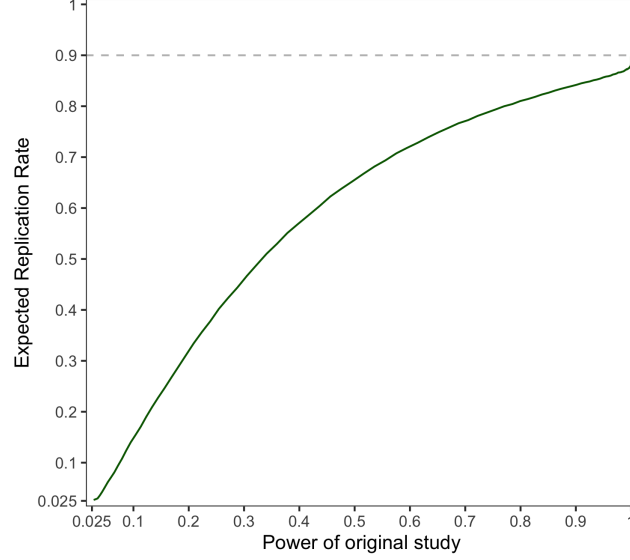


FIGURE 2. ORIGINAL POWER AND THE EXPECTED REPLICATION RATE UNDER THE COMMON POWER RULE

*Notes:* Original power and the expected replication rate under the common power rule are both functions of  $\omega = \theta/\sigma$  (normalized to be positive). Original power to obtain a significant effect with the same sign as the true effect is equal to  $1 - \Phi(1.96 - \omega)$ . The expected replication rate is calculated by taking  $10^6$  draws of  $Z$  from  $N(\omega, 1)$  and then calculating  $10^{-6} \sum_{i=1}^{10^6} [1 - \Phi(1.96 - \text{sign}(z_i) \frac{\omega}{\sigma_r(z_i, \beta^n)})]$ , with intended power equal to  $1 - \beta^n = 0.9$  and depicted by the horizontal dashed line. The replication standard error is calculated using the common power rule (Definition 1) to detect original effect sizes with 90% power, which is given by  $\sigma_r(z_i, \beta^n) = |z_i|/3.242$ . This figure assumes no  $p$ -hacking, no heterogeneity in true effects, no selective publication and random replication selection. Further details are provided in Section I.

2004; Kahneman, 2011)<sup>4</sup>, although the ultimate impact on the replication rate is ambiguous since conditioning on significance also tends to select larger true effects with higher replication probabilities. Appendix C derives and estimates a decomposition of the replication rate gap in economics and psychology, using the empirical methodology described in the next section, and finds that it is almost entirely explained by the concavity of  $RP(\cdot)$ .

Finally, a common perception is that selective publication favouring significant results – either by authors or journals – produces more ‘false-positives’ in the published literature, which are in turn harder to replicate. This theory is important to address because it enjoys substantial support: over 90% of researchers cite ‘selective reporting’ as a contributing factor to irreproducibility, more than any other factor (Baker, 2016). However, Andrews and Kasy (2019) and Kasy (2021) show that the replication rate in fact tells us very little about selective publication. Both provide examples showing that the replication rate can take on almost any value depending on the size of the underlying true effect, irrespective of how selective publication is. In fact, the replication rate in the model presented here and in Andrews and Kasy (2019) is completely

<sup>4</sup>For a formal statement and proof, see Proposition B1 in Appendix B.

insensitive to selective publication against null results.<sup>5</sup> This follows from the simple fact that the replication rate definition does not include statistically insignificant results. Thus, even if insignificant results were being widely published, they would not be included in the replication rate.<sup>6,7</sup>

## II. Empirical Applications

Given the range of explanations for low replication rates, to what extent can issues with power emphasized in Proposition 1 *alone* explain observed replication rates? To answer this question, the theory requires that we estimate the latent distribution of studies. This can then be used to generate replication rate predictions which can be compared to observed replication rates. The procedure is as follows:

1. Estimate the latent distribution of studies,  $\mu_{\Theta, \Sigma}$  using an augmented version of the [Andrews and Kasy \(2019\)](#) model applied to three large-scale replications. Estimation does not use any data from replications.
2. Use the estimated model to simulate replications and predict what fraction of significant results would replicate, absent any other issues such as  $p$ -hacking or heterogeneity.
3. Compare these predictions (which do not use any data from the replications) to actual replication outcomes.

Accurate predictions provide evidence that the issues with power calculations underlying Proposition 1 can adequately explain low observed replication rates. Discrepancies suggest factors not included in the model may also be important.

### A. Replication Studies

I examine three replication studies. [Camerer, Dreber et al. \(2016\)](#) replicate results from all 18 between subjects laboratory experiments published in *American Economic Review* and

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<sup>5</sup>For a formal statement, see Proposition B2 in Appendix B, which proves this more generally for measures that condition on statistical significance. Setting  $g(x, \sigma, x_r, \beta^n) = \mathbb{1}\left[\frac{|x_r|}{\sigma_r(x, \sigma, \beta^n)} \geq 1.96, \text{sign}(x_r) = \text{sign}(x)\right]$  gives the result for the replication rate measure.

<sup>6</sup>A caveat is that the model assumes a fixed distribution of latent studies, whereas in practice it may be endogenous, for example, if researchers engage in more specification searching when publication bias against null results is high ([Simonsohn, Nelson and Simmons, 2014](#); [Brodeur et al., 2016](#); [Brodeur, Cook and Heyes, 2020, 2022](#)). I examine the possible impact of manipulation on the replication rate in Section II.

<sup>7</sup>Appendix D examines measures of replication which may be more sensitive to changes in selective publication than the replication rate. For evaluating efforts to reduce selective publication, simulation results show that the prediction interval approach ([Patil, Peng and Leek, 2016](#)), when calculated over both significant and insignificant results, may provide a useful alternative to the replication rate, the confidence interval measure, and the meta-analysis approach.

*Quarterly Journal of Economics* between 2011 and 2014. [Open Science Collaboration \(2015\)](#) replicate results from 100 psychology studies in 2008 from *Psychological Science*, *Journal of Personality and Social Psychology*, and *Journal of Experimental Psychology: Learning, Memory, and Cognition*. Following [Andrews and Kasy \(2019\)](#), I consider a subsample of 73 studies with test statistics that are well-approximated by  $z$ -statistics. [Camerer, Dreber et al. \(2018\)](#) replicate 21 experimental studies in the social sciences published between 2010 and 2015 in *Science* and *Nature*.

In [Camerer, Dreber et al. \(2016\)](#), replicators set power to detect original effects with at least 90% power. In [Open Science Collaboration \(2015\)](#), replication teams were instructed to achieve at least 80% power, and encouraged to obtain higher power if feasible. Reported mean intended power was 92% in both cases. [Camerer, Dreber et al. \(2018\)](#) implemented a higher-powered design consisting of two stages. In the first stage, replicators implemented power to detect 75% of the original effect with 90% power. In the second stage, further data collection was undertaken for insignificant results from the first stage, such that the pooled sample from both stages was calibrated to detect half of the original effect size with 90% power. I predict replication outcomes in the first stage.<sup>8</sup>

### B. Estimation

To calculate the expected replication rate, it is necessary to estimate the latent distribution of studies  $\mu_{\Theta, \Sigma}$ . To do this, I estimate an augmented version of the empirical model in [Andrews and Kasy \(2019\)](#). Specifically, [Andrews and Kasy \(2019\)](#) develop an empirical model to estimate the marginal distribution of true effects  $\Theta^*$ , but not of standard errors  $\Sigma^*$ . Since predictions of the replication rate also require knowledge of the distribution of  $\Sigma^*$ , I augment the model to estimate the joint distribution of  $(\Theta^*, \Sigma^*)$ . Estimation is based on the ‘metastudy approach’, which only uses data from original studies. Identification requires that true effects are statistically independent of standard errors, a common assumption in meta-analyses. I assume that  $\Sigma^*$  follows a gamma distribution with shape and scale parameters denoted by  $\kappa_\sigma$  and  $\lambda_\sigma$ , respectively.

For all other aspects of the model, I implement identical model specifications as [Andrews and Kasy \(2019\)](#), whose focus is on estimating publication bias. Matching their specifications, I assume that  $|\Theta^*|$  follows a gamma distribution with shape and scale parameters  $(\kappa_\theta, \lambda_\theta)$ ; and that the joint probability of being published and chosen for replication,  $p(X/\Sigma) \times r(X/\Sigma)$ , is a step-function parameterized by  $\beta_p$ . The inclusion of steps at common significance levels (1.64, 1.96, 2.58) varies slightly across applications owing to different approaches for choosing

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<sup>8</sup>Predicting second-stage outcomes is complicated by the fact that one study that was ‘successfully’ replicated in the first stage was erroneously included in the second stage.

TABLE 1 – MAXIMUM LIKELIHOOD ESTIMATES

	Latent true effects $\Theta^*$		Latent standard errors $\Sigma^*$		Selection parameters		
	$\kappa_\theta$	$\lambda_\theta$	$\kappa_\sigma$	$\lambda_\sigma$	$\beta_{p1}$	$\beta_{p2}$	$\beta_{p3}$
<i>Economics experiments</i>							
Augmented model	1.426 (1.282)	0.148 (0.072)	2.735 (0.536)	0.103 (0.031)	0.000 (0.000)	0.039 (0.05)	– –
Andrews and Kasy (2019)	1.343 (1.285)	0.157 (0.075)	– –	– –	0.000 (0.000)	0.038 (0.05)	– –
<i>Psychology experiments</i>							
Augmented model	0.782 (0.782)	0.179 (0.179)	4.698 (4.698)	0.044 (0.044)	0.012 (0.012)	0.303 (0.303)	– –
Andrews and Kasy (2019)	0.734 (0.408)	0.185 (0.056)	– –	– –	0.012 (0.007)	0.300 (0.134)	– –
<i>Social science experiments</i>							
Augmented model	0.070 (0.091)	0.663 (0.326)	5.792 (1.754)	0.028 (0.009)	0.000 (0.000)	0.000 (0.000)	0.584 (0.419)
Andrews and Kasy (2019)	0.070 (0.091)	0.663 (0.327)	– –	– –	0.000 (0.000)	0.000 (0.000)	0.583 (0.418)

Notes: Maximum likelihood estimates for economics (Camerer, Dreber et al., 2016), psychology (Open Science Collaboration, 2015) and social sciences (Camerer, Dreber et al., 2018). Robust standard errors are in parentheses. Latent true effects and standard errors are assumed to follow a gamma distribution; parameters ( $\kappa, \lambda$ ) are the shape and scale parameters, respectively. In economics and psychology, joint publication and replication probability coefficients are measured relative to the omitted category of studies significant at 5 percent level. For example, in experimental economics, an estimate of  $\beta_{p2} = 0.038$  implies that results which are significant at the 5% level are 26.3 times more likely to be published and chosen for replication than results that are significant at the 10% level but insignificant at the 5% level. In social sciences, the omitted category is studies significant at the 1% level. Andrews and Kasy (2019) estimates are reproduced from accessible data and code from their analysis.

which studies to replicate.<sup>9</sup> Table 1 presents the maximum likelihood estimates together with reproduced estimates from Andrews and Kasy (2019) for comparison.<sup>10</sup> For common parameters, estimates are very close.

### C. The Predicted Replication Rate

Model parameters estimates in Table 1 can be used to generate replication rate predictions by simulating replications using the following procedure:

<sup>9</sup>Details on mechanisms for replication selection are outlined in Appendix E. With  $Z = X/\Sigma$ , the selection functions in each application are:  $r(X/\Sigma) \propto p(X/\Sigma) \mathbb{1}(1.64 \leq |Z| < 1.96) \beta_{p2} + \mathbb{1}(|Z| \geq 1.96)$  in economics;  $r(X/\Sigma) \propto p(X/\Sigma) \mathbb{1}(|Z| < 1.64) \beta_{p1} + \mathbb{1}(1.64 \leq |Z| < 1.96) \beta_{p2} + \mathbb{1}(|Z| \geq 1.96)$  in psychology; and  $r(X/\Sigma) \propto p(X/\Sigma) \mathbb{1}(1.96 \leq |Z| < 2.58) \beta_{p3} + \mathbb{1}(|Z| \geq 2.58)$  for social science experiments. Separate identification of the publication probability function,  $p()$ , requires that we specify the replication selection function  $r()$ .

<sup>10</sup>Estimates for psychology in this article are slightly different to the meta-study estimates reported in Andrews and Kasy (2019) (their Table 2). The difference is due to a misreported  $p$ -value in the raw psychology data for one study, which leads to an erroneous outlier in the distribution of original study standard errors. Table 1 in this article reproduces estimates of their model with the corrected data. Excluding this study in the augmented model leads to very similar replication rate predictions.

1. Draw  $10^6$  latent (published or unpublished) research questions and standard errors  $(\theta^{*sim}, \sigma^{*sim})$  from the estimated joint distribution  $\hat{\mu}_{\Theta, \Sigma}(\hat{\kappa}_\theta, \hat{\lambda}_\theta, \hat{\kappa}_\sigma, \hat{\lambda}_\sigma)$ .
2. Draw estimated effects  $x^{*sim} | \theta^{*sim}, \sigma^{*sim} \sim N(\theta^{*sim}, \sigma^{*sim2})$  for each latent study.
3. Use the estimated selection parameters  $\hat{\beta}_p$  to determine the subset of studies that are published and chosen for replication.
4. For studies chosen for replication, calculate the replication standard error  $\sigma_r^{sim}$  according to the following rule

$$\sigma_r^{sim}(x^{sim}, \beta^n, \psi) = \frac{\psi \cdot |x^{sim}|}{1.96 - \Phi^{-1}(\beta^n)} \quad (7)$$

where  $\psi = 1$  and  $1 - \beta^n = 0.92$  in economics and psychology, which corresponds to the common power rule; and  $\psi = \frac{3}{4}$  and  $1 - \beta^n = 0.9$  in social science experiments, which corresponds to a fractional power rule.<sup>11</sup>

5. Simulate replications by drawing replication effect sizes  $x_r^{sim} | \theta^{sim}, \sigma_r^{sim} \sim N(\theta^{sim}, \sigma_r^{sim2})$

Let  $\{x_i, \sigma_i, x_{r,i}, \sigma_{r,i}\}_{i=1}^{M_{sig}}$  be the (simulated) set of published, replicated original studies that are significant at the 5% level, and their corresponding replication results.<sup>12</sup>  $M_{sig}$  is the number of replicated originally-significant studies. The predicted replication rate is equal to

$$\frac{1}{M_{sig}} \sum_{i=1}^{M_{sig}} \mathbb{1}\left(|x_{r,i}| \geq 1.96\sigma_{r,i}, \text{sign}(x_{r,i}) = \text{sign}(x_i)\right) \quad (8)$$

### D. Results

*Common Power Rule (Economics and Psychology).*—In experimental economics, the predicted replication rate is 60%, which is very close to the observed rate of 61.1% (Table 2). This is an “out-of-sample” prediction in the sense that the model is estimated only using information from the original studies, and does not incorporate any information from the replications. The accuracy of this prediction provides evidence that replication power calculation issues in

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<sup>11</sup>This assumes all simulated replications set intended power equal to the mean of reported intended power. In practice, there was some variation in the application of the power rule around the mean. Appendix F reports predicted replication rates allowing for variation in intended power across studies that matches the empirical variation in each application. Results are very similar and in fact slightly more accurate in all three applications (61.5% in economics; 52.3% in psychology; and 56.5% in social science).

<sup>12</sup>In both experimental economics and psychology, a small number of original results whose  $p$ -values were slightly above 0.05 were treated as ‘positive’ results and included in the replication rate calculation. To match this, I set the cutoff for significant findings for the purposes of replication equal to the smallest  $z$ -statistic that was treated as a ‘positive’ result for replication. Predictions are almost identical with a strict 0.05 significance threshold.

conjunction with low power in original studies is sufficient to explain the observed replication rate. This is also consistent with little evidence of researcher manipulation in experimental settings (Brodeur et al., 2016; Brodeur, Cook and Heyes, 2020; Imai et al., 2020).

	Economics experiments	Psychology	Social sciences
Nominal target (intended power)	0.92	0.92	–
Observed replication rate	0.611	0.348	0.571
Predicted replication rate	0.600	0.545	0.553

TABLE 2 – REPLICATION RATE PREDICTIONS

*Notes:* Economics experiments refers to Camerer, Dreber et al. (2016), psychology experiments to Open Science Collaboration (2015) and social sciences to Camerer, Dreber et al. (2018). The replication rate is defined as the share of original estimate whose replications have statistically significant findings of the same sign. Figures in the first row report the mean intended power reported in both applications. The second row shows observed replication rates. The third row reports the predicted replication rate in equation (8) calculated using parameter estimates Table 1. In social sciences, power is set to detect three-quarters of the original effect size with 90% power. This approach does not have a fixed nominal target for the replication rate.

In psychology, the model predicts a replication rate of 54.5%. This is well below mean intended power of 92%, but higher than the observed replication rate of 34.8%. In this case, the model can account for around two-thirds of the replication rate gap. The unexplained portion of the gap in psychology suggests that problems with calculating replication power can account for some but not all of the replication rate gap. Other factors discussed in the literature and not incorporated in the model may be important, including heterogeneity in true effects,  $p$ -hacking, and measurement error. Another possibility is that the model should account for differences in replicating main effects and interaction effects, and differences across subfields (Open Science Collaboration, 2015; Altmejd et al., 2019).

*Fractional Power Rule (Social Sciences).*—A popular variant for the common power rule is the fractional power rule, where replication power is set to detect some fraction of the original effect size with a given level of statistical power (e.g. Camerer, Dreber et al. (2018) and Camerer et al. (2022)). Theoretically, under the specific rule applied in Camerer, Dreber et al. (2018), the expected replication rate can range anywhere between 0.025 and 0.99 depending on the power of original studies.<sup>13</sup> Empirically, the predicted replication rate for the experimental social sciences is 55.3%, which is very close to the observed rate of 57.1%. Similarly to experimental economics, this suggests that the interaction of power calculation issues in replication studies and low power in original studies can adequately explain the observed replication rate.

<sup>13</sup>Under the fractional power rule, the expected replication probability approaches  $1 - \Phi[1.96 - \frac{1}{\psi}(1.96 - \Phi^{-1}(\beta^n))]$  as  $\theta/\sigma$  approaches infinity. With the fraction of original effect size to detect equal to  $\psi = 3/4$ , and intended power set to  $1 - \beta^n = 0.9$ , this limit equals 0.99.

*Extensions.*—I examine four extensions. First, I augment the model to include  $p$ -hacking. The augmented model incorporates an egregious form of manipulation, where researchers who obtain a ‘marginally insignificant’ result (defined in the model as  $X^* \in (1.46\Sigma^*, 1.96\Sigma^*)$ ) misreport their findings as significant with some prespecified probability  $\beta_h$ . Specifically,  $p$ -hackers misreport a statistically significant estimate drawn from a uniform distribution over  $[1.96\Sigma^*, 2.46\Sigma^*]$ . The augmented model for  $p$ -hacking assumes that the estimated models in Table 1 accurately reflect the DGP, which notably do not incorporate  $p$ -hacking. This assumption is plausible for experimental economics and social science, where replication rate predictions are very accurate, but less so for psychology, where they are less accurate.

The main takeaway is that  $p$ -hacking lowers the replication rate, but that its total impact is relatively small when compared to problems related to power. A useful benchmark for what constitutes a realistic value for  $\beta_h$  comes from Brodeur et al. (2016) and Brodeur, Cook and Heyes (2022), who estimate that the proportion of ‘wrongfully claimed significant results’ is around 10%. In the augmented model, this implies that the probability of  $p$ -hacking marginally insignificant results,  $\beta_h$ , is between 0.22–0.26 across applications (Table G1 in Appendix G). Under these specifications, the replication rate falls by between 2–4 percentage points compared to the case with no manipulation. In economics and psychology, this implies that  $p$ -hacking of the form specified in the model would account for 5–6% of the total replication rate gap; issues related to power would account for the remainder. For further details, see Appendix G.

A second extension uses the estimated models in Table 1 to predict the average relative effect size, a complementary continuous measure of replication defined as the average of the ratio of the replication effect size and the original effect size. In Appendix G, I use the estimated models to generate predicted average relative effect sizes using a similar procedure to the replication rate predictions. The accuracy of the predictions in each field is qualitatively similar to the replication rate predictions.

A third extension considers the proposed rule of setting replication power equal to original power in Appendix F. In a review of 108 psychology replications by Anderson and Maxwell (2017), 19 (17.6%) implemented this approach. In all three applications, this approach leads to lower predicted replication rates than under the common power rule.

Given the issues that stem from conditioning on statistical significance, the fourth extension in Appendix H examines the suggestion of extending the replication rate definition to include null results that are ‘replicated’ if their replications are also insignificant. For empirical models in economics and psychology, this ‘extended’ replication rate remains below intended power under the common power rule.

### III. Conclusion

The prominence of the replication rate stems in part from its apparent transparency and ease of interpretation. However, caution should be applied when interpreting the replication rate from large-scale replication studies using the common power rule for setting replication power. In general, intended replication targets are not attainable in expectation. Moreover, the replication rate gap will be particularly large when original power is low. Empirical evidence supports the importance of these theoretical insights. In a model with neither heterogeneity nor  $p$ -hacking, problems with power calculations alone are sufficient to explain observed replication rates in experimental economics and social science.

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## Online Appendix

This appendix contain proofs and supplementary materials for “Can the Replication Rate Tell Us About Selective Publication?” Section A derives properties of the replication probability function. Section B contains proofs for results in the main text, in addition to other theoretical results. Section C presents an illustrative example of how the replication rate can vary with changes in selective publication above the 1.96 significance threshold. Section D details replication selection mechanisms implemented in the three applications. Section E presents extensions of the empirical results using alternative power calculations. Section F builds intuition for the empirical replication rate decomposition results. Section G examine two further extensions to the empirical results: examining the impact of  $p$ -hacking on the replication rate; and an analysis of the relative effect size measure of replication. Appendix H examines a generalization of the replication rate definition to include insignificant results.

### A. Properties of the Replication Probability Function

This Appendix derives properties of the replication probability function (Definition 1). The first ‘property’ simply provides a convenient, compact notation. The remaining properties consider the replication probability function under the common power rule to detect original effect sizes with  $1 - \beta^n$  intended power (Definition 3). Recall that the replication probability for original study  $(x, \sigma, \theta)$  is equal to

$$RP(x, \theta, \sigma_r(x, \sigma, \beta^n)) = \mathbb{P}\left(\frac{|X_r|}{\sigma_r(x, \beta^n)} \geq 1.96, \text{sign}(X_r) = \text{sign}(x)\right) \quad (9)$$

To provide intuition of the properties, Figure A1 provides an illustration of the replication probability function for different values of  $x$  under the common power rule for  $1 - \beta^n = 0.9$  and a fixed value of  $\theta$ .

**Lemma A1** (Properties of the replication probability function). *The replication probability function satisfies the following properties:*

1. *For any replication standard error  $\sigma_r(x, \sigma, \beta^n)$ , the replication probability for an original study  $(x, \sigma, \theta)$  can be written compactly as*

$$RP(x, \theta, \sigma_r(x, \sigma, \beta^n)) = 1 - \Phi\left(1.96 - \text{sign}(x) \frac{\theta}{\sigma_r(x, \sigma, \beta^n)}\right) \quad (10)$$

The remaining properties assume the replication standard error  $\sigma_r(x, \beta^n)$  is set using the

common power rule in Definition 3 with intended power  $1 - \beta^n$ :

2. If  $1 - \beta^n > 0.025$ , then  $RP(x, \theta, \sigma_r(x, \beta^n))$  is strictly decreasing in  $x$  over  $(-\infty, 0)$  and  $(0, \infty)$ .
3. If  $(1 - \beta^n) > 0.6628$ , then  $RP(x, \theta, \sigma_r(x, \beta^n))$  is strictly concave with respect to  $x$  over the open interval  $(\max\{0, [1 - r^*(\beta^n)]\theta\}, [1 + r^*(\beta^n)]\theta)$ , where

$$r^*(\beta^n) = -(2 + 1.96 \cdot h(\beta^n)) + \sqrt{\frac{(2 + 1.96 \cdot h(\beta^n))^2 - 4 \times (1 + 1.96 \cdot h(\beta^n) - h(\beta^n)^2)}{2}} > 0 \quad (11)$$

with  $h(\beta^n) = (1.96 - \Phi^{-1}(\beta^n))$ .

4. The limits of the replication probability function with respect to  $x$  are

$$\lim_{x \rightarrow \infty} RP(x, \theta, \sigma_r(x, \beta^n)) = 0.025 \text{ and } \lim_{x \rightarrow -\infty} RP(x, \theta, \sigma_r(x, \beta^n)) = 0.025 \quad (12)$$

$$\lim_{x \uparrow 0} RP(x, \theta, \sigma_r(x, \beta^n)) = 0 \text{ and } \lim_{x \downarrow 0} RP(x, \theta, \sigma_r(x, \beta^n)) = 1 \quad (13)$$

5. Suppose  $X^* \sim N(\theta, \sigma^2)$ . Then  $\mathbb{E}[RP(X, \theta, \sigma_r(X, \beta^n))] \rightarrow 1 - \beta^n$  as  $\theta \rightarrow \infty$  for fixed  $\sigma$ .

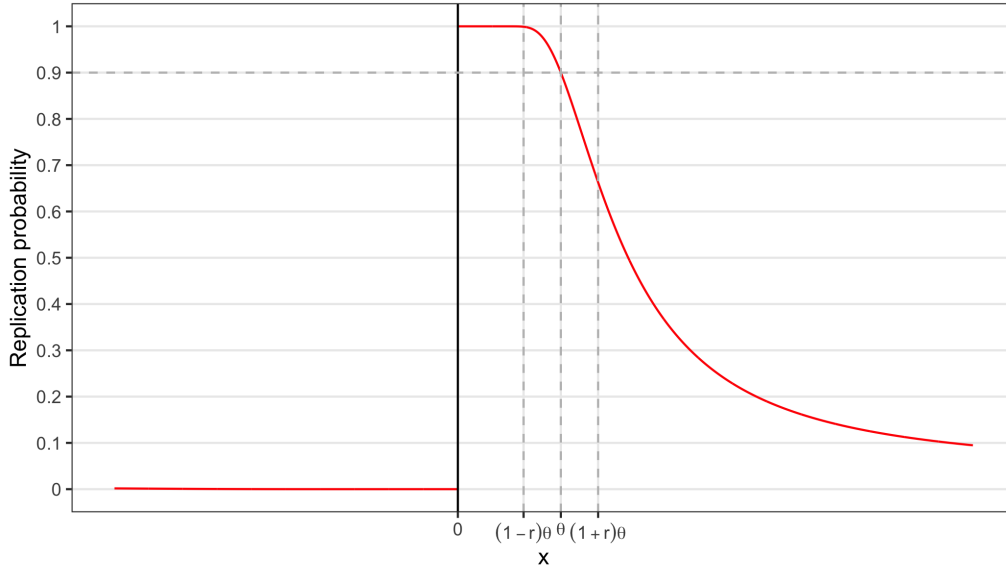


FIGURE A1. Example of the replication probability function under the common power rule with intended power  $(1 - \beta^n) = 0.9$ . The two vertical lines around  $\theta$  marks the open interval over which the replication probability function is strictly concave, where  $r^*$  is given by equation (11).

*Proof of 1.* The probability in equation (9) equals  $\left[\mathbb{1}(x/\sigma \geq 1.96) \times (1 - \Phi(1.96 - \frac{\theta}{\sigma_r}))\right] + \left[\mathbb{1}(x/\sigma \leq -1.96) \times \Phi(-1.96 - \frac{\theta}{\sigma_r})\right]$ . This captures the two requirements for ‘successful’ replication: the replication estimate must attain statistical significance and have the same sign as the original estimate. Equation (10) is obtained using the symmetry of the normal distribution, which implies that  $\Phi(t) = 1 - \Phi(-t)$  for any  $t$ .  $\square$

*Proof of 2.* The first derivative of the replication probability function with the common power rule is

$$\frac{\partial RP(x, \theta, \sigma_r(x, \beta^n))}{\partial x} = \begin{cases} -\frac{\theta}{x^2}(1.96 - \Phi^{-1}(\beta^n)) \times \phi\left(1.96 - \frac{\theta}{x}(1.96 - \Phi^{-1}(\beta^n))\right), & x > 0 \\ -\frac{\theta}{x^2}(1.96 - \Phi^{-1}(\beta^n)) \times \phi\left(-1.96 - \frac{\theta}{|x|}(1.96 - \Phi^{-1}(\beta^n))\right), & x < 0 \end{cases} \quad (14)$$

These are strictly negative whenever  $(1.96 - \Phi^{-1}(\beta^n)) > 0 \iff (1 - \beta^n) > 0.025$ .  $\square$

*Proof of 3.* First, note that for  $x > 0$ , the second derivative of the replication probability function with the common power rule is

$$\frac{\partial^2 RP(x, \theta, \sigma_r(x, \beta^n))}{\partial x^2} = \left(\frac{h(\beta^n)\theta}{x^3}\right)\phi\left(1.96 - \frac{h(\beta^n)\theta}{x}\right)\left[1 + \left(\frac{h(\beta^n)\theta}{x}\right)\left(1.96 - \frac{h(\beta^n)\theta}{x}\right)\right] \quad (15)$$

Let  $x = (1 + r)\theta$ . Substituting this into the previous equation and simplifying shows that equation (15) is strictly negative when the following inequality is satisfied

$$r^2 + (2 + 1.96h(\beta^n)).r + (1 + 1.96h(\beta^n) - h(\beta^n)^2) < 0 \quad (16)$$

The solution to the quadratic equation has a unique positive solution  $r^*(\beta^n)$  whenever  $(1 - \beta^n) > 0.6628$ . To see this, note that there exists a unique positive solution when  $(1 + 1.96h(\beta^n) - h(\beta^n)^2) < 0$ . This quadratic equation in  $h(\beta^n)$  must have a unique positive and negative solution in turn, since the parabola opens downwards and equals 1 when  $h(\beta^n) = 0$ . The positive root can be obtained from the quadratic formula, which gives 2.38014. Since the quadratic function opens downward, this implies that for any  $h(\beta^n) > 2.38014$ , we have  $(1 + 1.96h(\beta^n) - h(\beta^n)^2) < 0$ . Thus, a unique positive solution to equation (16) exists whenever this condition is satisfied. In particular, a unique positive solution exists whenever

$$h(\beta^n) = 1.96 - \Phi^{-1}(\beta^n) > 2.38014$$

$$\begin{aligned}
&\Longleftrightarrow \Phi(1.96 - 2.38014) > \beta^n \\
&\Longleftrightarrow (1 - \beta^n) > 0.6628
\end{aligned} \tag{17}$$

The unique positive solution for equation (16) can again be obtained by the quadratic formula, which gives equation (11). Note that for any  $r > 0$  where the inequality for concavity in equation (16) is satisfied, the same must also be true of  $-r$ , since it makes the left-hand-side strictly smaller. This implies that the replication probability function is strictly concave (since its second derivative is strict negative) over  $(\max\{0, [1 - r^*(\beta^n)]\theta\}, [1 + r^*(\beta^n)]\theta)$ , where the maximum is taken because the replication probability function is discontinuous at 0. This follows because of the properties of the quadratic function. Specifically, suppose  $f(x)$  is a parabola that opens upward and intersects the y-axis at a negative value. Then for any two points  $(a, b)$  with  $a < b$  and  $f(a), f(b) < 0$ , it must be that  $f(c) < 0$  for any  $c \in (a, b)$ .  $\square$

*Proof of 4.* Substituting the common power rule into the replication probability function gives

$$RP(x, \theta, \sigma_r(x, \beta^n)) = 1 - \Phi\left(1.96 - \frac{\theta}{x}(1.96 - \Phi^{-1}(\beta^n))\right) \tag{18}$$

The values of the limits can be seen immediately from this expression.  $\square$

*Proof of 5.* This proof consists of two steps. In the first step, I show that the replication probability function approaches linearity in  $x$  in an even interval around  $\theta$ , as  $\theta \rightarrow \infty$  for fixed  $\sigma$ . To see this, fix  $r \in (0, 1)$ . Then the second derivative evaluated at any point  $c\theta \in (r\theta, (1+r)\theta)$  equals

$$\left. \frac{\partial^2 RP(x, \theta, \sigma_r(x, \beta^n))}{\partial x^2} \right|_{x=c\theta} = \left( \frac{h(\beta^n)}{c^3\theta^2} \right) \phi\left(1.96 - \frac{h(\beta^n)}{c}\right) \left[ 1 + \left( \frac{h(\beta^n)}{c} \right) \left( 1.96 - \frac{h(\beta^n)}{c} \right) \right] \tag{19}$$

This approaches zero as  $\theta \rightarrow \infty$ , which implies that  $RP(x, \theta, \sigma_r(x, \beta^n))$  approaches linearity in  $x$  over the interval  $(r\theta, (1+r)\theta)$  in the limit.

For the second step, see that as  $\theta \rightarrow \infty$  with fixed  $\sigma$ , we have that

$$\mathbb{P}[X^* \in (r\theta, (1+r)\theta) | \theta, \sigma] = \Phi\left(\frac{(1+r)\theta - \theta}{\sigma}\right) - \Phi\left(\frac{r\theta - \theta}{\sigma}\right) \rightarrow 1 \tag{20}$$

That is, the probability of drawing  $X^*$  inside of the range  $(r\theta, (1+r)\theta)$  approaches one in the limit. But from the first step we know that the replication probability function is linear over this range as  $\theta \rightarrow \infty$  with fixed  $\sigma$ . This implies in the limit that  $\mathbb{E}[RP(X, \theta, \sigma_r(X, \beta^n))] =$

$RP(\mathbb{E}[X], \theta, \sigma_r(X, \beta^n)) = RP(\theta, \theta, \sigma_r(X, \beta^n)) = 1 - \beta^n$ , as shown in Lemma 1 in the main text.

## B. Proofs of Propositions

For convenience, some proofs use notation distinguishing the publication probability function  $p(\cdot)$  over significant and insignificant regions:

$$p(X^*/\Sigma^*) = \begin{cases} p_{sig}(X^*/\Sigma^*) & \text{if } S_X^* = 1 \\ p_{insig}(X^*/\Sigma^*) & \text{if } S_X^* = 0 \end{cases}$$

where  $S_X^*$  is an indicator variable that equals one if  $|X^*/\Sigma^*| \geq 1.96$  and zero otherwise.

**Lemma B1** (Justification of the common power rule). *Consider a published study  $(x, \sigma, \theta)$ . If  $x = \theta$  and a replication uses the common power rule to detect the original effect with intended power  $1 - \beta^n$ , then*

$$RP(\theta, \theta, \sigma_r(\theta, \beta^n)) = 1 - \beta^n \quad (21)$$

*Proof.* Substitute the common power rule in the replication probability function derived in Lemma A1.1 in Appendix A. If  $x = \theta$ , then

$$RP(\theta, \theta, \sigma_r(\theta, \beta^n)) = 1 - \Phi\left(1.96 - \text{sign}(\theta) \frac{\theta}{\sigma_r(\theta, \beta^n)}\right) = 1 - \Phi\left(1.96 - \frac{\theta}{\theta}(1.96 - \Phi^{-1}(\beta^n))\right) = 1 - \beta^n \quad (22)$$

□

**Proof of Proposition 1:** For notational convenience, let  $(X_{sig}, \Sigma_{sig}, \Theta_{sig})$  denote the distribution of latent studies  $(X^*, \Sigma^*, \Theta^*)$  conditional on being published ( $D = 1$ ) and statistically significant ( $|X^*/\Sigma^*| \geq 1.96$ ). The expected replication probability (Definition 2) under the common power rule (Definition 3) can be written as

$$\begin{aligned} & \mathbb{E}_{X^*, \Sigma^*, \Theta^* | D, R, S_X^*} \left[ RP(X^*, \Theta^*, \sigma_r(X^*, \beta^n)) \middle| D = 1, R = 1, |X^*/\Sigma^*| \geq 1.96 \right] \\ &= \mathbb{E}_{X, \Sigma, \Theta | S_X} \left[ RP(X, \Theta, \sigma_r(X, \Sigma, \beta^n)) \middle| |X/\Sigma| \geq 1.96 \right] \\ &= \mathbb{E}_{X_{sig}, \Sigma_{sig}, \Theta_{sig}} \left[ RP(X_{sig}, \Theta_{sig}, \sigma_r(X_{sig}, \beta^n)) \right] \\ &= \mathbb{E}_{\Sigma_{sig}, \Theta_{sig}} \left[ \mathbb{E}_{X_{sig} | \Sigma_{sig}, \Theta_{sig}} \left[ RP(X_{sig}, \Theta_{sig}, \sigma_r(X_{sig}, \beta^n)) \middle| \Theta_{sig} = \theta, \Sigma_{sig} = \sigma \right] \right] \end{aligned} \quad (23)$$



where the second inequality drops the conditioning on being chosen for replication ( $R$ ) because it is assumed that replication selection on significant results is random; and the last equality uses the Law of Iterated Expectations. The proof shows that the conditional expected replication probability satisfies  $\mathbb{E}_{X_{sig}|\Sigma_{sig},\Theta_{sig}}[RP(X_{sig}, \Theta_{sig}, \sigma_r(X_{sig}, \beta^n))|\Theta_{sig} = \theta, \Sigma_{sig} = \sigma] < 1 - \beta^n$  which implies that the expected replication probability is also less than intended power  $1 - \beta^n$ . For greater clarity in what follows, let  $\mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n)]$  be shorthand for  $\mathbb{E}_{X_{sig}|\Sigma_{sig},\Theta_{sig}}[RP(X_{sig}, \Theta_{sig}, \sigma_r(X_{sig}, \beta^n))|\Theta_{sig} = \theta, \Sigma_{sig} = \sigma]$ .

Note that the conditional expected replication probability can be written explicitly as

$$\mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n)] = \int \left(1 - \Phi\left(1.96 - \text{sign}(x) \frac{\theta}{|x|} (1.96 - \Phi^{-1}(\beta^n))\right)\right) \frac{p(\frac{x}{\sigma}) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) \mathbb{1}(|\frac{x}{\sigma}| \geq 1.96) dx}{\int_{x'} p(\frac{x'}{\sigma}) \frac{1}{\sigma} \phi\left(\frac{x'-\theta}{\sigma}\right) \mathbb{1}(|\frac{x'}{\sigma}| \geq 1.96) dx'} \quad (24)$$

where the integrand in equation (24) is obtained using the compact notation for the replication probability derived in Lemma A1.1 and then substituting the common power rule in Definition 3. This density differs from a normal density in two respects: (1) the publication probability function  $p(\frac{x}{\sigma})$  reweights the distribution; and (2) conditioning on statistical significance truncates original effects falling in the insignificant region  $(-1.96\sigma, 1.96\sigma)$ . The denominator is the normalization constant.

First, we introduce some notation. Lemma A1.3 shows that if  $(1 - \beta^n) > 0.6628$ , then  $RP(x, |\theta, \sigma, \beta^n)$  is strictly concave over the open interval  $(\max\{0, [1 - r^*(\beta^n)]\theta\}, [1 + r^*(\beta^n)]\theta)$ , where  $r^*(\beta^n)$  is given by equation (11). This Proposition assumes  $(1 - \beta^n) > 0.8314$ , so the condition is satisfied. To simplify the notation, define  $(l^*, u^*) = ((1 - r^*)\theta, (1 + r^*)\theta)$  when  $r^* \in (0, 1)$  and  $(l^*, u^*) = (0, 2\theta)$  when  $r^* \geq 1$ ; in both cases, the replication probability function is strictly concave over an interval with mid-point  $\theta$ .

Consider first the case where  $r^* \geq 1$  so that  $(l^*, u^*) = (0, 2\theta)$ . The conditional replication probability can be expressed as a weighted sum

$$\begin{aligned} \mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n)] &= \mathbb{P}(X_{sig} < l^*) \mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n) | X_{sig} < l^*] \\ &+ \mathbb{P}(l^* \leq X_{sig} \leq u^*) \mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n) | l^* \leq X_{sig} \leq u^*] + \mathbb{P}(X_{sig} > u^*) \mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n) | X_{sig} > u^*] \\ &< \mathbb{P}(X_{sig} < l^*) 0.025 + \mathbb{P}(l^* \leq X_{sig} \leq u^*) \mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n) | l^* \leq X_{sig} \leq u^*] + \mathbb{P}(X_{sig} > u^*) (1 - \beta^n) \end{aligned} \quad (25)$$

In the last line, the first term in the sum uses the fact that the maximum value of the replication probability when  $x < l^* = 0$  is 0.025 (Lemma A1.2 and Lemma A1.4 in Appendix A). The third term follows because  $RP(2\theta|\theta, \sigma, \beta^n)$  is the maximum value the function takes over  $x > u^* = 2\theta$ , since the function is strictly decreasing over  $x > 0$  (Lemma A1.2); and therefore that  $RP(2\theta|\theta, \sigma, \beta^n) < RP(\theta|\theta, \sigma, \beta^n) = 1 - \beta^n$ , where the equality is shown in

**Lemma 1.** From equation (25), we can see that  $\mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n)|l^* \leq X_{sig} \leq u^*] < 1 - \beta^n$  is a sufficient condition for  $\mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n)] < 1 - \beta^n$ .

Before showing that this sufficient condition is satisfied, we show that the same sufficient condition holds in the second case, where  $r^* \in (0, 1)$  so that  $(l^*, u^*) = ((1-r^*)\theta, (1+r^*)\theta)$ . This requires additional steps. First, express the conditional replication probability as a weighted sum

$$\begin{aligned} \mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n)] &= \mathbb{P}(X_{sig} \leq l^*)\mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n)|X_{sig} \leq l^*] \\ &+ \mathbb{P}(l^* \leq X_{sig} \leq u^*)\mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n)|l^* \leq X_{sig} \leq u^*] + \mathbb{P}(X_{sig} \geq u^*)\mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n)|X_{sig} \geq u^*] \\ &< \mathbb{P}(X_{sig} \leq l^*) + \mathbb{P}(l^* \leq X_{sig} \leq u^*)\mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n)|l^* \leq X_{sig} \leq u^*] + \mathbb{P}(X_{sig} \geq u^*)RP(u^*|\theta, \sigma, \beta^n) \end{aligned} \quad (26)$$

The strict inequality follows for two reasons. For the first term in the sum, one is the maximum value the function can take for any  $x$ . For the third term,  $RP(u^*|\theta, \sigma, \beta^n)$  is the function's maximum value over  $x \geq u^*$ , since the integrand is strictly decreasing over positive values (Lemma A1.2). With an additional step, we can write this inequality as

$$\begin{aligned} \mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n)] &< \frac{1}{2}\left(1 - \mathbb{P}(l^* \leq X_{sig} \leq u^*)\right)\left(1 + RP(u^*|\theta, \sigma, \beta^n)\right) \\ &+ \mathbb{P}(l^* \leq X_{sig} \leq u^*)\mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n)|l^* \leq X_{sig} \leq u^*] \end{aligned} \quad (27)$$

This follows because  $\mathbb{P}(X_{sig} \leq l^*) \leq \mathbb{P}(X_{sig} \geq u^*)$  and  $RP(u^*|\theta, \sigma, \beta^n) < 1$ . That is, increasing the relative weight on the maximum value of one, such that both tails are equally weighted, must lead to a (weakly) larger value. The weak inequality  $\mathbb{P}(X_{sig} \leq l^*) \leq \mathbb{P}(X_{sig} \geq u^*)$  required for this simplification is shown below:

**Lemma B2.** Suppose  $X|\theta, \sigma$  follows the truncated normal pdf in equation (24). Then for any  $r^* \in (0, 1)$ , the following inequality holds:  $\mathbb{P}(X_{sig} \leq (1-r^*)\theta) < \mathbb{P}(X_{sig} \geq (1+r^*)\theta)$ .

*Proof.* First, note that  $((1-r^*)\theta, (1+r^*)\theta)$  is an interval over the positive real line centered at  $\theta$ . Consider two cases:

*Case 1:* Let  $(1-r^*)\theta \leq 1.96\sigma$ . Define the normalization constant  $C = \int_{x'} p\left(\frac{x'}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x'-\theta}{\sigma}\right) \mathbb{1}\left(\left|\frac{x}{\sigma}\right| \geq 1.96\right) dx'$ . Then

$$\mathbb{P}(X_{sig} \leq (1-r^*)\theta) = \frac{1}{C} \int_{-\infty}^{-1.96\sigma} p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx' \leq \frac{1}{C} \int_{2\theta+1.96\sigma}^{\infty} p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx'$$

$$< \frac{1}{C} \int_{2\theta+1.96\sigma}^{\infty} p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx' + \frac{1}{C} \int_{\max\{1.96\sigma, (1+r^*)\theta\}}^{2\theta+1.96\sigma} p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx' = \mathbb{P}(X_{sig} \geq (1+r^*)\theta) \quad (28)$$

Consider the weak inequality. Note that the mid-point between  $-1.96\sigma$  and  $2\theta + 1.96\sigma$  is  $\theta$ . Thus, with no selective publication (i.e.  $p(t) = 1$  for all  $t$ ), we would have equality owing to the symmetry of the normal distribution. However, recall that  $p_{sig}()$  is symmetric about zero and weakly increasing in absolute value. It follows therefore that  $|2\theta + 1.96\sigma| > |-1.96\sigma|$  implies  $p_{sig}(|2\theta + 1.96\sigma|) \geq p_{sig}(|-1.96\sigma|)$ ; using this fact and symmetry of the normal distribution about  $\theta$  gives the weak inequality. The strict inequality follows because the additional term is strictly positive, since  $p_{sig}()$  is assumed to be non-zero.

*Case 2:* Let  $(1 - r^*)\theta > 1.96\sigma$ . The argument is similar to the first case:

$$\begin{aligned} \mathbb{P}(X_{sig} \leq (1 - r^*)\theta) &= \frac{1}{C} \int_{-\infty}^{-1.96\sigma} p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx' + \frac{1}{C} \int_{1.96\sigma}^{(1-r^*)\theta} p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx' \\ &< \frac{1}{C} \int_{2\theta+1.96\sigma}^{\infty} p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx' + \frac{1}{C} \int_{(1+r^*)\theta}^{2\theta-1.96\sigma} p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx' \\ &\quad + \frac{1}{C} \int_{2\theta-1.96\sigma}^{2\theta+1.96\sigma} p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx' = \mathbb{P}(X_{sig} \geq (1 + r^*)\theta) \end{aligned} \quad (29)$$

□

The inequality in equation (27) can be further simplified by placing restrictions on intended power. In particular, if intended power satisfies  $1 - \beta^n \geq 0.8314$ , then

$$\begin{aligned} \mathbb{E}\left[RP(X_{sig}|\theta, \sigma, \beta^n)\right] &< \left(1 - \mathbb{P}(l^* \leq X_{sig} \leq u^*)\right)(1 - \beta^n) \\ &+ \mathbb{P}(l^* \leq X_{sig} \leq u^*) \mathbb{E}\left[RP(X_{sig}|\theta, \sigma, \beta^n) \middle| l^* \leq X_{sig} \leq u^*\right] \end{aligned} \quad (30)$$

This follows because with  $u^* = (1 + r^*)\theta$ , we have

$$\begin{aligned} \frac{1}{2}\left(1 + RP(u^*|\theta, \sigma, \beta^n)\right) &= \frac{1}{2}\left(1 + \left(1 - \Phi\left(1.96 - \frac{1.96 - \Phi^{-1}(\beta^n)}{1 + r^*(\beta^n)}\right)\right)\right) \\ &\leq 1 - \beta^n \iff 1 - \beta^n \geq 0.8314 \end{aligned} \quad (31)$$

From equation (30), we can see that  $\mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n) | l^* \leq X_{sig} \leq u^*] < 1 - \beta^n$  is a sufficient condition for  $\mathbb{E}[RP(X_{sig}|\theta, \sigma, \beta^n)] < 1 - \beta^n$ . Thus, in both cases, the sufficient condition for the desired result is the same.

This sufficient condition is shown in two steps. In the first, I show that this inequality

holds even in the case where there is no selective publication and all published results are replicated (i.e. when  $X \sim N(\Theta, \Sigma^2)$ ). In the second, I show that this inequality remains true once we allow for selective publication and truncation of the distribution due to conditioning on statistical significance.

Lemma B3 states the first intermediate step. Its implications are of independent interest and discussed in the main text. It shows that even in the optimistic scenario where original estimates are unbiased, there is no selective publication, and all results are published and replicated, that the expected replication probability still falls below intended power.

**Lemma B3.** *Let published effects be distributed according to  $X|\theta, \sigma \sim N(\theta, \sigma^2)$ . Suppose  $p(t) = 1$  and  $r(t) = 1$  for all  $t \in \mathbb{R}$ . Assume all results are included in the replication rate calculation. Let power in replications is set according to the common power rule with intended power  $1 - \beta^n \geq 0.8314$ . Then  $\mathbb{E}[RP(X|\theta, \sigma, \beta^n)] < 1 - \beta^n$ .*

*Proof.* Recall that  $RP(x|\theta, \sigma, \beta^n)$  is strictly concave with respect to  $x$  over the interval  $(l^*, u^*)$ , where  $(l^*, u^*) = ((1 - r^*)\theta, (1 + r^*)\theta)$  when  $r^* \in (0, 1)$  and  $(l^*, u^*) = (0, 2\theta)$ ; in both cases, the mid-point of the interval is  $\theta$ . We have that

$$\mathbb{E}[RP(X|\theta, \sigma, \beta^n) | l^* \leq X \leq u^*] = \int_{l^*}^{u^*} RP(x|\theta, \sigma, \beta^n) \frac{\frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx}{\int_{l^*}^{u^*} \frac{1}{\sigma} \phi\left(\frac{x'-\theta}{\sigma}\right) dx'} < RP(\theta|\theta, \sigma, \beta^n) = 1 - \beta^n \quad (32)$$

where the strict inequality follows from Jensen's inequality and the fact that  $\mathbb{E}[X | l^* \leq X \leq u^*] = \theta$ . The final equality is a property of the replication probability function shown in Lemma 1 in the main text. This is the sufficient condition required for the desired result.

Note that the inequalities in equations (27) (for when  $r^* \geq 1$ ) and (30) (for when  $r^* \in (0, 1)$ ) were derived under more general conditions, where the normal distribution may be reweighted by  $p()$  and truncated based on significance. This setting is a special case with no selective publication (i.e.  $p(t) = 1$  for all  $t$ ), and no truncation such that all results are included in the replication rate irrespective of statistical significance.  $\square$

The same conclusions hold when we introduce selective publication (which reweights the normal distribution) and condition on statistical significance (which truncates the 'insignificant' regions of the density). Consider three cases. First, suppose that  $u^* \leq 1.96\sigma$ . Then  $\mathbb{E}(RP(X_{sig}|\theta, \sigma, \beta^n) | l^* \leq X_{sig} \leq u^*) = 0 < 1 - \beta^n$  because of truncation. Second, suppose that  $l^* \geq 1.96\sigma$ . Then

$$\begin{aligned}
\mathbb{E}\left[RP(X_{sig}|\theta, \sigma, \beta^n) \middle| l^* \leq X_{sig} \leq u^*\right] &= \int_{l^*}^{u^*} RP(x|\theta, \sigma, \beta^n) \frac{p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx}{\int_{l^*}^{u^*} p_{sig}\left(\frac{x'}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x'-\theta}{\sigma}\right) dx'} \\
&\leq \int_{l^*}^{u^*} RP(x|\theta, \sigma, \beta^n) \frac{\frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx}{\int_{l^*}^{u^*} \frac{1}{\sigma} \phi\left(\frac{x'-\theta}{\sigma}\right) dx'} < RP(\theta|\theta, \sigma, \beta^n) = 1 - \beta^n
\end{aligned} \tag{33}$$

Note that the distribution is invariant to the scale of  $p_{sig}()$ . Consider first the weak inequality. This follows because  $p_{sig}()$  is assumed to be weakly increasing over  $(l^*, u^*)$ . When it is a constant function over the interval, the equality holds. If  $p_{sig}(x/\sigma) > 0$  for some  $x \in (l^*, u^*)$  then the function redistributes weight to larger values of  $x$ . Since  $RP(x|\theta, \sigma, \beta^n)$  is strictly decreasing over positive values of  $x$  (Lemma A1.2), placing higher relative weight on lower values implies that the weak inequality becomes strict. As in the proof to Lemma B3, the strict inequality follows from Jensen's inequality, since  $RP(x|\theta, \sigma, \beta^n)$  is strictly concave over  $(l^*, u^*)$ , and the fact that the expected value of  $X$  over this interval is equal to the true value  $\theta$ . The last equality follows from Lemma 1 in the main text.

Finally, consider the case where  $l^* < 1.96\sigma < u^*$ . Then

$$\begin{aligned}
\mathbb{E}\left[RP(X_{sig}|\theta, \sigma, \beta^n) \middle| l^* \leq X_{sig} \leq u^*\right] &= \int_{1.96\sigma}^{u^*} RP(x|\theta, \sigma, \beta^n) \frac{p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx}{\int_{1.96\sigma}^{u^*} p_{sig}\left(\frac{x'}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x'-\theta}{\sigma}\right) dx'} \\
&= \int_{1.96\sigma}^{2\theta-1.96\sigma} RP(x|\theta, \sigma, \beta^n) \frac{p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx}{\int_{1.96\sigma}^{u^*} p_{sig}\left(\frac{x'}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x'-\theta}{\sigma}\right) dx'} + \int_{2\theta-1.96\sigma}^{u^*} RP(x|\theta, \sigma, \beta^n) \frac{p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx}{\int_{1.96\sigma}^{u^*} p_{sig}\left(\frac{x'}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x'-\theta}{\sigma}\right) dx'} \\
&= \omega \int_{1.96\sigma}^{2\theta-1.96\sigma} RP(x|\theta, \sigma, \beta^n) \frac{p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx}{\int_{1.96\sigma}^{2\theta-1.96\sigma} p_{sig}\left(\frac{x'}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x'-\theta}{\sigma}\right) dx'} + (1-\omega) \int_{2\theta-1.96\sigma}^{u^*} RP(x|\theta, \sigma, \beta^n) \frac{p_{sig}\left(\frac{x}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx}{\int_{2\theta-1.96\sigma}^{u^*} p_{sig}\left(\frac{x'}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x'-\theta}{\sigma}\right) dx'} \\
&= \omega \int_{1.96\sigma}^{2\theta-1.96\sigma} RP(x|\theta, \sigma, \beta^n) \frac{\frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx}{\int_{1.96\sigma}^{2\theta-1.96\sigma} \frac{1}{\sigma} \phi\left(\frac{x'-\theta}{\sigma}\right) dx'} + (1-\omega) \int_{2\theta-1.96\sigma}^{u^*} RP(x|\theta, \sigma, \beta^n) \frac{\frac{1}{\sigma} \phi\left(\frac{x-\theta}{\sigma}\right) dx}{\int_{2\theta-1.96\sigma}^{u^*} \frac{1}{\sigma} \phi\left(\frac{x'-\theta}{\sigma}\right) dx'} \\
&< \omega RP(\theta|\theta, \sigma, \beta^n) + (1-\omega) \cdot RP(2\theta-1.96\sigma|\theta, \sigma, \beta^n) < 1 - \beta^n
\end{aligned} \tag{34}$$

with

$$\omega = \frac{\int_{1.96\sigma}^{2\theta-1.96\sigma} p_{sig}\left(\frac{x'}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x'-\theta}{\sigma}\right) dx'}{\int_{1.96\sigma}^{u^*} p_{sig}\left(\frac{x'}{\sigma}\right) \frac{1}{\sigma} \phi\left(\frac{x'-\theta}{\sigma}\right) dx'} \tag{35}$$

The second row simply breaks up the integral. The third row rearranges the sum so that the conditional expectation of the replication probability appears in both terms. The third line

follows because, as in the previous case, the  $p_{sig}$  function redistributes weight to large values of  $x$  and hence lower values of  $RP(x|\theta, \sigma, \beta^n)$ . In the last line, the first term uses the concavity of  $RP(x|\theta, \sigma, \beta^n)$  over  $(1.96\sigma, 2\theta - 1.96\sigma) \subset (l^*, u^*)$ , Jensen's inequality, and the fact that the expected value of  $X$  over this interval is equal to  $\theta$ . The second term follows because  $2\theta - 1.96\sigma$  is the maximum value the function can take because  $RP(x|\theta, \sigma, \beta^n)$  is strictly decreasing in  $x$  over positive values. The final inequality follows because  $RP(\theta|\theta, \sigma, \beta^n) = 1 - \beta^n$  (Lemma 1) and  $RP(2\theta - 1.96\sigma|\theta, \sigma, \beta^n) < 1 - \beta^n$  because  $2\theta - 1.96\sigma > \theta$  and the function is strictly decreasing over positive values.

This covers all cases, proving the proposition.

**Proposition B1** (Regression to the mean in replications). *Suppose  $p_{sig}()$  is symmetric about zero, non-zero over all values, differentiable, and weakly increasing in absolute value. Allow  $p_{insig}()$  to take any form. Published original estimates  $X$  and corresponding replication estimates  $X_r$  satisfy*

$$\mathbb{E}[X|\Theta = \theta, S_X = 1] > \theta = \mathbb{E}[X_r|\Theta = \theta] \quad (36)$$

*Proof.* We have  $\mathbb{E}(X_r|\Theta = \theta) = \theta$  by assumption. Next, note that

$$\begin{aligned} \mathbb{E}_{X^*|\Theta^*, S_X^*, D}(X^*|\Theta^* = \theta, |X^*/\Sigma^*| \geq 1.96, D = 1) &= \mathbb{E}_{X|\Theta, S_X}(X|\Theta = \theta, |X/\Sigma| \geq 1.96) \\ &= \mathbb{E}_{\Sigma|\Theta, S_X}\left(\mathbb{E}_{X|\Theta, \Sigma, S_X}(X|\Theta = \theta, \Sigma = \sigma, |X/\sigma| \geq 1.96)\right) \end{aligned} \quad (37)$$

where the last line uses the Law of Iterated Expectations. We will prove  $\mathbb{E}_{X|\Theta, \Sigma, S_X^*}(X|\Theta = \theta, \Sigma = \sigma, |X/\sigma| \geq 1.96) > \theta$ , which implies that the expression in equation (37) is also greater than  $\theta$ . Recall that  $X|\theta, \sigma$  is the effect size of published studies and follows a truncated normal distribution:

$$\frac{p\left(\frac{x}{\sigma}\right)\frac{1}{\sigma}\phi\left(\frac{x-\theta}{\sigma}\right)\mathbb{1}\left(\left|\frac{x}{\sigma}\right| \geq 1.96\right)}{\int p\left(\frac{x'}{\sigma}\right)\frac{1}{\sigma}\phi\left(\frac{x'-\theta}{\sigma}\right)\mathbb{1}\left(\left|\frac{x'}{\sigma}\right| \geq 1.96\right)dx'} \quad (38)$$

Define  $X = \theta + \sigma Z$ . Then the density for the transformed random variable  $Z$  is

$$\frac{p\left(z + \frac{\theta}{\sigma}\right)\phi(z)\mathbb{1}\left(\left|z + \frac{\theta}{\sigma}\right| \geq 1.96\right)}{\int p\left(z' + \frac{\theta}{\sigma}\right)\phi(z')\mathbb{1}\left(\left|z' + \frac{\theta}{\sigma}\right| \geq 1.96\right)dz'} \quad (39)$$

For notational convenience, define the following normalization constants:

$$\bar{\eta} = \mathbb{P}(X \leq -1.96\sigma) + \mathbb{P}(X \geq 1.96\sigma) = \mathbb{P}\left(Z \leq -1.96 - \frac{\theta}{\sigma}\right) + \mathbb{P}\left(Z \geq 1.96 - \frac{\theta}{\sigma}\right) \quad (40)$$

$$\eta_1 = \mathbb{P}(X \leq -1.96\sigma) = \mathbb{P}\left(Z \leq -1.96 - \frac{\theta}{\sigma}\right) \quad (41)$$

$$\eta_2 = \mathbb{P}(X \geq 2\theta + 1.96\sigma) = \mathbb{P}\left(Z \geq \frac{\theta}{\sigma} + 1.96\right) \quad (42)$$

$$\eta_3 = \mathbb{P}(1.96\sigma \leq X \leq 2\theta - 1.96\sigma) = \mathbb{P}\left(1.96 - \frac{\theta}{\sigma} \leq Z \leq \frac{\theta}{\sigma} - 1.96\right) \quad (43)$$

*Case 1.* Consider two cases. First, suppose  $\theta \in (0, 1.96\sigma)$ . Conditional on  $(\theta, \sigma)$  (where we suppress the conditional notation on  $(\theta, \sigma)$  for clarity), the expected value of a published estimate conditional of statistical significance is

$$\begin{aligned} \mathbb{E}(X|1.96\sigma \leq |X|) &= \frac{1}{\bar{\eta}} \left( \eta_1 \mathbb{E}(X|X \leq -1.96\sigma) + \eta_2 \mathbb{E}(X|X \geq 2\theta + 1.96\sigma) \right. \\ &\quad \left. + (\bar{\eta} - \eta_1 - \eta_2) \mathbb{E}(X|1.96\sigma \leq X \leq 2\theta + 1.96\sigma) \right) \end{aligned} \quad (44)$$

First note that  $\mathbb{E}(X|1.96\sigma \leq X \leq 2\theta + 1.96\sigma) > \theta$  since we assume that  $\theta \in (0, 1.96\sigma)$  and  $p_{sig}() > 0$ . If  $\eta_1 \mathbb{E}(X|X \leq -1.96\sigma) + \eta_2 \mathbb{E}(X|X \geq 2\theta + 1.96\sigma) \geq (\eta_1 + \eta_2)\theta$ , it follows that  $\mathbb{E}(X|1.96\sigma \leq |X|) > \theta$ , which is what we want to show. Consider the first expectation in this expression:

$$\mathbb{E}(X|X \leq -1.96\sigma) = \mathbb{E}\left(\theta + \sigma Z|Z \leq -1.96 - \frac{\theta}{\sigma}\right) = \theta + \sigma \mathbb{E}\left(Z|Z \leq -1.96 - \frac{\theta}{\sigma}\right) \quad (45)$$

Evaluating the expectation in the right-hand-side of equation (45) gives

$$\begin{aligned} \mathbb{E}\left(Z|Z \leq -1.96 - \frac{\theta}{\sigma}\right) &= \frac{1}{\eta_1} \int_{-\infty}^{-1.96 - \frac{\theta}{\sigma}} z p_{sig}\left(z + \frac{\theta}{\sigma}\right) \phi(z) dz = -\frac{1}{\eta_1} \int_{-\infty}^{-1.96 - \frac{\theta}{\sigma}} p_{sig}\left(z + \frac{\theta}{\sigma}\right) \phi'(z) dz \\ &= -\frac{1}{\eta_1} \left[ p_{sig}(-1.96) \phi\left(-1.96 - \frac{\theta}{\sigma}\right) - p_{sig}(-\infty) \phi(-\infty) - \int_{-\infty}^{-1.96 - \frac{\theta}{\sigma}} p'_{sig}\left(z + \frac{\theta}{\sigma}\right) \phi(z) dz \right] \end{aligned}$$

$$= -\frac{1}{\eta_1} p_{sig}(-1.96) \phi\left(-1.96 - \frac{\theta}{\sigma}\right) + \frac{1}{\eta_1} \int_{-\infty}^{-1.96 - \frac{\theta}{\sigma}} p'_{sig}\left(z + \frac{\theta}{\sigma}\right) \phi(z) dz \quad (46)$$

where the second equality uses  $\phi'(z) = -z\phi(z)$ ; the third equality uses integration by parts; and the final equality follows because  $p_{sig}(-\infty)\phi(-\infty) = 0$  since  $p_{sig}()$  is bounded between zero and one. Substituting this into equation (45) gives

$$\mathbb{E}(X|X \leq -1.96\sigma) = \theta - \frac{\sigma}{\eta_1} p_{sig}(-1.96) \phi\left(-1.96 - \frac{\theta}{\sigma}\right) + \frac{\sigma}{\eta_1} \int_{-\infty}^{-1.96 - \frac{\theta}{\sigma}} p'_{sig}\left(z + \frac{\theta}{\sigma}\right) \phi(z) dz \quad (47)$$

Next, note that

$$\mathbb{E}(X|X \geq 2\theta + 1.96\sigma) = \theta + \sigma \mathbb{E}\left(Z|Z \leq \frac{\theta}{\sigma} + 1.96\right) \quad (48)$$

where

$$\mathbb{E}\left(Z|Z \leq \frac{\theta}{\sigma} + 1.96\right) = \frac{1}{\eta_2} \int_{1.96 + \frac{\theta}{\sigma}}^{\infty} z p_{sig}\left(z + \frac{\theta}{\sigma}\right) \phi(z) dz \geq \frac{1}{\eta_2} \int_{1.96 + \frac{\theta}{\sigma}}^{\infty} z p_{sig}\left(z - \frac{\theta}{\sigma}\right) \phi(z) dz \quad (49)$$

since  $p_{sig}(z + \theta/\sigma) \geq p_{sig}(z - \theta/\sigma)$  for all  $z \in (1.96 + \theta/\sigma, \infty)$  because  $p_{sig}(t)$  is weakly increasing over  $t > 1.96$ . For the right-hand-side of this equation, we can apply similar arguments used to derive equation (46). Substituting the result into equation (48) gives

$$\mathbb{E}(X|X \geq 2\theta + 1.96\sigma) \geq \theta + \frac{\sigma}{\eta_2} p_{sig}(1.96) \phi\left(1.96 + \frac{\theta}{\sigma}\right) + \frac{\sigma}{\eta_2} \int_{1.96 + \frac{\theta}{\sigma}}^{\infty} p'_{sig}\left(z - \frac{\theta}{\sigma}\right) \phi(z) dz \quad (50)$$

Equations (47) and (50) imply

$$\begin{aligned} & \eta_1 \mathbb{E}(X|X \leq -1.96\sigma) + \eta_2 \mathbb{E}(X|X \geq 2\theta + 1.96\sigma) \\ & \geq (\eta_1 + \eta_2) \theta + \sigma \left[ p_{sig}(1.96) \phi\left(1.96 + \frac{\theta}{\sigma}\right) - p_{sig}(-1.96) \phi\left(-1.96 - \frac{\theta}{\sigma}\right) \right] \\ & + \sigma \left[ \int_{-\infty}^{-1.96 - \frac{\theta}{\sigma}} p'_{sig}\left(z + \frac{\theta}{\sigma}\right) \phi(z) dz + \int_{1.96 + \frac{\theta}{\sigma}}^{\infty} p'_{sig}\left(z - \frac{\theta}{\sigma}\right) \phi(z) dz \right] = (\eta_1 + \eta_2) \theta \quad (51) \end{aligned}$$

In the second line, the second term in the sum equals zero because symmetry of  $p_{sig}()$  and  $\phi()$  about zero implies that both terms in the brackets are equal. To see why the third term in the sum equals zero, note that



$$\int_{-\infty}^{-1.96 - \frac{\theta}{\sigma}} p'_{sig}\left(z + \frac{\theta}{\sigma}\right) \phi(z) dz = \int_{1.96 + \frac{\theta}{\sigma}}^{\infty} p'_{sig}\left(-u + \frac{\theta}{\sigma}\right) \phi(u) du = - \int_{1.96 + \frac{\theta}{\sigma}}^{\infty} p'_{sig}\left(u - \frac{\theta}{\sigma}\right) \phi(u) du \quad (52)$$

The first equality follows from both changing the order of the integral limits and applying the substitution  $u = -x$ ; it also uses the symmetry of  $\phi(\cdot)$ . The final equality holds because symmetry of  $p_{sig}(\cdot)$  about zero implies that for any  $t > 1.96$ ,  $p'_{sig}(t) = -p'_{sig}(-t)$ .

*Case 2.* Consider the second case where  $\theta \geq 1.96\sigma$ . For a given  $(\theta, \sigma)$ , we have

$$\begin{aligned} \mathbb{E}(X|1.96\sigma \leq |X|) &= \frac{1}{\bar{\eta}} \left( \eta_1 \mathbb{E}(X|X \leq -1.96\sigma) + \eta_2 \mathbb{E}(X|X \geq 2\theta + 1.96\sigma) \right. \\ &\quad \left. \eta_3 \mathbb{E}(X|1.96\sigma \leq X \leq 2\theta - 1.96\sigma) + (\bar{\eta} - \eta_1 - \eta_2 - \eta_3) \mathbb{E}(X|2\theta - 1.96\sigma \leq X \leq 2\theta + 1.96\sigma) \right) \\ &> \frac{1}{\bar{\eta}} \left( \theta(\eta_1 + \eta_2) + (\bar{\eta} - \eta_1 - \eta_2 - \eta_3)\theta + \eta_3 \mathbb{E}(X|1.96\sigma \leq X \leq 2\theta - 1.96\sigma) \right) \end{aligned} \quad (53)$$

The inequality follows from two facts. First, the inequality proved in the first case:  $\eta_1 \mathbb{E}(X|X \leq -1.96\sigma) + \eta_2 \mathbb{E}(X|X \geq 2\theta + 1.96\sigma) \geq (\eta_1 + \eta_2)\theta$ . Second, the expectation in the third term of the sum satisfies  $\mathbb{E}(X|2\theta - 1.96\sigma \leq X \leq 2\theta + 1.96\sigma) > \theta$  because  $\theta \geq 1.96\sigma \iff 2\theta - 1.96\sigma \geq \theta$  and we assume that  $p_{sig}(\cdot) > 0$ .

It remains to show that  $\mathbb{E}(X|1.96\sigma \leq X \leq 2\theta - 1.96\sigma) \geq \theta$ . Then it follows that  $\mathbb{E}(X|1.96\sigma \leq |X|) > \theta$ , which is what we want to show. First, note that

$$\mathbb{E}(X|1.96\sigma \leq X \leq 2\theta - 1.96\sigma) = \theta + \sigma \mathbb{E}\left(Z \middle| 1.96 - \frac{\theta}{\sigma} \leq Z \leq -1.96 + \frac{\theta}{\sigma}\right) \quad (54)$$

It is therefore sufficient to show that  $\mathbb{E}\left(Z \middle| 1.96 - \frac{\theta}{\sigma} \leq Z \leq -1.96 + \frac{\theta}{\sigma}\right) \geq 0$ . Writing out the expectation in full gives

$$\begin{aligned} \mathbb{E}\left(Z \middle| 1.96 - \frac{\theta}{\sigma} \leq Z \leq -1.96 + \frac{\theta}{\sigma}\right) &= \frac{1}{\eta_3} \left( \int_{1.96 - \frac{\theta}{\sigma}}^0 z p_{sig}\left(z + \frac{\theta}{\sigma}\right) \phi(z) dz + \int_0^{\frac{\theta}{\sigma} - 1.96} z p_{sig}\left(z + \frac{\theta}{\sigma}\right) \phi(z) dz \right) \\ &= \frac{1}{\eta_3} \left( \int_0^{\frac{\theta}{\sigma} - 1.96} z \left[ p_{sig}\left(z + \frac{\theta}{\sigma}\right) - p_{sig}\left(-z + \frac{\theta}{\sigma}\right) \right] \phi(z) dz \right) \geq 0 \end{aligned} \quad (55)$$

The second equality follows because

$$\int_{1.96-\frac{\theta}{\sigma}}^0 z p_{sig}\left(z+\frac{\theta}{\sigma}\right) \phi(z) dz = - \int_0^{1.96-\frac{\theta}{\sigma}} z p_{sig}\left(z+\frac{\theta}{\sigma}\right) \phi(z) dz = - \int_0^{\frac{\theta}{\sigma}-1.96} u p_{sig}\left(-u+\frac{\theta}{\sigma}\right) \phi(u) du \quad (56)$$

which uses the substitution  $u = -x$  and the symmetry of  $\phi(\cdot)$ . The weak inequality in equation (55) follows because  $p_{sig}(\cdot)$  is assumed to be weakly increasing over positive values. Thus,  $z - \theta/\sigma > -z + \theta/\sigma$  for all  $z \in (0, \theta/\sigma - 1.96)$  implies  $p_{sig}(z + \theta/\sigma) - p_{sig}(-z + \theta/\sigma) \geq 0$ .

This covers all cases and proves the proposition.  $\square$

**Proposition B2** *For any function  $g(X, \Sigma, X_r, \beta^n), \mathbb{E}[g(X, \Sigma, X_r, \beta^n) | D = 1, R = 1, S_X = 1]$  does not depend on  $p_{insig}(\cdot)$ .*

**Proof of Proposition B2:** We can write  $\mathbb{E}[g(X, \Sigma, X_r, \beta^n) | D = 1, R = 1, S_X = 1]$  as

$$\begin{aligned} & \int g(x, \sigma, x_r, \beta^n) f_{X^*, \Sigma^*, \Theta^*, X_r | D, R, S_X^*}(x, \sigma, \theta, x_r | D = 1, R = 1, S_X^* = 1) dx d\sigma d\theta dx_r \\ &= \int_{x, \sigma, \theta} \left( \int_{x_r} g(x, \sigma, x_r, \beta^n) f_{X_r | X^*, \Sigma^*, \Theta^*}(x_r | \theta, \sigma_r(x, \sigma, \beta^n)) dx_r \right) f_{X^*, \Sigma^*, \Theta^* | D, R, S_X^*}(x, \sigma, \theta | D = 1, R = 1, S_X^* = 1) dx d\sigma d\theta \end{aligned} \quad (57)$$

The equality uses the Law of Iterated Expectations and  $f_{X_r | X^*, \Sigma^*, \Theta^*, D, R, S_X^*}(x_r | \theta, \sigma_r(x, \sigma, \beta^n)) = f_{X_r | X^*, \Sigma^*, \Theta^*}(x_r | \theta, \sigma_r(x, \sigma, \beta^n))$ . Replication estimates are not subject to selective publication, which implies this is a normal density that does not depend on  $p(\cdot)$ . Hence, the term in parentheses can only be affected by  $p(\cdot)$  indirectly through  $f_{X^*, \Sigma^*, \Theta^* | D, R, S_X^*}$ , which is the joint distribution of original studies conditional on being published, chosen for replication, and statistically significant at the 5% level. However, this distribution does not depend on the probability of publishing insignificant findings. To see this, apply Bayes rule twice to get

$$\begin{aligned} & f_{X^*, \Sigma^*, \Theta^* | D, R, S_X^*}(x, \sigma, \theta | D = 1, R = 1, S_X^* = 1) \\ &= \frac{\mathbb{P}(D = 1 | X^* = x, \Sigma^* = \sigma, \Theta^* = \theta, R = 1, S_X^* = 1)}{\mathbb{P}(D = 1 | R = 1, S_X^* = 1)} \times \frac{\mathbb{P}(R = 1 | X^* = x, \Sigma^* = \sigma, \Theta^* = \theta, S_X^* = 1)}{\mathbb{P}(R = 1 | S_X^* = 1)} \\ & \quad \times f_{X^*, \Theta, \Sigma^* | S_X^*}(x, \theta, \sigma | S_X^* = 1) \\ &= \frac{p_{sig}(x/\sigma)}{\mathbb{E}(p_{sig}(X^*/\Sigma^*) | S_X^* = 1)} \cdot \frac{r_{sig}(x/\sigma)}{\mathbb{E}(r_{sig}(X^*/\Sigma^*) | S_X^* = 1)} \cdot f_{X^*, \Sigma^*, \Theta^* | S_X^*}(\theta, x, \sigma | S_X^* = 1) \end{aligned} \quad (58)$$

In the final line, the first factor in the product includes only  $p_{sig}(\cdot)$ ; the denominator does not

condition on  $R$  because replication selection is assumed to be random for significant findings. The second factor equals one because replication selection for significant results is assumed to be random. The final factor in the product is the density of latent studies conditional on significance, which is not affected by selective publication.

### C. Replication Rate Gap Decomposition

How can we measure the relative importance of non-linearities as compared to distortions from selection on significance? To answer this question, I derive a decomposition of the replication rate gap, which I implement in the empirical section.

The decomposition is based on two regimes. Regime 1 ( $M1$ ) assumes use of the standard definition of the replication rate: only significant results are included, and replication selection is a random sample of significant results. Regime 2 ( $M2$ ) is based on a counterfactual scenario where all results are published and replication is random. This implies the distribution of published, replicated studies coincides with the distribution of latent studies. Formally, the expectation operators under both regimes are defined by:

$$\mathbb{E}_{M1}[RP(X, \Theta, \sigma_r(X, \beta^n))] = \int RP(x, \theta, \sigma_r(x, \beta^n)) f_{X^*, \Theta^* | D, R, S_X^*}(x, \theta | D = 1, R = 1, S_X^* = 1) dx d\theta \quad (59)$$

$$\mathbb{E}_{M2}[RP(X, \Theta, \sigma_r(X, \beta^n))] = \int RP(x, \theta, \sigma_r(x, \beta^n)) f_{X^*, \Theta^*}(x, \theta) dx d\theta \quad (60)$$

Using these, we have the following decomposition:

$$\begin{aligned} \underbrace{(1 - \beta^n) - \mathbb{E}_{M1}[RP(X, \Theta, \sigma_r(X, \beta^n))]}_{\text{replication rate gap}} &= \underbrace{(1 - \beta^n) - \mathbb{E}_{M2}[RP(X, \Theta, \sigma_r(X, \beta^n)) | X \geq 0]}_{\text{(i) concavity gap}} \\ &+ \underbrace{\mathbb{P}_{M1}(X < 0) \left( \mathbb{E}_{M1}[RP(X, \Theta, \sigma_r(X, \beta^n)) | X \geq 0] - \mathbb{E}_{M1}[RP(X, \Theta, \sigma_r(X, \beta^n)) | X < 0] \right)}_{\text{(ii) wrong-sign gap}} \\ &+ \underbrace{\mathbb{E}_{M2}[RP(X, \Theta, \sigma_r(X, \beta^n)) | X \geq 0] - \mathbb{E}_{M1}[RP(X, \Theta, \sigma_r(X, \beta^n)) | X \geq 0]}_{\text{(iii) selection-on-significance gap}} \end{aligned} \quad (61)$$

*Proof.* Write the expected replication probability under model 1 as

$$\begin{aligned} \mathbb{E}_{M1}[RP(X, \Theta, \sigma_r(X, \beta^n))] &= \mathbb{E}_{M1}[RP(X, \Theta, \sigma_r(X, \beta^n)) | X \geq 0] \\ &+ \mathbb{P}_{M1}(X < 0) \left( \mathbb{E}_{M1}[RP(X, \Theta, \sigma_r(X, \beta^n)) | X < 0] \right) - \mathbb{E}_{M1}[RP(X, \Theta, \sigma_r(X, \beta^n)) | X \geq 0] \end{aligned} \quad (62)$$

To arrive at equation (61), substitute equation (62) into the replication rate gap; add and subtract  $\mathbb{E}_{M2}[RP(X, \Theta, \sigma_r(X, \beta^n)) | X \geq 0]$ ; and rearrange the terms.  $\square$

Note that the concavity gap and the selection-on-significance gap condition on estimates with the same sign as the underlying true effect. This allows us to determine their contribution separate from the impact of attempting to replicate original estimates with the ‘wrong’ sign.

Table C1 presents the results. Panel A reproduces the results in the main text, and Panel B present the decomposition results. The empirical results for the decomposition show that failing to account for the concavity of the replication power function explains the overwhelming majority of the explained replication rate gap in both economics and psychology. The selection-on-significance gap is small, explaining only 3.1% of the gap in economics, while actually *decreasing* the replication rate in psychology. The latter outcome arises because conditioning on statistical significance tends to select larger true effects, which have higher replication probabilities than smaller true effects.

	Economics experiments	Psychology	Social sciences
<i>A. Replication rate predictions</i>			
Nominal target (intended power)	0.92	0.92	–
Observed replication rate	0.611	0.348	0.571
Predicted replication rate	0.600	0.545	0.553
<i>B. Decomposition of explained gap</i>			
Predicted replication rate gap	0.320 (100%)	0.375 (100%)	–
Concavity gap	0.292 (91.16%)	0.364 (97.16%)	–
Wrong-sign gap	0.018 (5.72%)	0.030 (8.03%)	–
Selection-on-significance gap	0.010 (3.12%)	-0.019 (-5.18%)	–

TABLE C1 – REPLICATION RATE PREDICTIONS AND DECOMPOSITION RESULTS

*Notes:* Economics experiments refers to [Camerer, Dreber et al. \(2016\)](#), psychology experiments to [Open Science Collaboration \(2015\)](#) and social sciences to [Camerer, Dreber et al. \(2018\)](#). The replication rate is defined as the share of original estimate whose replications have statistically significant findings of the same sign. Figures in the first row report the mean intended power reported in both applications. The second row shows observed replication rates. The third row reports the predicted replication rate in equation (8) calculated using parameter estimates Table 1. In social sciences, power is set to detect three-quarters of the original effect size with 90% power. This approach does not have a fixed nominal target for the replication rate.

Below I provide details underlying the intuition behind the decomposition results.

*Concavity gap.*—Figure C1 presents normal simulations showing that the non-linearity gap is largest for standardized true effects  $\omega \equiv \theta/\sigma$  which are close to 0, and remains above 0.2 for  $\omega \leq 1$ . It decreases monotonically as the true effect size  $\omega$  increases and approaches zero in the limit.<sup>14</sup> It follows that the size of the non-linearity gap depends on the distribution of  $\omega$ . The first row of graphs in Figure F2 plot the distribution of latent studies that have the

<sup>14</sup>See Lemma A1.5 in Appendix A for a proof which shows that the non-linearity issue vanishes as true effect sizes approach infinity.

‘correct’ sign (this corresponds to the expression for the ‘non-linearity’ gap in equation (??)). We see that a high fraction of latent studies have  $\omega < 1$ , which explains why the non-linearity gap explains such a large role.

*Wrong-sign gap.*—Random sampling variation means that original estimates will occasionally have the ‘wrong’ sign. When this occurs, the replication probability is bounded above by 0.025. The extent to which this issue contributes to low replication rates therefore depends on the share of studies that have the wrong sign among significant studies. This share will be higher in settings with small true effects and low statistical power (Gelman and Carlin, 2014; Ioannidis, Stanley and Doucouliagos, 2017). As power approaches 100%, the ‘wrong-sign gap’ approaches zero because the probability of drawing an estimate with the ‘wrong’ sign shrinks to zero.

Table C2 presents figures based on the estimated models, which show that significant results in experimental economics and psychology are relatively low-powered. The share of significant studies with the ‘wrong’ sign is 3% in economics, and 5% in psychology owing to lower statistical power. As a consequence, the wrong-sign gap is around 1 percentage point higher in psychology compared to economics.

TABLE C2 – POWER AND ESTIMATES WITH THE WRONG SIGN FOR STATISTICALLY SIGNIFICANT STUDIES

	Experimental economics	Experimental psychology
Mean normalized true effect	2.835	2.251
Mean power	0.550	0.486
Share with wrong sign	0.030	0.054
Wrong-sign gap	0.018	0.030

*Notes:* Figures are based on simulated draws from the estimated distribution of latent studies in Table 1 in the main text. All statistics are calculated on the subset of statistically significant studies. The normalized true effect is defined as  $\theta/\sigma$ . Power is defined as the probability of obtaining a statistically significant effect at the 5% level. The wrong-sign gap is defined in (??).

*Selection-on-significance gap.*—The Selection-on-significance gap is 1% in economics and slightly negative for psychology (i.e. conditioning on statistical significance increases the replication rate compared to when there is no conditioning). The sign of this gap is ambiguous because of two opposing effects from conditioning on statistical significance. To see these two effects, consider the figures in Table C2 which are based on the estimated empirical models. For the first effect, note that conditioning on significant findings increases mean bias in both applications.<sup>15</sup> This makes replication more difficult for any fixed level of  $\omega$ . For the second

<sup>15</sup>Bias is positive for latent studies because these statistics condition on original estimates  $X^*$  to have the same sign as true effects.

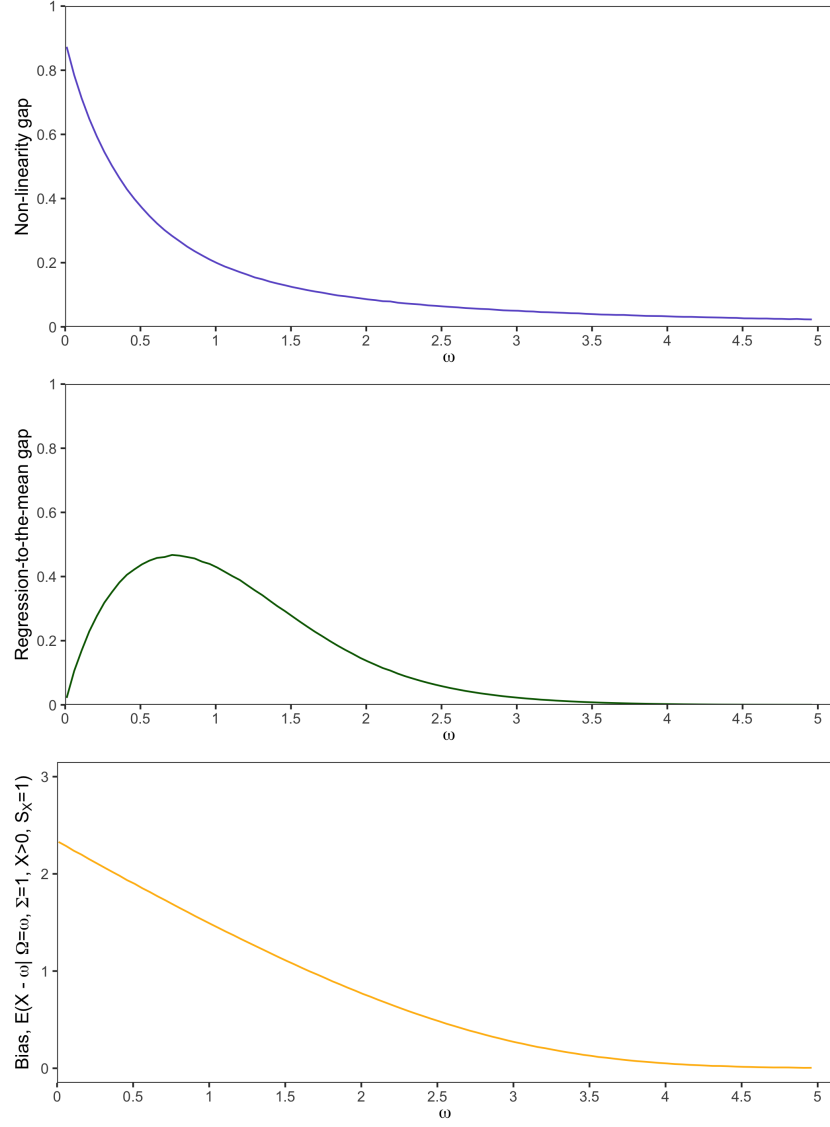


FIGURE C1. REPLICATION RATE GAP DECOMPOSITION: MONTE CARLO SIMULATIONS

*Notes:* Plots are based on simulating studies from an  $N(\omega, 1)$  distribution, for different values of  $\omega$ . Replication estimates are drawn from a  $N(\omega, \sigma_r(x, \beta^n)^2)$ , where  $\sigma_r(x, \beta^n)$  is set based on the common power rule to detect the original effect  $x$  with  $1 - \beta^n = 0.92$  intended power. The non-linearity gap and regression-to-the-mean gap are based on equation (??) and calculated using Monte Carlo methods.

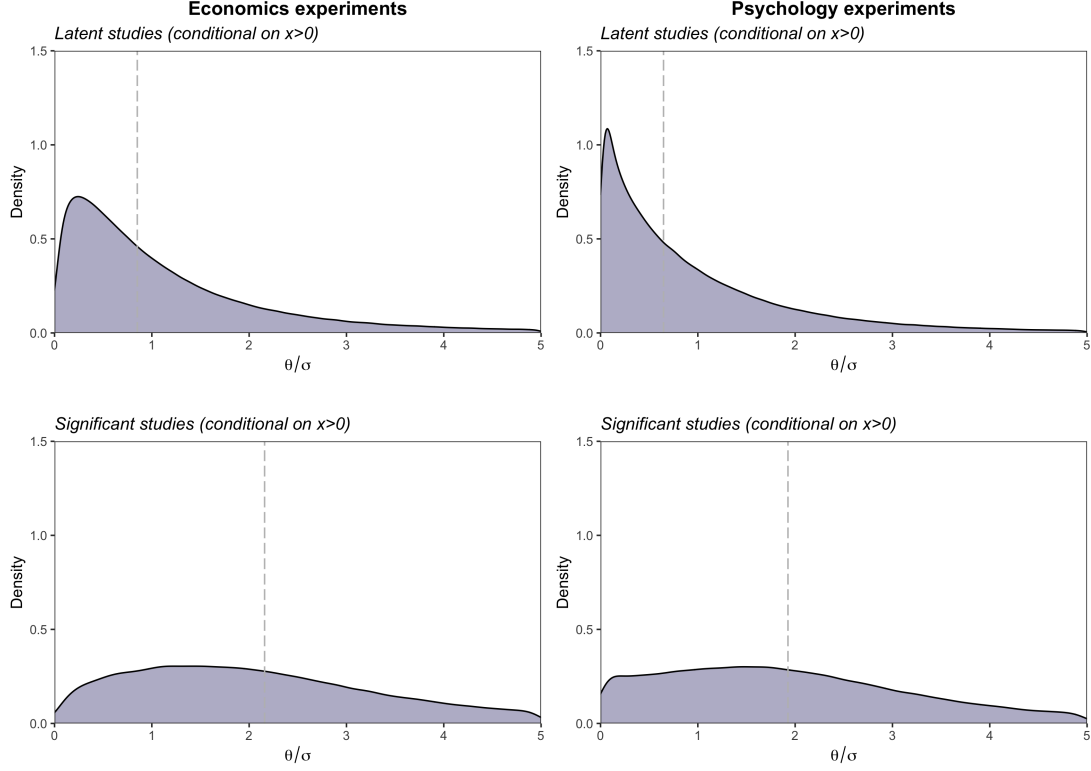


FIGURE C2. DISTRIBUTION OF NORMALIZED TRUE EFFECTS: LATENT STUDIES AND SIGNIFICANT STUDIES

*Notes:* Economics experiments refers to [Camerer, Dreber et al. \(2016\)](#) and psychology experiments to [Open Science Collaboration \(2015\)](#). Densities are based on simulated draws from the estimated distribution of latent studies in Table 1 in the main text. Dashed vertical lines show the median of the distribution.

effect, note that conditioning also tends to select studies with larger standardized true effects  $\omega$ , which have higher replication probabilities.<sup>16</sup> Higher replication probabilities arise because (i) bias is lower for larger true effects; and (ii) non-linearity effects are less severe for more highly powered studies.

The bottom panel in Figure C1 present normal simulations which show that mean bias decreases as the standard effect size increases, and approaches zero in the limit. The intuition is that censoring insignificant original estimates has little ‘bite’ when the true effect is very large, since the probability of drawing an insignificant estimate is very small. Thus, as true effects become very large, the regression-to-the-mean gap approaches zero because the expected replication probability of statistically significant findings with the ‘correct’ sign converges to the expected replication probability of latent studies with the ‘correct’ sign.

<sup>16</sup>The impact of conditioning on the full distribution of  $\omega$  can be seen in Figure C2.

TABLE C3 – TRUE EFFECT SIZES AND BIAS FOR STUDIES WITH THE ‘CORRECT’ SIGN

	Economics experiments		Psychology experiments	
	Latent	Published & significant	Latent	Published & significant
Mean bias	0.113	0.200	0.091	0.173
Mean standardized true effect	1.415	2.915	1.084	2.367

*Notes:* Economics experiments refers to [Camerer, Dreber et al. \(2016\)](#) and psychology experiments to [Open Science Collaboration \(2015\)](#). Figures are based on simulated draws from the estimated distribution of latent studies from Table 1 in the main text. The mean of the standardized true effect is equal to  $\mathbb{E}[\Omega^*|S_X^*, X^* > 0, D]$ . Mean Bias is equal to  $\mathbb{E}[X^* - \Omega^*|S_X^*, X^* > 0, D]$ . ‘Latent studies’ allow  $S_X^*$  and  $D$  to be either 0 or 1. ‘Published & significant studies’ set  $S_X^* = 1$  and  $D = 1$ .

## D. Alternative Measures of Selective Publication

Proposition 1 shows that the replication rate is unresponsive to the most salient form of selective publication. For journals and policymakers seeking to change current norms, this highlights the need for more informative measures. In this section, I conduct policy simulations using the estimated model to show how three alternative measures respond to changes in the selective publication of null results:

1. **Replication CI:** This measure counts a replication as ‘successful’ if its 95% confidence interval covers the original estimate:  $\mathbb{1}[X \in (X_r - 1.96\Sigma_r, X_r + 1.96\Sigma_r)]$ .
2. **Meta-analysis:** The standard criterion of replication with the same sign and significance is applied to a fixed-effect meta-analytic estimate combining the original and replication estimate (uncorrected for selective publication):  $\mathbb{1}[|X_m| \geq 1.96\Sigma_m, \text{sign}(X_m) = \text{sign}(X)]$  where  $X_m$  and  $\Sigma_m$  are the meta-analytic estimate and standard error, respectively.<sup>17</sup>
3. **Prediction interval:** Original and replication estimates are counted as ‘consistent’ under this approach if their difference is not statistically different from zero at the 5% level ([Patil, Peng and Leek, 2016](#)). This is equivalent to estimating a 95% ‘prediction interval’ for the original estimate and then determining if it covers the replication estimate:  $\mathbb{1}[X_r \in (X - 1.96\sqrt{\Sigma^2 + \Sigma_r^2}, X + 1.96\sqrt{\Sigma^2 + \Sigma_r^2})]$ .<sup>18</sup>

These alternative replication measures are frequently reported in large-scale replication studies ([Open Science Collaboration, 2015](#); [Camerer, Dreber et al., 2016, 2018](#)). In simulations,

<sup>17</sup>The fixed-effects meta-analytic estimate is a weighted average of original and replication estimates:  $X_m = (\omega_o X + \omega_r X_r) / (\omega_o + \omega_r)$ , where the weights are equal to the precision of each estimate i.e.  $(\omega_o, \omega_r) = (\Sigma^{-2}, \Sigma_r^{-2})$ . These weights minimize the mean-squared error of  $X_m$  ([Laird and Mosteller, 1990](#)). The variance of this estimator is given by  $\Sigma_m^2 = 1/(\omega_o + \omega_r)$ .

<sup>18</sup>This approach assumes that original and replication estimates share the same true effect and are statistically independent. For more details, see the Supplementary Materials for [Patil, Peng and Leek \(2016\)](#).



I calculate these measures over significant and insignificant published results, since conditioning on statistical significance makes them unresponsive to selective publication on null results (Proposition B2).

Simulations assume that all results significant at the 5% level are published, and that results insignificant at the 5% level are published with probability  $\beta_p$ . I then calculate how the various measures change with  $\beta_p$  to see how well they capture changes in selective publication (e.g. because of policy changes that reduce selective publication). Policymakers' successful efforts to increase the probability of publishing null results lead to an increase in the policy variable,  $\beta_p$ . Note that while model estimation assumes multiple cutoffs, policy simulations are performed assuming policymakers influence publication probabilities at a single cutoff (1.96) for simplicity (i.e. in the policy simulations I set  $\beta_p = \beta_{p1} = \beta_{p2}$  and  $\beta_{p3} = 1$  in social science).

Figure D1 shows the results. In line with Proposition 1, the replication rate is completely unresponsive to changes in the probability of publishing null results, making it a poor measure to evaluate efforts to reduce selective publication. Turning to alternative measures, note that the replication CI and meta-analysis measures actually *worsen* when more null results are published ( $\beta_p \rightarrow 1$ ). This is because less selective publication leads to more small effects being selected for replication, which have relatively low replication probabilities under these approaches. By contrast, the prediction interval measure is low when selective publication is high, and approaches close to 95% as the probability of publishing null results approach one.<sup>19</sup> The prediction interval measure performs well because it explicitly accounts for the decline in original power as more small effects are selected for replication. Noisy low-powered original studies contain limited information about true effects, which implies that a large range of replication estimates are statistically consistent with them.

Overall, for the purpose of evaluating efforts to reduce selective publication, these results suggest that calculating the prediction interval measure over a random sample of all published results could provide a useful alternative to the replication rate.

## E. Replication Selection in Empirical Applications

Replication selection is a multi-step mechanism that first selects studies, and then selects results within those studies to replicate (since studies typically report multiple results). It consists of three steps:

1. **Eligibility:** define the set of eligible studies (e.g. journals, time-frame, study designs).

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<sup>19</sup>When  $\beta_p = 1$ , the prediction interval measure is slightly higher than 95% in all applications. This is because it assumes that the original estimate  $X$  and the replication estimate  $X_r$  are uncorrelated. In practice, the replication standard error is a function of the original estimate via the common power rule, which generates some correlation between  $X$  and  $X_r$ .

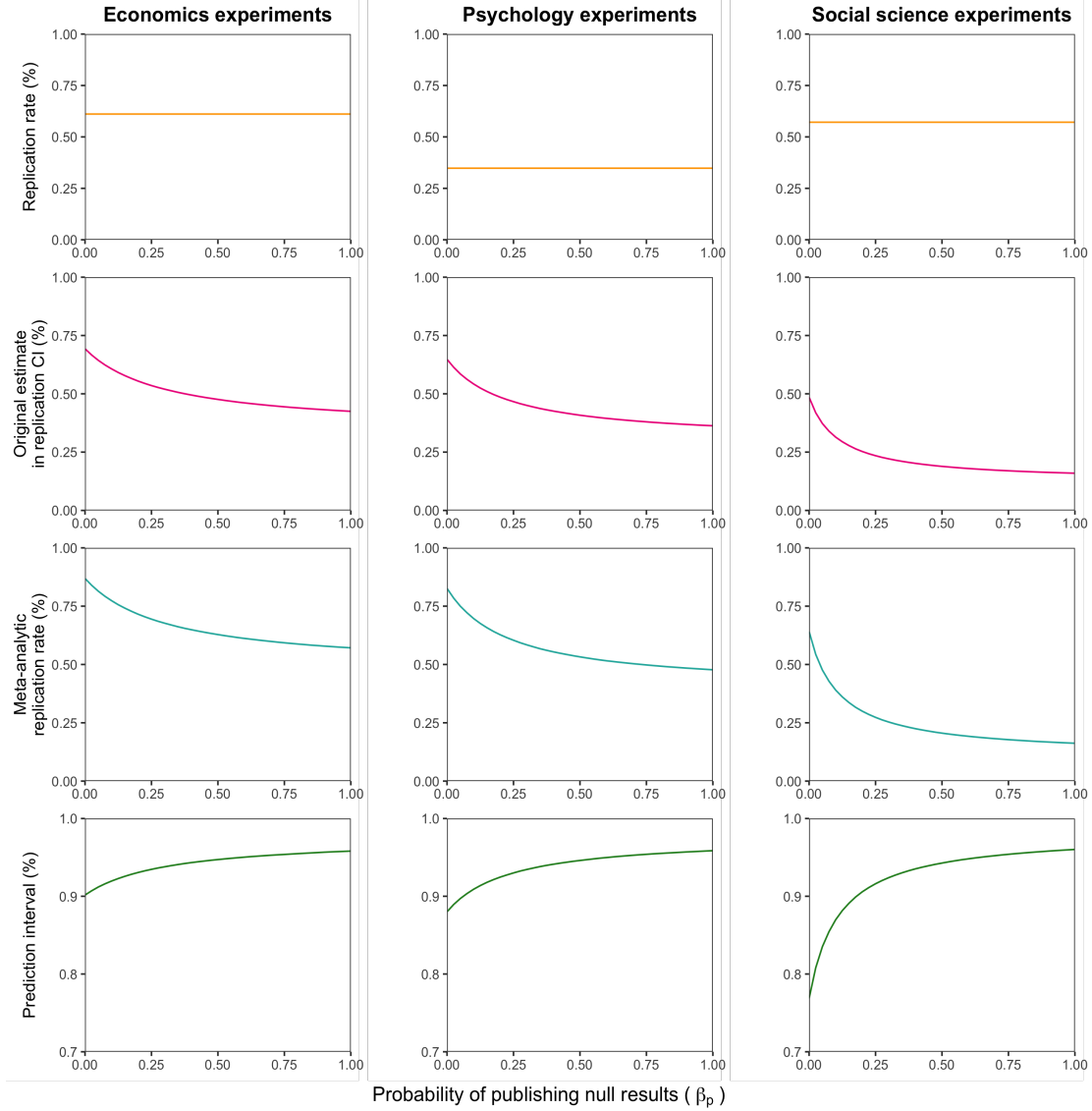


TABLE D1 – POLICY SIMULATIONS: ALTERNATIVE MEASURES OF REPLICATION AND SELECTIVE PUBLICATION

*Notes:* Details of each measure are provided in the main text. All measures except for the replication rate are calculated over significant and insignificant published results. Simulations use model estimates of the latent distribution of studies from Table 1 and set different levels of selective publication  $\beta_p$ . The first column reproduces replication rate predictions in Table 2.

2. **Study selection:** on the set of eligible studies, a mechanism that select which studies will be included in the replication study.
3. **Within-study replication selection:** for selected studies, a mechanism for selecting which result(s) to replicate.

These three features of the replication selection mechanism influence the interpretation of

the selection parameters  $(\beta_{p1}, \beta_{p2}, \beta_{p3})$ .

*Economics experiments.*—Consider these three steps in [Camerer, Dreber et al. \(2016\)](#):

1. **Eligibility:** Between-study laboratory experiments in *American Economic Review* and *Quarterly Journal of Economics* published between 2011 and 2014.
2. **Study selection:** [Camerer, Dreber et al. \(2016\)](#) select for publication all eligible studies that had ‘at least one significant between subject treatment effect that was referred to as statistically significant in the paper.’ [Andrews and Kasy \(2019\)](#) review eligible studies and conclude that no studies were excluded by this restriction. Thus, the complete set of eligible studies was selected for replication.
3. **Within-study replication selection:** the most important *statistically significant* result within a study, as emphasized by the authors, was chosen for replication. Further details are in the supplementary materials in [Camerer, Dreber et al. \(2016\)](#). Of the 18 replication studies, 16 were significant at the 5% level and two had  $p$ -values slightly above 0.05 but were treated as ‘positive’ results for replication and included in the replication rate calculation.

I assume replication selection is random with respect to the  $t$ -ratio for results whose  $p$ -values are below or only slightly above 0.05. This implies that  $\beta_{p2}$  measures the relative probability of being published and chosen for replication for a result whose  $p$ -value is slightly above 0.05, compared to if it were strictly below 0.05. Overall, the empirical results are valid for the population of ‘most important’ significant (or ‘almost significant’) results, as emphasized by authors, in experimental economics papers published in top economics journals between 2011 and 2014.

*Psychology.*—Next, consider replication selection in [Open Science Collaboration \(2015\)](#):

1. **Eligibility:** Studies published in 2008 in one of the following journals: *Psychological Science*, *Journal of Personality and Social Psychology*, and *Journal of Experimental Psychology: Learning, Memory, and Cognition*.
2. **Study selection:** [Open Science Collaboration \(2015\)](#) write: ‘The first replication teams could select from a pool of the first 20 articles from each journal, starting with the first article published in the first 2008 issue. Project coordinators facilitated matching articles with replication teams by interests and expertise until the remaining articles were difficult

to match. If there were still interested teams, then another 10 articles from one or more of the three journals were made available from the sampling frame.’ Importantly, the most common reason why an article was not matched was due to feasibility constraints (e.g. time, resources, instrumentation, dependence on historical events, or hard-to-access samples).

3. **Within-study replication selection:** the last experiment reported in each article was chosen for replication. [Open Science Collaboration \(2015\)](#) write that, ‘Deviations from selecting the last experiment were made occasionally on the basis of feasibility or recommendations of the original authors.’ A small number of results had  $p$ -values just above 0.05 but were treated as ‘positive’ results for replication, as in [Camerer, Dreber et al. \(2016\)](#).

This selection mechanism implies that the empirical results are valid for the distribution of last experiments in the set of eligible journals. Since neither studies nor results were selected based on statistical significance, it is reasonable to treat the ‘last experiment’ rule as effectively random. In this case, we can interpret the results as being valid for all results in the eligible set of journals.

*Social science experiments.*—Finally, consider replication selection in [Camerer, Dreber et al. \(2018\)](#):

1. **Eligibility:** Experimental studies in the social sciences published in *Nature* or *Science* between 2010 and 2015.
2. **Study selection:** [Camerer, Dreber et al. \(2018\)](#) include all studies that: ‘(1) test for an experimental treatment effect between or within subjects, (2) test at least one clear hypothesis with a statistically significant finding, and (3) were performed on students or other accessible subject pools. Twenty-one studies were identified to meet these criteria.’
3. **Within-study replication selection:** [Camerer, Dreber et al. \(2018\)](#) write, ‘We used the following three criteria in descending order to determine which treatment effect to replicate within each of these 21 papers: (a) select the first study reporting a significant treatment effect for papers reporting more than one study, (b) from that study, select the statistically significant result identified in the original study as the most important result among all within- and between-subject treatment comparisons, and (c) if there was more than one equally central result, randomly select one of them for replication.’ All results selected for replication had  $p$ -values strictly below 0.05.

This selection mechanism implies that the empirical results are valid for the population of statistically significant between- or within-subject treatment comparisons in experimental social science, which were identified by authors as the most ‘important’ and published in *Nature* or *Science* between 2010 and 2015.

## F. Predicted Replication Rates Under Alternative Power Calculations

This appendix presents several extensions to the main empirical results on predicting replication rates in experimental economics, psychology and social science. The first extension allows for variation in the application of the common power rule around mean intended power. Results are similar to those in the main text, which assume no variability in the application of the common power rule. The second extension generates replication rate predictions under the rule of setting replication power equal to original power. This delivers lower replication rates than the common power rule.

*Alternative power calculation rules.*—Consider first the rule used for calculating replication power in the main text, and then two additional approaches. For concreteness, suppose we want to calculate the replication standard error for a simulated original study  $(x^{sim}, \sigma^{sim}, \theta^{sim})$ .

1. **Common power rule (mean):** This is the rule reported in the results in the main text. It assumes no variability in the application of the common power rule, such that all replications have mean intended power  $1 - \beta^n$ . This rule implies

$$\sigma_r^{sim}(x^{sim}, \beta^n) = \frac{|x^{sim}|}{1.96 - \Phi^{-1}(\beta^n)} \quad (63)$$

2. **Common power rule (realized):** Intended power for individual replications varied around mean intended power for at least two reasons. First, replication teams were instructed to meet minimum levels of statistical power, and encouraged to obtain higher power if feasible. Second, a number of replication in [Open Science Collaboration \(2015\)](#) did not meet this requirement. Figure E1 shows the distribution of realized intended power in replications for experimental economics and psychology. Realized intended power is right-skewed for psychology. In experimental economics and social science, realized intended power is distributed more tightly around mean.

To capture variability in the application of the common power rule, take a random draw from the empirical distribution of  $|x|/\sigma_r$  and denote it  $1.96 - \hat{\beta}^n$ . Then realized intended

power for simulated study  $(x^{sim}, \sigma^{sim}, \theta^{sim})$  is equal to

$$\sigma_r^{sim}(x^{sim}, \hat{\beta}^n) = \frac{|x^{sim}|}{1.96 - \Phi^{-1}(\hat{\beta}^n)} \quad (64)$$

3. **Original power:** Set replication power equal to the power in the original study:

$$\sigma_r^{sim}(\sigma^{sim}) = \sigma^{sim} \quad (65)$$

This rule has been proposed as a straightforward, intuitive approach for designing replication studies. In a review of replication studies by [Anderson and Maxwell \(2017\)](#), 19 of 108 studies used this approach.

*Results.*—Table [F1](#) presents the results for all three applications. Panel A shows that allowing intended power to vary across replications (‘Realized power’) yields similar replication rate prediction to assuming all replications have intended power equal to the report mean (‘92% on  $X$ ’). In fact, in all three applications, the accuracy improves very slightly under the realized power rule. The biggest differences is in psychology, because the realized power rule accounts for the fact that the distribution of intended power is right skewed.

Panel B examines the proposed rule of setting replication power equal to original power. In all three cases, the expected replication rate is lower than under the common power rule.

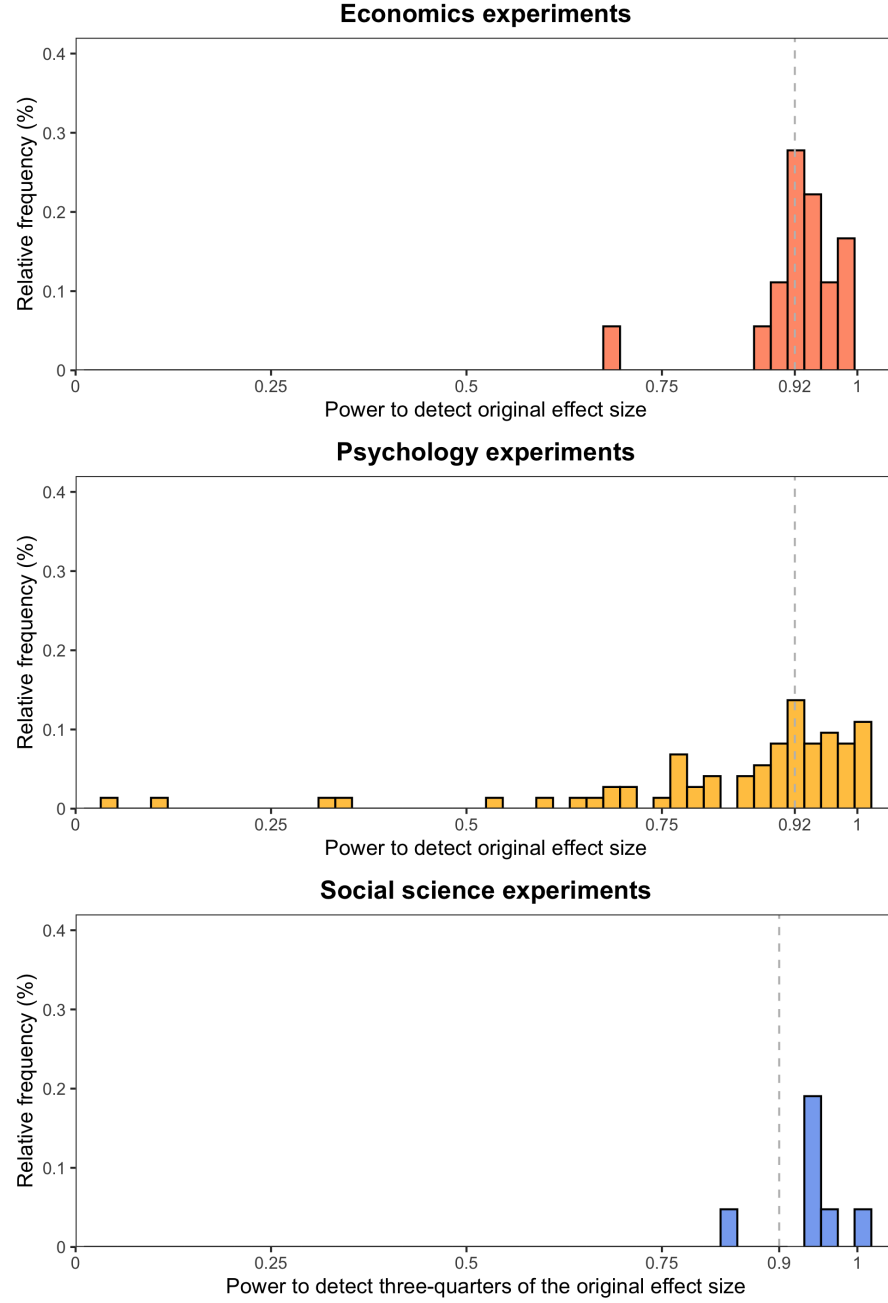


FIGURE F1. Histograms of realized intended power in replication studies in experimental economics, psychology, and social science. Data are from [Camerer, Dreber et al. \(2016\)](#), [Open Science Collaboration \(2015\)](#), and [Camerer, Dreber et al. \(2018\)](#), respectively. Realized intended power is defined as  $1 - \Phi(1.96 - \psi \cdot \frac{x}{\sigma_r})$  with  $\psi = 1$  in economics and psychology and  $\psi = 3/4$  in social science. The horizontal dashed line is reported mean power in each application. In economics and psychology, this is 92% to detect the original effect size. In social science, this is 90% to detect three quarters of the effect size.

TABLE F1 – REPLICATION RATE PREDICTIONS UNDER ALTERNATIVE REPLICATION POWER RULES

	Economics	Psychology	Social science
<i>A. Replication rate predictions</i>			
Nominal target (intended power)	0.92	0.92	—
Observed replication rate	0.611	0.348	0.571
92% on $X$	0.600	0.545	0.553
Realized power	0.615	0.523	0.565
<i>B. Alternative rule</i>			
Same power	0.550	0.486	0.494

*Notes:* Economics experiments refer to [Camerer, Dreber et al. \(2016\)](#), psychology experiments to [Open Science Collaboration \(2015\)](#), and social science experiments to [Camerer, Dreber et al. \(2018\)](#). The replication rate is defined as the share of original estimate whose replications have statistically significant findings of the same sign. Figures in the first row are observed outcomes from large-scale replication studies. Remaining rows report predicted replication rates using parameter estimates Table 1 in the main text and assuming different rules for calculating replication power.

## G. Extensions to the Empirical Model

This appendix presents results from two extensions. The first extension incorporates  $p$ -hacking and manipulation into the empirical model. In the second extension, I use the model (without  $p$ -hacking) to predict relative effect sizes, a continuous measure of the replication that is complementary to the replication rate.

### A. Augmented Model with $p$ -Hacking and Manipulation

*The augmented model.*— $p$ -hacking captures a wide range of researcher behaviors to obtain ‘more favorable’  $p$ -values e.g. the way data are cleaned, adopting convenient variable definitions, reporting favorable specifications, dropping inconvenient observations etc. I augment the empirical model to incorporate a particularly egregious form of manipulation, where researchers who obtain a marginally insignificant standardized estimates misreport their result as significant with some prespecified probability.

Formally, the augmented model adds one step in the model outlined in the main text. The  $p$ -hacking step occurs after the first stage where researchers draw a latent estimate  $X^*$ . The model of  $p$ -hacking assumes that researchers misreport marginally insignificant results, defined as any  $X^* \in (1.46\Sigma^*, 1.96\Sigma^*)$ , with probability  $\beta_h$ . Misreported results  $\tilde{X}^*$  are drawn from a uniform distribution over  $[1.96\Sigma^*, 2.46\Sigma^*]$ . Publication and replication selection depend on the reported estimate  $\tilde{X}^*$ . When  $\beta_h = 0$  we have the original model. This augmented model allows us to show how the predicted replication rate decreases as we vary  $\beta_h$  from zero (no  $p$ -hacking) to one (all marginally insignificant results are  $p$ -hacked). Note that the replication rate in this exercise is defined as the share of *reported* significant results that are replicated with the same



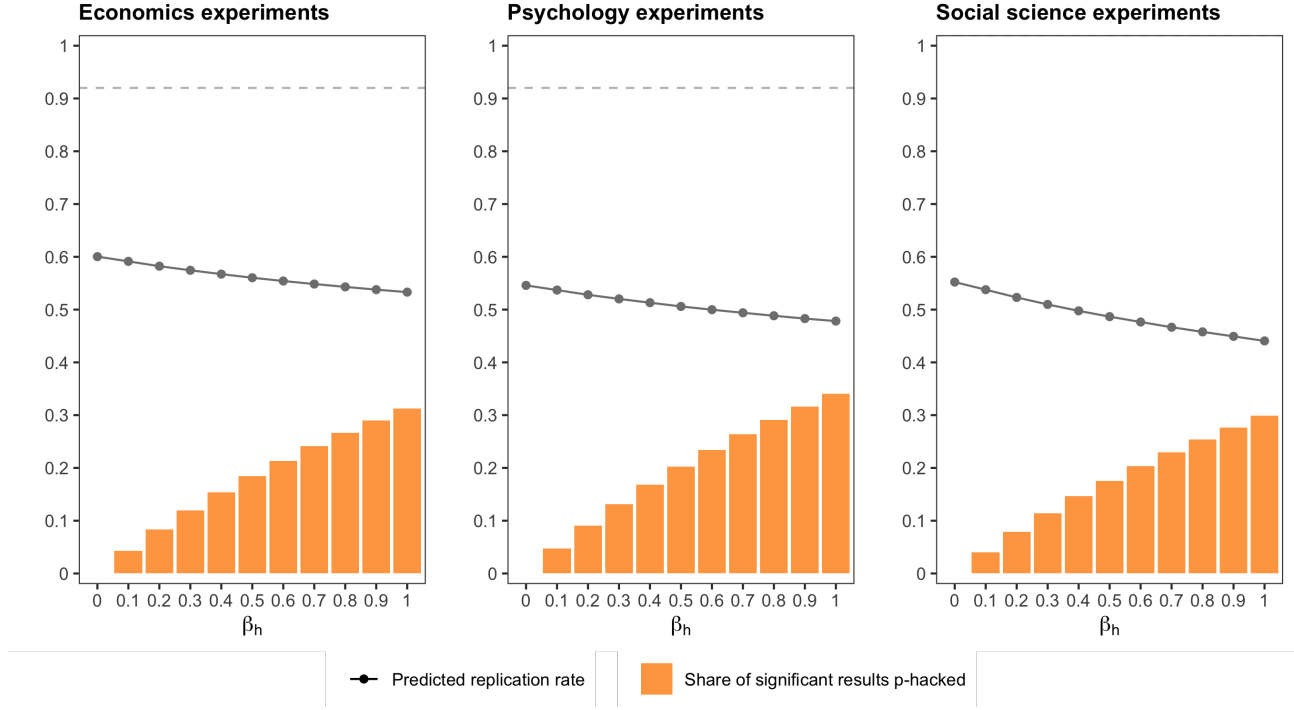
sign.

*Remark.*—Predictions are based on latent distribution of studies estimated in the standard model with no  $p$ -hacking. Thus, the augmented model assumes that the estimated standard models reflect the true DGP. A close model fit in economics and social science is consistent with no manipulation in these fields. This suggests that it may be reasonable to use estimates of the latent distribution of studies as a basis for the augmented  $p$ -hacking model, since the assumed  $p$ -hacking step occurs *after* latent studies are drawn. More caution should be applied when interpreting the results in psychology, where the model can only explain two-thirds of the replication rate gap.

Additionally, note that the accompanying code is available. It allows any one interested to test alternative specifications e.g. modifying the ranges over which marginally insignificant results are defined and misreported results are drawn from.

*Results.*—Figure G1 shows the results for specifications with  $\beta_h = 0, 0.1, 0.2, \dots, 1$ . Across all three applications, the replication rate decreases and the share of misreported results increases from no  $p$ -hacking ( $\beta_h = 0$ ) to complete  $p$ -hacking ( $\beta_h = 1$ ). Figure G1 shows that the magnitude of the decline in the replication rate is relatively small for most values of  $\beta_h$  when compared to the overall distance between the replication rate and intended power (dashed grey line) in economics and psychology. If all marginally insignificant results are manipulated ( $\beta_h = 1$ ), then the share of significant results that are  $p$ -hacked is around 30%.

A useful benchmark for what might constitute a realistic value for  $\beta_h$  comes from Brodeur et al. (2016) and Brodeur, Cook and Heyes (2022), who estimate that the proportion of ‘wrongfully claimed significant results’ is around 10%. In our model, this implies that between 22–26% of significant results are  $p$ -hacked (Table G1). Under these specifications, the replication rate falls by between 2-4 percentage points. In economics and psychology, this implies that  $p$ -hacking accounts for between 5-6% of the total gap between the predicted replication rate and mean intended power of 92%.

FIGURE G1. PREDICTED REPLICATION RATE WITH  $p$ -HACKING

Notes: Economics experiments refers to [Camerer, Dreber et al. \(2016\)](#), psychology experiments to [Open Science Collaboration \(2015\)](#) and social sciences to [Camerer, Dreber et al. \(2018\)](#). See text for details on the augmented model. The horizontal dashed line denotes mean intended power in economics and psychology. Replications for social science experiments implemented the fractional power rule to detect three-quarters of original effects with 90% power in its first stage. This rule for setting power does not have a well-defined replication target.

TABLE G1 – SPECIFICATIONS WHERE 10% OF SIGNIFICANT RESULTS ARE  $p$ -HACKED

	Implied $\beta_h$	Replication rate ( $p$ -hacking)	Replication rate (no $p$ -hacking)	Share of gap explained by $p$ -hacking
Economics	0.25	0.578	0.600	0.063
Psychology	0.22	0.526	0.545	0.047
Social sciences	0.26	0.515	0.553	—

Notes: Economics experiments refers to [Camerer, Dreber et al. \(2016\)](#), psychology experiments to [Open Science Collaboration \(2015\)](#) and social science experiments to [Camerer, Dreber et al. \(2018\)](#). The implied  $\beta_h$  is the probability of  $p$ -hacking which is consistent with having 10% of significant results wrongfully claimed as  $p$ -hacked. The share of the gap explained by  $p$ -hacking is defined as the difference between the non- $p$ -hacked replication rate and the  $p$ -hacked replication rate divided by the difference between intended power and the  $p$ -hacked replication rate.

### B. Relative Effect Size

The main focus of this article is the binary measure of replication based on the statistical significance criterion. This is because of its status as the primary replication indicator in the large-scale replication studies.<sup>20</sup> However, complementary measures are frequently presented alongside the replication rate. Perhaps the most common is the relative effect size, a continuous measure of replication defined as the ratio of replication effect size and original effect size. Relative effect sizes typically range between 0.35 and 0.7. Below, I include a brief theoretical discussion of the relative effect size and then present predictions of this measure using the estimated models.

*Theoretical discussion.*—The relative effect size for individual studies may be informative about biases affecting original studies, especially when original studies are well-powered. However, as an *aggregate* measure of reproducibility, the relative effect size measure may be subject to similar issues to the replication rate, at least in the case where it is defined exclusively over significant findings.

First, if the relative effect size is defined over significant original results, then it will be largely uninformative about the ‘file-drawer’ problem (Proposition B2).<sup>21</sup> Second, non-random sampling of significant results for replication mechanically induces inflationary bias in original estimates and regression to the mean in replication estimates, such that relative effect sizes are below one in expectation. Thus, similar to the replication rate, it has no natural benchmark against which to judge deviations, making it challenging to interpret. Relatedly, the average relative effect size is also very sensitive to power in original studies, which is unobserved. Figure G2 provides an illustration with intended power set to 0.9, which shows that the expected relative effect size for significant results is increasing in the power of original studies, and approaches one only as statistical power approaches 100%.

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<sup>20</sup>Power calculations in replications are themselves typically designed to measure a binary notation of replication ‘success’ or ‘failure’.

<sup>21</sup>Defining it over null results may present its own difficulties. For a perfectly measured null effect, the denominator in the statistic is equal to zero and the statistic is not well defined. On the other hand, if it is close but not equal to zero, then the statistic is highly sensitive to the precision of replication estimates; this raises questions about how one should set replication power when replicating a null effect.

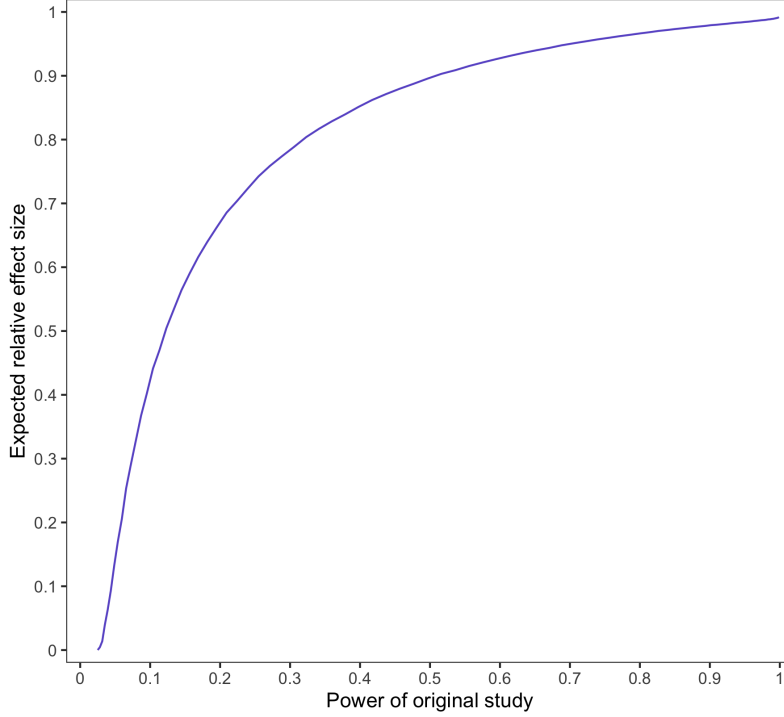


FIGURE G2. EXPECTED RELATIVE EFFECT SIZE OF SIGNIFICANT ORIGINAL STUDIES AND THEIR STATISTICAL POWER

*Notes:* Illustration for the relationship between original power and the expected relative effect size of significant findings under the common power rule are both functions of  $\omega = \theta/\sigma$  (normalized to be positive). Original power to obtain a significant effect with the same sign as the true effect is equal to  $1 - \Phi(1.96 - \omega)$ . The expected relative effect size is calculated by taking  $10^6$  draws of  $Z$  from  $N(\omega, 1)$  and then calculating  $\frac{1}{M_{sig}} \sum_{i=1}^{M_{sig}} \rho_{i,r}^{sig} / \rho_i^{sig}$ , where  $\rho = \tanh z$  denotes the Pearson correlation coefficient obtained by transforming the Fisher-transformed correlation coefficient (Fisher, 1915); and  $M_{sig}$  is the number of significant latent studies. The superscript *sig* reflects the fact that only statistically significant original results at the 5% level and their replications are included in the calculation. Replication estimates  $z_{i,r}$  are drawn from an  $N(\omega, \sigma_{r,i}(z_i, \beta^n)^2)$  distribution. The replication standard error is calculated using the common power rule to detect original effect sizes with 90% power (i.e.  $1 - \beta^n = 0.9$ ), which is given by  $\sigma_r(z_i, \beta^n) = |z_i|/[1.96 - \Phi^{-1}(\beta^n)] = |z_i|/3.242$ .

*Empirical results.*—The estimated models in Table 1 in the main text can be used to generate predictions of the average relative effect sizes. To procedure for simulating replications is identical to the procedure outlined in the main text for the replication rate case. Let  $\{x_i, \sigma_i, x_{r,i}, \sigma_{r,i}\}_{i=1}^{M_{sig}}$  be the set of simulated original studies that are published and significant, and their corresponding replication results;  $M_{sig}$  is the size of the set. The predicted relative effect size is equal to

$$\frac{1}{M_{sig}} \sum_{i=1}^{M_{sig}} \frac{\rho_{i,r}^{sig}}{\rho_i^{sig}} \quad (66)$$

where  $\rho = \tanh z$  denotes the Pearson correlation coefficient which is obtained by transforming

the Fisher-transformed correlation coefficient (Fisher, 1915). Results are presented in Table G2. The predicted average relative effect size is relatively close to observed average relative effect size in economics and social science, although somewhat optimistic in both cases. In psychology, the predicted average relative effect size is very optimistic compared to the observed value.

	Economics	Psychology	Social Sciences
Observed average relative effect size	0.657	0.374	0.443
Predicted average relative effect size	0.703	0.637	0.542

TABLE G2. AVERAGE RELATIVE EFFECT SIZE PREDICTIONS

*Notes:* Economics experiments refers to Camerer, Dreber et al. (2016), psychology experiments to Open Science Collaboration (2015) and social science experiments to Camerer, Dreber et al. (2018). Observed relative effect sizes are based on data from large-scale replication studies. Predicted average relative effect sizes are calculated using equation (66) and the procedure outlined in the text.

## H. Extending the Replication Rate Definition

This appendix analyzes a generalization of the replication rate definition that extends to insignificant results. It outlines a number of issues with this proposal.

*The Generalized Replication Rate.*—Suppose we extend the definition of the replication rate such that insignificant original results are counted as ‘successfully replicated’ if they are also insignificant in replications. Assume replication selection is a random sample of published results. Then we have the following definitions:

**Definition H1** (Generalized replication probability of a single study). *The replication probability of a study  $(X, \Sigma, \Theta)$  which is published ( $D = 1$ ) and chosen for replication ( $R = 1$ ) is*

$$\widetilde{RP}(X, \Theta, \sigma_r(X, \Sigma, \beta^n)) = \begin{cases} \mathbb{P}\left(\frac{|X_r|}{\sigma_r(X, \Sigma, \beta^n)} \geq 1.96, \text{sign}(X) = \text{sign}(X_r) \middle| X, \Theta, \sigma_r(X, \Sigma, \beta^n)\right) & \text{if } 1.96.\Sigma \leq |X| \\ \mathbb{P}\left(\frac{|X_r|}{\sigma_r(X, \Sigma, \beta^n)} < 1.96 \middle| X, \Theta, \sigma_r(X, \Sigma, \beta^n)\right) & \text{if } 1.96.\Sigma > |X| \end{cases} \quad (67)$$

**Definition H2** (Expected generalized replication probability). *The expected generalized replication probability equals*

$$\begin{aligned} \mathbb{E}[\widetilde{RP}(X, \Theta, \sigma_r(X, \Sigma, \beta^n))] &= \mathbb{P}(1.96.\Sigma \leq |X|) \mathbb{E}\left[\widetilde{RP}(X, \Theta, \sigma_r(X, \Sigma, \beta^n) \middle| X, \Theta, \sigma_r(X, \Sigma, \beta^n), 1.96.\Sigma \leq |X|]\right] \\ &+ \left(1 - \mathbb{P}(1.96.\Sigma \leq |X|)\right) \mathbb{E}\left[\widetilde{RP}(X, \Theta, \sigma_r(X, \Sigma, \beta^n) \middle| X, \Theta, \sigma_r(X, \Sigma, \beta^n), 1.96.\Sigma > |X|]\right] \end{aligned} \quad (68)$$

First, note that Definition H2 equals the standard replication rate definition when the expectation is taken only over significant studies because, in this case,  $\mathbb{P}(|X| \leq 1.96.\Sigma) =$

0. Thus, the degree to which the expected generalized replication probability differs from the standard expected replication probability depends on two factors. First, the share of published results that are insignificant. Second, the expected probability that replications will be insignificant conditional on original estimates being insignificant.<sup>22</sup>

*Empirical Results.*—To analyze the generalized replication rate, we can apply the empirical approach outlined in the main text, but using the generalized definition in place of the original definition. Recall that the original replication rate is invariant to publication bias against null results. The generalized replication rate, by contrast, does vary as the degree of selective publication against null results changes. Thus, two sets of results are presented for comparison. The first set assumes selective publication using estimated selection parameters in Table 1 in the main text. The second set assumes no selective publication (i.e. that all results are published with equal probability). We examine two rules for calculating replication power: the common power rule and the original power rule (where the replication standard error is set equal to the original standard error). For more details on different rules for calculation replication power, see Appendix E.

Table H1 reports the results for both applications. Under the common power rule, the simulated generalized replication rate remains below intended power in both publication regimes. Under the original power rule, it is relatively low when there is selective publication and around 80% when there is no selective publication.

These generalized replication rate predictions differs from the standard replication rate predictions for two reasons: (i) the share of insignificant results in the published literature and (ii) the replication probability when results are insignificant, which depends on the power rule used in replication studies. On the first point, moving from the selective publication regime to the no selective publication regime implies a dramatic increase in the share of insignificant published results; in both applications, null results change from a minority of published results to a majority. On the second point, the results show that the replication power rules considered here have some undesirable properties. First, note that the common power rule is designed to detect original estimates with high statistical power. This implies that low-powered, insignificant original results will be high-powered in replications, which increases the probability that they are significant and thus counted as replication ‘failures’ under the generalized definition. The original power rule has the reverse problem. On the one hand, low-powered, insignificant original studies are likely to be insignificant in replications, which counts as a ‘successful’ replication under the generalized definition. However, on the other hand, low-powered, significant

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<sup>22</sup>Additionally, note that this definition implies that if  $\theta = 0$ , then  $\widehat{RP}(X, \Theta, \sigma_r(X, \Sigma, \beta^n) | \Theta = 0) = 0.90375$ . That is, the replication probability of null results is constant and independent of power in original studies and replication studies.

original studies will have low replication probabilities when the same low-powered design is repeated in replications. The generalized replication rate therefore depends crucially on the share of significant and insignificant findings in the published literature, and the distribution of standard errors. Under the original power rule with no selective publication, the generalized replication rate is around 80% in both applications; however, with greater power in original studies, the replication rate would fall.

While the generalized replication rate changes as selective publication is reduced, the direction of this change depends on which replication power rule is used: with the original power rule the replication rate increases, while with the common power rule it decreases.

Overall, generalizing the replication rate with Definition H2 does not deliver replication rates close to intended power under the common power rule. For the original power rule, it is

TABLE H1 – PREDICTED GENERALIZED REPLICATION RATE RESULTS

	<i>Simulated statistics</i>	
	92% for $X$	Original power
<b>A Economics experiments</b>		
<i>Selective publication</i>		
Generalized replication rate	0.600	0.553
$\mathbb{P}(\text{Replicated} S_X = 1)$	0.600	0.551
$\mathbb{P}(\text{Replicated} S_X = 0)$	0.574	0.789
$\mathbb{P}(S_X = 1)$	0.993	0.993
$\mathbb{P}(S_X = 0)$	0.007	0.007
<i>No selective publication</i>		
Generalized replication rate	0.432	0.773
$\mathbb{P}(\text{Replicated} S_X = 1)$	0.582	0.515
$\mathbb{P}(\text{Replicated} S_X = 0)$	0.378	0.867
$\mathbb{P}(S_X = 1)$	0.268	0.268
$\mathbb{P}(S_X = 0)$	0.732	0.732
<b>B Psychology experiments</b>		
<i>Selective Publication</i>		
Generalized replication rate	0.546	0.526
$\mathbb{P}(\text{Replicated} S_X = 1)$	0.544	0.487
$\mathbb{P}(\text{Replicated} S_X = 0)$	0.563	0.839
$\mathbb{P}(S_X = 1)$	0.890	0.890
$\mathbb{P}(S_X = 0)$	0.110	0.110
<i>No selective publication</i>		
Generalized replication rate	0.490	0.798
$\mathbb{P}(\text{Replicated} S_X = 1)$	0.535	0.469
$\mathbb{P}(\text{Replicated} S_X = 0)$	0.478	0.886
$\mathbb{P}(S_X = 1)$	0.209	0.209
$\mathbb{P}(S_X = 0)$	0.791	0.791

*Notes:* Economics experiments refer to [Camerer, Dreber et al. \(2016\)](#) and psychology experiments to [Open Science Collaboration \(2015\)](#). The generalized replication rate is defined in the text. The indicator variable  $S_X$  equals one for significant results and zero otherwise. Economics experiments refers to [Camerer, Dreber et al. \(2016\)](#) and psychology experiments to [Open Science Collaboration \(2015\)](#). Simulated statistics are based on parameter estimates in Table 1 in the main text. Different column represent different rules for calculating power in replications.

higher when there is no selective publication because replications repeat low-power designs for low-powered original studies with insignificant results. The generalized replication rate under this original power rule will therefore be sensitive to the distribution of power in original studies.