Running head: BETWEEN-STUDY HETEROGENEITY TEST

A Bootstrap Based Between-Study Heterogeneity Test in Meta-Analysis

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Abstract

Meta-analysis combines pertinent information from existing studies to provide an overall estimate of population parameters/effect sizes, as well as to quantify and explain the differences between studies. However, testing the between-study heterogeneity is one of the most troublesome topics in meta-analysis research. Additionally, no methods have been proposed to test whether the size of the heterogeneity is larger than a specific level. The existing methods, such as the Q test and likelihood ratio (LR) tests, are criticized for their failure to control the Type I error rate and/or failure to attain enough statistical power. Although better reference distribution approximations have been proposed in the literature, the expression is complicated and the application is limited. In this article, we propose bootstrap based heterogeneity tests combining the restricted maximum likelihood (REML) ratio test or Q test with bootstrap procedures, denoted as B-REML-LRT and B-Q respectively. Simulation studies were conducted to examine and compare the performance of the proposed methods with the regular LR tests, the regular Q test, and the improved Q test in both the random-effects meta-analysis and mixed-effects meta-analysis. Based on the results of Type I error rates and statistical power, B-Q is recommended. An R package `boot.heterogeneity` is provided to facilitate the implementation of the proposed method.

Keywords: Meta-analysis, Between-study heterogeneity, bootstrap method

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1. Introduction

Meta-analysis is a popular statistical technique that combines the results across multiple studies (Glass et al., 1984; Hedges & Olkin, 1985; Hunter et al., 1982). Besides providing a pooled estimate of the unknown population effect sizes, modern meta-analysis methods also have the capacity to quantify and explain the differences between studies. Fixed-effects meta-analysis assumes that all studies have the same true effect, whereas random-effects and mixed-effects models assume that there is variability (i.e., between-study heterogeneity) across study-specific population/true effects. Researchers have suggested that one should decide whether to include the between-study heterogeneity based on the assumption and intended generalization of the obtained results (Borenstein et al., 2009b; Coughlin et al., 2015). More specifically, if one would like to generalize the conclusion from a specific set of studies, the model with the between-study heterogeneity is preferred.

Although we cannot rely on the heterogeneity test to select the fixed-effects model versus random-effects model, testing between-study heterogeneity is the research of interest in some studies and widely used in practice. There are two types of between-study heterogeneity tests. In the first type of test, the null hypothesis is “true effects are the same (no heterogeneity; $\tau^2 = 0$)” and the alternative hypothesis is “true effects vary ($\tau^2 \neq 0$)”. We refer to this type of test as the *heterogeneity test*. In the second type of test, the null hypothesis that “the size of heterogeneity is equal to a specific level ($\tau^2 = \lambda$)” and the alternative hypothesis is that “the size heterogeneity is larger than a specific level ($\tau^2 > \lambda$)”. We refer to this type of test as the *heterogeneity magnitude test*. To the best of our knowledge, no existing method can handle the latter type of hypothesis testing. There are methods for the former type of hypothesis testing, but those heterogeneity tests are criticized by their failure to control the Type I error rate and low power.

Hence, the goal of the current student is to propose a more reliable and powerful test for both hypotheses.

The importance of the two types of between-study heterogeneity tests is three-fold. First, “in a meta-analysis, it is usual to conduct a homogeneity test to determine whether the effects measured by the included studies are sufficiently similar to justify their combination” (Kulinskaya et al., 2011a, p. 254). When the between-study heterogeneity is large, researchers may be more interested in the effect size of each specific study (e.g., Thalib et al., 2011). Field (2003) illustrated one example. The effect sizes of cognitive behavior therapy for social phobia in the United Kingdom, United States, and Netherlands are found to be different, but the overall effect size across countries may be 0. In this case, interpreting the overall effect size only may not provide any practical meaning, whereas estimating the between-study heterogeneity is important and “it is sometimes useful to test the hypothesis that the effect size variance is zero in addition to estimating this quantity” (Hedges & Olkin, 1985, p.197). Furthermore, in the heterogeneity magnitude test, with a significant test result, we can claim that the heterogeneity is larger than a specific level (e.g., a scientifically or clinically meaningful difference). Second, one may be interested in what factors could explain the between-study heterogeneity. To address this research question, relevant study-level covariates (also referred to as moderators) are included in a mixed-effects model (meta-regression) to explain whether there are systematic reasons why effect sizes are different (Field, 2003). For example, studies on gender differences in marital satisfaction may have discrepancy due to cohort effects. Marriage may look very different today than it did 10 or 20 years ago. Thus, the year in which each study was initiated can be considered as a moderator to explain why effect sizes vary across studies. We don’t anticipate that the covariates can explain the entire residual heterogeneity, but “the size of this residual or unexplained heterogeneity is one consideration in evaluating the adequacy of the moderation model” (Card, 2015, p. 218). The test of (unexplained) heterogeneity in meta-regression allows for the evaluation of more complex moderation hypothesis. “For example, one can test interactive combinations of moderators by creating product terms. Similarly one can evaluate nonlinear moderation by

the creation of power polynomial terms” (Card, 2015, p. 218). Borenstein et al. (2009b) called this test the *goodness of fit test* (p. 198). Third, heterogeneity tests are particularly important in replication research. When one wants to know whether the replication is exact (studies have identical effect sizes), the heterogeneity test can be used (Hedges & Schauer, 2018). However, it is usually too strict to require exact replications in scientific practice, and therefore we can test whether the heterogeneity is negligible (approximate replication; Hedges & Schauer, 2018; Schauer & Hedges, 2020). “Small differences in the magnitude of effects may not lead to different interpretations of a finding”, so that the replications can be treated “almost the same” (Hedges & Schauer, 2018, p. 559). In this case, the heterogeneity magnitude test can be used.

Testing between-study heterogeneity is widely used in real data analysis. We reviewed studies published in the flagship journal *Psychological Bulletin*. Specifically, we searched for articles through PsycINFO using the keywords “meta-regression”, “mixed-effects model”, and “meta-analysis” from January 1, 2014 to March 15, 2019. The initial research returned 160 articles, 34 of which were identified as reply articles, comment articles, or erratum announcements. Those 34 articles were set aside for reference, bringing down the total number of articles to 126. Out of the 126 articles, no article only used fixed-effect model, 3 articles (2.4%) used both random-effects and fixed-effects, and the remaining 123 articles (97.6%) all chose the random-effects model or mixed-effects models. 77 articles out of the 126 articles (61.1%) reported the results of testing between-study heterogeneity (with no decline trend from 2014 to 2019), suggesting that the tests of between-study heterogeneity are widely used in real meta-analyses. Among the 77 articles, 75 used the Q test (Hedges & Olkin, 1985; Hedges, 1983), one used a likelihood ratio test, and one used a Wald test.

Despite the theoretical and practical significance of studying between-study heterogeneity, addressing the between-study heterogeneity is one of the most troublesome topics in meta-analysis research. There are three limitations. First, the existing methods, such as the Q test (Hedges & Olkin, 1985;

Hedges, 1983) and likelihood ratio test (Viechtbauer, 2007), are limited in testing for the homogeneity of effect sizes ($H_0 : \tau^2 = 0$) and they cannot test whether the heterogeneity is larger than a specific level (i.e., heterogeneity magnitude test). Second, the existing methods are criticized for their failure to control the Type I error rate and/or failure to attain enough statistical power (e.g., Borenstein et al., 2009a; Huedo-Medina et al., 2006). The main reason is that the reference distributions of the Q test and likelihood ratio test do not have exact analytic expression, and the asymptotic approximation requires a large sample size within each study. Although better reference distribution approximations have been proposed in the literature, the expression is “sufficiently complicated” (Hoaglin, 2016, p. 488). Third, few research has studied the performance of heterogeneity test in the mixed-effect meta-analysis. To overcome these limitations, we propose a bootstrap based heterogeneity test combining the likelihood ratio tests and Q test with bootstrap simulation. That is, we propose to obtain the critical value of the likelihood ratio test or Q test using a bootstrap simulation to construct an empirical reference distribution.

The outline of this paper is as follows: in Section 2, an overview of the meta-analysis models and effect sizes is provided. Section 3 presents several existing methods that test the between-study heterogeneity, some of which motivated the current study. In Section 4, we present the proposed bootstrap based heterogeneity test and a corresponding R package. In Section 5, the Type I error rate and statistical power of the bootstrap based heterogeneity test with different types of effect sizes are thoroughly examined via simulations. In Section 6, real data examples are provided to illustrate the bootstrap based heterogeneity test. We end the paper with some concluding remarks in Section 7.

2. Meta-Analysis Models and Effect Sizes

Suppose K unbiased effect size estimates x_j ($j = 1, \dots, K$) are included in a meta-analysis. A typical random-effects meta-analysis model (e.g., Hedges & Olkin, 1985) is given by

$$\begin{aligned} x_j &= \delta_j + e_j, e_j \sim N(0, \sigma_j^2) \\ \delta_j &= \mu_\delta + u_j, u_j \sim N(0, \tau^2) \end{aligned} \tag{1}$$

where e_j is the deviation of the observed study effect size x_j from the true/population study effect size δ_j and its variance σ_j^2 represents within-study sampling variability of study j , u_j is the deviation of the true study effect size δ_j from the true/population overall effect size μ_δ , and τ^2 represents between-study heterogeneity ($\tau^2 = 0$ in a fixed-effects meta-analysis model). The calculation of σ_j^2 varies depending on the type of effect size. If we incorporate study-level covariates (e.g., sampling and design characteristics), the between-study discrepancies could be explained by the covariates to some degree. This model is referred to as a mixed-effects model or a meta-regression model (Card, 2015; Hedges & Vevea, 1998). τ^2 in the mixed-effects meta-analysis is the between-study heterogeneity that cannot be explained by the study-level covariates. When all the between-study discrepancies can be explained by the covariates and hence $\tau^2 = 0$, the model becomes a fixed-effects meta-regression. Besides, fixed- and random-effects meta-analysis model and fixed- and random-effects meta-regression, Stanley & Doucouliagos (2015) and Stanley & Doucouliagos (2017) proposed an unrestricted weighted least squares (WLS). The WLS model is neither the fixed-effects model nor the random-effects model. It does not estimate τ^2 and assume the study variance v_j can be estimated up to some unknown multiplicative constant ϕ so that $v_j = \phi\sigma_j^2$. Stanley & Doucouliagos (2015) and Stanley & Doucouliagos (2017) found that the WLS model outperformed the conventional random-effects meta-analysis when there is publication bias and outperformed the fixed-effects model when there is heterogeneity.

In the current article, we focus on three types of effect sizes: standardized mean difference, Pearson's correlation, and odds ratio. When the effects are standardized mean differences, the effect size estimate is $g_j = \frac{\bar{y}_{1j} - \bar{y}_{2j}}{s_j}$, where $\bar{y}_{1j}$ and $\bar{y}_{2j}$ are the sample means of the two groups in study j , and $s_j = \sqrt{\frac{(n_{1j}-1)s_{1j}^2 + (n_{2j}-1)s_{2j}^2}{n_{1j} + n_{2j} - 2}}$ where s_{1j}^2 and s_{2j}^2 are the sample variances of the two groups of study j and n_{1j} and n_{2j} are the sample sizes of the two groups. However, g_j is biased especially when the per-study

sample size is small (Hedges, 1981; Hedges & Olkin, 1985). The unbiased effect size estimate is

$d_j = (1 - \frac{3}{4(n_{1j}+n_{2j})-9})g_j$ (Hedges, 1981).¹ Using this effect size, d_j asymptotically follows $N\left(\delta_j, \frac{n_{1j}+n_{2j}}{n_{1j}n_{2j}} + \frac{\delta_j^2}{2(n_{1j}+n_{2j})}\right)$ where δ_j is the study-specific true effect size (Hedges, 1981; Hedges & Olkin, 1985). In practice, we usually use d_j to replace δ_j to calculate the within-study sampling variance.

When the effects are Pearson correlations, we first transfer correlation in each study r_j to a Fisher's z score $z_{r,j}$ by $z_{r,j} = \frac{1}{2}\ln\left(\frac{1+r_j}{1-r_j}\right)$ (Hedges & Olkin, 1985). $z_{r,j}$ asymptotically follows $N\left(\delta_j, \frac{1}{n_j-3}\right)$ where n_j is the per-study sample size and δ_j is the study-specific true effect size.

When the effects are odds ratios, the effect size estimate is $o_j = \frac{n_{11j}}{n_{10j}} / \frac{n_{01j}}{n_{00j}} = \frac{n_{00j}n_{11j}}{n_{01j}n_{10j}}$ where n_{11j} is the number of participants with $Y = 1$ in Group 1, n_{10j} is the number of participants with $Y = 0$ in Group 1, n_{01j} is the number of participants with $Y = 1$ in Group 2, and n_{00j} is the number of participants with $Y = 0$ in Group 2. Since odds ratios cannot be negative and hence the distribution of sample estimates is skewed, odds ratios are usually transformed by natural logarithm (i.e., $\log(o_j)$; log odds ratios). The approximated standard error of the log odds ratio is $\hat{\sigma}_j = \sqrt{1/n_{00j} + 1/n_{10j} + 1/n_{01j} + 1/n_{11j}}$ (Morris & Gardner, 1988).

3. Heterogeneity Testing in Meta-Analysis

To test heterogeneity of effect sizes ($H_0 : \tau^2 = 0$ versus $H_0 : \tau^2 \neq 0$), various hypothesis tests have been proposed. We focus on the Q test, the improved Q test, likelihood ratio test, and Bootstrap methods in the main text. There are also other heterogeneity tests and we illustrate them in the supplemental materials. None of the illustrated tests were proposed or have been generalized to the heterogeneity magnitude test: $H_0 : \tau^2 = \lambda$ versus $H_a : \tau^2 > \lambda$. To provide an overall picture of different methods, the strengths and limitations of each method are presented in Table 1 and will be elaborated further in the following sections.

Q test

The Q test is historically the most widely used test. In our literature review on *Psychological Bulletin*, 75 articles out of 77 articles which tested the between-study heterogeneity used the Q test. The test statistic Q follows a χ^2 distribution with $K - 1$ degrees of freedom in the random-effects meta-analysis and with $K - P - 1$ (P is the number of covariates) degrees of freedom in the mixed-effects meta-analysis when the effect size is normally distributed and the sampling variance, σ_j^2 , is known (Hoaglin, 2016; Hedges, 1983; Hedges & Olkin, 1985). When σ_j^2 is unknown and a consistent estimator of σ_j^2 is used, Q asymptotically approximates the χ^2 distribution. The approximation is not accurate for small and medium study-level sample sizes (Kulinskaya et al., 2011a). The Type I error rate and power of the Q test have been widely studied with different types of effect size, and the unified conclusion is that the Type I error rate and power of the Q test are influenced by the number of studies and the study-level sample sizes (e.g., Chang, 1993; Harwell, 1997; Hedges & Olkin, 1985; S. B. Morris, 2000; Takkouche et al., 1999; Viechtbauer, 2007). More specifically, when the study-level sample sizes are small and the number of studies is large, the Type I error rate and power (usually not powerful enough) deviate from the theoretical values (e.g., Chang, 1993; Harwell, 1997; Hedges & Olkin, 1985; Viechtbauer, 2007). The direction of deviation depends on the type of effect size. Regardless of the type of effect size, with a large number of studies and larger study-level sample sizes, the Q test approximates the nominal α level (e.g., Harwell, 1997; S. B. Morris, 2000; Viechtbauer, 2007). Viechtbauer (2007) found that more studies, large study-level sample sizes, and larger τ^2 led to higher power across different types of effect size, however, Harwell (1997) found that power values decreased with more studies, especially for smaller study-level sample sizes for standardized mean difference. We present the strengths and limitations of the Q test in Table 1.

[Table 1]

Kulinskaya (2011a; 2011b; 2015) pointed out that there are better approximations than χ^2_{K-1} for the Q test in meta-analysis, and the approximated distribution of Q depends on the type of effect size.² When

effect sizes are standardized mean differences, Kulinskaya et al. (2011b) estimated the degrees of freedom of the χ^2 distribution using an approximation of $E(Q)$. However, the improved Q test needs the sample mean and sample variances information from each study, which may not be provided in meta-analysis. When effect sizes are risk differences and log odds ratios, Kulinskaya et al. (2011a) and Kulinskaya & Dollinger (2015) approximated the reference distribution of the Q test by matching Gamma distributions. Kulinskaya (2011a; 2011b; 2015) found that the Type I error rate and statistical power from the improved Q test is much better than the Q test. In order to achieve a high approximation accuracy, mathematical expressions from Kulinskaya (2011a; 2011b; 2015) are all quite complicated, and the generalization of Kulinskaya's approximation to other types of effect sizes needs to be resolved based on each specific effect size. As independent work, Breslow et al. (1991) proposed a χ^2 test for odds ratios (no logarithm transformation needed) and the reference distribution is χ^2_{K-1} . Interested readers can find more details in Takkouche et al. (1999), Viechtbauer (2007), and Viechtbauer et al. (2015).

Likelihood ratio (LR) test

The likelihood ratio (LR) test is not very widely used but frequently mentioned in the literature probably because it is relatively new (Takkouche et al., 1999; Viechtbauer, 2007). The LR test for the between-study heterogeneity is based on comparing two models: a full model in which τ^2 is freely estimated (the random-effects model or the mixed-effects model) and a reduced model in which $\tau^2 = 0$ (the fixed-effects model or the fixed-effect meta-regression). There are two maximum likelihood based estimators: the maximum likelihood (ML) estimator is based on the regular log-likelihood function and the restricted maximum likelihood (REML) estimator is based on the restricted log-likelihood function. Generally, REML is preferred over ML for variance component estimation.

There is a boundary (i.e., 0) in the variances' parameter space when only nonnegative variances are allowed (i.e., constrained estimation). This is the so-called boundary issue. If we allow negative variances

and the boundary issue does not exist (i.e., unconstrained estimation), the LR test follows a χ_1^2 distribution. With constrained estimation, the reference distribution is a 0.5:0.5 mixture of chi-squared distributions, and the critical value is 2.71 ($.5\chi_0^2 + .5\chi_1^2$ at $\alpha = 0.05$) (Stram & Lee, 1994, 1995; Stoel et al., 2006). Similar to the Q test, the simulation in Viechtbauer (2007) showed that the performance of ML based LR test and the REML based LR test also depended on the type of effect sizes, the number of studies, and the study-level sample sizes. He found that a large number of studies and large study-level sample sizes, especially the latter, improved the Type I error rate, except in the Fisher-transformed correlation case (Viechtbauer, 2007). But even with a large number of studies and large study-level sample sizes, the ML based LR test still could be too conservative. The REML based LR test was slightly better than the ML based LR test (Viechtbauer, 2007). The statistical power of the Q test, the ML based LR test, and the REML based LR test were very similar (Viechtbauer, 2007).

Bootstrap methods

Because the approximation of the LR test is sensitive to the number of studies and study-level sample sizes, when the number of studies is small and/or the study-level sample sizes are small, the reference distribution may be far different from a 0.5:0.5 mixture of χ_0^2 and χ_1^2 . Takkouche et al. (1999) proposed a parametric bootstrap version of the LR test. The procedure of testing the between-study heterogeneity in a random-effects model is as follows: (1) draw with replacement from the original K studies; (2) estimate μ_δ and the sampling variances, σ_j^2 , for each study within each bootstrap sample; (3) simulate K observed study effect sizes using a fixed-effects model based on the estimated μ_δ and σ_j^2 s; (4) calculate the LR test statistic, LR^B ; (5) repeat steps 1 to 4 for B times; (6) the empirical p -value is calculated as the proportion of LR^B s larger than the LR in the original sample. However, there are two things unclear in Takkouche et al. (1999). First, they did not describe which model was used to obtain $\hat{\mu}_\delta$ (i.e., a fixed- or random-effects model). Second and more importantly, it is unclear which ML estimator was used and whether REML- or

ML based LR test was used. To the best of our knowledge, the ML and REML estimators were first proposed or clearly defined by Viechtbauer in 2005 (Viechtbauer, 2005), after the study of Takkouche et al. (1999). Takkouche et al. (1999) examined the performance of the regular LR test, the bootstrap based LR test, and the Q test with the odds ratio. They found that the Q test and the bootstrap based LR test appropriately controlled the Type I error rate, but the regular LR test was too conservative. The power of the Q test and the bootstrap based LR test were similar to each other, whereas the LR test had lower power. Sinha et al. (2012) and Van den Noortgate & Onghena (2003) had independent work on parametric bootstrap tests for testing the between-study heterogeneity. Their procedures were similar to that of Takkouche et al. (1999). We illustrate the details in the supplemental materials.

4. A Bootstrap Based Heterogeneity Test

Given the importance of testing the between-study heterogeneity, researchers have worked on proposing better heterogeneity tests. However, there are several limitations in the existing methods as summarized in Table 1. First, the existing methods focus on testing homogeneity ($\tau^2 = 0$ versus $\tau^2 \neq 0$). But none of the existing methods can test whether the heterogeneity is larger than a specific level ($\tau^2 = \lambda$ versus $\tau^2 > \lambda$), which is a quite important research question when researchers are interested in the heterogeneity magnitude or replication. More specifically, the reference distribution of the Q test when $\tau^2 \neq 0$ is a weighted linear combination of chi-square distributions and this reference distribution only can be approximated when the sampling variances are the same across studies (Hedges & Pigott, 2001). Hence, the regular Q test is not applicable in the heterogeneity magnitude test. The reference distribution of ML-LRT and REML-LRT (i.e., 0.5:0.5 mixture of chi-squared distributions) cannot accommodate inequality constraints like $\tau^2 > \lambda$. Second, despite the widespread use of different tests of homogeneity, the existing methods are criticized by their failure to control the Type I error rate and attain enough statistical power (e.g., Borenstein et al., 2009a; Huedo-Medina et al., 2006). Third, although Kulinskaya's

improved Q test perform better than the regular Q test, the improved approximations cannot handle effect size measures other than standardized mean difference, risk difference, and log odds ratio, and the mathematical expressions for the proposed approximated distributions are all quite complicated. Additionally, the improved Q test requires the raw data information for the standardized mean difference effect size, which may not be available in the literature. Fourth, the bootstrap version of the LR test in Takkouche et al. (1999) cannot be replicated given important details missing. In addition, the parametric bootstrap methods by Takkouche et al. (1999), Sinha et al. (2012), and Van den Noortgate & Onghena (2003) were only examined in small simulation studies. Fifth, the existing research of between-study heterogeneity tests mainly focuses on random-effects meta-analysis, and the mixed-effects meta-analysis does not get enough attention. In a mixed-effects model, the heterogeneity test detects the residual heterogeneity that is not accounted for by the moderators, which can be used to search for important moderators and evaluate more complex moderation hypothesis.

We now propose a parametric bootstrap based heterogeneity test. The basic procedure of our bootstrap method is to simulate the empirical reference distribution of a chosen test statistic (i.e., ML or REML based LR test or the Q test) to get a critical value for null hypothesis testing. This method can be used with or without covariates. When the bootstrap procedure is coupled with the ML estimator, the REML estimator, or the Q statistic, we refer to it as B-ML-LRT, B-REML-LRT, or B-Q, respectively. Different from Sinha et al. (2012) and Van den Noortgate & Onghena (2003), we simulate bootstrap samples based on effect size's asymptotic sampling distributions, whereas Sinha et al. (2012) and Van den Noortgate & Onghena (2003) simulated raw data first and calculate simulated effect sizes based on the simulated data.

The general procedure of testing τ^2 in a meta-analysis with K studies using the proposed method is as follows.

- (1) For the bootstrap based LR tests, we calculate the ML or REML based LR test statistic based on

the corresponding log-likelihood function in the original sample LR^O . For the bootstrap based Q test, we calculate the Q statistic in the original sample Q^O .

(2) We use the original sample sizes and covariate values, treat the REML estimates of μ_δ and the influence of covariates as the true parameters, and simulate K sample study effect sizes ($x_j^B; j \in [1, K]$) based on the null hypothesis (e.g., $H_0 : \tau^2 = 0.05$). The sampling variances used in simulation are the sampling variances in the original study.

(3) With the simulated effect size x_j^B , we calculate the ML or REML based LR test statistic LR^B and the Q statistic Q^B . Note that the heterogeneity magnitude test is a one-sided test ($\tau^2 = \lambda$ verses $\tau^2 > \lambda$), and therefore when $\hat{\tau}^2$ in the simulated data was smaller than λ , the difference between the log-likelihoods was counted as 0.³

(4) We repeat steps 2 and 3 B times.

(5) The critical value $LR^{critical}$ is the $(1 - \alpha)$ -quantile of the B LR^B s where α is the pre-specified Type I error rate, and the critical value $Q^{critical}$ is the $(1 - \alpha)$ -quantile of the B Q^B s.

(6) In the heterogeneity test, the null assumption is rejected if LR^O is larger than $LR^{critical}$ or Q^O is larger than $Q^{critical}$. In the heterogeneity magnitude test, the null assumption is rejected if LR^O is larger than $LR^{critical}$ or Q^O is larger than $Q^{critical}$, and $\hat{\tau}^2$ in the original data was larger than λ . When $\hat{\tau}^2$ in the original data was smaller than λ , we fail to reject the null assumption.

Note that the log-likelihood function should match the LR test. That is, when the ML estimator is used to estimate τ^2 , $LRT = -2 \left(L_{\hat{\mu}_\delta^{(ML)}, \tau^2=0} - L_{\hat{\mu}_\delta^{(ML)}, \hat{\tau}^2(ML)} \right)$ based on the regular log-likelihood function, and when the REML estimator is used to estimate τ^2 , $LRT = -2 \left(L_{\tau^2=0}^{restricted} - L_{\hat{\tau}^2(REML)}^{restricted} \right)$ based on the restricted log-likelihood function.

R package for Implementing the Bootstrap Based Between-Study Heterogeneity Test

We provide an R package for the bootstrap based between-study heterogeneity test, `boot.heterogeneity`. This package can be downloaded from GitHub and CRAN (The Comprehensive R Archive Network). The code, example illustrations, and the help manual are in the supplemental materials. `boot.heterogeneity` builds on the `metafor` package by Viechtbauer (2010). It implements the heterogeneity test for the standardized mean difference, Fisher-transformed Pearson correlation, and log odds ratio. For example, `boot.d` is a function for testing the between-study heterogeneity with the standardized mean difference.

```
boot.d(n1, n2, est, model = "random", mods = NULL, nrep = 104, p_cut = 0.05, adjust = FALSE, lambda = 0)
```

`est` reads in a vector of unbiased estimates of standardized mean differences from individual studies. If the biased estimates g_j are read in for `est`, `adjust = TRUE` must be specified to obtain the corresponding unbiased standardized mean difference estimates d_j . `n1` and `n2` read in two vectors of sample sizes from group 1 and group 2 in each of the included studies, respectively. `model` defines a random- or mixed-effects model. `mods` can read in one or more moderators in the model. `nrep` defines the number of replications used in bootstrap simulations and the default value is 10^4 . `p_cut` defines the alpha level and the default value is 0.05. `lambda` defines the null hypothesis ($H_0 : \tau^2 = \lambda$). Bootstrap based tests of the between-study heterogeneity for Fisher-transformed Pearson correlations and log odds ratios are implemented in functions `boot.fcor()` and `boot.lnOR()`, respectively.

Each function for testing the between-study heterogeneity provides the testing results from the bootstrap based REML LR test (B-REML-LRT), the bootstrap based Q test (B-Q), and the Q test. In addition, for odds ratio, the standard error will be infinite if any one of the four cells is zero. In this case, Haldane and Anscombe's correction is used by adding 0.5 to each cell value automatically (Anscombe, 1956; Haldane, 1940).

5. Simulation studies

5.1 Simulation Design

Five factors that have been found to have an impact on the performance of the between-study heterogeneity tests are manipulated in the simulation study: (1) the type of effect size (standardized mean difference, Fisher-transformed Pearson correlation, or log odds ratio), (2) the number of studies, ($K=10, 20, 30, 50, 100$, or 300), (3) the per-group study-level sample size ($SZ = 24, 91$, or 370), (4) the size of the between-study heterogeneity τ^2 , and (5) the number of the study-level covariates (1 or 3) in the mixed-effects model. The overall effect size μ_δ was not manipulated because previous research has found that its influence on the performance of the between-study heterogeneity tests is negligible (e.g., Takkouche et al., 1999; Viechtbauer, 2005). Our pilot simulation results confirmed this. Please see the supplemental materials for more details. Therefore, we specified $\mu_\delta = 0$ in the simulation.

Specifically, sample sizes were chosen based on three real meta-analyses on standardized mean differences in order to mimic the realistic setting: a 55-study meta-analysis with the per-group sample sizes ranging from 5 to 83 and a median of 24 (Deci et al., 1999), a 33-study meta-analysis with the per-group sample sizes ranging from 10 to 489 and a median of 91 (Becker, 1986), and a 186-study meta-analysis with the per-group sample sizes ranging from 12 to 96267 and a median of 370 (Hyde et al., 1990). These chosen study-level sample sizes and number of studies are also representative values based on our literature review of the 126 studies in *Psychological Bulletin*.

The size of the between-study heterogeneity τ^2 is determined in the following way. Pigott (2012) provided benchmarks for the size of a relative heterogeneity index I^2 (the proportion of total variation in the observed effect sizes that is due to the between-study heterogeneity Higgins et al., 2002): small (25%), medium (50%) and large (75%). We first calculated the average within-study variance per sample size set for each type of effect, and we compute τ^2 based on the within-study variance and the above-mentioned benchmarks. Hence, the size of the between-study heterogeneity is different for different types of effect

size. The condition of $SZ=370$ was not considered because τ^2 was too small (<0.001). We illustrate different levels of τ^2 in Table 2. Additionally, we checked heterogeneity review papers to verify the appropriateness of τ^2 's specification (Rhodes et al., 2015 and van Erp et al., 2017 for the standardized mean difference; Turner et al., 2015 and Gunhan et al., 2020 for the log odds ratio; van Erp et al., 2017 for the correlation).

[Table 2]

In the mixed-effects meta-analyses, the study-level covariates were simulated from a normal distribution $N(0, 1)$ and the regression coefficients for study-level covariates, β , were specified to 0.5. The number of replications was 1000 for each condition. The study specific true effect size was simulated by $\delta_j \sim N(\mu_\delta, \tau^2)$ without covariates or $\delta_j \sim N(\mu_\delta + \beta_1 Z_{1j} + \dots + \beta_P Z_{Pj}, \tau^2)$ with covariates. The number of bootstrap samples, M , was set to 10,000 to find the critical value of the LR test or Q test.⁴ We consider a Type I error rate between 0.025 and 0.075 as satisfactory (Bradley, 1978).

In the simulation of standardized mean differences, we randomly drew K sample sizes from the aforementioned three real meta-analysis datasets. Based on each simulated δ_j , the raw data were simulated as $y_{1ij} \sim N(0, 1)$ and $y_{2ij} \sim N(\delta_j, 1)$ where i indicates the i th individual and j indicates the j th study. Based on the sample means and sample variances of each study, we calculated the observed standardized mean differences.

In the simulation of Fisher-transformed Pearson correlations, we randomly drew K per-group sample sizes from one of the two groups in the real meta-analysis data. The simulated study specific true Fisher-transformed Pearson correlations δ_j were converted to Pearson correlations ρ_j . For each simulated study, the raw data were simulated as $\begin{pmatrix} y_{1ij} \\ y_{2ij} \end{pmatrix} \sim MV \left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} 1 & \rho_j \\ \rho_j & 1 \end{pmatrix} \right)$. Based on the simulated raw data, we calculated the observed Fisher-transformed Pearson correlations r_j .

In the simulation of log odds ratio, we used the total sample sizes of the two groups in the real meta-analysis datasets as the total sample sizes of the odds ratios (N_j). We randomly drew n_{00j} and n_{01j} ,

and calculated n_{10j} and n_{11j} based on the simulated study specific true log odds ratio (δ_j). The current n_{00j} , n_{10j} , n_{01j} , and n_{11j} were the true cell sizes without sampling error. Accordingly, the estimated proportions of the four cells $p_{00j} = \frac{n_{00j}}{N_j}$, $p_{01j} = \frac{n_{01j}}{N_j}$, $p_{10j} = \frac{n_{10j}}{N_j}$, and $p_{11j} = \frac{n_{11j}}{N_j}$ were calculated. We used binomial distributions to simulate the raw data, add sampling error, and update n_{00j} , n_{10j} , n_{01j} , and n_{11j} : $n_{01j.update} \sim \text{binomial}\left(n_{00j} + n_{01j}, \frac{n_{01j}}{N_j}\right)$, $n_{00j.update} = n_{00j} + n_{01j} - n_{01j.update}$, $n_{11j.update} \sim \text{binomial}\left(n_{10j} + n_{11j}, \frac{n_{11j}}{N_j}\right)$, and $n_{10j.update} = n_{10j} + n_{11j} - n_{11j.update}$. The updated cell sizes contained sampling errors. Based on the simulated raw data, we calculated the observed log odds ratios.

We considered the bootstrap based ML LR test (B-ML-LRT), the bootstrap based REML LR test (B-REML-LRT), and the bootstrap based Q test (B-Q). The proposed bootstrap based methods were compared with the regular LR tests (ML-LRT and REML-LRT), the Q test, the improved Q test by Kulinskaya (2011a; 2011b; 2015) (Q_2 was for the standardized mean difference and log odds ratio only), and the Breslow-Day's test by Breslow et al. (1991) (BD was for the odds ratio only).

As pilot studies, we have tried some other tests. The first test was the bootstrap based ML and REML LR tests with unconstrained estimation of τ^2 . Although unconstrained estimation should avoid the boundary issue, this method failed to control the Type I error rates. It could lead to too small or too large Type I error rates, especially when the study-level sample sizes were large. Second, we examined the Type I error rates of using confidence intervals of the heterogeneity indexes I^2 and H coupled with the different estimators of τ^2 . The Type I error rates were too small across all combinations. Please see the supplemental materials for more details.

5.2 Type I error rates

5.2.1 Type I error rates in meta-analyses for heterogeneity test. In this section, we evaluate the performance of the heterogeneity tests when the true size of heterogeneity is zero, i.e., $\tau^2 = 0$. The Type I

error rates of the bootstrap based ML LR test (Q-ML-LRT), the bootstrap based REML LR test (Q-REML-LRT), the bootstrap based Q test (B-Q), the improved Q test by Kulinskaya & Dollinger (2015) (Q_2), and the Breslow-Day's test by Breslow et al. (1991) for odds ratios (BD), the regular ML based LR test (ML-LRT), the regular REML based LR test (REML-LRT), and the regular Q test are presented in Table 3 with their corresponding Monte Carlo standard errors. ML-LRT produced too conservative Type I error rates in most conditions. Consistent with the results from Viechtbauer (2007), more studies and larger study-level sample sizes improved the Type I error rates of ML-LRT, but even with a relatively large number of studies and study-level sample sizes (e.g., $SZ = 370$ and $K = 50$), ML-LRT was too conservative in the examined conditions. REML-LRT performed better than ML-LRT, but it also could provide too conservative Type I error rates, especially with the log odds ratio. The Q test could fail to control the Type I error rates when the study-level sample sizes were small ($SZ = 24$). Especially in the case of log odds ratio, with small study-level sample sizes, large number of studies failed to boost the Type I error rates of the Q test; in contrast, more studies tended to decrease the Type I error rates. The improved Q test appropriately controlled the Type I error rates across different conditions of study-level sample size, number of studies, and type of effect size. The Breslow-Day's test also could appropriately control the Type I error rates for the odds ratio. The two bootstrap LR methods, B-ML-LRT and B-REML-LRT, could appropriately control the Type I error rates regardless of the type of effect size, the number of studies, and the study-level sample sizes, except when the effect size was the log odds ratio and the study-level sample sizes were small ($SZ = 24$). Their performances were similar. The bootstrap based Q test (B-Q) could appropriately control the Type I error rates across all examined conditions. Comparing to the Monte Carlo standard errors which were smaller than 0.01, the methods that could control the Type I error rates and the methods that failed to control the Type I error rates yielded quite different Type I error rates. The difference between their Type I error rates should not be just due to random errors.

[Table 3]

5.2.2 Type I error rates in mixed-effects models for heterogeneity test. Here we evaluate the performance of heterogeneity tests when the true size of residual heterogeneity is zero, i.e., $\tau^2 = 0$. The Type I error rates of B-ML-LRT, B-REML-LRT, B-Q, ML-LRT, REML-LRT, and the Q test (Q) with one and three covariates are presented in Tables 4 and 5 with their corresponding Monte Carlo standard errors. The improved Q test and Breslow-Day's test are not applicable in mixed-effects models. Similar to the model without covariates, ML-LRT was too conservative in most examined conditions, regardless of which type of effect size was of interest and the number of covariates. REML-LRT and the Q test also could be too conservative, especially when the effect size of interest was the log odds ratio. A larger number of studies could aggravate the conservative Type I error rates of ML-LRT, REML-LRT, and the Q test when the effect size was the log odds ratio and the study-level sample sizes were small ($SZ = 24$). B-REML-LRT generally performed well, except when the study-level sample sizes were small ($SZ = 24$) and the effect size was the log odds ratio. B-Q outperformed B-ML-LRT and B-REML-LRT, and appropriately controlled the Type I error rates regardless of the type of effect size, the number of studies, and the study-level sample sizes. Unlike the cases without covariates, B-ML-LRT could fail to control the Type I error rates when the study-level sample sizes were large ($SZ = 370$). Since the Monte Carlo standard errors were all smaller than 0.01, the difference between the methods that could control the Type I error rates and the methods that failed to control the Type I error rates was not due to random errors.

[Table 4]

[Table 5]

5.2.3 Type I error rates in meta-analysis for heterogeneity magnitude test. We explore the Type I error rates of the proposed methods in testing whether the heterogeneity is larger than a specific level ($\tau^2 = \lambda$ versus $\tau^2 > \lambda$) in this section. The null hypothesis was $H_0 : \tau^2 = \lambda$ and λ was specified as the small level of heterogeneity in Table 2 to mimic real data. The Type I error rates of B-ML-LRT, B-REML-LRT, and B-Q are presented in Table 6 with their corresponding Monte Carlo standard errors.

B-ML-LRT and B-REML-LRT could appropriately control the Type I error rates, except when the study-level sample sizes were small ($SZ = 24$) and the effect size was the log odds ratio. B-Q could appropriately control the Type I error rates across all conditions.

[Table 6]

5.2.4 Summary of the performance of the tests on Type I error rates. In sum, across different conditions and types of effect sizes, although the performance of REML-LRT and Q test was better than ML-LRT, the three methods could be too conservative in the examined conditions and they are not applicable in the heterogeneity magnitude test. Without covariates, the improved Q test and the Breslow-Day's test could appropriately control the Type I error rates in the heterogeneity test but they are not applicable in the heterogeneity magnitude test. The bootstrap methods are applicable in both the heterogeneity test and the heterogeneity magnitude test. Among the three bootstrap methods, B-Q was superior to the other two, and B-REML-LRT could appropriately control Type I error rates except when the study-level sample sizes were small ($SZ = 24$) and the effect size was the log odds ratio. B-Q was competitive with the improved Q test and the Breslow-Day test when the improved Q test and the Breslow-Day test were applicable.

5.3 Statistical power

REML-LRT and ML-LRT will not be discussed further due to their failure to control Type I error rates. We varied the between-study heterogeneity (τ^2) as mentioned above (see Table 2 for the small, medium, and large levels of heterogeneity) and considered the same simulation conditions as in examining Type I error rates, except the cases where the power values from B-ML-LRT, B-REML-LRT, B-Q, the improved Q test, the Breslow-Day test, and the Q test were all nearly 1.

5.3.1 Statistical power in meta-analyses for heterogeneity test. In random-effects meta-analyses, the statistical power values of B-ML-LRT, B-REML-LRT, B-Q, the improved Q test (Q_2), the Breslow-Day

test, and the Q test are presented in Table 7, with the highest power values under each condition bolded and the Monte Carlo standard errors inside the parentheses. More studies, larger study-level sample sizes, and/or larger τ^2 increased power, which supported the conclusion from Viechtbauer (2007), but not Harwell (1997). As a parametric method, the improved Q test was more powerful than B-REML-LRT and B-ML-LRT in a number of conditions. In terms of the proposed bootstrap based methods, when the effect of interest was the standardized mean difference, compared to the Q test, the maximum power increments of B-ML-LRT, B-REML-LRT, and B-Q compared to the regular Q test were 0.117, 0.119, and 0.029 respectively, which were much larger than the Monte Carlo standard errors. The maximum percentage increments of B-ML-LRT, B-REML-LRT, and B-Q was 34.0%, 31.6%, and 7.5% respectively. When the effect size was the Fisher-transformed Pearson correlation, the maximum power increments of B-ML-LRT, B-REML-LRT, and B-Q were 0.076, 0.076, and 0.003 respectively and the maximum percentage increments of B-ML-LRT, B-REML-LRT, and B-Q was 17.6%, 17.6%, and 1% respectively. B-Q barely boosted power, whereas B-ML-LRT and B-REML-LRT boosted power based on the increased percentages and Monte Carlo standard errors. When the effect size was the log odds ratio, the maximum power increments of B-ML-LRT, B-REML-LRT, and B-Q were 0.148, 0.149, and 0.203 respectively, which were much larger than the Monte Carlo standard errors, and the maximum percentage increments of B-ML-LRT, B-REML-LRT, and B-Q were 67.1%, 66.4%, and 81.9% respectively.

[Table 7]

5.3.2 Statistical power in mixed-effects models for heterogeneity test. In mixed-effects meta-analyses, the statistical power values of B-ML-LRT, B-REML-LRT, B-Q, and the Q test with one covariate are presented in Table 8. The highest power values under each condition are bolded and the Monte Carlo standard errors are presented inside the parentheses. Similar to the cases without covariates, more studies, larger study-level sample sizes, and/or larger τ^2 produced higher power. In terms of the proposed bootstrap based methods, when the effect of interest was the standardized mean difference,

compared to the Q test, the maximum power increments of B-ML-LRT, B-REML-LRT, and B-Q compared to the Q test were 0.098, 0.098, and 0.082 respectively, and the maximum percentage increments of B-ML-LRT, B-REML-LRT, and B-Q were 25.8%, 25.8%, and 15.7% respectively. When the effect size was the Fisher-transformed Pearson correlation, the maximum power increments of B-ML-LRT, B-REML-LRT, and B-Q were 0.108, 0.108, and 0.004 respectively and the maximum percentage increments of B-ML-LRT, B-REML-LRT, and B-Q were 23.8%, 23.8%, and 1.7% respectively. When the effect sizes was the log odds ratio, the maximum power increments of B-ML-LRT, B-REML-LRT, and B-Q were 0.142, 0.142, and 0.193 respectively, and the maximum percentage increments of B-ML-LRT, B-REML-LRT, and B-Q were 65.1%, 65.1%, and 88.5% respectively. Based on both the percentage increments and Monte Carlo standard errors, the bootstrap methods increased power compared to the regular Q test except the case of B-Q with the Fisher-transformed Pearson correlation.

[Table 8]

5.3.3 Statistical power in meta-analysis for heterogeneity magnitude test. τ^2 under the null hypothesis was specified as the small level of heterogeneity in Table 2 and the true τ^2 which generated data was specified as the medium level of heterogeneity. We explore the power of the proposed methods in testing whether the heterogeneity is larger than a specific level ($\tau^2 = \lambda$ verses $\tau^2 > \lambda$) in this section. The power values are presented in Table 9 with the highest power values under each condition bolded and the Monte Carlo standard errors inside the parentheses. Based on Monte Carlo standard errors, different bootstrap methods did not differ noticeably.

[Table 9]

5.3.3 Summary of the performance of the tests on statistical power. In sum, regardless of the method and type of effect size, more studies, larger study-level sample sizes, and/or larger τ^2 provided higher power. The bootstrap based methods boosted power compared to the regular Q test. The increments of power were the highest when the log odds ratio was of interest. The improved Q test demonstrated the

highest power in a number of conditions when it was applicable.

6. Empirical Illustration

We applied the proposed bootstrap based heterogeneity test in three real meta-analyses. Specifically, we considered the bootstrap based REML LR test (B-REML-LRT) and bootstrap based Q test (B-Q) which generally appropriately controlled Type I error rates. Datasets used in the three examples along with the R codes are provided in the supplemental materials. The first meta-analysis consists of 13 studies that studied the correlation between sensation seeking scores and levels of monoamine oxidase (Zuckerman, 1994). The sample sizes range from 10 to 125 with a median of 40. In this example, the REML estimate of the overall Fisher-transformed Pearson correlation is -0.26 and the REML estimate of the between-study heterogeneity is 0.03. The Q test result is $Q(df = 12) = 29.06$, $p = 0.004$. Similarly, B-REML-LRT and B-Q also reject the assumption of homogeneity with $p = 0.004$ and 0.002 respectively when $\alpha = 0.05$.

The second meta-analysis consists of 18 studies in which the effect of open versus traditional education on students' self-concept was studied (Hedges et al., 1981). The per-group sample sizes range from 13.5 to 140 with a median of 64.5. In this example, the REML estimate of the overall standardized mean difference is 0.01 and the REML estimate of the between-study heterogeneity is 0.02. The Q test result is $Q(df = 17) = 23.39$, $p = 0.137$, and thus the test fails to reject the assumption of homogeneity when $\alpha = 0.05$ or 0.1. B-REML-LRT and B-Q fail to reject the assumption of homogeneity when $\alpha = 0.05$ with $p = 0.053$. The improved Q test is not applicable because the sample mean and sample variance information is not available in this meta-analysis.

The third meta-analysis consists of 26 studies on nicotine replacement therapy (nicotine chewing gum) for smoking cessation (Silagy et al., 2004). The numbers of participants in the control group and treatment group (i.e., nicotine replacement therapy) who stopped smoking for at least 6 months after treatment were recorded. The total sample sizes for the control and treatment groups range from 47 to 1217

with a median of 175. The REML estimate of the overall log odds ratio is 0.56 and the REML estimate of the between-study heterogeneity is 0.05. The Q test result is $Q(df = 25) = 34.87$, $p = 0.091$, and thus the test fails to reject the assumption of homogeneity when $\alpha = 0.05$. The improved Q test and B-Q also fail to reject the assumption of homogeneity with $p = 0.073$ and 0.088 , respectively. In contrast, B-REML-LRT rejects the assumption of homogeneity with $p = 0.037$. This real data analysis result is consistent with the simulation results: when the effect size was the log odds ratio and $SZ = 91$, B-REML-LRT was the most powerful method.

7. Discussion

Testing the between-study heterogeneity is the research of interest in some studies and widely used in practice. We considered two types of between-study heterogeneity tests. In the heterogeneity test, we test $\tau^2 = 0$ versus $\tau^2 \neq 0$. In the heterogeneity magnitude test, we test $\tau^2 = \lambda$ versus $\tau^2 > \lambda$. The heterogeneity test can be viewed as the a special case of heterogeneity magnitude test with $\lambda = 0$. Researchers may be interested in the heterogeneity test and the heterogeneity magnitude test for three reasons. First, with a large between-study heterogeneity, interpreting study-specific effect size is more practically meaningful than the overall effect size. Second, researchers may also be interested in exploring factors that can explain the systematic between-study heterogeneity. The relevant test can be viewed as the goodness of fit test. If the unexplained heterogeneity in mixed-effects model is significantly, we can consider interactive combinations of moderators by creating product terms. Third, in replication research, the heterogeneity test can be used in exact replication test and the heterogeneity magnitude test can be used in approximate replication test. For both heterogeneity test and heterogeneity magnitude test, failure of controlling Type I error rates and low power will lead to misleading testing results. Hence, the goal of the current student is to propose a powerful and reliable test for both hypothesis testings.

The existing methods, such as the Q test (Hedges & Olkin, 1985; Hedges, 1983) and likelihood ratio

test (Viechtbauer, 2007), are criticized by their failure to control the Type I error rate and/or failure to attain enough statistical power. Kulinskaya's improved Q test, as a more recently proposed test could better control Type I error rates and improve power, but so far it is limited to standardized mean differences, risk differences, and log odds ratios. Additionally, few studies have examined the performance of the heterogeneity test in the mixed-effect meta-analysis and the R functions for the improved Q test cannot accommodate covariates yet. To the best of our knowledge, no existing methods can handle heterogeneity magnitude test so far. Consistent with the findings from Harwell (1997), Hedges & Olkin (1985), and Viechtbauer (2007), we found that the regular ML based LR test (ML-LRT), the regular REML based LR test (REML-LRT), and the Q test could be too conservative with the examined types of effect sizes. We have summarized the strengths and limitations of the existing methods in Table 1.

We propose a bootstrap based heterogeneity test combining the likelihood ratio test (ML- and REML based) and the Q test with parametric bootstrap procedures. The proposed method has several advantages over the existing approaches. First, the proposed bootstrap based heterogeneity test can be applied to heterogeneity magnitude test while other methods cannot. Second, based on our simulation results, B-REML-LRT and B-Q outperformed ML-LRT, REML-LRT, and the Q test in terms of controlling the Type I error rate. B-REML-LRT and B-Q appropriately controlled the Type I error rate and were competitive with the improved Q test, except a few cases (B-REML-LRT was too conservative when the study-level sample sizes were small and the effect size was the log odds ratio).

Third, the proposed bootstrap based heterogeneity test increased statistical power compared to the Q test, especially when the study-level sample sizes were small. The increment in power was the largest with the log odds ratio. As a parametric method, the improved Q test had the highest power in a number of conditions when it was applicable. We also found that for both the bootstrap methods and Q test, more studies, larger study-level sample sizes, and/or larger τ^2 provided higher power.

Another strength of the proposed method is its flexibility. Unlike the improved Q test which requests

a sophisticated analytical form for each type of effect size and the information of raw data, the bootstrap based heterogeneity test can be easily extended to cases where effect sizes are not the types of effect size discussed in the current paper, such as standardized mean change. The only place that would need modification is the analytical form of the sampling distribution. We have summarized the strengths and limitations of the proposed bootstrap methods in Table 1.

Overall, based on both the Type I error rate and statistical power, we recommend B-Q for the heterogeneity test and heterogeneity magnitude test. If one wants to pursue higher power, B-REML-LRT is also recommended as long as study-level sample sizes are not too small. One also can consider the improved Q test when it is applicable. Future work could look into developing the bootstrap based heterogeneity test for multivariate meta-analysis. The generalization is feasible when the analytical form of the sampling distribution of multivariate meta-analysis is given. Furthermore, published studies may not be truly representative of all valid studies undertaken on a research topic which is known as publication bias or the file-drawer problem (Hunter & Schmidt, 2004; Iyengar & Greenhouse, 1988; Rosenthal, 1979). Conclusions based on the meta-analyses of published studies without correcting for publication bias compromise the validity of a systematic review (Du et al., 2017; Klein et al., 2018; Kvarven et al., 2020; Collaboration et al., 2015; Sutton et al., 2000). Future research should look into developing the bootstrap based heterogeneity test while reducing the bias caused by the file-drawer problem.

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Footnotes

¹We used the notations in the original work of Hedges (1981) and Hedges & Olkin (1985). The notations vary across different researchers. For example, Borenstein et al. (2009b) called the biased estimate the Cohen's d and unbiased estimate the Hedge's g .

²The improved approximations were proposed in the cases where the effects are not normally distributed (but can be asymptotically normally distributed) and the estimators of the weights are not independent of the estimators of the effects. But Kulinskaya still assumed that raw data are normally distributed.

³When the effect size is the log odds ratio, the effect size is a function of four cell sizes. The four cell sizes in the original data will not be consistent with the simulated x_j^B , therefore there is one more step. We keep three of the simulated cell sizes n_{00j}^B , n_{10j}^B , n_{11j}^B and n_{01j}^B the same as n_{00j} , n_{10j} , n_{11j} and n_{01j} in the original data, and the fourth one is updated based on x_j^B . For example, we fix n_{00j}^B , n_{10j}^B , and n_{11j}^B to be the same as n_{00j} , n_{10j} and n_{11j} , n_{01j}^B is calculated as $\frac{n_{11j}^B n_{00j}^B}{n_{10j}^B \exp(x_j^B)}$. Each cell size has a 25% chance to be calculated across the permutations. Based on the calculated fourth cell size, the sampling variances of simulated studies are updated, and then we calculate the ML- or REML based LR test statistic and the Q statistic.

⁴In our simulation and real data analyses, we found 10^4 bootstrap samples provided relatively stable critical values.

Table 1: Summary of the methods

Method	Strengths	Limitations
Regular Q test	Widely used in different software and R packages	Type I error rates are not appropriately controlled; Power can be low; Cannot conduct heterogeneity magnitude test
Kulinskaya's improved Q test (Q_2)	Appropriately control Type I error rates; High power; R package available	Require raw data information such as sample means and sample standard deviations; Complicated mathematical expressions; Cannot handle effect size measures other than standardized mean difference, risk difference, and log odds ratio so far; Cannot incorporate covariates; Cannot conduct heterogeneity magnitude test
Breslow-Day's test	Appropriately control Type I error rates; High power R package available	Cannot handle effect size measures other than odds ratio; Cannot incorporate covariates; Cannot conduct heterogeneity magnitude test
ML- and REML based LR tests	R package available	Type I error rates are not appropriately controlled; Power can be low; Cannot conduct heterogeneity magnitude test
Takkouche's, Van den Noortgate-Onghena, and Sinha's bootstrap tests	Appropriately control Type I error rates in the small scale simulations	Cannot be replicated given important details missing; Performance has not been examined by large scale simulations; Cannot incorporate covariates; Cannot conduct heterogeneity magnitude test
Bootstrap based ML LR test (B-ML-LRT), Bootstrap based REML LR test (B-REML-LRT)	Control Type I error rates relatively well; Higher power compared to the regular Q test Can incorporate covariates; Can be used in heterogeneity magnitude test; Can be easily generalized to other types of effect sizes; R package available	Can fail to control Type I error rates when the study-level sample sizes were small ($SZ = 24$) and the effect size was the log odds ratio; Can be less powerful than the improved Q test B-REML-LRT performs slightly better than B-ML-LRT
Bootstrap based Q test (B-Q)	Appropriately control Type I error rates Higher power compared to the regular Q test Can incorporate covariates; Can be used in heterogeneity magnitude test; Can be easily generalized to other types of effect sizes; R package available	Can be less powerful than B-REML-LRT and the improved Q test

Table 2: Levels of heterogeneity of τ^2

	Sample size (SZ)	Levels of heterogeneity		
		Small	Medium	Large
Standardized mean difference	24	0.03	0.1	0.3
	91	0.006	0.02	0.05
Fisher-transformed Pearson correlation	24	0.01	0.03	0.1
	91	0.006	0.02	0.05
Log odds ratio	24	0.1	0.3	0.9
	91	0.03	0.1	0.3

Table 3: Type I error rates of the bootstrap based ML LR test (Q-ML-LRT), the bootstrap based REML LR test (Q-REML-LRT), the bootstrap based Q test (B-Q), the improved Q test (Q_2), the Breslow-Day's test (BD), the regular ML based LR test (ML-LRT), the regular REML based LR test (REML-LRT), and the regular Q test in meta-analyses

Sample size (SZ)	Number of Studies (K)	B-ML-LRT	B-REML-LRT	B-Q	Q_2	BD	ML-LRT	REML-LRT	Q
Standardized mean difference									
24	10	0.052 (0.007)	0.052 (0.007)	0.053 (0.007)	0.044 (0.006)	-	0.015 (0.004)	0.026 (0.005)	0.039 (0.006)
	20	0.052 (0.007)	0.052 (0.007)	0.055 (0.007)	0.051 (0.007)	-	0.013 (0.004)	0.022 (0.005)	0.041 (0.006)
	30	0.049 (0.007)	0.05 (0.007)	0.049 (0.007)	0.044 (0.006)	-	0.017 (0.004)	0.026 (0.005)	0.033 (0.006)
	50	0.062 (0.008)	0.062 (0.008)	0.06 (0.008)	0.055 (0.007)	-	0.028 (0.005)	0.034 (0.006)	0.042 (0.006)
	100	0.065 (0.008)	0.064 (0.008)	0.063 (0.008)	0.058 (0.007)	-	0.031 (0.005)	0.037 (0.006)	0.045 (0.007)
	300	0.058 (0.007)	0.058 (0.007)	0.057 (0.007)	0.058 (0.007)	-	0.019 (0.004)	0.022 (0.005)	0.028 (0.005)
91	10	0.054 (0.007)	0.056 (0.007)	0.049 (0.007)	0.046 (0.007)	-	0.012 (0.003)	0.032 (0.006)	0.046 (0.007)
	20	0.041 (0.006)	0.041 (0.006)	0.04 (0.006)	0.038 (0.006)	-	0.019 (0.004)	0.03 (0.005)	0.035 (0.006)
	30	0.051 (0.007)	0.052 (0.007)	0.053 (0.007)	0.048 (0.007)	-	0.03 (0.005)	0.038 (0.006)	0.045 (0.007)
	50	0.054 (0.007)	0.055 (0.007)	0.052 (0.007)	0.05 (0.007)	-	0.028 (0.005)	0.037 (0.006)	0.046 (0.007)
	100	0.055 (0.007)	0.056 (0.007)	0.052 (0.007)	0.049 (0.007)	-	0.03 (0.005)	0.035 (0.006)	0.046 (0.007)
	300	0.043 (0.006)	0.043 (0.006)	0.045 (0.007)	0.04 (0.006)	-	0.032 (0.006)	0.032 (0.006)	0.032 (0.006)
370	10	0.043 (0.006)	0.049 (0.007)	0.051 (0.007)	0.05 (0.007)	-	0.007 (0.003)	0.022 (0.005)	0.05 (0.007)
	20	0.048 (0.007)	0.051 (0.007)	0.041 (0.006)	0.04 (0.006)	-	0.017 (0.004)	0.032 (0.006)	0.04 (0.006)
	30	0.052 (0.007)	0.054 (0.007)	0.053 (0.007)	0.053 (0.007)	-	0.021 (0.005)	0.037 (0.006)	0.051 (0.007)
	50	0.053 (0.007)	0.055 (0.007)	0.048 (0.007)	0.047 (0.007)	-	0.021 (0.005)	0.04 (0.006)	0.047 (0.007)
	100	0.058 (0.007)	0.056 (0.007)	0.059 (0.007)	0.057 (0.007)	-	0.029 (0.005)	0.044 (0.006)	0.057 (0.007)
	300	0.055 (0.007)	0.054 (0.007)	0.051 (0.007)	0.049 (0.007)	-	0.037 (0.006)	0.046 (0.007)	0.046 (0.007)
Fisher-transformed Pearson correlation									
24	10	0.073 (0.008)	0.074 (0.008)	0.074 (0.008)	-	-	0.032 (0.006)	0.05 (0.007)	0.077 (0.008)
	20	0.054 (0.007)	0.053 (0.007)	0.053 (0.007)	-	-	0.029 (0.005)	0.043 (0.006)	0.053 (0.007)
	30	0.046 (0.007)	0.048 (0.007)	0.048 (0.007)	-	-	0.031 (0.005)	0.037 (0.006)	0.048 (0.007)
	50	0.06 (0.008)	0.061 (0.008)	0.064 (0.008)	-	-	0.043 (0.006)	0.057 (0.007)	0.063 (0.008)
	100	0.047 (0.007)	0.047 (0.007)	0.052 (0.007)	-	-	0.031 (0.005)	0.04 (0.006)	0.053 (0.007)
	300	0.056 (0.007)	0.056 (0.007)	0.043 (0.006)	-	-	0.044 (0.006)	0.051 (0.007)	0.043 (0.006)
91	10	0.053 (0.007)	0.053 (0.007)	0.05 (0.007)	-	-	0.024 (0.005)	0.038 (0.006)	0.049 (0.007)
	20	0.055 (0.007)	0.055 (0.007)	0.053 (0.007)	-	-	0.023 (0.005)	0.04 (0.006)	0.055 (0.007)
	30	0.038 (0.006)	0.037 (0.006)	0.047 (0.007)	-	-	0.024 (0.005)	0.032 (0.006)	0.047 (0.007)
	50	0.047 (0.007)	0.048 (0.007)	0.061 (0.008)	-	-	0.03 (0.005)	0.037 (0.006)	0.062 (0.008)
	100	0.046 (0.007)	0.046 (0.007)	0.046 (0.007)	-	-	0.041 (0.006)	0.045 (0.007)	0.044 (0.006)
	300	0.044 (0.006)	0.044 (0.006)	0.043 (0.006)	-	-	0.037 (0.006)	0.039 (0.006)	0.044 (0.006)
370	10	0.041 (0.006)	0.042 (0.006)	0.052 (0.007)	-	-	0.008 (0.003)	0.026 (0.005)	0.052 (0.007)
	20	0.049 (0.007)	0.045 (0.007)	0.047 (0.007)	-	-	0.016 (0.004)	0.026 (0.005)	0.049 (0.007)
	30	0.046 (0.007)	0.053 (0.007)	0.048 (0.007)	-	-	0.018 (0.004)	0.032 (0.006)	0.048 (0.007)
	50	0.05 (0.007)	0.052 (0.007)	0.049 (0.007)	-	-	0.022 (0.005)	0.034 (0.006)	0.048 (0.007)
	100	0.047 (0.007)	0.047 (0.007)	0.047 (0.007)	-	-	0.026 (0.005)	0.035 (0.006)	0.047 (0.007)
	300	0.048 (0.007)	0.047 (0.007)	0.056 (0.007)	-	-	0.031 (0.005)	0.037 (0.006)	0.055 (0.007)
Log odds ratio									
24	10	0.037 (0.006)	0.036 (0.006)	0.041 (0.006)	0.04 (0.006)	0.037 (0.006)	0.009 (0.003)	0.017 (0.004)	0.024 (0.005)
	20	0.03 (0.005)	0.03 (0.005)	0.036 (0.006)	0.042 (0.006)	0.03 (0.005)	0.003 (0.002)	0.005 (0.002)	0.008 (0.003)
	30	0.032 (0.006)	0.033 (0.006)	0.031 (0.005)	0.044 (0.006)	0.037 (0.006)	0.007 (0.003)	0.012 (0.003)	0.016 (0.004)
	50	0.027 (0.005)	0.028 (0.005)	0.039 (0.006)	0.039 (0.006)	0.037 (0.006)	0.007 (0.003)	0.008 (0.003)	0.011 (0.003)
	100	0.015 (0.004)	0.015 (0.004)	0.043 (0.006)	0.036 (0.006)	0.03 (0.005)	0.006 (0.002)	0.009 (0.003)	0.009 (0.003)
	300	0.012 (0.003)	0.012 (0.003)	0.04 (0.006)	0.034 (0.006)	0.035 (0.006)	0.001 (0.001)	0.001 (0.001)	0.002 (0.001)
91	10	0.039 (0.006)	0.039 (0.006)	0.045 (0.007)	0.05 (0.007)	0.05 (0.007)	0.015 (0.004)	0.028 (0.005)	0.039 (0.006)
	20	0.047 (0.007)	0.048 (0.007)	0.048 (0.007)	0.052 (0.007)	0.052 (0.007)	0.02 (0.004)	0.03 (0.005)	0.036 (0.006)
	30	0.041 (0.006)	0.039 (0.006)	0.042 (0.006)	0.041 (0.006)	0.043 (0.006)	0.019 (0.004)	0.023 (0.005)	0.032 (0.006)
	50	0.029 (0.005)	0.03 (0.005)	0.047 (0.007)	0.048 (0.007)	0.056 (0.007)	0.015 (0.004)	0.021 (0.005)	0.027 (0.005)
	100	0.038 (0.006)	0.039 (0.006)	0.058 (0.007)	0.053 (0.007)	0.056 (0.007)	0.02 (0.004)	0.022 (0.005)	0.034 (0.006)
	300	0.033 (0.006)	0.033 (0.006)	0.049 (0.007)	0.038 (0.006)	0.05 (0.007)	0.022 (0.005)	0.022 (0.005)	0.023 (0.005)
370	10	0.035 (0.006)	0.039 (0.006)	0.054 (0.007)	0.054 (0.007)	0.051 (0.007)	0.009 (0.003)	0.028 (0.005)	0.05 (0.007)
	20	0.033 (0.006)	0.036 (0.006)	0.045 (0.007)	0.046 (0.007)	0.045 (0.007)	0.011 (0.003)	0.021 (0.005)	0.044 (0.006)
	30	0.049 (0.007)	0.053 (0.007)	0.048 (0.007)	0.05 (0.007)	0.052 (0.007)	0.015 (0.004)	0.031 (0.005)	0.043 (0.006)
	50	0.051 (0.007)	0.049 (0.007)	0.034 (0.006)	0.035 (0.006)	0.035 (0.006)	0.019 (0.004)	0.034 (0.006)	0.033 (0.006)
	100	0.039 (0.006)	0.04 (0.006)	0.044 (0.006)	0.049 (0.007)	0.048 (0.007)	0.021 (0.005)	0.032 (0.006)	0.039 (0.006)
	300	0.054 (0.007)	0.054 (0.007)	0.053 (0.007)	0.055 (0.007)	0.054 (0.007)	0.035 (0.006)	0.043 (0.006)	0.042 (0.006)

Note: Monte Carlo standard errors are presented in the parentheses. All unacceptable Type I error rates (i.e., <0.025 or >0.075) are bold. ML or REML estimation sometimes did not have converged results, but the nonconvergence rates across all conditions were within 1%. “-” indicate not available.

Table 4: With one covariate, Type I error rates of the bootstrap based ML LR test (Q-ML-LRT), the bootstrap based REML LR test (Q-REML-LRT), the bootstrap based Q test (B-Q), the regular ML based LR test (ML-LRT), the regular REML based LR test (REML-LRT), and the regular Q test in mixed-effects models

Sample size (SZ)	Number of Studies (K)	B-ML-LRT	B-REML-LRT	B-Q	ML-LRT	REML-LRT	Q
Standardized mean difference							
24	10	0.063 (0.008)	0.059 (0.007)	0.059 (0.007)	0.015 (0.004)	0.046 (0.007)	0.059 (0.007)
	20	0.06 (0.008)	0.055 (0.007)	0.047 (0.007)	0.017 (0.004)	0.04 (0.006)	0.046 (0.007)
	30	0.051 (0.007)	0.05 (0.007)	0.05 (0.007)	0.026 (0.005)	0.042 (0.006)	0.049 (0.007)
	50	0.059 (0.007)	0.059 (0.007)	0.055 (0.007)	0.033 (0.006)	0.045 (0.007)	0.055 (0.007)
	100	0.053 (0.007)	0.052 (0.007)	0.043 (0.006)	0.036 (0.006)	0.045 (0.007)	0.043 (0.006)
	300	0.047 (0.007)	0.047 (0.007)	0.044 (0.006)	0.028 (0.005)	0.042 (0.006)	0.043 (0.006)
91	10	0.048 (0.007)	0.05 (0.007)	0.05 (0.007)	0.009 (0.003)	0.03 (0.005)	0.05 (0.007)
	20	0.047 (0.007)	0.044 (0.006)	0.039 (0.006)	0.016 (0.004)	0.032 (0.006)	0.036 (0.006)
	30	0.055 (0.007)	0.053 (0.007)	0.049 (0.007)	0.018 (0.004)	0.039 (0.006)	0.046 (0.007)
	50	0.059 (0.007)	0.059 (0.007)	0.052 (0.007)	0.026 (0.005)	0.046 (0.007)	0.044 (0.006)
	100	0.049 (0.007)	0.048 (0.007)	0.044 (0.006)	0.027 (0.005)	0.035 (0.006)	0.04 (0.006)
	300	0.044 (0.006)	0.044 (0.006)	0.034 (0.006)	0.033 (0.006)	0.042 (0.006)	0.034 (0.006)
370	10	0.03 (0.005)	0.053 (0.007)	0.051 (0.007)	0.006 (0.002)	0.032 (0.006)	0.048 (0.007)
	20	0.041 (0.006)	0.046 (0.007)	0.05 (0.007)	0.008 (0.003)	0.037 (0.006)	0.051 (0.007)
	30	0.058 (0.007)	0.053 (0.007)	0.064 (0.008)	0.012 (0.003)	0.035 (0.006)	0.063 (0.008)
	50	0.051 (0.007)	0.052 (0.007)	0.047 (0.007)	0.012 (0.003)	0.037 (0.006)	0.045 (0.007)
	100	0.05 (0.007)	0.05 (0.007)	0.051 (0.007)	0.025 (0.005)	0.039 (0.006)	0.051 (0.007)
	300	0.053 (0.007)	0.054 (0.007)	0.052 (0.007)	0.032 (0.006)	0.044 (0.006)	0.048 (0.007)
Fisher-transformed Pearson correlation							
24	10	0.063 (0.008)	0.06 (0.008)	0.058 (0.007)	0.015 (0.004)	0.046 (0.007)	0.059 (0.007)
	20	0.06 (0.008)	0.056 (0.007)	0.049 (0.007)	0.017 (0.004)	0.04 (0.006)	0.046 (0.007)
	30	0.048 (0.007)	0.048 (0.007)	0.05 (0.007)	0.026 (0.005)	0.042 (0.006)	0.049 (0.007)
	50	0.059 (0.007)	0.058 (0.007)	0.055 (0.007)	0.033 (0.006)	0.045 (0.007)	0.055 (0.007)
	100	0.053 (0.007)	0.053 (0.007)	0.042 (0.006)	0.036 (0.006)	0.045 (0.007)	0.043 (0.006)
	300	0.048 (0.007)	0.048 (0.007)	0.044 (0.006)	0.028 (0.005)	0.042 (0.006)	0.043 (0.006)
91	10	0.057 (0.007)	0.06 (0.008)	0.063 (0.008)	0.015 (0.004)	0.038 (0.006)	0.062 (0.008)
	20	0.051 (0.007)	0.051 (0.007)	0.056 (0.007)	0.016 (0.004)	0.038 (0.006)	0.054 (0.007)
	30	0.049 (0.007)	0.045 (0.007)	0.046 (0.007)	0.017 (0.004)	0.038 (0.006)	0.046 (0.007)
	50	0.052 (0.007)	0.053 (0.007)	0.054 (0.007)	0.024 (0.005)	0.045 (0.007)	0.056 (0.007)
	100	0.051 (0.007)	0.05 (0.007)	0.048 (0.007)	0.026 (0.005)	0.047 (0.007)	0.047 (0.007)
	300	0.044 (0.006)	0.044 (0.006)	0.033 (0.006)	0.033 (0.006)	0.042 (0.006)	0.034 (0.006)
370	10	0.032 (0.006)	0.051 (0.007)	0.06 (0.008)	0.004 (0.002)	0.033 (0.006)	0.06 (0.008)
	20	0.048 (0.007)	0.051 (0.007)	0.047 (0.007)	0.008 (0.003)	0.03 (0.005)	0.048 (0.007)
	30	0.048 (0.007)	0.047 (0.007)	0.055 (0.007)	0.007 (0.003)	0.031 (0.005)	0.057 (0.007)
	50	0.049 (0.007)	0.049 (0.007)	0.047 (0.007)	0.013 (0.004)	0.036 (0.006)	0.047 (0.007)
	100	0.049 (0.007)	0.048 (0.007)	0.044 (0.006)	0.017 (0.004)	0.037 (0.006)	0.047 (0.007)
	300	0.052 (0.007)	0.053 (0.007)	0.045 (0.007)	0.027 (0.005)	0.042 (0.006)	0.044 (0.006)
Log odds ratio							
24	10	0.033 (0.006)	0.033 (0.006)	0.04 (0.006)	0.005 (0.002)	0.013 (0.004)	0.018 (0.004)
	20	0.028 (0.005)	0.026 (0.005)	0.033 (0.006)	0.009 (0.003)	0.014 (0.004)	0.017 (0.004)
	30	0.03 (0.005)	0.031 (0.005)	0.045 (0.007)	0.008 (0.003)	0.014 (0.004)	0.012 (0.003)
	50	0.032 (0.006)	0.03 (0.005)	0.033 (0.006)	0.009 (0.003)	0.016 (0.004)	0.015 (0.004)
	100	0.017 (0.004)	0.016 (0.004)	0.034 (0.006)	0.004 (0.002)	0.007 (0.003)	0.007 (0.003)
	300	0.013 (0.004)	0.013 (0.004)	0.04 (0.006)	0 (0<0.001)	0.001 (0.001)	0.004 (0.002)
91	10	0.046 (0.007)	0.048 (0.007)	0.049 (0.007)	0.009 (0.003)	0.028 (0.005)	0.037 (0.006)
	20	0.04 (0.006)	0.041 (0.006)	0.046 (0.007)	0.01 (0.003)	0.02 (0.004)	0.036 (0.006)
	30	0.046 (0.007)	0.046 (0.007)	0.051 (0.007)	0.021 (0.005)	0.029 (0.005)	0.033 (0.006)
	50	0.044 (0.006)	0.041 (0.006)	0.044 (0.006)	0.011 (0.003)	0.02 (0.004)	0.024 (0.005)
	100	0.036 (0.006)	0.035 (0.006)	0.048 (0.007)	0.015 (0.004)	0.026 (0.005)	0.026 (0.005)
	300	0.047 (0.007)	0.046 (0.007)	0.051 (0.007)	0.024 (0.005)	0.032 (0.006)	0.025 (0.005)
370	10	0.023 (0.005)	0.042 (0.006)	0.039 (0.006)	0.002 (0.001)	0.02 (0.004)	0.036 (0.006)
	20	0.042 (0.006)	0.044 (0.006)	0.047 (0.007)	0.009 (0.003)	0.028 (0.005)	0.041 (0.006)
	30	0.04 (0.006)	0.047 (0.007)	0.05 (0.007)	0.01 (0.003)	0.03 (0.005)	0.045 (0.007)
	50	0.052 (0.007)	0.053 (0.007)	0.059 (0.007)	0.013 (0.004)	0.043 (0.006)	0.051 (0.007)
	100	0.045 (0.007)	0.044 (0.006)	0.044 (0.006)	0.012 (0.003)	0.029 (0.005)	0.036 (0.006)
	300	0.061 (0.008)	0.057 (0.007)	0.047 (0.007)	0.033 (0.006)	0.047 (0.007)	0.026 (0.005)

Note: Monte Carlo standard errors are presented in the parentheses. All unacceptable Type I error rates (i.e., <0.025 or >0.075) are bold. ML or

REML estimation sometimes did not have converged results, but the nonconvergence rates across all conditions were within 0.8%.

Table 5: With three covariates, Type I error rates of the bootstrap ML-based LR test (Q-ML-LRT), the bootstrap REML-based LR test (Q-REML-LRT), the bootstrap Q test (B-Q), the regular ML-based LR test (ML-LRT), the regular REML-based LR test (REML-LRT), and the regular Q test in mixed-effects models

Sample size (SZ)	Number of Studies (K)	B-ML-LRT	B-REML-LRT	B-Q	ML-LRT	REML-LRT	Q
Standardized mean difference							
24	10	0.052 (0.007)	0.058 (0.007)	0.054 (0.007)	0.002 (0.001)	0.032 (0.006)	0.049 (0.007)
	20	0.037 (0.006)	0.035 (0.006)	0.036 (0.006)	0.006 (0.002)	0.024 (0.005)	0.031 (0.005)
	30	0.054 (0.007)	0.055 (0.007)	0.052 (0.007)	0.008 (0.003)	0.032 (0.006)	0.046 (0.007)
	50	0.045 (0.007)	0.047 (0.007)	0.049 (0.007)	0.01 (0.003)	0.029 (0.005)	0.04 (0.006)
	100	0.045 (0.007)	0.043 (0.006)	0.049 (0.007)	0.015 (0.004)	0.031 (0.005)	0.041 (0.006)
	300	0.034 (0.006)	0.034 (0.006)	0.039 (0.006)	0.012 (0.003)	0.019 (0.004)	0.028 (0.005)
91	10	0.044 (0.006)	0.05 (0.007)	0.049 (0.007)	0.001 (0.001)	0.025 (0.005)	0.048 (0.007)
	20	0.063 (0.008)	0.061 (0.008)	0.058 (0.007)	0.007 (0.003)	0.047 (0.007)	0.058 (0.007)
	30	0.054 (0.007)	0.051 (0.007)	0.051 (0.007)	0.009 (0.003)	0.038 (0.006)	0.048 (0.007)
	50	0.055 (0.007)	0.054 (0.007)	0.059 (0.007)	0.02 (0.004)	0.042 (0.006)	0.054 (0.007)
	100	0.041 (0.006)	0.042 (0.006)	0.044 (0.006)	0.014 (0.004)	0.03 (0.005)	0.04 (0.006)
	300	0.053 (0.007)	0.055 (0.007)	0.049 (0.007)	0.023 (0.005)	0.047 (0.007)	0.043 (0.006)
370	10	0.01 (0.003)	0.063 (0.008)	0.051 (0.007)	0.001 (0.001)	0.036 (0.006)	0.053 (0.007)
	20	0.021 (0.005)	0.053 (0.007)	0.047 (0.007)	0.002 (0.001)	0.032 (0.006)	0.046 (0.007)
	30	0.046 (0.007)	0.053 (0.007)	0.045 (0.007)	0.003 (0.002)	0.034 (0.006)	0.046 (0.007)
	50	0.048 (0.007)	0.051 (0.007)	0.042 (0.006)	0.003 (0.002)	0.033 (0.006)	0.038 (0.006)
	100	0.054 (0.007)	0.053 (0.007)	0.056 (0.007)	0.011 (0.003)	0.043 (0.006)	0.053 (0.007)
	300	0.049 (0.007)	0.051 (0.007)	0.043 (0.006)	0.013 (0.004)	0.039 (0.006)	0.041 (0.006)
Fisher-transformed Pearson correlation							
24	10	0.054 (0.007)	0.057 (0.007)	0.06 (0.008)	0.004 (0.002)	0.04 (0.006)	0.061 (0.008)
	20	0.047 (0.007)	0.047 (0.007)	0.045 (0.007)	0.006 (0.002)	0.035 (0.006)	0.045 (0.007)
	30	0.059 (0.007)	0.058 (0.007)	0.059 (0.007)	0.013 (0.004)	0.047 (0.007)	0.059 (0.007)
	50	0.05 (0.007)	0.051 (0.007)	0.054 (0.007)	0.018 (0.004)	0.041 (0.006)	0.054 (0.007)
	100	0.053 (0.007)	0.053 (0.007)	0.049 (0.007)	0.021 (0.005)	0.048 (0.007)	0.047 (0.007)
	300	0.05 (0.007)	0.05 (0.007)	0.046 (0.007)	0.04 (0.006)	0.048 (0.007)	0.045 (0.007)
91	10	0.044 (0.006)	0.049 (0.007)	0.048 (0.007)	0.006 (0.002)	0.033 (0.006)	0.047 (0.007)
	20	0.036 (0.006)	0.035 (0.006)	0.048 (0.007)	0.005 (0.002)	0.027 (0.005)	0.046 (0.007)
	30	0.046 (0.007)	0.043 (0.006)	0.049 (0.007)	0.007 (0.003)	0.037 (0.006)	0.046 (0.007)
	50	0.049 (0.007)	0.047 (0.007)	0.059 (0.007)	0.013 (0.004)	0.034 (0.006)	0.059 (0.007)
	100	0.047 (0.007)	0.046 (0.007)	0.046 (0.007)	0.017 (0.004)	0.044 (0.006)	0.046 (0.007)
	300	0.053 (0.007)	0.053 (0.007)	0.055 (0.007)	0.038 (0.006)	0.049 (0.007)	0.054 (0.007)
370	10	0.009 (0.003)	0.052 (0.007)	0.048 (0.007)	0.001 (0.001)	0.023 (0.005)	0.048 (0.007)
	20	0.021 (0.005)	0.051 (0.007)	0.049 (0.007)	0.001 (0.001)	0.034 (0.006)	0.049 (0.007)
	30	0.037 (0.006)	0.055 (0.007)	0.047 (0.007)	0.003 (0.002)	0.035 (0.006)	0.047 (0.007)
	50	0.041 (0.006)	0.046 (0.007)	0.054 (0.007)	0.004 (0.002)	0.026 (0.005)	0.054 (0.007)
	100	0.059 (0.007)	0.055 (0.007)	0.044 (0.006)	0.012 (0.003)	0.041 (0.006)	0.045 (0.007)
	300	0.044 (0.006)	0.044 (0.006)	0.047 (0.007)	0.02 (0.004)	0.037 (0.006)	0.048 (0.007)
Log odds ratio							
24	10	0.038 (0.006)	0.035 (0.006)	0.035 (0.006)	0.002 (0.001)	0.017 (0.004)	0.027 (0.005)
	20	0.029 (0.005)	0.028 (0.005)	0.03 (0.005)	0.001 (0.001)	0.014 (0.004)	0.016 (0.004)
	30	0.022 (0.005)	0.023 (0.005)	0.032 (0.006)	0.001 (0.001)	0.007 (0.003)	0.009 (0.003)
	50	0.021 (0.005)	0.02 (0.004)	0.028 (0.005)	0.001 (0.001)	0.006 (0.002)	0.012 (0.003)
	100	0.017 (0.004)	0.018 (0.004)	0.034 (0.006)	0 (0<0.001)	0.001 (0.001)	0.005 (0.002)
	300	0.012 (0.003)	0.012 (0.003)	0.033 (0.006)	0 (0<0.001)	0 (0<0.001)	0 (0<0.001)
91	10	0.039 (0.006)	0.044 (0.006)	0.042 (0.006)	0.001 (0.001)	0.024 (0.005)	0.037 (0.006)
	20	0.049 (0.007)	0.048 (0.007)	0.042 (0.006)	0.005 (0.002)	0.034 (0.006)	0.035 (0.006)
	30	0.043 (0.006)	0.041 (0.006)	0.046 (0.007)	0.004 (0.002)	0.024 (0.005)	0.034 (0.006)
	50	0.048 (0.007)	0.05 (0.007)	0.054 (0.007)	0.01 (0.003)	0.029 (0.005)	0.033 (0.006)
	100	0.042 (0.006)	0.041 (0.006)	0.043 (0.006)	0.013 (0.004)	0.028 (0.005)	0.024 (0.005)
	300	0.035 (0.006)	0.034 (0.006)	0.04 (0.006)	0.007 (0.003)	0.013 (0.004)	0.013 (0.004)
370	10	0.009 (0.003)	0.052 (0.007)	0.049 (0.007)	0.001 (0.001)	0.022 (0.005)	0.043 (0.006)
	20	0.019 (0.004)	0.046 (0.007)	0.051 (0.007)	0 (0<0.001)	0.034 (0.006)	0.046 (0.007)
	30	0.033 (0.006)	0.046 (0.007)	0.044 (0.006)	0.004 (0.002)	0.029 (0.005)	0.036 (0.006)
	50	0.045 (0.007)	0.045 (0.007)	0.047 (0.007)	0.004 (0.002)	0.034 (0.006)	0.04 (0.006)
	100	0.042 (0.006)	0.041 (0.006)	0.049 (0.007)	0.007 (0.003)	0.032 (0.006)	0.036 (0.006)
	300	0.065 (0.008)	0.063 (0.008)	0.059 (0.007)	0.02 (0.004)	0.052 (0.007)	0.042 (0.006)

Note: Monte Carlo standard errors are presented in the parentheses. All unacceptable Type I error rates (i.e., <0.025 or >0.075) are bold. ML or

REML estimation sometimes did not have converged results, but the nonconvergence rates across all conditions were within 0.8%.

Table 6: Type I error rates of the bootstrap based ML LR test (Q-ML-LRT), the bootstrap based REML LR test (Q-REML-LRT), and the bootstrap based Q test (B-Q) in meta-analyses for heterogeneity magnitude test

Sample size (SZ)	Number of Studies (K)	B-ML-LRT	B-REML-LRT	B-Q
		Standardized mean difference		
24	10	0.049 (0.007)	0.048 (0.007)	0.048 (0.007)
	20	0.047 (0.007)	0.045 (0.007)	0.048 (0.007)
	30	0.057 (0.007)	0.059 (0.007)	0.058 (0.007)
	50	0.05 (0.007)	0.05 (0.007)	0.058 (0.007)
	100	0.047 (0.007)	0.048 (0.007)	0.048 (0.007)
	300	0.056 (0.007)	0.056 (0.007)	0.055 (0.007)
91	10	0.038 (0.006)	0.039 (0.006)	0.04 (0.006)
	20	0.053 (0.007)	0.052 (0.007)	0.057 (0.007)
	30	0.045 (0.007)	0.044 (0.006)	0.039 (0.006)
	50	0.054 (0.007)	0.055 (0.007)	0.046 (0.007)
	100	0.061 (0.008)	0.062 (0.008)	0.067 (0.008)
	300	0.043 (0.006)	0.043 (0.006)	0.053 (0.007)
Fisher-transformed Pearson correlation				
24	10	0.045 (0.007)	0.044 (0.006)	0.041 (0.006)
	20	0.061 (0.008)	0.062 (0.008)	0.058 (0.007)
	30	0.067 (0.008)	0.066 (0.008)	0.059 (0.007)
	50	0.046 (0.007)	0.045 (0.007)	0.051 (0.007)
	100	0.059 (0.007)	0.058 (0.007)	0.06 (0.008)
	300	0.053 (0.007)	0.053 (0.007)	0.042 (0.006)
91	10	0.05 (0.007)	0.051 (0.007)	0.049 (0.007)
	20	0.065 (0.008)	0.064 (0.008)	0.064 (0.008)
	30	0.04 (0.006)	0.039 (0.006)	0.039 (0.006)
	50	0.059 (0.007)	0.059 (0.007)	0.062 (0.008)
	100	0.055 (0.007)	0.055 (0.007)	0.054 (0.007)
	300	0.071 (0.008)	0.071 (0.008)	0.069 (0.008)
Log odds ratio				
24	10	0.04 (0.006)	0.041 (0.006)	0.046 (0.007)
	20	0.025 (0.005)	0.026 (0.005)	0.034 (0.006)
	30	0.034 (0.006)	0.033 (0.006)	0.039 (0.006)
	50	0.025 (0.005)	0.025 (0.005)	0.035 (0.006)
	100	0.031 (0.005)	0.03 (0.005)	0.051 (0.007)
	300	0.018 (0.004)	0.018 (0.004)	0.065 (0.008)
91	10	0.051 (0.007)	0.05 (0.007)	0.06 (0.008)
	20	0.061 (0.008)	0.062 (0.008)	0.057 (0.007)
	30	0.058 (0.007)	0.058 (0.007)	0.058 (0.007)
	50	0.043 (0.006)	0.043 (0.006)	0.044 (0.006)
	100	0.047 (0.007)	0.047 (0.007)	0.05 (0.007)
	300	0.047 (0.007)	0.047 (0.007)	0.049 (0.007)

Note: Monte Carlo standard errors are presented in the parentheses. All unacceptable Type I error rates (i.e., <0.025 or >0.075) are bold. ML or REML estimation sometimes did not have converged results, but the nonconvergence rates across all conditions were within 0.3%.

Table 7: Statistical power of the bootstrap based ML LR test (Q-ML-LRT), the bootstrap based REML LR test (Q-REML-LRT), the bootstrap based Q test (B-Q), the improved Q test (Q_2), the Breslow-Day's test (BD), and the regular Q test in meta-analyses

	Number of Studies	B-ML-LRT	B-REML-LRT	B-Q	Q_2	BD	Q	B-ML-LRT	B-REML-LRT	B-Q	Q_2	BD	Q
Standardized mean difference	$\tau^2 = 0.03$							$\tau^2 = 0.1$					
	10	0.174 (0.012)	0.174 (0.012)	0.171 (0.012)	0.214 (0.013)	-	0.147 (0.011)	0.542 (0.016)	0.541 (0.016)	0.543 (0.016)	0.53 (0.016)	-	0.514 (0.016)
	20	0.304 (0.015)	0.305 (0.015)	0.286 (0.014)	0.341 (0.015)	-	0.248 (0.014)	0.795 (0.013)	0.796 (0.013)	0.791 (0.013)	0.78 (0.013)	-	0.766 (0.013)
	30	0.342 (0.015)	0.342 (0.015)	0.326 (0.015)	0.39 (0.015)	-	0.279 (0.014)	0.885 (0.01)	0.886 (0.01)	0.881 (0.01)	0.875 (0.01)	-	0.852 (0.011)
	50	0.503 (0.016)	0.507 (0.016)	0.493 (0.016)	0.56 (0.016)	-	0.42 (0.016)	0.981 (0.004)	0.981 (0.004)	0.984 (0.004)	0.982 (0.004)	-	0.975 (0.005)
	100	0.742 (0.014)	0.743 (0.014)	0.706 (0.014)	0.794 (0.013)	-	0.625 (0.015)						
	$\tau^2 = 0.3$												
	10	0.877 (0.01)	0.88 (0.01)	0.881 (0.01)	0.873 (0.011)	-	0.866 (0.011)						
	20	0.986 (0.004)	0.986 (0.004)	0.988 (0.003)	0.987 (0.004)	-	0.986 (0.004)						
	$\tau^2 = 0.006$							$\tau^2 = 0.02$					
	10	0.153 (0.011)	0.153 (0.011)	0.133 (0.011)	0.149 (0.011)	-	0.125 (0.01)	0.474 (0.016)	0.478 (0.016)	0.452 (0.016)	0.46 (0.016)	-	0.446 (0.016)
	20	0.284 (0.014)	0.279 (0.014)	0.228 (0.013)	0.243 (0.014)	-	0.212 (0.013)	0.715 (0.014)	0.715 (0.014)	0.685 (0.015)	0.689 (0.015)	-	0.672 (0.015)
Fisher- transformed Pearson correlation	30	0.346 (0.015)	0.347 (0.015)	0.317 (0.015)	0.341 (0.015)	-	0.3 (0.014)	0.841 (0.012)	0.842 (0.012)	0.821 (0.012)	0.816 (0.012)	-	0.809 (0.012)
	50	0.457 (0.016)	0.458 (0.016)	0.415 (0.016)	0.441 (0.016)	-	0.4 (0.015)	0.957 (0.006)	0.96 (0.006)	0.948 (0.007)	0.955 (0.007)	-	0.941 (0.007)
	100	0.694 (0.015)	0.695 (0.015)	0.606 (0.015)	0.653 (0.015)	-	0.577 (0.016)	0.996 (0.002)	0.996 (0.002)	0.998 (0.001)	0.997 (0.002)	-	0.997 (0.002)
	$\tau^2 = 0.05$												
	10	0.799 (0.013)	0.798 (0.013)	0.796 (0.013)	0.791 (0.013)	-	0.791 (0.013)						
	20	0.962 (0.006)	0.962 (0.006)	0.961 (0.006)	0.961 (0.006)	-	0.959 (0.006)						
	$\tau^2 = 0.01$							$\tau^2 = 0.03$					
	10	0.161 (0.012)	0.158 (0.012)	0.143 (0.011)	-	-	0.145 (0.011)	0.396 (0.015)	0.398 (0.015)	0.368 (0.015)	-	-	0.37 (0.015)
	20	0.225 (0.013)	0.223 (0.013)	0.197 (0.013)	-	-	0.195 (0.013)	0.575 (0.016)	0.578 (0.016)	0.55 (0.016)	-	-	0.548 (0.016)
	30	0.286 (0.014)	0.282 (0.014)	0.257 (0.014)	-	-	0.256 (0.014)	0.737 (0.014)	0.74 (0.014)	0.693 (0.015)	-	-	0.69 (0.015)
	50	0.348 (0.015)	0.348 (0.015)	0.296 (0.014)	-	-	0.296 (0.014)	0.887 (0.01)	0.887 (0.01)	0.851 (0.011)	-	-	0.849 (0.011)
	100	0.615 (0.015)	0.615 (0.015)	0.539 (0.016)	-	-	0.539 (0.016)	0.994 (0.002)	0.994 (0.002)	0.986 (0.004)	-	-	0.986 (0.004)
Natural- logarithm- transformed odds	$\tau^2 = 0.1$												
	10	0.815 (0.012)	0.815 (0.012)	0.804 (0.013)	-	-	0.802 (0.013)						
	20	0.965 (0.006)	0.965 (0.006)	0.964 (0.006)	-	-	0.963 (0.006)						
	$\tau^2 = 0.06$							$\tau^2 = 0.2$					
	10	0.318 (0.015)	0.315 (0.015)	0.292 (0.014)	-	-	0.289 (0.014)	0.755 (0.014)	0.756 (0.014)	0.732 (0.014)	-	-	0.735 (0.014)
	20	0.512 (0.016)	0.512 (0.016)	0.461 (0.016)	-	-	0.461 (0.016)	0.942 (0.007)	0.942 (0.007)	0.931 (0.008)	-	-	0.931 (0.008)
	30	0.624 (0.015)	0.626 (0.015)	0.586 (0.016)	-	-	0.586 (0.016)	0.985 (0.004)	0.985 (0.004)	0.981 (0.004)	-	-	0.98 (0.004)
	50	0.791 (0.013)	0.791 (0.013)	0.746 (0.014)	-	-	0.744 (0.014)						
	100	0.97 (0.005)	0.97 (0.005)	0.946 (0.007)	-	-	0.945 (0.007)						
	$\tau^2 = 0.5$												
	10	0.946 (0.007)	0.948 (0.007)	0.942 (0.007)			0.942 (0.007)						
SZ=24	$\tau^2 = 0.1$							$\tau^2 = 0.3$					
	10	0.115 (0.01)	0.115 (0.01)	0.128 (0.011)	0.132 (0.011)	0.11 (0.01)	0.087 (0.009)	0.308 (0.015)	0.31 (0.015)	0.328 (0.015)	0.341 (0.015)	0.14 (0.011)	0.245 (0.014)
	20	0.141 (0.011)	0.142 (0.011)	0.168 (0.012)	0.182 (0.012)	0.173 (0.012)	0.11 (0.01)	0.43 (0.016)	0.432 (0.016)	0.459 (0.016)	0.48 (0.016)	0.216 (0.013)	0.355 (0.015)
	30	0.192 (0.012)	0.19 (0.012)	0.21 (0.013)	0.226 (0.013)	0.196 (0.013)	0.125 (0.01)	0.56 (0.016)	0.565 (0.016)	0.592 (0.016)	0.627 (0.015)	0.256 (0.014)	0.481 (0.016)
	50	0.249 (0.014)	0.248 (0.014)	0.266 (0.014)	0.284 (0.014)	0.249 (0.014)	0.149 (0.011)	0.725 (0.014)	0.725 (0.014)	0.766 (0.013)	0.794 (0.013)	0.355 (0.015)	0.652 (0.015)
	100	0.396 (0.015)	0.397 (0.015)	0.451 (0.016)	0.463 (0.016)	0.426 (0.016)	0.248 (0.014)	0.898 (0.01)	0.898 (0.01)	0.909 (0.009)	0.938 (0.008)	0.914 (0.009)	0.818 (0.012)
	$\tau^2 = 0.9$												
	10	0.658 (0.015)	0.662 (0.015)	0.689 (0.015)	0.718 (0.014)	0.686 (0.015)	0.601 (0.015)						
	20	0.856 (0.011)	0.854 (0.011)	0.889 (0.01)	0.899 (0.01)	0.881 (0.01)	0.822 (0.012)						
	30	0.941 (0.007)	0.942 (0.007)	0.955 (0.007)	0.963 (0.006)	0.951 (0.007)	0.915 (0.009)						
	50	0.987 (0.004)	0.987 (0.004)	0.992 (0.003)	0.997 (0.002)	0.993 (0.003)	0.98 (0.004)						
SZ=91	$\tau^2 = 0.03$							$\tau^2 = 0.1$					
	10	0.176 (0.012)	0.177 (0.012)	0.174 (0.012)	0.174 (0.012)	0.174 (0.012)	0.16 (0.012)	0.457 (0.016)	0.46 (0.016)	0.446 (0.016)	0.449 (0.016)	0.443 (0.016)	0.406 (0.016)
	20	0.269 (0.014)	0.268 (0.014)	0.227 (0.013)	0.242 (0.014)	0.229 (0.013)	0.196 (0.013)	0.693 (0.015)	0.696 (0.015)	0.686 (0.015)	0.676 (0.015)	0.674 (0.015)	0.649 (0.015)
	30	0.327 (0.015)	0.331 (0.015)	0.311 (0.015)	0.308 (0.015)	0.297 (0.014)	0.263 (0.014)	0.831 (0.012)	0.83 (0.012)	0.828 (0.012)	0.834 (0.012)	0.821 (0.012)	0.797 (0.013)
	50	0.45 (0.016)	0.453 (0.016)	0.415 (0.016)	0.41 (0.016)	0.408 (0.016)	0.346 (0.015)	0.934 (0.008)	0.934 (0.008)	0.915 (0.009)	0.922 (0.008)	0.913 (0.009)	0.893 (0.01)
	100	0.652 (0.015)	0.652 (0.015)	0.602 (0.015)	0.601 (0.015)	0.597 (0.016)	0.521 (0.016)						
	$\tau^2 = 0.3$												
	10	0.833 (0.012)	0.836 (0.012)	0.839 (0.012)	0.834 (0.012)	0.828 (0.012)	0.82 (0.012)						
	20	0.965 (0.006)	0.965 (0.006)	0.969 (0.005)	0.973 (0.005)	0.97 (0.005)	0.962 (0.006)						

Note: Monte Carlo standard errors are presented in the parentheses. SZ is the average per-group sample sizes. The largest power values under each condition are highlighted as bold. ML or REML estimation sometimes did not have converged results, but the nonconvergence rates across all conditions were within 0.4%.

Table 8: With one covariate, statistical power of the bootstrap and ML based LR test (B-ML-LRT), the bootstrap and REML based LR test (B-REML-LRT), the bootstrap based Q test (B-Q), the regular ML based LR test (ML-LRT), and the Q test (Q) in mixed-effects models

	Number of Studies	B-ML-LRT	B-REML-LRT	B-Q	Q	B-ML-LRT	B-REML-LRT	B-Q	Q		
Standardized mean difference	SZ=24	$\tau^2 = 0.03$				$\tau^2 = 0.1$					
		10	0.173 (0.012)	0.169 (0.012)	0.162 (0.012)	0.144 (0.011)	0.457 (0.016)	0.459 (0.016)	0.439 (0.016)	0.425 (0.016)	
		20	0.265 (0.014)	0.263 (0.014)	0.243 (0.014)	0.22 (0.013)	0.72 (0.014)	0.726 (0.014)	0.728 (0.014)	0.696 (0.015)	
		30	0.365 (0.015)	0.365 (0.015)	0.337 (0.015)	0.303 (0.015)	0.852 (0.011)	0.851 (0.011)	0.845 (0.011)	0.832 (0.012)	
		50	0.473 (0.016)	0.476 (0.016)	0.435 (0.016)	0.376 (0.015)	0.976 (0.005)	0.979 (0.005)	0.976 (0.005)	0.971 (0.005)	
		100	0.701 (0.014)	0.701 (0.014)	0.685 (0.015)	0.603 (0.015)					
		$\tau^2 = 0.3$									
		10	0.867 (0.011)	0.871 (0.011)	0.873 (0.011)	0.856 (0.011)					
		20	0.984 (0.004)	0.987 (0.004)	0.99 (0.003)	0.99 (0.003)					
	Fisher- Pearson correlation	SZ=91	$\tau^2 = 0.006$				$\tau^2 = 0.02$				
			10	0.157 (0.012)	0.147 (0.011)	0.144 (0.011)	0.142 (0.011)	0.419 (0.016)	0.42 (0.016)	0.399 (0.015)	0.389 (0.015)
			20	0.259 (0.014)	0.262 (0.014)	0.246 (0.014)	0.235 (0.013)	0.677 (0.015)	0.682 (0.015)	0.645 (0.015)	0.638 (0.015)
			30	0.302 (0.015)	0.3 (0.014)	0.257 (0.014)	0.251 (0.014)	0.822 (0.012)	0.827 (0.012)	0.818 (0.012)	0.808 (0.012)
			50	0.462 (0.016)	0.461 (0.016)	0.424 (0.016)	0.408 (0.016)	0.941 (0.007)	0.941 (0.007)	0.929 (0.008)	0.924 (0.008)
100			0.631 (0.015)	0.631 (0.015)	0.572 (0.016)	0.552 (0.016)					
		$\tau^2 = 0.05$									
		10	0.758 (0.014)	0.766 (0.013)	0.763 (0.013)	0.755 (0.014)					
		20	0.948 (0.007)	0.95 (0.007)	0.949 (0.007)	0.947 (0.007)					
Natural- logarithm- transformed odds		SZ=24	$\tau^2 = 0.01$				$\tau^2 = 0.03$				
			10	0.128 (0.011)	0.131 (0.011)	0.119 (0.01)	0.117 (0.01)	0.359 (0.015)	0.367 (0.015)	0.351 (0.015)	0.348 (0.015)
			20	0.19 (0.012)	0.188 (0.012)	0.158 (0.012)	0.159 (0.012)	0.538 (0.016)	0.538 (0.016)	0.503 (0.016)	0.503 (0.016)
			30	0.279 (0.014)	0.281 (0.014)	0.241 (0.014)	0.237 (0.013)	0.725 (0.014)	0.725 (0.014)	0.684 (0.015)	0.688 (0.015)
			50	0.365 (0.015)	0.364 (0.015)	0.328 (0.015)	0.331 (0.015)	0.881 (0.01)	0.884 (0.01)	0.847 (0.011)	0.847 (0.011)
	100		0.561 (0.016)	0.56 (0.016)	0.454 (0.016)	0.453 (0.016)	0.983 (0.004)	0.983 (0.004)	0.974 (0.005)	0.975 (0.005)	
		$\tau^2 = 0.1$									
		10	0.766 (0.013)	0.774 (0.013)	0.769 (0.013)	0.768 (0.013)					
		20	0.955 (0.007)	0.961 (0.006)	0.956 (0.006)	0.956 (0.006)					
		30	0.991 (0.003)	0.991 (0.003)	0.988 (0.003)	0.988 (0.003)					
	SZ=91		$\tau^2 = 0.006$				$\tau^2 = 0.02$				
			10	0.302 (0.015)	0.308 (0.015)	0.297 (0.014)	0.297 (0.014)	0.696 (0.015)	0.699 (0.015)	0.682 (0.015)	0.681 (0.015)
			20	0.491 (0.016)	0.49 (0.016)	0.441 (0.016)	0.44 (0.016)	0.932 (0.008)	0.934 (0.008)	0.92 (0.009)	0.917 (0.009)
			30	0.595 (0.016)	0.595 (0.016)	0.566 (0.016)	0.567 (0.016)	0.977 (0.005)	0.978 (0.005)	0.976 (0.005)	0.976 (0.005)
50			0.805 (0.013)	0.806 (0.013)	0.763 (0.013)	0.764 (0.013)					
100			0.952 (0.007)	0.952 (0.007)	0.933 (0.008)	0.934 (0.008)					
		$\tau^2 = 0.05$									
		10	0.909 (0.009)	0.917 (0.009)	0.915 (0.009)	0.916 (0.009)					
SZ=24			$\tau^2 = 0.1$				$\tau^2 = 0.3$				
			10	0.102 (0.01)	0.101 (0.01)	0.099 (0.009)	0.07 (0.008)	0.254 (0.014)	0.252 (0.014)	0.278 (0.014)	0.205 (0.013)
			20	0.138 (0.011)	0.139 (0.011)	0.143 (0.011)	0.084 (0.009)	0.443 (0.016)	0.445 (0.016)	0.441 (0.016)	0.359 (0.015)
			30	0.174 (0.012)	0.177 (0.012)	0.184 (0.012)	0.108 (0.01)	0.549 (0.016)	0.55 (0.016)	0.574 (0.016)	0.431 (0.016)
			50	0.221 (0.013)	0.22 (0.013)	0.25 (0.014)	0.14 (0.011)	0.71 (0.014)	0.711 (0.014)	0.752 (0.014)	0.609 (0.015)
			100	0.36 (0.015)	0.359 (0.015)	0.411 (0.016)	0.218 (0.013)	0.902 (0.009)	0.902 (0.009)	0.893 (0.01)	0.79 (0.013)
		$\tau^2 = 0.9$									
		10	0.607 (0.015)	0.619 (0.015)	0.64 (0.015)	0.566 (0.016)					
		20	0.851 (0.011)	0.854 (0.011)	0.881 (0.01)	0.826 (0.012)					
		30	0.923 (0.008)	0.924 (0.008)	0.947 (0.007)	0.909 (0.009)					
		50	0.987 (0.004)	0.988 (0.003)	0.989 (0.003)	0.974 (0.005)					
		SZ=91	$\tau^2 = 0.03$				$\tau^2 = 0.1$				
	10		0.16 (0.012)	0.157 (0.012)	0.144 (0.011)	0.119 (0.01)	0.412 (0.016)	0.416 (0.016)	0.392 (0.015)	0.372 (0.015)	
	20		0.228 (0.013)	0.224 (0.013)	0.215 (0.013)	0.182 (0.012)	0.652 (0.015)	0.651 (0.015)	0.627 (0.015)	0.587 (0.016)	
30	0.314 (0.015)		0.314 (0.015)	0.297 (0.014)	0.245 (0.014)	0.767 (0.013)	0.77 (0.013)	0.744 (0.014)	0.703 (0.014)		
50	0.42 (0.016)		0.42 (0.016)	0.388 (0.015)	0.333 (0.015)	0.922 (0.008)	0.923 (0.008)	0.913 (0.009)	0.885 (0.01)		
100	0.622 (0.015)		0.623 (0.015)	0.574 (0.016)	0.494 (0.016)						
	$\tau^2 = 0.3$										
	10	0.789 (0.013)	0.795 (0.013)	0.809 (0.012)	0.784 (0.013)						
	20	0.955 (0.007)	0.957 (0.006)	0.967 (0.006)	0.954 (0.007)						

Note: Monte Carlo standard errors are presented in the parentheses. SZ is the average per-group sample sizes. The largest power values under each condition are highlighted as bold. ML or REML estimation sometimes did not have converged results, but the nonconvergence rates across all conditions were within 0.4%.

Table 9: Statistical power of the bootstrap based ML LR test (Q-ML-LRT), the bootstrap based REML LR test (Q-REML-LRT), and the bootstrap based Q test (B-Q) in meta-analyses for heterogeneity magnitude test

		Number of Studies	B-ML-LRT	B-REML-LRT	B-Q
$\lambda = 0.03, \tau^2 = 0.1$					
Standardized mean difference	SZ=24	10	0.284 (0.014)	0.285 (0.014)	0.286 (0.014)
		20	0.476 (0.016)	0.477 (0.016)	0.471 (0.016)
		30	0.566 (0.016)	0.565 (0.016)	0.565 (0.016)
		50	0.75 (0.014)	0.751 (0.014)	0.756 (0.014)
		100	0.94 (0.008)	0.94 (0.008)	0.94 (0.008)
	$\lambda = 0.006, \tau^2 = 0.02$				
	SZ=91	10	0.243 (0.014)	0.247 (0.014)	0.242 (0.014)
		20	0.418 (0.016)	0.418 (0.016)	0.409 (0.016)
		30	0.528 (0.016)	0.528 (0.016)	0.505 (0.016)
		50	0.702 (0.014)	0.704 (0.014)	0.698 (0.015)
100		0.911 (0.009)	0.911 (0.009)	0.902 (0.009)	
$\lambda = 0.1, \tau^2 = 0.3$					
Fisher- transformed Pearson correlation	SZ=24	10	0.211 (0.013)	0.213 (0.013)	0.215 (0.013)
		20	0.314 (0.015)	0.315 (0.015)	0.301 (0.015)
		30	0.425 (0.016)	0.422 (0.016)	0.414 (0.016)
		50	0.535 (0.016)	0.535 (0.016)	0.513 (0.016)
		100	0.815 (0.012)	0.815 (0.012)	0.807 (0.012)
	$\lambda = 0.03, \tau^2 = 0.1$				
	SZ=91	10	0.391 (0.015)	0.391 (0.015)	0.388 (0.015)
		20	0.613 (0.015)	0.617 (0.015)	0.609 (0.015)
		30	0.753 (0.014)	0.754 (0.014)	0.751 (0.014)
		50	0.909 (0.009)	0.91 (0.009)	0.907 (0.009)
$\lambda = 0.01, \tau^2 = 0.03$					
Natural- logarithm- transformed odds	SZ=24	10	0.157 (0.012)	0.158 (0.012)	0.175 (0.012)
		20	0.202 (0.013)	0.203 (0.013)	0.236 (0.013)
		30	0.286 (0.014)	0.286 (0.014)	0.326 (0.015)
		50	0.394 (0.015)	0.391 (0.015)	0.468 (0.016)
		100	0.569 (0.016)	0.568 (0.016)	0.648 (0.015)
	$\lambda = 0.006, \tau^2 = 0.02$				
	SZ=91	10	0.286 (0.014)	0.285 (0.014)	0.291 (0.014)
		20	0.389 (0.015)	0.388 (0.015)	0.395 (0.015)
		30	0.519 (0.016)	0.516 (0.016)	0.526 (0.016)
		50	0.668 (0.015)	0.667 (0.015)	0.67 (0.015)
100		0.877 (0.01)	0.877 (0.01)	0.889 (0.01)	

Note: Monte Carlo standard errors are presented in the parentheses. λ indicates the heterogeneity level under the null hypothesis. τ^2 is the true data generating heterogeneity level. SZ is the average per-group sample sizes. The largest power values under each condition are highlighted as bold.

ML or REML estimation sometimes did not have converged results, but the nonconvergence rates across all conditions were within 0.4%.