A New Standard for the Analysis and Design of Replication Studies

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Abstract: A new standard is proposed for the evidential assessment of replication studies. The approach combines a specific reverse-Bayes technique with prior-predictive tail probabilities to define replication success. The method gives rise to a quantitative measure for replication success, called the sceptical $p$-value. The sceptical $p$-value integrates traditional significance of both the original and replication study with a comparison of the respective effect sizes. It incorporates the uncertainty of both the original and replication effect estimates and reduces to the ordinary $p$-value of the replication study if the uncertainty of the original effect estimate is ignored. The proposed framework can also be used to determine the power or the required replication sample size to achieve replication success. Numerical calculations highlight the difficulty to achieve replication success if the evidence from the original study is only suggestive. An application to data from the Open Science Collaboration project on the replicability of psychological science illustrates the proposed methodology.

Key Words: Power; Prior-Data Conflict; Replication Success; Reverse-Bayes; Sample Size; Sceptical $p$-value

1. Introduction

Replicability of research findings is crucial to the credibility of all empirical domains of science. As a consequence of the so-called replication crisis (Ioannidis, 2005; Begley and Ioannidis, 2015), the past years have witnessed increasing interest in large-scale replication projects, e.g. Open Science Collaboration (2015); Camerer et al. (2016, 2018). Such efforts help assess to what extent claims of new discoveries can be confirmed in independent replication studies whose procedures are as closely matched to the original studies as possible.
However, there is no established standard for the statistical evaluation of replication success. Standard significance of the replication study is often used as a criterion, but significance alone does not take the effect sizes of the original and replication study into account and can easily lead to conclusions opposite to what the evidence warrants (Simonsohn, 2015). A comparison of the effect sizes of the original and replication study is also common, where a smaller replication effect estimate decreases the credibility of the original study result. A modification of this is to investigate whether the replication effect estimate is compatible with the original effect estimate (Bayarri and Mayoral, 2002; Patil et al., 2016). Meta-analytic approaches take the results from the original and replication study at face value and combine them into an overall effect size estimate. However, when conducting a replication, researchers are challenging the original study and asking whether they should take it at face value. This is an inherently asymmetric question, where exchangeability assumptions are not appropriate and alternative methods for evidence quantification are needed.

Recently the lack of a single accepted definition of replicability has been emphasized by Goodman et al. (2016) who call for a better understanding of the relationship between reproducibility and the truth of scientific claims. Researchers have started to develop Bayesian methods to analyse and design replication studies (Verhagen and Wagenmakers, 2014; van Aert and van Assen, 2017; Schönbrodt and Wagenmakers, 2018), but there is a lack of appropriate methodology based on traditional metrics (effect estimates, confidence intervals and $p$-values). To address this deficiency, I propose a principled approach, combining the analysis of credibility (Matthews, 2001a,b) with the prior criticism approach by Box (1980) and Evans and Moshonov (2006) to define replication success (Section 2). This gives rise to a new quantitative measure of replication success, the sceptical $p$-value (Section 3).

The sceptical $p$-value has attractive properties. It takes into account the results from both the original and the replication study and is always larger than the ordinary
Statistical power is of central importance in assessing the reliability of science (Button et al., 2013). Appropriate design of a replication study is key to tackling the replication crisis as many such studies are currently severely under-powered, even by traditional standards (Anderson and Maxwell, 2017). Methods to calculate the power for replication success are proposed in Section 4. Numerical calculations highlight the difficulty to achieve replication success if the evidence from the original study is only suggestive. The framework is also used to determine the required sample size to achieve replication success with appropriate power. Section 5 presents a reanalysis of data from the Open Science Collaboration (2015) project on the replicability of psychological science to illustrate the usefulness of the proposed methodology. I close with some comments in Section 6.

2. Assessment of Replication Success

Analysis of credibility (Matthews, 2001a,b) is a reverse-Bayes procedure originally designed to assess the credibility of significant findings in the light of existing evidence. The discussion of Matthews (2001b) and the author’s response provide additional insights on the philosophy and detail of this method. The idea to use Bayes’s theorem in reverse originates in the work of I.J. Good (Good, 1950, 1983) and is increasingly used to assess the plausibility of scientific findings (Greenland, 2006, 2011; Held, 2013;
2.1. Reverse-Bayes Analysis

Analysis of credibility combines a significant effect estimate from the original study with a sceptical prior (Spiegelhalter et al., 1994, Section 4.1.3), a normal distribution centered around zero to represent doubts about large effect estimates. A sceptical prior shrinks the original effect estimate towards zero, where the amount of shrinkage depends on the sceptical prior variance. Fletcher et al. (1993) have argued for the use of sceptical priors for original clinical study results, which often show a tendency for overoptimism.

In order to challenge the original study it is natural to ask how sceptical we would have to be not to find its apparently positive effect estimate convincing. This leads to a reverse-Bayes approach, where the posterior is fixed to have a lower (or upper) credible limit exactly equal to zero and the sceptical prior variance is chosen accordingly. The approach thus represents the objection by a sceptic who argues that the original result would no longer be ‘significant’ if combined with a sufficiently sceptical prior. The goal is now to persuade the sceptic by showing that this prior is unrealistic. To do so, a replication study is conducted. If the data from the replication study are in conflict with the sufficiently sceptical prior, the original study result is confirmed.

Suppose the original study gives rise to a conventional confidence interval for the unknown effect size $\theta$ at level $1 - \alpha$ with lower limit $L$ and upper limit $U$. Assume that $L$ and $U$ are symmetric around the original point estimate $\hat{\theta}_o$ (assumed to be normally distributed) and that both are either positive or negative, i.e. the original effect is significant at significance level $\alpha$. After a suitable transformation this framework covers a large number of commonly used effect measures such as differences in means, odds ratios, relative risks and correlations.

We first need to compute the variance of the sufficiently sceptical prior. Matthews
(2001a) has shown that the equi-tailed credible interval of the sufficiently sceptical prior at level $1 - \alpha$ has limits $\pm S$ where

$$S = \frac{(U - L)^2}{4\sqrt{UL}}$$  \quad (1)

is the scepticism limit (Matthews, 2018). Note that (1) holds for any value of $\alpha$, not just for the traditional 5% level. The sufficiently sceptical prior variance $\tau^2$ can be derived from (1) and expressed as a function of the variance $\sigma_0^2$ (the squared standard error, assumed to be known) of the estimate $\hat{\theta}_o$, the corresponding test statistic $t_o = \hat{\theta}_o / \sigma_o$ and $z_{\alpha/2}$, the $1 - \alpha/2$ quantile of the standard normal distribution (Held, 2019, Appendix):

$$\tau^2 = \frac{\sigma_0^2}{t_o^2 / z_{\alpha/2}^2 - 1}$$  \quad (2)

where $t_o^2 > z_{\alpha/2}^2$ holds due to significance of the original study at level $\alpha$.

Equation (2) shows that the sufficiently sceptical prior variance $\tau^2$ can be both smaller or larger than $\sigma_0^2$, depending on the value of $t_o^2$. For a “borderline” significant result where $t_o^2$ is close to $z_{\alpha/2}^2$, the sufficiently sceptical prior variance will be relatively large. If $t_o^2$ is substantially larger than $z_{\alpha/2}^2$, then the sufficiently sceptical prior variance will be relatively small.

The left part of Figure 1 shows an example of this procedure. The original study has effect estimate $\hat{\theta}_o = 0.57$ (95% CI from $L = 0.25$ to $U = 0.89$) and two-sided $p$-value $p_o = 0.0005$. The scepticism limit, calculated from (1), turns out to be $S = 0.22$.

2.2. Assessing Prior-Data Conflict

The replication study shown in the right part of Figure 1 has an effect estimate of $\hat{\theta}_r = 0.33$ (95% CI from 0.01 to 0.65, $p_r = 0.046$). If the replication result is in conflict
Figure 1: Example of the assessment of replication success. The original study has effect estimate $\hat{\theta}_o = 0.57$ (95% CI from $L = 0.25$ to $U = 0.89$) and two-sided $p$-value $p_o = 0.0005$. The left part of the figure illustrates the reverse-Bayes derivation of the sufficiently sceptical prior with scepticism limit $S = 0.22$ based on the original study result and the posterior with lower credible limit fixed at zero. The comparison of the sufficiently sceptical prior with the replication study result ($\hat{\theta}_r = 0.33$, 95% CI from 0.01 to 0.65, $p_r = 0.046$) in the right part of the figure is used to assess potential prior-data conflict.

with the sufficiently sceptical prior, the original result is deemed credible. A visual comparison of the sufficiently sceptical prior with the replication study result in the right part of Figure 1 can be useful to assess potential conflict, but in general a more principled statistical approach is needed.

One option is to consider the original study as credible, if the absolute value of the effect estimate $\hat{\theta}_r$ from the replication study is larger than the scepticism limit $S$ (Matthews, 2001a,b). In the above example the effect estimate in the replication study ($\hat{\theta}_r = 0.33$) is larger than the scepticism limit ($S = 0.22$), so the original study would be considered credible at the 5% level. However, a disadvantage of this approach is that it does not take the (known) variance $\sigma^2_r$ of the replication estimate $\hat{\theta}_r$ (in the following also assumed to be normally distributed) into account. To address this issue, I propose to quantify prior-data conflict based on the prior-predictive distribution of $\hat{\theta}_r$, a normal distribution with mean zero and variance $\tau^2 + \sigma^2_r$ (Spiegelhalter et al., 2004, Section 5.8). This is the established way to check the compatibility of prior and
data (Box, 1980; Evans and Moshonov, 2006) and leads to the test statistic

\[ t_{\text{Box}} = \frac{\hat{\theta}_r}{\sqrt{\tau^2 + \sigma_r^2}} \]  

(3)

and the tail probability \( p_{\text{Box}} = \Pr(\chi^2(1) \geq t^2_{\text{Box}}) \) as the corresponding upper tail of a \( \chi^2 \)-distribution with one degree of freedom. Small values of \( p_{\text{Box}} \) indicate a conflict between the sufficiently sceptical prior and the estimate from the replication study and I define replication success at level \( \alpha \) if \( p_{\text{Box}} \leq \alpha \), or equivalently \( t^2_{\text{Box}} \geq z^2_{\alpha/2} \).

In the example shown in Figure 1, the prior-predictive assessment of conflict gives \( t_{\text{Box}} = 1.65 \) with Box’s tail probability \( p_{\text{Box}} = 0.098 > 0.05 \), so the replication study is not successful at the 5% level, although both the original and the replication study are significant at that level. This illustrates that replication success is a more stringent criterion than significance alone. For \( \alpha = 10\% \), Box’s tail probability is somewhat smaller (\( p_{\text{Box}} = 0.078 \)), and we can declare replication success at the 10% level.

The example illustrates how Box’s tail probability can be used to assess replication success at level \( \alpha \). However, it is difficult to interpret the actual value of \( p_{\text{Box}} \) as it depends on the choice of \( \alpha \). Furthermore, assessment of replication success is only possible if the original study result is significant at level \( \alpha \) as otherwise the sufficiently sceptical prior would not exist and \( p_{\text{Box}} \) could not be computed. These issues motivate the work described in the next section where I introduce the sceptical \( p \)-value, a quantitative measure for replication success that is independent of the level \( \alpha \).

3. The Sceptical \( p \)-Value

Instead of dichotomizing replication studies into successful yes/no at some arbitrary level \( \alpha \), I now propose the sceptical \( p \)-value \( p_S \) to assess replication success quantitatively. The idea is to determine the largest confidence level \( 1 - p_S \) for the original confidence interval, at which we are able to declare replication success at level \( p_S \).
This parallels the duality of ordinary $p$-values and confidence intervals, where the largest confidence level $1 - p$ at which we are able to declare significance can be used to compute the ordinary $p$-value $p$. Replication success at any pre-specified level $\alpha$ is then equivalent to $p_S \leq \alpha$, just as significance at level $\alpha$ is equivalent to $p \leq \alpha$.

To determine $p_S$, let $c = \sigma_o^2 / \sigma_r^2$ denote the ratio of the variances of the original and replication effect estimates and let $t_r = \hat{\theta}_r / \sigma_r$ denote the test statistic of the replication study. With (2) we can derive the prior-predictive variance of $\hat{\theta}_r$:

$$
\tau^2 + \sigma_r^2 = \sigma_r^2 \left( \frac{c}{t_o^2 / z_{\alpha/2}^2 - 1} + 1 \right).
$$

Using (3) and (4), the requirement $t_{Box}^2 = \hat{\theta}_o^2 / (\tau^2 + \sigma_r^2) \geq z_{\alpha/2}^2$ for replication success at level $\alpha$ can be written as

$$
(t_o^2 / z_{\alpha/2}^2 - 1) (t_r^2 / z_{\alpha/2}^2 - 1) \geq c,
$$

see Appendix A for a derivation. Significance of the original study implies that $z_{\alpha/2}^2 < t_o^2$ holds, therefore $z_{\alpha/2}^2 < t_r^2$ must also hold to ensure that the left hand side of equation (5) is positive. The required squared quantile $z_S^2 = z_{p_S/2}^2$ to obtain equality in (5) must therefore fulfill

$$
0 \leq z_S^2 < \min\{t_o^2, t_r^2\}
$$

and defines the sceptical $p$-value $p_S = 2 \left[ 1 - \Phi(z_S) \right]$ via

$$
(t_o^2 / z_S^2 - 1) (t_r^2 / z_S^2 - 1) = c.
$$

The requirement $p_S \leq \alpha$ for replication success at level $\alpha$ now translates to $z_S^2 \geq z_{\alpha/2}^2$.

Equation (7) can be re-written as

$$
(c - 1) z_S^4 + 2 z_S^2 t_A^2 = t_A^2 t_H^2,
$$

where $t_A = t_o^2 / t_H$.
where $t^2_A = (t^2_o + t^2_r)/2$ is the arithmetic and $t^2_H = 2/(1/t^2_o + 1/t^2_r)$ the harmonic mean of the squared test statistics $t^2_o$ and $t^2_r$. The only solution of (8) that fulfills (6) is

$$z^2_S = \begin{cases} \frac{t^2_H}{2} & \text{for } c = 1 \\ \frac{1}{c-1} \left\{ \sqrt{t^2_A [t^2_A + (c-1)t^2_H]} - t^2_A \right\} & \text{for } c \neq 1. \end{cases} \quad (9)$$

In the introductory example the original and the replication confidence intervals have the same width, so $c = 1$ and $z^2_S$ is simply half the harmonic mean of $t^2_o = 12.19$ and $t^2_r = 3.99$, i.e. $z^2_S = 3.00$, $|z_S| = 1.73$ and $p_S = 2[1 - \Phi(1.73)] = 0.083$. We can thus declare replication success at any pre-specified level $\alpha \geq 0.083$.

### 3.1. Properties

Inequation (6) implies that the sceptical $p$-value $p_S$ is always larger than both the original and the replication $p$-values $p_o$ and $p_r$. Closer inspection of equation (7) shows that $z^2_S$ is increasing with increasing $t^2_o$ (for fixed $t^2_r$ and $c$) and also with increasing $t^2_r$ (for fixed $t^2_o$ and $c$). Therefore, the smaller $p_o$ (or $p_r$), the smaller $p_S$ (for fixed $c$). Furthermore, for fixed test statistics $t_o$ and $t_r$ (so fixed $p$-values $p_o$ and $p_r$), the solution $z^2_S$ of (7) will decrease with increasing variance ratio

$$c = \frac{\sigma^2_o}{\sigma^2_r} = \frac{t^2_o / \hat{\theta}^2_o}{t^2_r / \hat{\theta}^2_r}$$

Since $t^2_r / t^2_o$ is fixed, $c$ increases with decreasing squared effect size ratio $\hat{\theta}^2_o / \hat{\theta}^2_r$. In other words, for the same ordinary $p$-values $p_o$ and $p_r$, the sceptical $p$-value $p_S$ increases with decreasing absolute replication effect estimate relative to the original effect estimate. This is a desired property, as replication studies with smaller effect estimates than the original estimates are considered less credible (Simonsohn, 2015).

To illustrate the dependence of $p_S$ on the variance ratio $c$, consider a scenario where $p_o = p_r = 0.01$, so $t^2_o = t^2_r$ and therefore $c = \hat{\theta}^2_o / \hat{\theta}^2_r$. First assume equal effect sizes
\( \hat{\theta}_r = \hat{\theta}_o \), so \( c = 1 \). The sceptical \( p \)-value turns out to be \( p_S = 0.069 \). For \( \hat{\theta}_r = \hat{\theta}_o / 2 \) \((c = 4)\) we obtain a larger value \((p_S = 0.14)\) because the effect estimate \( \hat{\theta}_r \) of the replication study is just half as large as the original estimate \( \hat{\theta}_o \). On the other hand, for \( \hat{\theta}_r = 2 \hat{\theta}_o \) \((c = 1/4)\) the sceptical \( p \)-value gets smaller \((p_S = 0.035)\). This asymmetry in the incorporation of the original and replication study data is natural, placing less weight on replication studies with relatively small effect estimates. This is the case in the introductory example, where substantial shrinkage of the replication effect estimate leads to a relatively large sceptical \( p \)-value.

It is also interesting to study limiting values of the sceptical \( p \)-value. If we let \( \sigma^2_o \downarrow 0 \) for fixed \( \hat{\theta}_o \neq 0 \), (8) reduces to the requirement \( z_S^2 = t_r^2 \), as shown in Appendix B. Thus, the ordinary \( p \)-value of the replication study is a special case of the sceptical \( p \)-value if the uncertainty of the original effect estimate is ignored. On the other hand, ignoring the uncertainty of \( \hat{\theta}_r \neq 0 \) via \( \sigma^2_r \downarrow 0 \) leads to \( z_S^2 \downarrow z_M^2 \) where

\[
z_M^2 = \frac{\sqrt{d(d+4)} - d}{2} t_o^2,
\]

with \( d = \hat{\theta}_r^2 / \hat{\theta}_o^2 \), see Appendix B for a proof. Using the criterion \( z_M^2 \geq z_{a/2}^2 \) rather than \( z_S^2 \geq z_{a/2}^2 \) to assess replication success corresponds to the Matthews (2001a,b) approach mentioned in Section 2.2. For any value of \( d \), \( z_M^2 \) is smaller than \( t_o^2 \) but can be larger than \( t_r^2 \). Ignoring the uncertainty of the replication effect estimate may thus lead to the declaration of replication success, even if the replication study is not conventionally significant on its own.

We may also consider the case \( c \downarrow 0 \) for fixed \( t_o^2 \) and \( t_r^2 \), where (9) increases with limit

\[
z_S^2 \uparrow \min\{t_o^2, t_r^2\},
\]

as shown in Appendix C. Therefore \( p_S \downarrow \max\{p_o, p_r\} \) for \( c \downarrow 0 \), which we will use in Section 3.4.
3.2. Relationship to Intrinsic Credibility

The concept of intrinsic credibility has been proposed in Matthews (2018) to check the credibility of "out of the blue" findings without any prior support. In the present context this corresponds to an original study in the absence of a replication study. The idea is to evaluate the credibility of the original study if we would be able to observe exactly the same result in the replication study.

The approach by Matthews (2018) corresponds to the case where we ignore the uncertainty of the (hypothetical) replication study and thus leads to (10) with \( d = 1 \):

\[
z_M^2 = \left( \sqrt{5} - 1 \right) / 2 t_o^2 \approx 0.618 t_o^2.
\]

However, if we incorporate the uncertainty using the prior-predictive approach by Box (1980), then we obtain \( z_S^2 = 0.5 t_o^2 \) as a special case of (9) for \( c = 1 \) and \( t_o^2 = t_r^2 \). Now \( p_S \) reduces to the \( p \)-value for intrinsic credibility,

\[
p_{IC} = 2 \left[ 1 - \Phi \left( t_o / \sqrt{2} \right) \right],
\]

as proposed in Held (2019) for the assessment of claims of new discoveries. Intrinsic credibility at level \( \alpha \) is achieved if \( p_{IC} \leq \alpha \), i.e. \( t_o^2 \geq 2 z_{\alpha/2}^2 \), which is equivalent to \( p_o \leq \alpha_{IC} \), where

\[
\alpha_{IC} = 2 \left\{ 1 - \Phi \left( \sqrt{2} z_{\alpha/2} \right) \right\}
\]

is the \( p \)-value threshold for intrinsic credibility. For \( \alpha = 0.05 \) we obtain \( \alpha_{IC} = 0.0056 \), for \( \alpha = 0.10 \) we have \( \alpha_{IC} = 0.02 \). These thresholds will become important in Section 4.

3.3. One-Sided Sceptical \( p \)-Values

The procedure described above is designed for standard two-sided confidence intervals and assesses replication success in a two-sided fashion, as the sign of \( \hat{\theta}_r \) does not matter in the computation of the sceptical \( p \)-value. In extreme cases, it may therefore
happen that a replication study is classified as successful although the signs of \( \hat{\theta}_o \) and \( \hat{\theta}_r \) differ. This “replication paradox” may also occur in a Bayes factor approach, see Ly et al. (2018) for details.

It is therefore of interest to adapt the sceptical \( p \)-value to the one-sided setting. Without loss of generality consider the one-sided alternative \( H_1: \theta > 0 \) to \( H_0: \theta = 0 \) and assume, that \( \hat{\theta}_o > 0 \). We now start with a one-sided confidence interval for \( \theta \) at level \( 1 - \tilde{\alpha} \) whose lower limit \( \hat{\theta}_o - z_{\tilde{\alpha}} \sigma_0 \) equals the lower limit \( L \) of the corresponding two-sided confidence interval at level \( 1 - 2\tilde{\alpha} \). The variance \( \tau^2 \) of the sufficiently sceptical prior therefore is (2) with \( z_{\alpha/2} \) replaced by \( z_{\tilde{\alpha}} \).

The obvious one-sided requirement for replication success \( t_{\text{Box}} = \hat{\theta}_r / \sqrt{\tau^2 + \sigma_r^2} \geq z_{\tilde{\alpha}} \) now replaces the two-sided requirement \( t_{\text{Box}}^2 \geq z_{\alpha/2}^2 \) and ensures that the replication paradox cannot occur. If \( \hat{\theta}_r \geq 0 \), we can hence still use (9) to compute \( z_S^2 \) from \( t_o, t_r \) and \( \epsilon \) and the one-sided sceptical \( p \)-value turns out to be \( \tilde{p}_S = 1 - \Phi(z_S) \), so half of the two-sided sceptical \( p \)-value: \( \tilde{p}_S = p_S/2 \). If the replication effect estimate is in the wrong direction, \( i.e. \hat{\theta}_r < 0 \), it is natural to set \( \tilde{p}_S = 1 - p_S/2 \). The same relationship holds between ordinary one- and two-sided \( p \)-values, of course, which implies that the one-sided sceptical \( p \)-value \( \tilde{p}_S \) is always larger than the ordinary one-sided \( p \)-values \( \tilde{p}_o \) and \( \tilde{p}_r \) from the two studies.

One-sided sceptical \( p \)-values are appropriate if the study protocol of the original study is already formulated in a one-sided fashion. A post-hoc (after the original study result is known) formulation of a one-sided alternative would require halving the original two-sided significance level \( \alpha \) to \( \tilde{\alpha} = \alpha/2 \). The one-sided assessment of replication success at level \( \tilde{\alpha} \) is then equivalent to the two-sided procedure at the original level \( \alpha \), if the signs of the original and replication effect estimates agree. This procedure ensures that the replication paradox cannot occur.

However, the one-sided \( p \)-value mapping (from \( \tilde{p}_o \) and \( \tilde{p}_r \) to \( \tilde{p}_S \)) will be different from the corresponding two-sided mapping (from \( p_o \) and \( p_r \) to \( p_S \)) because the same
ordinary one- and two-sided $p$-values correspond to different test statistics $t_o$ and $t_r$. For numerical illustration suppose that $\hat{\theta}_o = \hat{\theta}_r = 0.01$ (so $p_o = p_r = 0.02$ and $t_o = t_r = 2.33$) and that $\hat{\theta}_o$ and $\hat{\theta}_r$ are both positive. Then $\hat{p}_S = 0.05$ for $c = 1$, $\hat{p}_S = 0.09$ for $c = 4$ and $\hat{p}_S = 0.029$ for $c = 1/4$. These one-sided sceptical $p$-values are slightly smaller than the two-sided sceptical $p$-values for two-sided $p_o = p_r = 0.01$ (where $t_o = t_r = 2.58$), as reported in Section 3.1 ($p_S = 0.069$, 0.14 and 0.035 for $c = 1$, 4 and 1/4, respectively). This illustrates that the one-sided assessment of replication success based on one-sided ordinary $p$-values is slightly less stringent than the two-sided assessment based on two-sided ordinary $p$-values, if the original and replication effect estimates have the same sign.

3.4. The Distribution Under The Null

It is interesting to compare the distributions of $p_o$ (or $p_r$), $p_{IC}$ and $p_S$ under the assumption of no effect, where the ordinary $p$-value is uniformly distributed. We can easily derive the density of $p_{IC}$ with a change-of-variables using (12): $f(p_{IC}) = 2\sqrt{\pi} \phi \{t(p_{IC})\}$, here $\phi(\cdot)$ is the standard normal density function and $t(p_{IC}) = \Phi^{-1}(1 - p_{IC}/2)$.

The distribution of $p_S$ can be studied via stochastic simulation. Density estimates are displayed in Figure 2 for different values of the variance ratio $c$ based on $5 \times 10^6$ samples each. We can see that the risk of small “false positive” sceptical $p$-values is drastically reduced, compared to ordinary $p$-values based on one study only. Note that the variance is usually inversely proportional to the sample size of each study, i.e. $\sigma_o^2 = \kappa^2/n_o$ and $\sigma_r^2 = \kappa^2/n_r$ for some unit variance $\kappa^2$, say. Then $c = n_r/n_o$, so the variance factor $c$ is increasing with increasing sample size $n_r$ of the replication study. The distribution of $p_S$ in Figure 2 is shifted to the right with increasing $c$, so an increasing sample size of the replication study reduces the risk of a false claim of replication success.
From (11) we know that for $c \downarrow 0$ we have $p_S \downarrow \max\{p_o, p_r\}$, which follows a triangular $\text{Be}(2,1)$ distribution if $p_r$ and $p_o$ are independently uniform. The corresponding density function is shown in Figure 2, as well as the density function of $p_o$ and $p_{IC}$. The triangular distribution gives the upper bound $\alpha^2$ for the tail probability $\Pr(p_S \leq \alpha | H_0)$ for sufficiently small $\alpha$ and any value of the variance ratio $c$. For example, for $\alpha = 0.05$ we obtain $\Pr(p_S \leq 0.05 | H_0) \leq 0.0025$ for any $c$. This is to be compared with $\Pr(p_o \leq 0.05 | H_0) = 0.05$ and $\Pr(p_{IC} \leq 0.05 | H_0) = 0.0056$. However, $\alpha^2$ is not a particularly sharp bound. If, for example, the replication sample size equals the original sample size ($c = 1$), then $\Pr(p_S \leq 0.05 | H_0) \approx 0.0001$ is much smaller than 0.0025.
4. Power and Sample Size Calculations

Replication success is not only a function of the two \( p \)-values from the original and replication study, but also of sample size, which enters in the variance ratio \( c \). The computation of the power or the required replication sample size to achieve replication success is hence more challenging than in standard sample size calculations. A larger sample size will be required since replication success (defined as \( p_S \leq \alpha \)) implies significance of the replication study (\( p_r \leq \alpha \)). Furthermore, the required sample size will depend on the \( p \)-value \( p_o \) from the original study.

The Bayesian assessment of sample size uses a design prior (O’Hagan and Stevens, 2001; O’Hagan et al., 2005) to express prior beliefs about the true effect size. In order to power a study for replication success, the results from the original study will thus enter in two ways, as design prior for the effect size and in the subsequent assessment of replication success. For the former I will distinguish two cases, a normal prior with mean \( \hat{\theta}_o \) and variance \( \sigma^2_o \) and a point prior at \( \hat{\theta}_o \). The normal prior incorporates the uncertainty of \( \hat{\theta}_o \) while the point prior does not, in analogy to the concepts of predictive and conditional power in clinical trials (Spiegelhalter and Freedman, 1986; Spiegelhalter et al., 1986).

Suppose \( n_o \) is the size of the original study sample and \( n_r \) the sample size of the replication study, so \( \sigma^2_o = \kappa^2/n_o \) and \( \sigma^2_r = \kappa^2/n_r \), where \( \kappa^2 \) is the unit variance from one observation. Then \( c = n_r/n_o \), which would also hold in a balanced two-sample design with respective sample sizes \( n_o \) and \( n_r \) per group. Under an initial uniform prior for \( \theta \), the sampling distribution \( \hat{\theta}_o \sim \text{N}(\theta, \sigma^2_o) \) of the original study now serves as prior distribution \( \theta \mid \hat{\theta}_o \sim \text{N}(\hat{\theta}_o, \sigma^2_o) \) with prior-predictive distribution

\[
\hat{\theta}_r \mid \hat{\theta}_o \sim \text{N} \left( \hat{\theta}_o, \sigma^2_o + \sigma^2_r = \kappa^2 \left\{ \frac{1}{n_o} + \frac{1}{n_r} \right\} \right) \tag{14}
\]

for the observed effect \( \hat{\theta}_r \) in the replication study. Then \( t^2_r \) follows a scaled non-central
\( \chi^2 \)-distribution with one degree of freedom, scaling factor \( 1 + c \) and non-centrality parameter \( t_0^2/(1 + 1/c) \), as shown in Appendix D. For the alternative point prior at \( \theta = \hat{\theta}_o \), \( t_r^2 \) follows a non-central \( \chi^2 \)-distribution with one degree of freedom and non-centrality parameter \( \lambda = c t_o^2 \).

To compute the power for replication success, the relative sample size \( c = n_r/n_o \) in (9) is fixed. Then \( z^2 \) and the sceptical \( p \)-value \( p_S \) are monotone functions of \( t_r^2 \) and we can compute the power for replication success at any level \( \alpha \). We can also calculate the required relative sample size \( c = n_r/n_o \) at some pre-defined power for replication success. Both tasks require application of numerical root-finding algorithms. Computational details are omitted here.

### 4.1. Power calculations

Figure 3 compares the power for significance with the power for replication success for a replication study with sample size equal to the original study \( (c = 1) \) at level \( \alpha = 5\% \) as a function of the two-sided \( p \)-value \( p_o \) of the original study. Power calculations for significance aim to detect the effect estimate \( \hat{\theta}_o \) from the original study with a standard two-sided significance test. Not accounting for the associated uncertainty corresponds to the concept of conditional power, whereas predictive power calculations are based on a normal design prior with mean \( \hat{\theta}_o \) and variance \( \sigma_o^2 \) (Spiegelhalter et al., 2004, Equation 6.4). The results are in accordance with those reported in Goodman (1992). In particular, for \( p_o = 0.05 \) the power is 50\% both for conditional and predictive power with conditional power increasing faster than predictive power for smaller \( p \)-values \( p_o \).

Conditional and predictive power for replication success is also shown in Figure 3 for two-sided \( \alpha = 5\% \) respectively one-sided \( \tilde{\alpha} = 5\% \). As expected, the power for replication success is lower than for significance, and drops quickly to zero for values of \( p_o \) close to 0.05. Remarkably, in the two-sided case, a conditional and predictive
Figure 3: Power calculations for a replication study with sample size equal to the original study ($c = 1$). Shown is conditional and predictive power for significance (two-sided) and for replication success (one- and two-sided) at level $\alpha = 5\%$ as a function of the two-sided $p$-value of the original study. The one- and two-sided thresholds for intrinsic credibility are marked in red on the x-axis.

A power of 50% is attained at $p_o = 0.0056$. This is the threshold (13) for intrinsic credibility at level $\alpha = 5\%$, as described in Section 3.2. In the one-sided case a power of...
50% is obtained at $p_o = 0.02$, the threshold for intrinsic credibility at two-sided level $\alpha = 10\%$. Therefore, only intrinsically credible results (based on the threshold (13)) ensure that the power for success of an identically designed replication study exceeds 50%. This intriguing feature highlights the difficulty to achieve replication success if the evidence from the original study is only suggestive and provides a new argument for more stringent $p$-value thresholds for claims of new discoveries (Johnson, 2013; Benjamin et al., 2018; Ioannidis, 2018; Held, 2019).

This surprising result can be explained as follows: If the non-centrality parameter $\lambda$ of a non-central $\chi^2(1)$-distribution is reasonably large, say $\lambda > 4$, then the median is approximately equal to $\lambda$. Under the point prior and for $c = 1$, the non-centrality parameter of the distribution of $t_H^2$ is $\lambda = t_o^2$, so $\text{Med}(t_H^2) \approx t_o^2$. The sceptical $p$-value $p_S = 2[1 - \Phi(z_S)]$ is then defined through $z_S^2 = t_H^2 / 2 = (1/t_o^2 + 1/t_r^2)^{-1}$, so the median of $z_S^2$ is approximately $t_o^2 / 2$. Replication success is thus achieved with 50% probability for $z_{s/2}^2 = z_S^2 \approx t_o^2 / 2$, i.e. $t_o^2 \approx 2z_{s/2}^2$. This corresponds to the intrinsic credibility threshold (13) for the ordinary $p$-value $p_o$ from the original study.

We obtain essentially the same result under the normal prior, where now $\lambda = t_o^2 / 2$, which combined with a scaling factor of 2 also leads to $\text{Med}(t_H^2) \approx t_o^2$ for sufficiently large $t_o^2$. The rest of the argument is as above, but note that this approximation is slightly less precise, because the non-centrality parameter is half as large than under the point prior. The approximation is, however, still very good: For $\alpha = 5\%$, the exact power for replication success at $p_o = 2[1 - \Phi(\sqrt{2}z_{0.025})]$ is 50.00001% for the point prior and 50.00459% for the normal prior.

### 4.2. Sample size calculations

Figure 4 compares different strategies to determine the replication sample size for two-sided $\alpha = 5\%$ respectively one-sided $\tilde{\alpha} = 5\%$ and original two-sided $p$-values $p_o$ between 0.0001 and 0.05. The power to achieve significance respectively replication
Two-sided sample size calculations based on conditional power give relative sample sizes between 0.52 and 2, depending on the $p$-value $p_o$ of the original study. Incorporating the uncertainty from the original study based on predictive power gives
relative sample sizes between 0.61 and 3.7.

The required relative sample size for two-sided replication success is larger than
the one for significance alone and depends more drastically on the \( p \)-value \( p_o \) of the
original study. First consider the case of conditional power. If \( p_o \) is smaller than
0.001, the required relative sample size \( c \) is smaller one, so the replication sample
size \( n_r \) does not need to be larger than the original sample size \( n_o \). However, the
required sample size explodes for larger \( p \)-values with an asymptote around \( p_o =
0.012 \). This highlights the difficulty to achieve 80% power for replication success with
original \( p \)-values between 0.01 and 0.05. Even larger sample sizes are required based
on predictive rather than conditional power with an asymptote around \( p_o = 0.005 \).

The curves shift a little to the right when we assess replication success in a one-sided
fashion, pushing the asymptotes towards \( p_o = 0.035 \) for conditional and \( p_o = 0.017 \)
for predictive power. The predictive power asymptotes are remarkably close to the
corresponding thresholds 0.0056 and 0.02 for intrinsic credibility, also shown in Figure
4. Of course, the asymptotes would change for power values different from 80%.

5. Replication Success in Psychological Science

I now re-analyse data from the Open Science Collaboration (2015) project, a multi-
year endeavour that replicated 100 scientific studies selected from three prominent
psychology journals. Effect sizes have been transformed to correlation coefficients
\( \hat{\rho} \) where application of Fisher’s \( z \)-transformation \( \hat{\theta} = \tanh^{-1}(\hat{\rho}) \) justifies a normal
assumption with the standard error being a function only of the nominal study sample
size \( n \): \( \text{se}(\hat{\theta}) = 1/\sqrt{n - 3} \), so the effective sample size is \( n - 3 \) with variance ratio
\( c = (n_r - 3)/(n_o - 3) \). Effective sample sizes are available for 73 studies, the so-called
Meta-Analytic subset (Johnson et al., 2016). Two-sided \( p \)-values \( p_o \) and \( p_r \) have been
calculated assuming normality of the corresponding test statistics \( t_o = \hat{\theta}_o / \text{se}(\hat{\theta}_o) \) and
\( t_r = \frac{\hat{\theta}_r}{\text{se}(\hat{\theta}_r)} \), respectively. I have not included the remaining 27 studies where a normal approximation is questionable since the standard errors of \( \hat{\theta}_o \) and \( \hat{\theta}_r \) are not available.

Figure 5 displays the replication versus the original correlation estimates. Eight of the 73 original studies are not significant at the standard \( \alpha = 5\% \) level, three of them with \( p \)-values between 0.05 and 0.06. There have been 21 significant replication studies following from the 65 significant original studies. The sceptical \( p \)-value allows us to rank the studies by the degree of replication success. Table 1 lists the 24 most successful replication studies with \( p_S \leq 0.15 \) of which the top 11 have been successful at the two-sided 5\% level (\( p_S \leq 0.05 \)). The remaining 13 studies in Table 1 (with \( p_S > 0.05 \)) show some interesting features. For example, study 18 has a non-significant replication result but still leads to replication success at the 10\% level. Conversely, there are several studies with both \( p_o \leq 0.05 \) and \( p_r \leq 0.05 \) but \( p_S > 0.10 \). This illustrates once again, that the sceptical \( p \)-value does not only take significance of the original and replication study into account, but also effect and sample sizes, both entering in the variance ratio \( c \).

6. Discussion

Science would proceed more efficiently if statistical approaches to inference are better aligned with scientific needs and practice (Goodman, 2016). The traditional dichotomy between ‘Bayesians’ and ‘frequentists’ may not always be useful to achieve this goal. The proposed approach follows the spirit of “evolution rather than revolution” (Matthews, 2018) and provides a framework for extracting more insight from replication studies based on standard metrics (effect estimates, confidence intervals and \( p \)-values). Instead of synthesising original and replication study results through a meta-analysis, the original study result is challenged with the sufficiently sceptical prior. Replica-
Figure 5: Application to Open Science Collaboration (2015) data: The circles represent the effect estimates (correlations) of original and replication studies. The circle size represents the predictive power for replication success at the two-sided 5% level. Replication success and significance is also assessed at the two-sided 5% level and indicated by the color of the circles.
Table 1: Results for the 24 most successful replication studies with $p_s \leq 0.15$, as listed in the last column. The penultimate column gives the predictive power for replication success (in %) at level $\alpha = 5\%$.

|          | Original study | Replication study | Replication Success | Power | $p_s$ |
|----------|----------------|-------------------|---------------------|-------|-------|
| $n_o$    | $\hat{p}_o$    | $p_o$             | $n_r$               | $\hat{p}_r$ | $p_r$ | $\Power$ | $p_s$ |
| 1        | 126            | 0.68              | < 0.0001            | 177   | 0.76  | < 0.0001 | > 99.9 | < 0.0001 |
| 2        | 78             | 0.77              | < 0.0001            | 38    | 0.65  | < 0.0001 | > 99.9 | < 0.0001 |
| 3        | 30             | 0.70              | < 0.0001            | 31    | 0.78  | < 0.0001 | 95.3   | 0.0005   |
| 4        | 174            | 0.29              | 0.0001              | 141   | 0.32  | < 0.0001 | 82.6   | 0.005    |
| 5        | 32             | 0.57              | 0.0005              | 32    | 0.65  | < 0.0001 | 78.7   | 0.007    |
| 6        | 22             | 0.71              | < 0.0001            | 22    | 0.68  | 0.0003  | 87.6   | 0.008    |
| 7        | 38             | 0.62              | < 0.0001            | 39    | 0.48  | 0.002   | 93.7   | 0.011    |
| 8        | 30             | 0.69              | < 0.0001            | 27    | 0.53  | 0.004   | 92.1   | 0.015    |
| 9        | 117            | 0.21              | 0.023               | 236   | 0.50  | < 0.0001 | 14.0   | 0.033    |
| 10       | 23             | 0.67              | 0.0003              | 31    | 0.47  | 0.007   | 88.4   | 0.038    |
| 11       | 9              | 0.72              | 0.026               | 18    | 0.92  | < 0.0001 | 9.3    | 0.048    |
| 12       | 154            | 0.36              | < 0.0001            | 50    | 0.28  | 0.045   | 69.7   | 0.052    |
| 13       | 40             | 0.37              | 0.017               | 95    | 0.41  | < 0.0001 | 29.0   | 0.06     |
| 14       | 11             | 0.70              | 0.014               | 11    | 0.75  | 0.006   | 28.9   | 0.067    |
| 15       | 25             | 0.59              | 0.001               | 33    | 0.40  | 0.022   | 76.7   | 0.072    |
| 16       | 41             | 0.52              | 0.0004              | 41    | 0.32  | 0.044   | 79.6   | 0.08     |
| 17       | 9              | 0.74              | 0.021               | 16    | 0.70  | 0.002   | 18.9   | 0.091    |
| 18       | 33             | 0.56              | 0.0005              | 21    | 0.40  | 0.071   | 64.5   | 0.096    |
| 19       | 33             | 0.38              | 0.029               | 72    | 0.38  | 0.0009  | 4.3    | 0.10     |
| 20       | 25             | 0.45              | 0.025               | 39    | 0.42  | 0.007   | 10.9   | 0.10     |
| 21       | 57             | 0.33              | 0.011               | 118   | 0.23  | 0.014   | 44.5   | 0.11     |
| 22       | 96             | 0.20              | 0.057               | 243   | 0.25  | < 0.0001 | 0.0    | 0.12     |
| 23       | 16             | 0.65              | 0.005               | 13    | 0.50  | 0.085   | 45.9   | 0.13     |
| 24       | 69             | 0.35              | 0.003               | 178   | 0.15  | 0.045   | 78.0   | 0.14     |
success. Specifically, a large sceptical $p$-value can occur if the replication sample size was too small, even if original and replication effect sizes are approximately equal, and should not be taken as evidence for no effect (Altman and Bland, 1995). This is not the only reason why it would be interesting to compare the sceptical $p$-value to direct “forward-Bayes” approaches, such as the replication Bayes Factor (Verhagen and Wagenmakers, 2014; Ly et al., 2018), which quantifies the change in evidence brought about by observing the results from the replication study, given that the evidence from the original study is already available.

Significance of both the original and the replication study is a necessary but not sufficient requirement for replication success. The proposed framework thus extends the “two pivotal study paradigm” requiring two significant findings from two independent confirmatory trials for regulatory drug approval, see Kennedy-Shaffer (2017) for a recent review. However, the difficulty to achieve replication success if the evidence from the original study is only suggestive underlines the need for more stringent $p$-value thresholds for claims of new discoveries. The threshold for intrinsic credibility (13) is a natural choice for this task.

It would be interesting to extend the approach to a setting where several replication studies are available. For example, a summary estimate based on a meta-analysis of all available replication studies may be used to assess replication success. If results from replication studies become sequentially available, an alternative approach is to combine original and replication effect estimates into an overall summary measure. Some down-weighting of the original study result would in general be required depending on the degree of replication success. The overall estimate could then be used as a new “original” effect estimate in order to assess the success of a second replication study. This would open up new ways to iteratively challenge existing knowledge through a series of replication studies and would provide an interesting alternative to traditional evidence synthesis methods.
The proposed reverse-Bayes approach assumes a simple mathematical framework, where likelihood, prior and posterior are all assumed to be normal. It will be of interest to extend this framework to other settings, for example to the t-distribution.

**Data and Software Availability** Data analyzed in this article are originally from Open Science Collaboration (2015) and have been downloaded from https://osf.io/fgjvw/. Software to compute the sceptical p-value and the power or required sample size to achieve replication success are available in the R-package pCalibrate available on the Comprehensive R Archive Network (https://CRAN.R-project.org/package=pCalibrate).

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Appendix

A. Proof of equation (5)

We have

\[ t_{\text{Box}}^2 = \frac{\hat{\theta}_x^2}{t^2 + \sigma_r^2} = \frac{\hat{\theta}_x^2}{\sigma_r^2} \left( \frac{c}{t_0^2/z_{\alpha/2}^2} + 1 \right)^{-1} = t_r^2 \left( \frac{t_0^2/z_{\alpha/2}^2 - 1}{c + t_0^2/z_{\alpha/2}^2 - 1} \right), \]

so the requirement \( t_{\text{Box}}^2 \geq z_{\alpha/2}^2 \) for replication success at level \( \alpha \) is equivalent to

\[ \frac{t_r^2}{z_{\alpha/2}^2} \left( \frac{t_0^2}{z_{\alpha/2}^2} - 1 \right) \geq c + t_0^2/z_{\alpha/2}^2 - 1. \]

Subtracting \( t_0^2/z_{\alpha/2}^2 - 1 \) on both sides leads to (5).

B. The limiting cases \( \sigma_o^2 \downarrow 0 \) and \( \sigma_r^2 \downarrow 0 \)

Equation (8) can be re-written as

\[ \frac{c - 1}{t_A^2} z_5^4 + 2 z_5^2 = t_H^2, \tag{15} \]

where

\[ \frac{c - 1}{t_A^2} = \frac{\sigma_o^2 - \sigma_r^2}{\sigma_r^2} \frac{2}{t_A^2 + t_r^2} = \frac{2 \sigma_o^2 (\sigma_o^2 - \sigma_r^2)}{\hat{\theta}_o^2 \sigma_r^2 + \hat{\theta}_r^2 \sigma_o^2}. \]

For \( \sigma_o^2 \downarrow 0 \) we thus have \( (c - 1)/t_A^2 \rightarrow 0 \) and \( t_H^2 \rightarrow 2 t_r^2 \) so equation (15) reduces to \( 2 z_5^2 = 2 t_r^2 \) and hence \( z_5^2 = t_r^2 \). For \( \sigma_r^2 \downarrow 0 \) we have \( (c - 1)/t_A^2 \rightarrow (2 \sigma_o^2) / \hat{\theta}_r^2 \) and \( t_H^2 \rightarrow 2 t_o^2 \) so equation (15) reduces to \( (\sigma_o^2 / \hat{\theta}_r^2) z_5^4 + z_5^2 = t_o^2 \). The solution of this equation is

\[ z_5^2 = \frac{\hat{\theta}_r^2}{2 \sigma_o^2} \left\{ \sqrt{1 + 4 \sigma_o^2 t_0^2 / \hat{\theta}_r^2} - 1 \right\} \]

\[ = \frac{\hat{\theta}_r^2}{2 \sigma_o^2} \left\{ \sqrt{1 + 4/d} - 1 \right\} \]

\[ = \frac{\hat{\theta}_r^2}{2 \sigma_o^2} \left\{ \sqrt{d^2 + 4 d} - d \right\} \]

\[ = \frac{t_0^2}{2} \left\{ \sqrt{d(d + 4)} - d \right\}, \]
which is equation (10) with \( d = \hat{\theta}_r^2 / \hat{\theta}_o^2 \).

\[ \text{C. Proof of result (11)} \]

Equation (9) reduces for \( c \downarrow 0 \) to

\[
z_S^2 = t_A^2 - \sqrt{t_A^2 [t_A^2 - t_H^2]} = t_A^2 - \sqrt{\frac{t_A^2 (t_o^2 - t_r^2)}{2 t_o^2 + t_r^2}} = t_A^2 - \frac{|t_o^2 - t_r^2|}{2} = \min\{t_o^2, t_r^2\}.
\]

The derivative of (9) with respect to \( c \) is (for \( c \neq 1 \))

\[
\frac{d z_S^2}{d c} = - \frac{1}{c - 1} \left\{ z_S^2 - \frac{1}{2} \frac{t_A^2 t_H^2}{(c - 1) z_S^2 + t_A^2} \right\} = - \frac{z_S^2}{c - 1} \left\{ 1 - \frac{1}{2} \frac{(c - 1) z_S^2 + 2 t_A^2}{(c - 1) z_S^2 + t_A^2} \right\} = - \frac{1}{2} \frac{z_S^4}{(c - 1) z_S^2 + t_A^2}
\]

where the middle line follows from (8) and the last line also holds for \( c = 1 \). It is easy to see from (9) that \((c - 1) z_S^2 + t_A^2 > 0\) for all \( c \), and therefore (16) is negative for all \( c \).

\[ \text{D. Proof of results in Section 4} \]

For notational simplicity I omit the conditioning on \( \hat{\theta}_o \) in the following. Equation (14) implies a distribution on \( t_r = \hat{\theta}_r / \sigma_r = \sqrt{n_r} \hat{\theta}_r / \kappa_r \)

\[ t_r \sim \text{N} \left( \sqrt{n_r} \frac{\hat{\theta}_r}{\kappa_r} \cdot \frac{n_o + n_r}{n_o} \right), \]

so \( t_r = \sqrt{(n_o + n_r) / n_o} \bar{t}_r \), where

\[ \bar{t}_r \sim \text{N} \left( \sqrt{\frac{n_o n_r}{n_o + n_r} \frac{\hat{\theta}_o}{\kappa}}, 1 \right). \]

Therefore \( t_r^2 = (n_o + n_r) / n_o \cdot \bar{t}_r^2 \) follows a scaled non-central \( \chi^2 \)-distribution with 1 degree of freedom, scaling factor \((n_o + n_r) / n_o = 1 + c\) and non-centrality parameter \( \lambda = (n_o n_r) / (n_o + n_r) \cdot \hat{\theta}_o^2 / \kappa^2 = t_o^2 / (1 + 1/c) \).
Things simplify somewhat for a point prior $\theta = \hat{\theta}_o$ at the estimate from the original study. Then $\hat{\theta}_r | \hat{\theta}_o \sim N(\hat{\theta}_o, \kappa^2/n_r)$ so $t_r \sim N(\sqrt{n_r} \hat{\theta}_o / \kappa, 1)$. Now $t_r^2$ follows a non-central $\chi^2$-distribution with 1 degree of freedom and non-centrality parameter $\lambda = n_r \hat{\theta}_o^2 / \kappa^2 = c t_o^2$. 