A peer-reviewed version of this preprint was published in PeerJ on 30 November 2017.

View the peer-reviewed version (peerj.com/articles/4115), which is the preferred citable publication unless you specifically need to cite this preprint.

Schneck A. 2017. Examining publication bias—a simulation-based evaluation of statistical tests on publication bias. PeerJ 5:e4115
https://doi.org/10.7717/peerj.4115

Examining publication bias – A simulation-based evaluation of statistical tests on publication bias

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Background

Publication bias is a form of scientific misconduct. It threatens the validity of research results and the credibility of science. Although several tests on publication bias exist, no in-depth evaluations are available that suggest which test to use for the specific research problem.

Methods

In the study at hand four tests on publication bias, Egger's test (FAT), p-uniform, the test of excess significance (TES), as well as the caliper test, were evaluated in a Monte Carlo simulation. Two different types of publication bias, as well as its degree (0%, 50%, 100%), were simulated. The type of publication bias was defined either as *file-drawer*, meaning the repeated analysis of new datasets, or *p-hacking*, meaning the inclusion of covariates in order to obtain a significant result. In addition, the underlying effect (β = 0, 0.5, 1, 1.5), effect heterogeneity, and the number of observations in the simulated primary studies (N =100, 500), as well as in the number of observations for the publication bias tests (K =100, 1000), were varied.

Results

All tests evaluated were able to identify publication bias both in the *file-drawer* and *p-hacking* condition. The false positive rates were, with the exception of the 15%- and 20%-caliper test, unbiased. The FAT had the largest statistical power in the *file-drawer* conditions, whereas under *p-hacking* the TES was, except under effect heterogeneity, slightly better. The caliper test was, however, inferior to the other tests under effect homogeneity and had a decent statistical power only in conditions with 1000 primary studies.

Discussion

The FAT is recommended as a test for publication bias in standard meta-analyses with no or only small effect heterogeneity. If no clear direction of publication bias is suspected the TES is the first alternative to the FAT. The 5%-caliper tests is recommended under conditions of effect heterogeneity, which may be found if publication bias is examined in a discipline-wide setting when primary studies cover different research problems.

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Introduction

All scientific disciplines try to uncover truth by systematically examining their surrounding environment (Descartes 2006: 17). Natural scientists try to observe regularities in nature, whereas social scientists try to uncover patterns in the social behaviour of humans. This could be, for example, the development of pharmaceuticals or the evaluation of political interventions, such as the effect of minimum wages on employment. The success, as well as the reputation, of science rests on the accuracy as well as unbiasedness of scientific results. Publication bias, the publication of only positive results confirming the researcher's hypothesis (cf. Dickersin & Min 1993: 135), threatens this validity. Under publication bias results showing either statistical significance and/or the desired direction of the effects are published. The published literature in this case is merely a selective (and too optimistic) part of all existing scientific knowledge.

The study at hand examines the performance of four methods to identify publication bias: Egger's Test/FAT (Egger et al. 1997; Stanley & Doucouliagos 2014), p-uniform (PU; van Aert et al. 2016; van Assen et al. 2015), the test for excess significance (TES; Ioannidis & Trikalinos 2007a) and the caliper test (CT; Gerber & Malhotra 2008a; Gerber & Malhotra 2008b). In order to compare the performance of these tests, the false positive rate (α-error, type I error) and the statistical power (true positive rate) were examined with a Monte Carlo approach. This makes it possible to assess the performance of the four tests under different conditions of publication bias (*file-drawer* vs. *p-hacking*), as well as study settings (underlying true effect, effect heterogeneity, number of observations in primary studies and in meta-analyses).

The issue of publication bias

The classification of inferential statistics relies on the truth table (Table 1). An estimator (rows) tries to derive conclusions about the underlying true data (columns). The diagonal from the top left

to the lower right (in bold) describes a situation in which the estimator describes the underlying data correctly. This can be either stating no existing effect (true negative) or stating an existing effect (true positive). In the opposite situation the estimator states the wrong result, either an effect if none is present (false positive) or no effect if one is present (false negative).

The false positive rate of a test (commonly called *p*-value) is the probability of the estimator rejecting H_0 despite this being true. The *p*-value is therefore the probability that the observed estimate is at least as extreme given there is no effect as assumed by H_0 (Wasserstein & Lazar 2016). The larger the *p*-value the higher the risk of assuming an effect if none exists in the data. *p*-values below a certain threshold are called statistically significant, whereas values above the threshold are labelled as non-significant. In the empirical sciences the 5%-significance threshold is mostly used (Cohen 1994; Labovitz 1972; Nuzzo 2014). The difference between 0.049 and 0.051 in the error probability is, however, marginal. Nevertheless, from the standpoint of the 5%-significance threshold the first would be a significant effect, whereas the latter would be a non-significant effect. In both of these two cases on average around 1 in 20 null-hypotheses of no difference would be rejected, albeit true. If empirical researchers select their data/models until they find, just by chance, significant evidence that seems worth publishing, publication bias is on the rise, leading to inflated or even artificial effects.

Rosenthal (1979) constructs a worst case scenario in which only the 5% of false positive studies that are "significant" solely by pure chance are published. In this case, misinterpreted results shape the scientific discourse and finally result in (medical or political) interventions. Although Rosenthal's example is extreme, a multitude of evidence for publication bias exists in various

disciplines and research fields.[1] Godlee (2012) therefore warns that scientific misconduct may also physically harm patients. Chalmers (1990) also counts publication bias among general forms of scientific misconduct because the consequences for the society as well as for science are similar.

In addition to the societal consequences, publication bias also has severe implications for the evolution of knowledge. All scientific progress relies on the rejection of theories (Popper 1968: 215) but under publication bias no such rejection occurs, which leads to a state of "undead theory" (Ferguson & Heene 2012: 559) where all existing theories are confirmed irrespective of their truth.[2]

Statistically significant results furthermore stress the originality of research findings (Merton 1957). Both authors and scientific journals[3] have large incentives to maximise their significant results to survive in a publish or perish research environment. Authors especially want to increase their publication chances, notably in top-tier journals where low acceptance rates of 5%–10% are quite common (for the top interdisciplinary ournals Nature 2017; Science 2017; cf. for the political sciences Yoder & Bramlett 2011: 266). There are, in particular, two distinct strategies to achieve significant results by means of publication bias practices. Firstly, non-significant findings can be suppressed (cf. the classical file-drawer effect described by Rosenthal 1979) and significant results are then searched for in another dataset. Secondly, small bits in the model of analysis can be changed (e.g. adding covariates) until a significant result is obtained – this method is known as *p*-

[1] One of the most prominent examples in medicine is the case of Tamiflu, which is commonly used to treat influenza. The meta-analysis of Jefferson et al. (2012) shows that the results in the published literature are far too optimistic, especially considering side effects. But the problem is also of concern in the social sciences, where in the case of minimum wage research the literature mostly suggests no or only very low minimum wages in order to prevent negative side effects on employment, whereas no such negative effects exist (Doucouliagos & Stanley 2009).

[2] Existing true effects are then indistinguishable from false positives. Schoenfeld & Ioannidis (2013) demonstrate this in their meta-analysis in which they unsurprisingly find that in most of the studies they included in their meta-analysis nearly all commonly used cooking ingredients are labelled carcinogenic.

[3] For evidence on publication bias by reviewers and editors (Coursol & Wagner 1986; Epstein 1990, 2004; Mahoney 1977). In contrast other studies found no evidence (Dickersin et al. 1992; Lee et al. 2006; Olson et al. 2002).

hacking (cf. "fishing" Gelman 2013; or "researchers degree of freedom" Simmons et al. 2011: 1359). Whereas the *file-drawer* strategy can be utilised by authors as well as by editors and reviewers, *p-hacking* can only be committed by authors/researchers. Nonetheless, *p-hacking* strategies can be recommended by actors other than authors (e.g. editors, reviewers, etc.).[4]

Evidence on the prevalence of publication bias

So far, there are two strategies for identifying publication bias: the first traces studies through the publication process, the second asks authors, reviewers, or editors about their publication practices via surveys. In the first strategy, most of the analyses trace conference papers or ethics committee decisions if those results get published or remain in the file-drawer. Callaham et al. (1998) trace all papers submitted to a medical conference and find that significant findings have nearly twice the chance of being published. Coursol & Wagner (1986) report from a retrospective survey of psychological studies that in total positive findings are over three times more likely to be published. However, full conference papers may already be biased because authors might submit their results to a conference only if they are already significant (Callaham et al. 1998: 256). This problem of underestimating publication bias might be overcome if the starting point is set more early on in the research process.[5] Therefore, another approach is to trace the studies directly after an ethics committee vote. The studies of Dickersin et al. (1992) and Easterbrook et al. (1991) confirm the previous findings by reporting 2.32 higher chance of getting published and 2.54 higher publication rates for significant studies. Also, Ioannidis (1998), who traces the study protocols of a large

[4] In contrast to fraudulently manipulated data, although publication bias is heavily punished, it is nearly impossible to detect at the individual level (Stroebe et al. 2012: 681) and in the case of *p-hacking* it is almost wholly without any costs, as data analysis tools/packages become increasingly easy to apply (Paldam 2013). Feigenbaum & Levy (1996) therefore even postulate the technological obsolescence of fraud. For evidence on the prevalence of fraud see (Baerlocher et al. 2010; John et al. 2012; Nuijten et al. 2016).

[5] Timmer et al. (2002) and Hua et al. (2016) report no evidence of unequal publication chances in the medical field of gastroenterology and dentistry. Both of these studies rely only on conference abstracts and therefore might be restricted in respect of the research results, because abstracts have the disadvantage that only a selection of results are presented in them, while in full papers all results are reported.

medical network over 10 years from 1986–1996, finds that significant studies have, beside their 3.7 times higher publication rate, a substantially higher publication speed, meaning a shorter time between completion of the study and the final publication.

The second approach asks directly about the publication practices of the involved actors. In a survey of psychologists that used a sensitive question technique up to 50% of the respondents claimed that they exercised publication bias (John et al. 2012: 525). Franco et al. (2014) also note that most non-significant findings go to the file-drawer right after the analysis and are not even written up. Also, other less harshly sanctioned forms of misbehaviour, like optional stopping (stopping data collection when significance is reached) or erroneous rounding of *p*-values to reach significant results, are alarmingly widespread (prevalence rate arround 22.5% John et al. 2012: 525). These results are in line with the survey of Ulrich & Miller (2017: 9), who report that researchers in the field of psychology prefer significant results over non-significant results, and, furthermore, attribute more value to results with smaller *p*-values. These estimates may even be conservative because it is known from the survey literature that sensitive behaviours like scientific misconduct may be underreported (Kreuter et al. 2008: 848). According to the presented research results *file-drawer* and *p-hacking* behaviour is therefore quite widespread.

Methods

Statistical tests on publication bias

So far, the presented detection strategies ask either directly for publication preferences or examine the publication fate of conference papers. Both approaches have the weakness that they either rely on the potentially biased answers of the actors involved or require an immense effort to follow the publication process, while publication bias may have happened before the paper is submitted to a conference. Statistical tests on publication bias circumvent this problem by relying only on the

117 published literature. In the paper at hand the regression-based Egger's test (Egger et al. 1997;
118 Stanley & Doucouliagos 2014), PU (van Assen et al. 2015), an extended version of p-curve
119 (Simonsohn et al. 2014a, b; Simonsohn et al. 2015), the TES (Ioannidis & Trikalinos 2007b) and
120 the CT (Gerber & Malhotra 2008a, b) are discussed.[6]

121 All of these tests are applied mostly in a discipline-specific context: the Egger's test is routinely
122 used in classical meta-analyses across all disciplines (cf. the Cochrae Handbook Higgins & Green
123 2008: 314), PU (for applications see Blázquez et al. 2017; Head et al. 2015; Simmons &
124 Simonsohn 2017), as well as the TES (for applications see Francis 2012a, b, c, d, e, 2013) are more
125 widely used in psychology. The CT is mostly implemented in the general social sciences (for
126 further applications in Soiology see Auspurg & Hinz 2011; Auspurg et al. 2014; Berning & Weiß
127 2015; in Psychology see Hartgerink et al. 2016; Kühberger et al. 2014). The discipline-specific
128 use of the tests is therefore to a certain degree path dependent on the practices involved in testing
129 publication bias in the specific fields.

130 Funnel asymmetry test (FAT)
131 The first class of tests makes it possible to address publication bias by the association of the effect
132 sizes and their variance. Because the variance *(se^2)* of an effect size in a primary study *(es)* is
133 strongly related to the sample size, small studies with a low number of observations (*N*) show an
134 increased variation of effects around the unobserved true effect. The larger the *N*, the smaller the
135 variation and thus the more precise is the effect size of the study. Under publication bias small
136 non-significant studies are mostly omitted, whereas small but precise effects with a large *N* still
137 remain in the analysis. When this pattern for a small positive effect is represented through a

[6] Because for Fail-save-N (Rosenthal 1979) only rules of thumbs instead of a formal statistical test exist it was not included in the simulation at hand. Although it is still widely applied (Banks et al. 2012: 183; Ferguson & Brannick 2012: 4), this benchmark is not recommended in the *Cochrane Handbook*, a guideline for conducting meta-analyses (Higgins & Green 2008: 321f.).

138 scatterplot graph a typical inverted funnel-shaped pattern can be observed (called "funnel plot"
139 Light & Pillemer 1984: 63-69). In the exemplary Figure 1 on the right, studies in the lower left
140 side are missing because of publication bias with a preference for significant positive effects. On
141 the left side, in contrast, a symmetric funnel with no publication bias is shown.

142 Relying only on subjective graphical information, as provided by funnel plots, might be misleading
143 (Tang & Liu 2000). Begg & Mazumdar (1994: 1089) examine the rank correlation of the
144 standardised effect ($t = es/se$) and its variance (se^2). A similar approach by Egger et al. (1997)[7]
145 regresses t on the inverse standard error ($1/se$). t is chosen as the dependent variable in order to
146 account for the unequal variance across the effects (heteroscedasticity) by weighting each
147 observation by the inverse of its variance. Compared to the regression of *se* on *es* this changes the
148 interpretation.

149 $$t_i = \beta_0 + \beta_1 \frac{1}{se_i} + \varepsilon_i$$

150 The constant β_0 is the test on publication bias (FAT stating publication bias if $\beta_0 \neq 0$), whereas β_1
151 makes it possible to identify a true empirical effect controlling for publication bias (Egger et al.
152 1997: 632). In the left graph of Figure 2 a primary study (depicted as dots), with almost no
153 precision, is not able to find an effect (H_0: $\beta_0 = 0$ could not be rejected). In contrast, in the right
154 graph under publication bias studies with no precision also find a substantial effect.

155 In the following simulation only the FAT is used because of its better statistical power, as found
156 in previous simulations (Hayashino et al. 2005; Kicinski 2014; Macaskill et al. 2001; Sterne et al.

[7] This estimator is equivalent to the bivariate FAT-PET recommended by Stanley & Doucouliagos (2014). The FAT-PET furthermore makes it possible to also include "potential effect modifiers" (Deeks et al. 2008: 284) in a meta-regression model. This is especially necessary if the literature being studies has, besides its theoretical meaningful overall effect, systematic differences (e.g. different implementations of an experimental stimulus, different experimental populations, etc.).

157 2000), compared to the rank correlation test of Begg & Mazumdar (1994).[8] Despite its strengths,
158 the central weaknesses of the FAT lies in its low statistical power in a setting with only a small
159 number of primary studies (Macaskill et al. 2001 simmulated the performace only based on 20
160 primary studies).[9]

161 p-uniform (PU)
162 The tests discussed so far focus on the empirical effect sizes, whereas the p-curve method,
163 proposed by Simonsohn et al. (2014b), and the similar PU, a method proposed by van Assen et al.
164 (2015), focus entirely on the distribution of significant *p*-values. All non-significant values are
165 therefore dropped from the analysis. The sample is, furthermore, restricted to the direction of
166 suspected publication bias: that means only positive or negative effects are examined (Simonsohn
167 et al. 2014a: 677). In the first step the *p*-value of the estimate in the primary study is rescaled in
168 respect to the significance threshold. For the present study the 5%-significance threshold ($p = 0.05$)
169 rescales the *pp*-values to the range [0,1]. This *p*-value of *p*-values (*pp*-value) reflects the probability
170 under the null hypothesis of a non-existing effect that a *p*-value would be as small as, or even
171 smaller than, the observed one.[10]

172 $$pp_i = \frac{p_i}{0.05} = \frac{1 - \Phi\left(\frac{es_i}{se_i}\right)}{0.05} \quad if\ p_i < 0.05$$

[8] The regression-based test also shows superior properties compared to the trim and fill technique (Bürkner & Doebler 2014; Kicinski 2014; Moreno et al. 2009; Renkewitz & Keiner 2016), which tries to obtain a symmetrical funnel plot by imputing studies that might be missing due to publication bias (Duval & Tweedie 2000).
[9] In addition to the performance of the FAT, multiple simulation studies (Alinaghi & Reed 2016; Paldam 2015; Reed 2015) also examine the unbiasedness of the effect estimate (PET - the estimated underlying effect size corrected on publication bias) which is not of interest in the study at hand. The PET is especially threatened by an increased false positive rate under effect heterogeneity (Deeks et al. 2005; Stanley 2017), the properties of the FAT in these conditions have not yet been examined.
[10] Φ represents the standard normal distribution

In a second step the skewness of the *pp*-distribution is tested (Simonsohn et al. 2015: 1149). Right skewness shows an overrepresentation of findings with a substantial statistical significance and indicates a genuine empirical effect. Left skewness, in contrast, shows an overrepresentation of just significant estimates that barely pass the significance threshold (in this case 5%) and indicates publication bias under the null hypothesis (Simonsohn et al. 2014b: 536).

Whereas p-curve by Simonsohn et al. (2014b) only allows to identify publication bias under a true underlying null effect, PU (van Assen et al. 2015) allows to also identify publication bias under an empirically observed effect. Therefore, p-curve is a special case of PU. For PU the underlying effect has to be estimated empirically by a fixed-effect meta-analysis (FE-MA)[11] with all primary studies. In a second step, and equivalent to p-curve, only *k* estimates with $p < 0.05$ and the direction of the suspected publication bias remain in the analysis (van Aert et al. 2016: 727). By adjusting on the existing underlying effect, the fixed-effect estimate μ, it is possible to test the skewness of the distribution conditional on the underlying empirical effect (van Assen et al. 2015). In the numerator, the effect size estimate is conditioned on the underlying effect (μ), similar to a one-sample *z*-test. The denominator of the *pp*-value is not fixed to 0.05 as in p-curve, but is also conditioned on the underlying effect (μ), which is subtracted from the effect threshold (*et*) an effect has to reach to become statistically significant given its standard error (*se*).

$$pp_i^{\mu} = \frac{1 - \Phi\left(\frac{es_i - \mu}{se_i}\right)}{1 - \Phi\left(\frac{et_i - \mu}{se_i}\right)} \quad if\ p_i < 0.05$$

[11] Mean effect size across all included studies weighted by the inverse study variance.

The test statistic is gamma-distributed with k degrees of freedom.[12] Because the skewness is now conditional on the underlying empirical effect left skewness observed by PU identifies publication bias across all underlying empirical effects, as depicted in Figure 3.

Because PU rests on the average effect size estimated by a fixed-effects meta-analysis it is sensible to effect heterogeneity. The degree of heterogeneity which invalidates the test is, however, unclear. Whereas Simonsohn et al. (2014a: 680) state that p-curve, and therefore also PU, is also appropriate under effect heterogeneity, van Aert et al. (2016: 718) note exactly the opposite.

van Assen et al. (2015) evaluate the performance of PU and the TES (a publication bias test, discussed in the next section), and trim-and-fill, and conclude that PU has a greater statistical power than the other methods (van Assen et al. 2015: 303). Also, Renkewitz & Keiner (2016) evaluate the PU publication bias test and observe its slightly better performance compared to the FAT and the TES. However, in both studies the number of studies in the meta-analyses (max. 160), as well as the number of observations (max. 80) in the primary studies, is relatively small.[13]

Test for excess significance (TES)

The TES (Ioannidis & Trikalinos 2007b; also called ic-index see Schimmack 2012) builds on the observed power of every single study to uncover the true total effect. This true effect is estimated by a fixed-effect meta-analysis, as in PU. Observed power analyses make it possible to compute

[12] $p = \Gamma\left(k, \ -\sum_{i=1}^{k} \log\left(pp_i^{\mu}\right)\right)$

[13] Similar to the FAT-PET, evaluations of PU center mainly on the estimated overall effect. While van Assen et al. (2015) show a good coverage of the estimated overall effect, McShane et al. (2016) state, in contrast, that while "p-curve and p-uniform approaches have increased awareness about the consequences of publication bias in meta-analysis, they fail to improve upon, and indeed are inferior to, methods proposed decades ago" (McShane et al. 2016: 744).

the post hoc power (pw_i) of a study. This allows to specify the expected number of significant effects E, given the average effect as well as the significance threshold (in this case $\alpha = 0.05$).[14]

$$E = \sum_{i=1}^{k} (pw_i)$$

E may even be a conservative estimate of the expected number of significant studies because it heavily relies on the fixed-effect estimate, which suffers from an eventual publication bias. In relation to O, the empirically observed number of significant studies ($p_i < 0.05$) the TES tests whether more significant results than expected are reported in the literature. To test whether the share of observed positive outcomes $\left(\frac{O}{K}\right)$ is larger than the share of expected positive outcomes $\left(\frac{E}{K}\right)$ a one-sided binomial test is used (Ioannidis & Trikalinos 2007b: 246).

On exemplary datasets the TES performs considerably better under moderate effect heterogeneity in large meta-analyses, where the FAT in particular failed to uncover publication bias (Ioannidis & Trikalinos 2007b: 248). Nevertheless, Johnson & Yuan (2007: 254) ask if the TES makes it possible to dissect between publication bias and study-heterogeneity accurately. Therefore, the authors of the *Cochrane Handbook* (Higgins & Green 2008: 323) express the need for further evaluations.

Caliper test (CT)

In contrast to the aforementioned three tests, the CT, developed by Gerber and Malhotra (2008a, b), ignores most of the information provided by the studies included and looks only at a narrow interval (caliper = c) around the significance threshold (th) in a distribution of absolute z-values. In case of a continuous distribution of z-values, studies in the interval below the significance

[14] Although Hoenig & Heisey (2001) criticise the application of post-hoc power analyses in primary studies for the good reason that the observed power estimate may be biased, meta-analyses circumvent this critique because a distribution of power estimates allows to infer more accurately the power of a set of studies.

threshold (in the so-called over-caliper; $x_z = 1$) should be as likely as just non-significant studies (in the so-called under-caliper; $x_z = 0$).

$$x_z = \begin{cases} 0 \; if \; th - c * th < z \leq th \\ 1 \; if \; th < z < th + c * th \end{cases}$$

Gerber and Malhotra (2008a, b) use a 5%, 10%, 15% and 20% interval (*c*) proportional to the significance threshold (*th*). In particular, the widest 20% caliper may be too wide because the 10%-significance level that could be another target threshold for publication bias is fully overlapped. The higher the overrepresentation in the over-caliper, the higher the likelihood of publication bias. This is also shown in Figure 4: in the left graph with no publication bias no discontinuities are seen around the arbitrary 5% significance threshold (dashed line), whereas in the right graph a stepwise increase of just significant results indicates publication bias. As with the TES, a one-sided binomial test is used to test the equal distribution of *z*-values in the over- and under-caliper.[15]

Publication bias tests in comparison

In order to compare the different publication bias tests presented, four different criteria have to be established: the measurement level, the sample used, the assumptions in connection with the test method, and its according limitations (cf. Table 2).

Whereas the FAT and PU explicitly model the test value distribution, the TES and the CT rely only on dichotomous classifiers. The PU and the CT furthermore restrict their sample to either significant estimates of positive or negative sign (PU) or estimates in a close interval around the significance threshold (CT). Both criteria lead to a hierarchy of information: the FAT relies on all available information, whereas the TES and PU, and the CT most strongly, rely only on limited

[15] Masicampo & Lalande (2012) and Leggett et al. (2013) test the deviance of values around the significance threshold from a fitted exponential curve on *p*-values in a broader range from 0.1 – 0.10 to counter the huge loss of observations in the CT. This may be problematic, because a single distributive function may not be able to describe the pattern well enough across the suspected jump points (cf. Lakens 2015). In the case of substantial effect heterogeneity this problem would be aggravated even further.

information on the published estimates. This may reduce the statistical power of the tests despite being a useful means of circumventing certain limitations or fulfilling assumptions, as discussed later on.

The FAT has the assumption that study precision drives publication bias (there is more publication bias in smaller and less precise studies). This has the disadvantage that variation in the number of observations in primary studies is necessary. Another disadvantage is that only directed publication bias either in favour of a positive or negative significant effect can be tested. In the extreme case of only a significant positive and significant negative effects due to publication bias no publication bias can be detected by the test. Publication bias in this case is nonetheless visible in the funnel plot. In addition, PU is only able to detect directed publication bias. The TES and the CT do not have this limitation of either a preference for positive or negative estimates. In contrast to the FAT, all the other tests are only able to test for publication bias in respect of a specific significance threshold (e.g. 5%).

The FAT has the central assumption that all variation of the effect should be independent of the study precision and therefore *N* (number of observations in the primary studies). PU has the assumption that every left skewness in the *pp*-value distribution is caused by publication bias. This assumption is, however, grounded mainly on the -effect estimate, which is very sensitive to effect heterogeneity. The same problem applies to the TES, which also relies on a fixed-effect estimate. Despite having the disadvantage of a vast amount of information and thus statistical power, the CT has the advantage of being unaffected by underlying effect distribution, as well as publication bias direction. Therefore, no assumption, despite being continuous, has to be made. In particular, jumps in the distribution around the significance threshold should therefore be highly unlikely. The assumption of a uniform distribution ($P(x_z) = 0.5$) of the under- and over-caliper is stronger,

the narrower the caliper (c) is set. This is because narrower calipers are less sensitive to the overall shape of the z-value distribution.

No evaluation of these tests exists based on a larger number of primary studies. In particular, the newer publication bias tests like PU, as well as the TES and the CT, are in need of an evaluation under different conditions. For the CT also no studies exist regarding the best caliper width to use. Despite the existence of some simulation studies on publication bias tests, so far no direct comparison exists that evaluates the performance of all available publication bias tests. Such an evaluation can particularly guide the choice of publication bias tests under substantial effect heterogeneity.

Simulation setup

In order to examine the performance of the four publication bias tests, a Monte Carlo simulation approach is used. For the simulation two different processes have to be distinguished: firstly, the data generation process (DGP), and, secondly, the meta-analytical estimation method (EM). The DGP provides the ground for the hypothetical data used by the simulated actors, as well as the results they report, whereas the EM applies the tests on publication bias reported in the previous section. The central advantage of using Monte Carlo simulations is that controlling the DGP allows us to identify which simulated studies suffer from publication bias and which do not. Similar to the case in experiments, different conditions can be defined to ensure a controlled setting. The performance of the estimators can then be examined under the different conditions.

The first step of the DGP (cf. Table 3) defines different effect size conditions that underlie the analyses of the simulated actors. In order to cover low to medium effects, as defined by the large-scale literature survey of Bosco et al. (2015: 436), as a first condition the underlying true effect was specified by a linear relationship with β = 0, 0.5, 1.0, 1.5. In addition to the homogenous

conditions with a common effect size, a heterogeneous condition was added that assumes no fixed distribution of an underlying effect but a uniform mixture of all four effect sizes, as defined above, plus an additional effect of $\beta = 2.0$ in order to ensure enough variation. The specified linear relationship between the dependent variable y and the independent variable x had a normally distributed regression error term of $\varepsilon = \Phi(0,10)$, while the variation of the independent variable was defined as $\sigma_x = 2$ (for a similar setup see Alinaghi & Reed 2016; Paldam 2015).

In order to quantify effect heterogeneity the I^2 measure (Higgins & Thompson 2002) is used. This approach allows us to differentiate between the random variation that is driven by N and the variation that is caused by underlying effect heterogeneity. I^2 allows us to specify the share of variation caused by true effect heterogeneity and the total variation that consists of random and true effect heterogeneity. Although effect heterogeneity is best addressed directly in meta-regression models this approach is not possible if the sources of heterogeneity are unknown or too diverse. This may be the case when analysing publication bias in a discipline-wide literature with no common underlying effect (e.g. sociology, psychology, etc.).

Because the FAT is based on study precision, which is mainly driven by the number of observations (N) of the primary studies, N was computed as a second condition by an absolute normal distribution with a mean of 100 (small N) or 500 (large N) and a standard deviation of 150. In order to ensure an adequate statistical analysis for the primary studies, Ns equal to or smaller than 30 were excluded.[16] This procedure resulted in a right skewed distribution with a mean N of roughly 500 for the large N, and 165 for the small N condition.

[16] The statistical power in studies with N<=30 is very small and therefore the normality assumption of the regression error term is not met.

Due to the two manipulated conditions, the true effect and the study N, the statistical power to detect an underlying true effect varied widely across the simulated primary studies with an underlying true effect, ranging from effectively powerless studies (9.7%) to very powerful ones (92.7%). On average, a study had a statistical power of 42.6%. 16.6% of the studies were adequately powered with at least 80% power (Cohen 1988: 56). The setting produced by the DGP also reflects the results of Ioannidis et al. (2016: 15), who report that only 10% of the studies in economics are adequately powered.

In addition to the number of observations in the primary studies (N) the number of primary studies that were included in the meta-analysis and form the basis of the publication bias tests (K) was varied in the third condition. A setting with 100 studies was used as a lower condition, whereas 1000 studies were set as an upper condition. The small K as well as large K condition define the space in which meta-analyses are applicable with an adequate statistical power.

Building on this data setup stage of the DGP the behavioural setup adds publication bias to the simulation in a fourth step. In the simulation setup publication bias is defined as the willingness to collect new data or run additional analyses if statistical significance failed ($p \geq 0.05$) or a negative effect occurred. Five different conditions have to be distinguished. Firstly, the condition without publication bias: in this ideal case all estimates (βx) are estimated by a bivariate ordinary least squares (OLS) model and afterwards published. Publishing in terms of the simulation model means that all estimates enter the final meta-analysis. Therefore, in the condition without publication bias either 100 or 1000 regression results are estimated and enter the meta-analysis. It is important to note that publication bias in the simulation model at hand is only the intention to commit publication bias. The actual publication bias depends on the data setup itself: how large is the true

effect size (β) and the number of observations (N) in the primary studies? Or, in short: is there already a significant positive result which does not need a publication bias treatment?

In the second and third conditions publication bias is present with a 50% probability. That means that 50% of the actors are willing to run additional analyses in order to obtain significant results. These conditions seem closest to the behavioural benchmark of the empirical studies presented.

If a non-significant result is obtained, actors operating under the second condition choose to collect new data in order to obtain significant results that can be published. This second condition therefore models publication bias under the *file-drawer* scenario, because the datasets not used remain unpublished. An actor tries to run analyses on the basis of up to nine additional datasets and only stops earlier if a significant result with a positive sign is obtained. If none of the 10 datasets yields a significant relationship with a positive sign, the estimate which is closest to the significant threshold is published. This rule serves two purposes: firstly, it seems plausible that an actor who has tried that many analyses wants to get the results published in the end to compensate for the invested effort and to avoid sunk costs (Thaler 1980). Secondly, from a technical point of view, this allows to keep the number of observations in a meta-analysis K constant across all simulation conditions.

In the third condition an actor does not try to achieve significant results by running the same bivariate analysis on different samples, but rather tries to run different model specifications on the same data by including control variables (z_j) to achieve statistical significance of the coefficient of interest (βx). The third condition therefore models publication bias as *p-hacking*, because the existing dataset is optimised to receive a significant p-value. The actor is able to add three different control variables to the model. The control variables are defined as collider variables that are both

an effect of x as well as y, which biases the effect of interest (Cole et al. 2009; Greenland et al. 1999). The effect of x and y on z_j is, however, only small ($\gamma = 0.5$). The error term of the equation defining z is normally distributed $\Phi(0,10)$. With three available control variables z_j an actor has seven different combinations to improve the research results to obtain a significant effect of x on y.

In contrast to the second and third conditions, where 50% of the actors have the intention to commit publication bias, in the fourth and fifth conditions all actors have the intention to engage in publication bias practices, once again either through *file-drawer* (fourth condition) or *p-hacking* behaviour (fifth condition). Part from the higher degree of intention to engage in publication bias practices the settings remain the same. Although the two conditions where all actors have the intent to engage in publication bias are far too pessimistic, they allow us to evaluate the performance of the tests in the most extreme publication bias environment. Tests that are not able to detect publication bias even under such extreme conditions are of low utility to the research community to identify publication bias.

The resulting design matrix has 100 different combinations resulting from 20 data setup conditions multiplied by the five publication bias conditions (see Table 3). In order to obtain reliable estimates similarly to in an experiment (Carsey & Harden 2013: 4f.), every single cell of the design matrix has to be replicated multiple times. In order to specify the numbers of replications that are necessary to achieve a sufficient statistical power of at least 80% (Cohen 1988: 56) a power analysis was conducted for the statistical power, as well as the false positive rate estimates (see Table 4). For the false positive rate a small deviation of 1 percentage point from the set 5%-false positive rate has to be correctly identified with at least an 80% chance. To achieve this goal, every condition without publication bias had to be supported with 3,729 runs. As deviations in power

are, though important, not as essential as the false positive rate (Cohen 1988: 56) a difference of 3 percentage points is set as acceptable. In order to identify a 3 percentage point deviation from the target power of 80% each of the 80 conditions with existing publication bias needed 1,545 runs. In total, 198,080 runs were necessary, resulting in nearly 109 million primary studies that in the case of publication bias contained up to 10 different regression models.[17]

The aim of the simulation study at hand is to compare the performance of the four tests in respect of: A) their capability to detect publication bias if present (true positive, statistical power), as well as B) consistent false positive classification (α-error) as shown in the second row of the truth table (cf. Table 1). Because the conditions with and without publication bias are known in a simulation study, the power of the tests as well as the false positive rate is computable (Mooney 1997: 77-79). In a first step, a dummy variable (s) is constructed, with the value 1 for a significant test result below the significance threshold (5% significance level). To obtain the power, the first estimate is restricted to the conditions with publication bias and sums up the indicator variable s over all runs (r) in that condition. The power is than defined as the proportion of significant results s in respect to r (see Table 4). The false positive rate is computed equivalently but only in conditions without any publication bias.

Results

The simulation process took about five days on a medium-performance computer that executed more than 232 million regressions. Because the publication bias in the experimental setup was implemented by the intent to commit publication bias (which does not necessarily need to result in actual publication bias), two further variables are useful to interpret the results: the share of

[17] The simulation routine was written in Stata and builds heavily on the fastreg.ado of Geertsema (2014), which makes it possible to speed up the process immensely by providing a stripped-down OLS-regression command. The analysis files are available for replication purposes.

actual studies per meta-analysis that suffer from publication bias (if $p < 0.05$ or negative result in the first trial) and the share of studies that achieve their goal of a significant positive result by publication bias.

In the two conditions where 50% of the actors have the intention to commit publication bias to reach their goal of a significant positive effect, on average 21.7% did so (cf. Table 5). Also in the two conditions where 100% are willing to commit publication bias only 43.4% engaged in publication bias practices because they already had achieved significant results beforehand. In other words: over all conditions, 56.6% of the primary studies already achieved significant results that did not call for a publication bias treatment. The success rate of those actors committing publication bias were, because of the limited number of trials (either by the maximum number of datasets or control variable combinations), with around 58% for both *file-drawer* and *p-hacking* far from a guaranteed success. In the 50% publication bias condition, on average 12.5% of the studies achieved significant results with the help of publication bias practices, while in the 100% publication bias condition 25% did so. Neither of these results differ between *file-drawer* and *p-hacking*. This means that the leverage of both publication bias conditions to get significant results does not differ in the chosen setting, which allows us to compare the performance of the publication bias tests to identify both strategies.

In the condition with no effect heterogeneity, on average only a negligible 4.3% of the variation was attributed to effect heterogeneity, whereas in the condition with effect heterogeneity 73.3% of the variation was attributed in this way. In terms of Higgins & Thompson (2002: 1553), an I^2 larger than 50% has to be modelled explicitly in meta-analyses and cannot be ignored.

Table 6 shows the false positive rates of the publication bias tests across all simulated conditions. The false positive rate of the test was fixed in the simulation setting to 0.05 (again, see Table 4),

so all false positive rates should be equal to, or even smaller than, 0.05. Positive deviations from 0.05 point to inflated false positive rates, which lead to more false conclusions than expected. In Table 6 these values are highlighted in bold. Over all conditions the FAT, PU, the TES, as well as the narrower CTs (3%, 5%), had a consistent false positive rate. The FAT was closest to the expected 5% error rate. PU and the TES, as well as the 3% and 5% CTs, in contrast, are in most cases very conservative because they fall far below 0.05. This over-conservatism may be problematic in respect to a decreased statistical power, a matter which is discussed later on. The wider 10% and 15% CTs suffered under inflated false positive rates because, due to the large caliper width, the assumption of a uniform distribution in both calipers was violated.[18] For the 10% CT the specified false positive rate doubles to more than 10%, whereas in case of the 15% CT it more than quadruples.

Looking at conditions with 50% publication bias in the *file-drawer* condition (see Table 7), the FAT had a superior power compared to other tests in 14 of 20 conditions, as indicated by the underlined numbers. The FAT is, however, closely followed by the TES, which had a larger number of conditions with a satisfactory power (> 0.8) compared to the FAT (7 vs. 6). In the first condition with $N = 100$ as well as $K = 100$ the TES was superior in the case of an underlying small or moderate effect ($\beta = 0.5; 1; 1.5$). The large variability of the primary study effect, which was caused by the low-N and low-K in the meta-analyses, resulted in an overall minor statistical power. A sufficient power (highlighted in bold) was only reached in conditions with a low or moderate underlying true effect ($\beta = 0.5, 1$). None of the CTs yielded a sufficient power. This picture changes if more studies were included in the meta-analysis. With $K = 1000$ most of the tests yielded a sufficient power. In particular, the FAT had a statistical power close to 100%, also under effect

[18] This means that an asymmetry between over- and under-caliper is not caused by publication bias rather than by an underlying effect distribution that is skewed in the caliper width.

heterogeneity. The PU and the TES failed to uncover *file-drawer* behaviour under effect heterogeneity, but performed well under homogeneity. PU was only able to discover *file-drawer* behaviour under low underlying true effects. The CTs profited the most from an increased K, the wider caliper (10, 15%) had a larger statistical power than the narrower ones but also had inflated false positive rates (see Table 6) that might invalidate the conclusions (grey shaded area). The narrower caliper had a sufficient power only in studies with no or small underlying effects (β = 0; 0.5). K = 100 and N = 500 decreased the power of all tests drastically. In this condition the FAT had the largest, but still not satisfactory power. With K = 1000 a sufficient power is yielded in conditions with a low overall effect (β = 0; 0.5).

The statistical power of the tests increased if the intent to engage in *file-drawer* behaviour is set to 100% (see Table 8). Overall, more publication bias tests achieved a satisfactory statistical power to detect publication bias. Also, in these conditions, the FAT dominated in 13 of 20 conditions. As before, neither the TES nor the PU were able to detect publication bias under effect heterogeneity. The TES was, furthermore, not able to detect publication bias with an underlying null effect. Similar to the 50% *file-drawer* condition, the CTs showed a drastically decreased power in conditions with K = 100.

The dominance of the FAT weakened when looking at the 50% *p-hacking* condition (see Table 9). Instead, the TES was besides the 15% CT superior under most conditions but had the advantage that its false positive rate was not inflated. The overall pattern was, however, quite similar: both PU and TES had almost no power to detect *p-hacking* under effect heterogeneity. Also, the statistical power was only satisfactory for PU when K = 100. With a large number of included studies, however, the power of the CT was close to, or even outperformed, the FAT, PU and the TES.

In the 100% *p-hacking* condition (see Table 10) the FAT caught up with the TES and yielded an increased power, especially in the case of $K = 100$. Despite the dominance of the 15% CT, the TES and the FAT closely followed. The CT had a similar strength to that demonstrated in the earlier conditions under effect heterogeneity and $K = 1000$. The underperformance of all tests in the condition with $N = 500$ and moderate underlying effects ($\beta = 1; 1.5$) is caused by the already existing significance of most results in this condition.

Overall, the FAT dominated under the *file-drawer* condition. The TES, in contrast, had a slightly higher statistical power than the FAT under the *p-hacking* condition without effect heterogeneity. However, the differences between both tests were quite small. The CTs performed well under the *file-drawer* as well as *p-hacking* condition with heterogeneous effect sizes and large numbers of studies included ($K = 1000$). Although the 10% and 15% caliper had the highest power to detect *p-hacking* these tests should not be applied due to their increased false positive rate.

In order to evaluate the tests on publication bias by their underlying risk factors (already significant results), rather than the conditions of the simulation, a regression model limited either on non-publication bias conditions or publication bias conditions was run. The dependent variable in both cases was the dummy variable (s in Table 4) for $p < 0.05$ for each publication bias test under examination. Linear probability models were used for the estimation.[19]

Table 11 shows the false positive rate in dependence of the number of studies included ($K = 100|1000$) and the effect heterogeneity (I^2). In the constant condition of a meta-analysis with $K = 100$ and no effect heterogeneity none of the tests had larger false positive rates than the expected 0.05. In particular, the TES and the 3% and 5% CTs were very conservative. A larger meta-

[19] Although the effect of the different conditions could be achieved with logistic regressions and average marginal effects, an intercept cannot be estimated in these models.

analytical sample increased the false positive rates for the TES and the CTs. The broadest 15% CT missed the expected significance threshold of 5%, with 7.8%. Increasing effect heterogeneity resulted in more conservative false positive rates for PU and 15% CT, and to a smaller extent also for the FAT, the TES and the 10% CT. The narrower 3% and 5% CTs were unaffected by effect heterogeneity. The overall influence of the varied conditions on the false positive rate was small, as can be seen by the small R^2 (< 1.7%).

The following regression model (Table 12) addresses the statistical power. Starting from the baseline condition of a meta-analysis with $K = 100$, a mean share of publication bias committed (32.6%. mean of the 50% and 100% publication bias condition, cf. Table 5), as well as successfully applied (18.8%, cf. Table 5) via a *file-drawer* procedure and no effect heterogeneity, the FAT had a superior power of 56.9%, followed by the TES (51.5%) and the PU (48.3%). The CTs performed worst and yielded only a power of 0.0%–38.6%. The underperformance of the CTs is largely explained by the small number of studies in the meta-analyses. With $K = 100$ hardly any study falls within the small caliper around the significance threshold. This limitation on just significant or non-significant effects also led to missing values, because without observations in the caliper no CT could be performed. The underperformance of the CT changed if 1000 studies were included, which improved the estimated power substantially, by 30.7–57.3 percentage points, while smaller calipers profited most. The FAT, as well as the TES and the PU, profited moderately from an increased number of studies, by 24.4, 23.8 and 16.5 percentage points, respectively. When focussing on the influence of heterogeneity in the meta-analyses the PU and the TES showed a drastic drop in power, by 6.5 and 6.4 percentage points, if the heterogeneity rose by 10 percentage points. This decrease in power shows that neither PU nor TES were able to cope with heterogeneity. In contrast, the FAT and the CTs actually showed a slight increased statistical

power. Varying the publication bias procedure from a *file-drawer* mechanism to *p-hacking,* which is less related to the standard error of the effect estimates, increased the power of PU, TES and the CTs. The CTs profited most, increasing the statistical power by around 18 percentage points. The TES and PU showed a smaller increase of power, by 7.5 and 4.8 percentage points. The FAT, in contrast lost about 11 percentage points compared to its power under a *p-hacking* procedure.

The structural difference between tests based on a continuous effect distribution (FAT, PU) and tests that focus only on a dichotomous classification (TES, CTs)[20] becomes clear when looking at the effect of the proportion of studies that underwent a publication bias treatment in the simulation and the proportion of studies that had a successful outcome after publication bias. Increasing the share of studies under publication bias lifted the power by 3.0 (FAT) and 5.1 (PU) percentage points. A 10 percentage point increase in studies successfully applying publication bias increases the power by 9.9 (FAT) and 10.3 percentage points (PU). The TES and the CTs, however, were only able to detect successful publication bias. An increase only in studies committing publication bias (whether successful or not) reduced the statistical power. Both tests were therefore not able to detect all outcomes of publication bias. This is especially problematic as non-successful publication bias may also increase the overall estimated effect in meta-analyses. All effects presented are statistically significant ($p < 0.05$).

In contrast to the influence of the varied conditions on the false positive rate, the influence on statistical power was substantial, varying from 30.6% in the case of the FAT to 57.2% for the PU. This finding underlines the fact that all publication bias tests have their strengths and weaknesses in specific conditions.

Discussion & Conclusions

[20] Significant or not (TES) over- or under-caliper (CTs).

In the simulation at hand, the performance of four different tests (PU, FAT, TES, CTs) was evaluated by a Monte Carlo simulation. Different conditions were varied: the underlying true effect size, including effect heterogeneity, the number of observations in the primary studies, the number of studies in the meta-analyses, and the degree of publication bias and its form as either *file-drawer* or *p-hacking*. Based on a Monte Carlo simulation with 100 different conditions and nearly 200,000 simulated meta-analyses the following recommendations can be made (cf. Table 13).

Firstly, for different research settings in a meta-analysis and with publication bias favouring only effects in one direction (directional), and irrespective of effect heterogeneity and the number of primary studies (K) in the meta-analysis or all significant estimates, the FAT is recommended due to its most consistent false positive rate as well as its superior statistical power in most conditions. Secondly, the TES should be preferred to the FAT if *p-hacking* is suspected. The application of the FAT and PU is limited in situations where the direction of publication bias is defined. If publication bias focusses not on positive or negative results but on both, the FAT loses its diagnostic value completely.

Therefore, thirdly, the TES is recommended under effect homogeneity if publication bias is suspected to be non-directional. Fourthly, in the case of heterogeneous effect sizes and a sufficient number of observations in the meta-analysis the 5% caliper provides the best trade-off between a conservative false positive rate and a decent statistical power. This test is therefore best used to identify publication bias in an effect heterogeneous discipline-wide setting which relies per definition on completely different underlying effects but offers enough studies to compensate for the low statistical power. Because the wider 10% and 15% CTs yield inflated false positive rates, at least in some conditions, they are not recommended to identify publication bias.

Identifying publication bias in substantial meta-analyses as well as focussing on publication as a general problem within the scientific domain is necessary in order to establish and retain trust in scientific results. Further research, however, should not only focus on the diagnosis of publication bias, but also examine the risk factors, either on the side of the authors or with regard to the incentive structure within the discipline (see for example Auspurg et al. 2014).

Acknowledgements

I thank Katrin Auspurg for her valuable comments on drafts of this article.

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Table 1 (on next page)

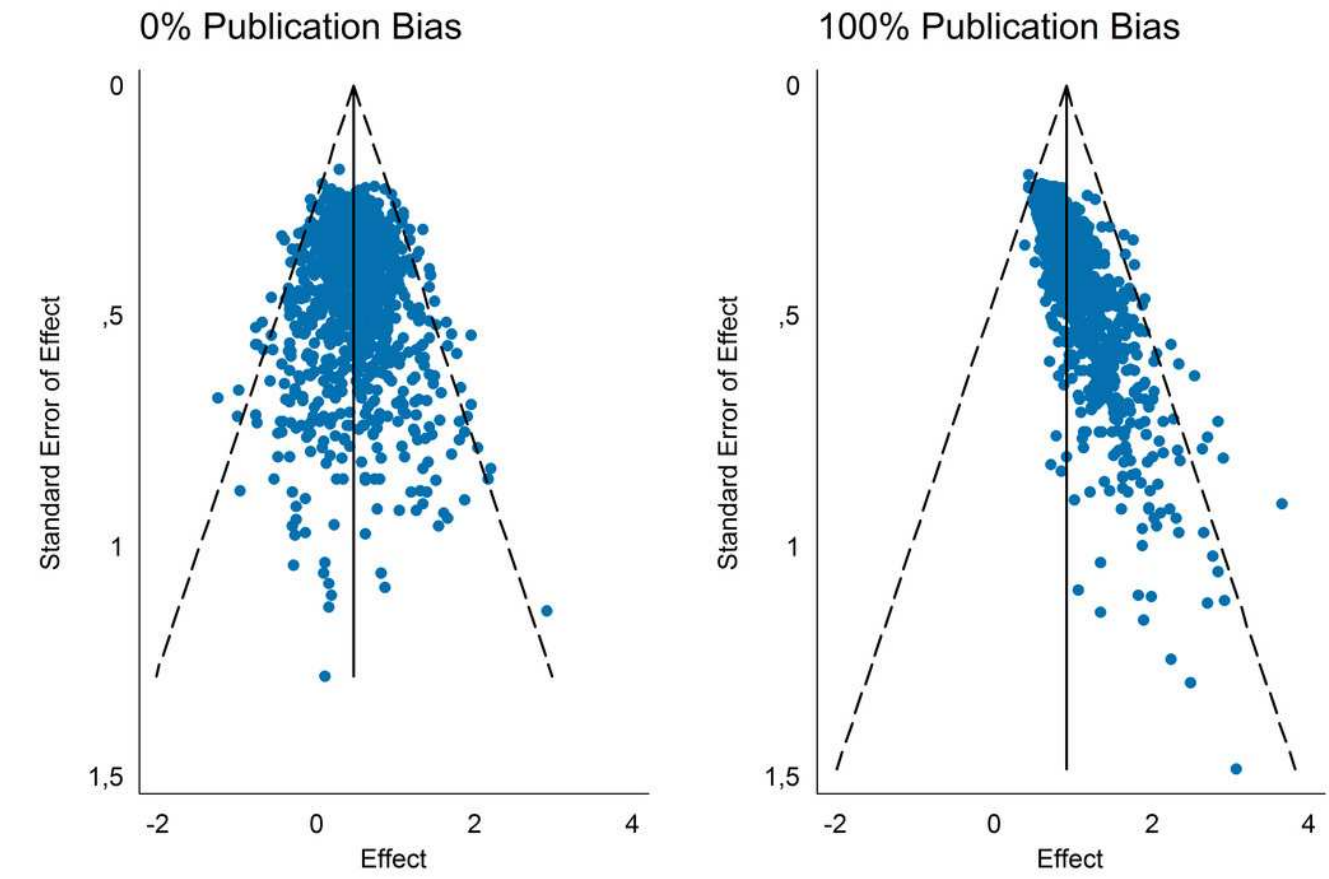
Truth table

Estimator	Data	
	No effect (false)	Effect (true)
No effect detected	**True negative (1-α)**	False negative (β)
Effect detected	False positive (α)	**True positive (1-β)**

1

Figure 1

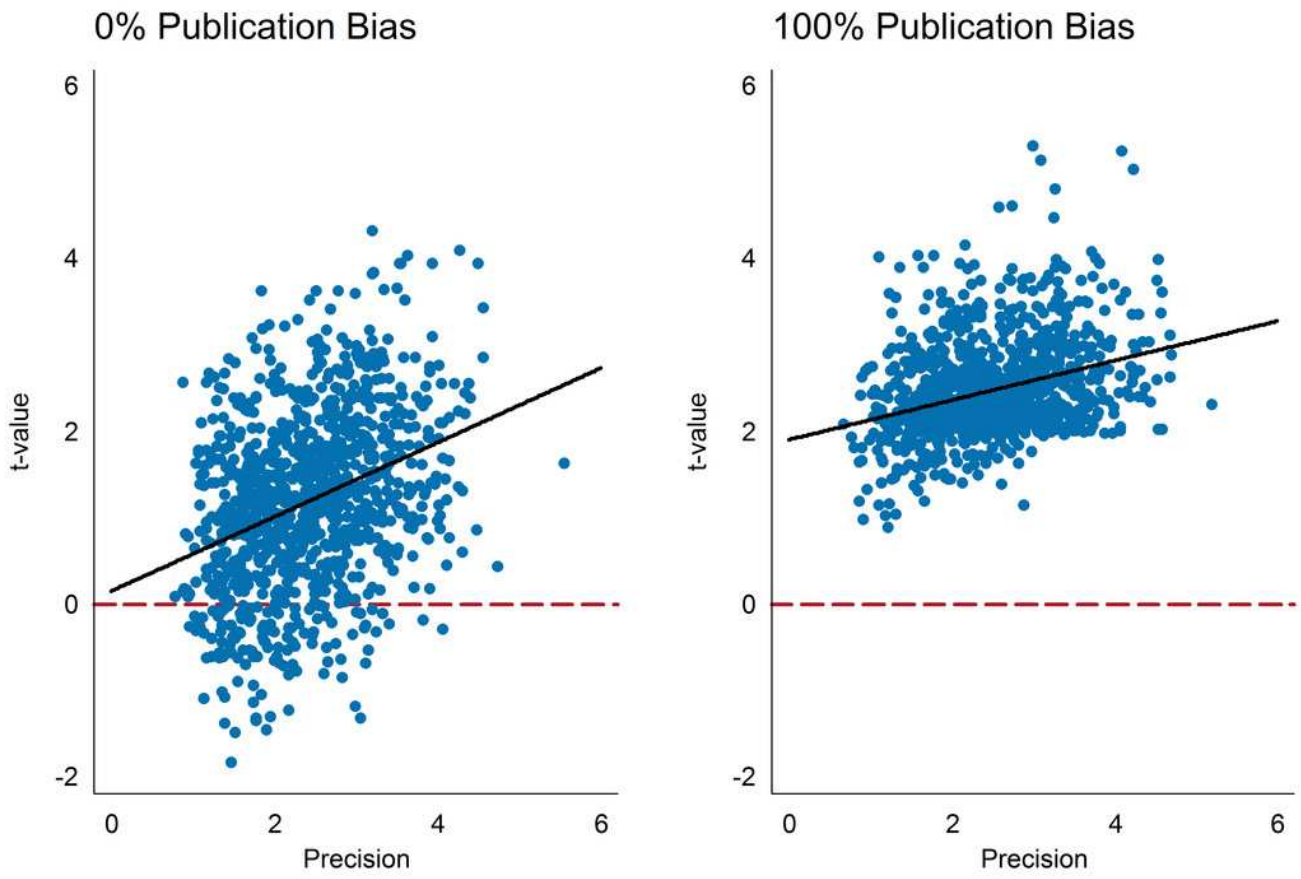
Funnel plot



es=0.5, k=1000, n=100, file-drawer

Figure 2

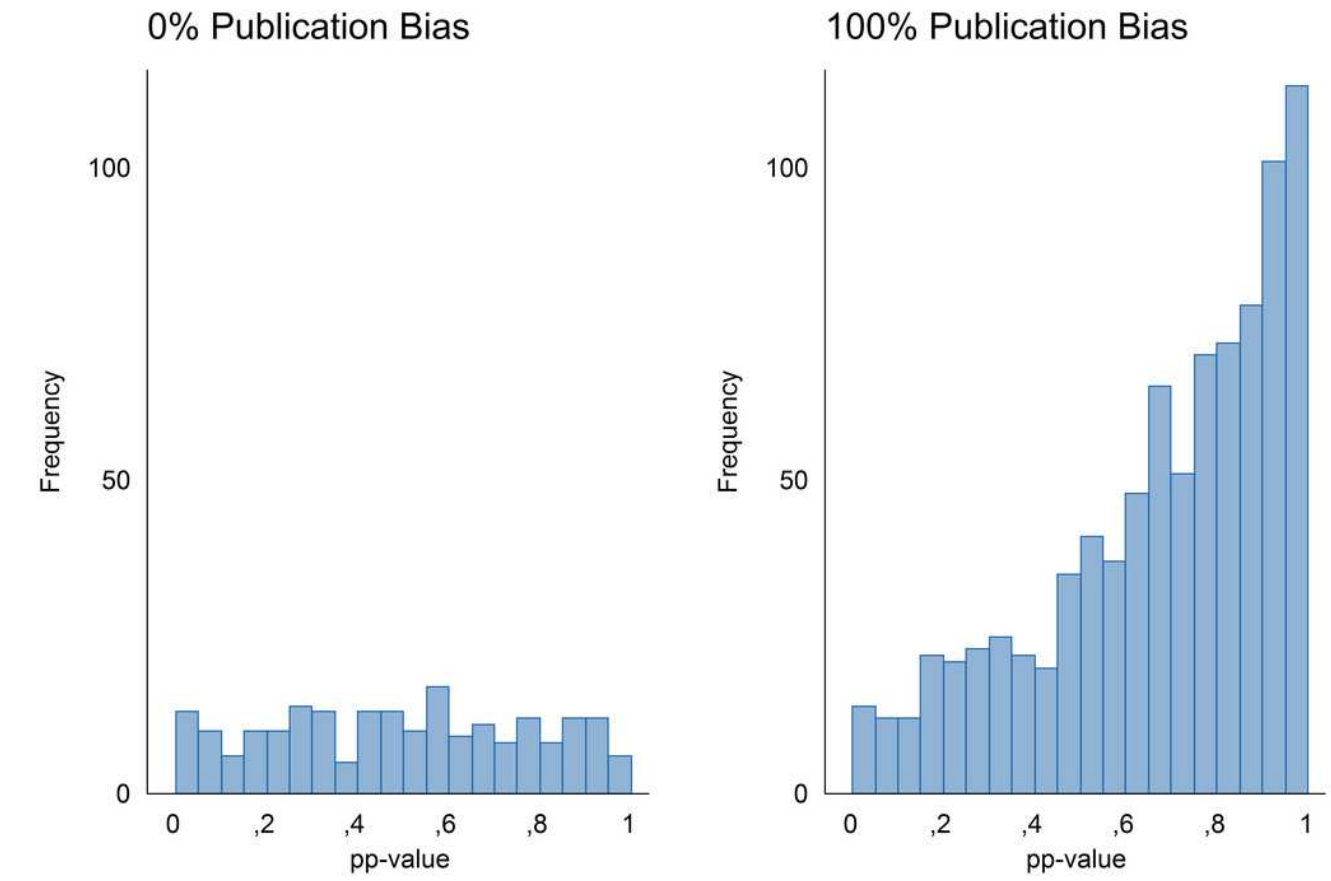
Funnel asymmetry test (FAT)



es=0.5, k=1000, n=100, file-drawer

Figure 3

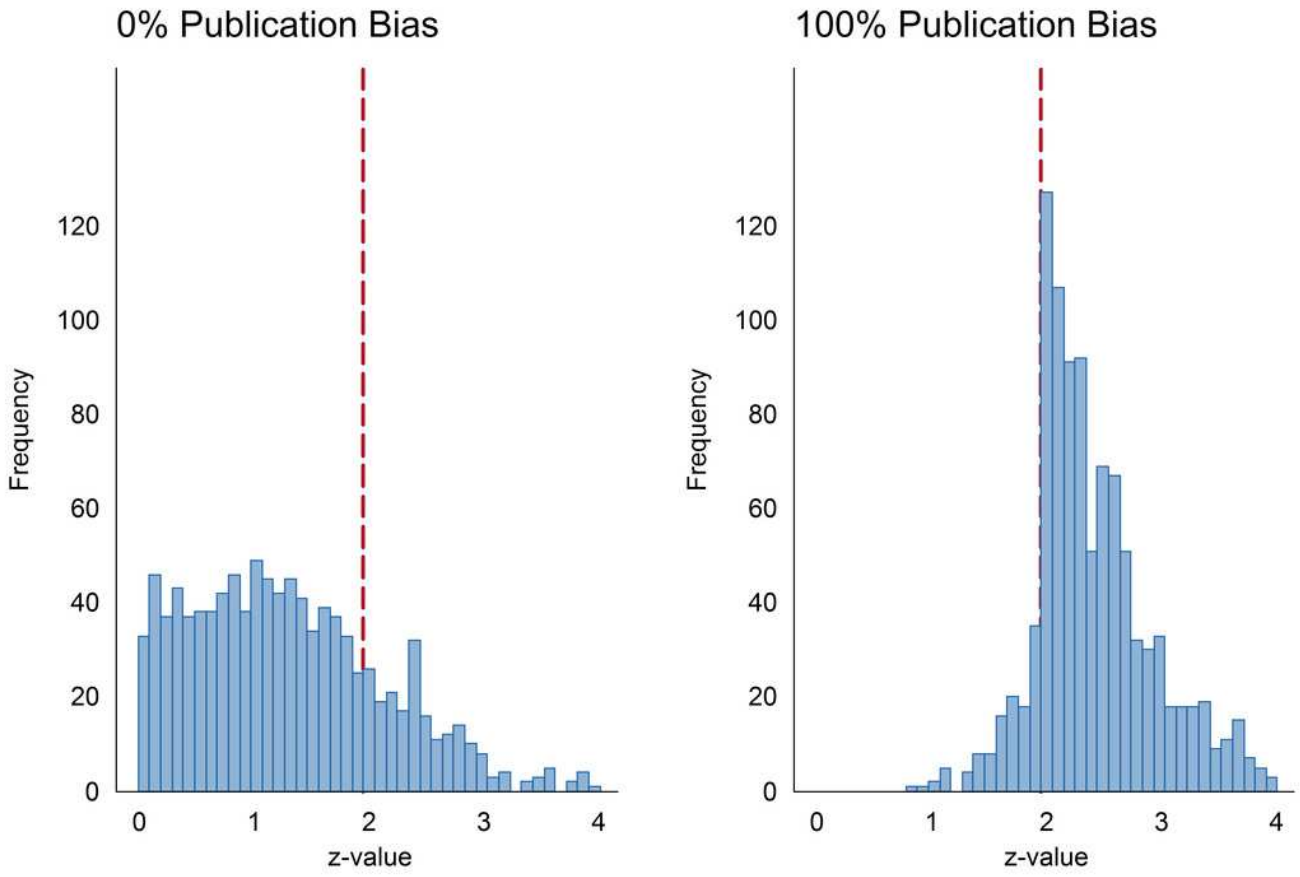
p-uniform (PU)



es=0.5, k=1000, n=100, file-drawer

Figure 4

Caliper test (CT with 5% caliper)



es=0.5, k=1000, n=100, file-drawer

Table 2 (on next page)

Publication bias tests in comparison

Test	Measurement level	Sample	Assumption	Limitation	Test method
FAT	Continuous [-∞,∞]	All	Cov(es, se) = 0	Only directed publication bias (PB) detectable	Weighted-Least-Squares
PU	Continuous [0,1]	$p < 0.05$, effects of same sign	Uniform or right skewed Skewness >= 0	Only directed PB detectable Only levelled (e.g. p=0.05) PB testable Effect homogeneity (FE-MA)	Skewness test (Gamma)
TES	Dichotomous [0,1]	All	E = O	Only levelled (e.g. p=0.05) PB Effect homogeneity (FE-MA)	Binomial test
CT	Dichotomous [0,1]	Threshold ± caliper width	P(UC) = P(OC)	Only levelled (e.g. p=0.05) PB testable	Binomial test

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Table 3 (on next page)

DGP of Monte Carlo simulation

Conditions	**Values**	**Functional form**	***N* (conditions)**
Data setup:			
1. True effects β:	$\boldsymbol{\beta = 0;0.5;1.0;1.5;Het}$		**5**
2. Number of observations *N*:	$\boldsymbol{\mu_N = 100;500}$	$\lvert NV(\mu_N,150)\rvert\ N > 30$	**2**
3. Number of studies *K*:	$\boldsymbol{K = 100;1000}$		**2**
Behavioural setup:			
4. Publication bias:	$\boldsymbol{PB = 0;0.5;1}$	$\beta > 0$ & $p < 0.05$ Take best result after maximum runs	**1+2*2=5**
4a. File-drawer	Draw new sample size *N*	*(maximum 9 additional samples)*	
4b. p-hacking	Run new analyses with same dataset	$y = \beta x + \gamma_j z_j + \varepsilon$ $z = 0.5x + 0.5y + \varepsilon$ max. 3 z's = 7 combinations	
			5*2*2*5 = 100

1

Table 4(on next page)

EM of Monte Carlo simulation

Implemented tests:

- Funnel asymmetry-test (FAT)
- p-uniform (PU)
- Test of excess significance (TES)
- Caliper Test (CT)
  3%-, 5%-, 10%-, 15%-caliper

➢ Seven tests

Outcomes:

- Statistical power (1-β))
- False positive rate (α)

$s = 0$ if $p \geq 0.05$
$s = 1$ if $p < 0.05$

$$\sum_{i=1}^{r} s_i / r \;\; if\; PB > 0$$

$$\sum_{i=1}^{r} s_i / r \;\; if\; PB = 0$$

➢ 100 different conditions for seven tests = 700 power/error estimates

Run Monte Carlo design:

- Power calculations in order to identify deviations from expected α and the statistical power for each element of the experimental design matrix (two-sided z-test of proportion)
- 3729 runs for each 20 α-error estimates (80% power, expected 0.05)
- 1545 runs for each 80 power estimates (80% power, expected 0.8)

➢ 198,080 meta-analyses containing 108,900,000 primary studies

Table 5 (on next page)

Descriptive results

		Runs	Mean	Median	Minimum	Maximum
Publication bias committed						
	50%	61,760	0.217	0.19	0	0.66
	100%	61,760	0.434	0.387	0	1
Publication bias successful						
file-drawer	50%	30.880	0.125	0.105	0	0.48
	100%	30.880	0.250	0.209	0	0.83
p-hacking	50%	30.880	0.125	0.112	0	0.49
	100%	30.880	0.251	0.225	0	0.84
Heterogeneity (I^2)						
	0%	158,464	0.043	0	0	0.542
	100%	39,616	0.733	0.766	0.066	0.908

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Table 6 (on next page)

False positive rate

0% FD/PH	PU	FAT	TES	3% CT	5% CT	10% CT	15% CT
N100/K100							
0.0	0.045	0.043	0.024	0.001	0.001	0.003	0.002
0.5	0.039	0.045	0.004	0.004	0.011	0.013	0.012
1.0	0.014	0.056	0.005	0.005	0.017	0.033	0.040
1.5	0.001	0.047	0.010	0.000	0.005	0.026	0.041
Het	0.000	0.042	0.001	0.002	0.012	0.021	0.025
N100/K1000							
0.0	0.032	0.051	0.036	0.020	0.012	0.000	0.000
0.5	0.020	0.046	0.005	0.031	0.023	0.007	0.001
1.0	0.008	0.048	0.003	0.043	0.049	**0.067**	**0.092**
1.5	0.002	0.046	0.013	0.040	0.049	**0.101**	**0.204**
Het	0.000	0.047	0.000	0.032	0.032	0.028	0.030
N500/K100							
0.0	0.051	0.051	0.024	0.000	0.002	0.003	0.002
0.5	0.025	0.050	0.002	0.010	0.019	0.039	0.043
1.0	0.000	0.045	0.007	0.000	0.000	0.001	0.010
1.5	0.000	0.047	0.000	0.000	0.000	0.000	0.000
Het	0.000	0.037	0.000	0.000	0.002	0.011	0.018
N500/K1000							
0.0	0.043	0.052	0.037	0.019	0.009	0.001	0.000
0.5	0.024	0.042	0.004	0.039	0.045	**0.070**	**0.104**
1.0	0.000	0.054	0.033	0.018	0.043	**0.108**	**0.244**
1.5	0.000	0.048	0.003	0.000	0.000	0.002	0.007
Het	0.000	0.035	0.000	0.031	0.036	0.038	0.035

Bold numbers > 0.05 at $p < 0.05$

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Table 7 (on next page)

Statistical power for 50% file-drawer

50% FD	PU	FAT	TES	3% CT	5% CT	10% CT	15% CT
N100/K100							
0.0	0.179	0.662	0.148	0.007	0.013	0.015	0.005
0.5	0.691	**0.822**	**0.912**	0.108	0.220	0.416	0.563
1.0	0.348	**0.823**	**0.881**	0.052	0.149	0.415	0.594
1.5	0.034	0.457	0.537	0.007	0.032	0.164	0.285
Het	0.000	0.370	0.042	0.032	0.082	0.220	0.321
N100/K1000							
0.0	0.720	**1.000**	0.737	0.029	0.019	0.001	0.000
0.5	**1.000**	**1.000**	**1.000**	**0.894**	**0.981**	**1.000**	**1.000**
1.0	**1.000**	**1.000**	**1.000**	**0.859**	**0.976**	**0.999**	**1.000**
1.5	0.521	**0.999**	**1.000**	0.530	0.763	**0.981**	**0.999**
Het	0.000	**0.997**	0.125	0.639	**0.839**	**0.977**	**0.996**
N500/K100							
0.0	0.238	0.245	0.104	0.007	0.010	0.013	0.003
0.5	0.580	0.499	0.736	0.080	0.201	0.442	0.671
1.0	0.001	0.110	0.039	0.000	0.001	0.003	0.021
1.5	0.000	0.056	0.000	0.000	0.000	0.000	0.000
Het	0.000	0.058	0.000	0.005	0.029	0.095	0.166
N500/K1000							
0.0	**0.905**	**0.950**	0.544	0.043	0.028	0.001	0.000
0.5	**1.000**	**0.999**	**1.000**	**0.911**	**0.987**	**1.000**	**1.000**
1.0	0.004	0.396	**0.874**	0.068	0.165	0.529	**0.826**
1.5	0.001	0.064	0.019	0.000	0.000	0.005	0.019
Het	0.000	0.214	0.000	0.373	0.569	**0.855**	**0.950**
Best / Satisfactory	3 / 4	14 / 6	8 / 7	0 / 3	0 / 4	2 / 6	3 / 7

Bold numbers: > 0.8 $p < 0.05$. Underlined: best estimator. Grey shaded: inflated false positive rate, cf. Table 6

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Table 8 (on next page)

Statistical power for 100% file-drawer

100% FD	PU	FAT	TES	3% CT	5% CT	10% CT	15% CT
N100/K100							
0.0	0.756	**1.000**	0.000	0.013	0.016	0.012	0.005
0.5	**1.000**	**1.000**	**1.000**	0.328	0.569	**0.891**	**0.981**
1.0	**0.958**	**1.000**	**1.000**	0.222	0.618	**0.962**	**0.999**
1.5	0.177	**0.975**	**1.000**	0.028	0.138	0.621	**0.898**
Het	0.000	**0.962**	**0.882**	0.124	0.278	0.595	0.790
N100/K1000							
0.0	**1.000**	**1.000**	0.000	0.047	0.021	0.002	0.000
0.5	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**
1.0	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**
1.5	**0.999**	**1.000**	**1.000**	**0.999**	**1.000**	**1.000**	**1.000**
Het	0.000	**1.000**	**1.000**	**0.981**	**0.999**	**1.000**	**1.000**
N500/K100							
0.0	**0.888**	**0.990**	0.000	0.011	0.012	0.015	0.003
0.5	**1.000**	**0.999**	**1.000**	0.351	0.755	**0.992**	**1.000**
1.0	0.001	0.221	0.235	0.000	0.000	0.006	0.047
1.5	0.001	0.059	0.000	0.000	0.000	0.000	0.000
Het	0.000	0.129	0.000	0.026	0.092	0.290	0.473
N500/K1000							
0.0	**1.000**	**1.000**	0.000	0.039	0.021	0.003	0.000
0.5	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**
1.0	0.093	**0.898**	**1.000**	0.233	0.669	**0.995**	**1.000**
1.5	0.000	0.108	0.145	0.000	0.000	0.009	0.041
Het	0.000	0.628	0.000	**0.829**	**0.957**	**1.000**	**1.000**
Best / Satisfactory	7 / 10	13 / 15	12 / 11	3 / 6	4 / 6	6 / 10	9 / 11

Bold numbers: > 0.8 $p < 0.05$. Underlined: best estimator. Grey shaded: inflated false positive rate, cf. Table 6

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Table 9(on next page)

Statistical power for 50% p-hacking

50% PH	PU	FAT	TES	3% CT	5% CT	10% CT	15% CT
N100/K100							
0.0	0.764	0.006	0.598	0.077	0.139	0.196	0.166
0.5	**0.870**	0.371	0.490	0.152	0.288	0.465	0.527
1.0	0.321	0.395	0.422	0.079	0.168	0.396	0.528
1.5	0.018	0.129	0.166	0.015	0.058	0.154	0.259
Het	0.000	0.369	0.020	0.068	0.139	0.292	0.380
N100/K1000							
0.0	**1.000**	0.000	**1.000**	0.767	**0.874**	**0.929**	**0.846**
0.5	**1.000**	**0.992**	**1.000**	**0.973**	**0.995**	**1.000**	**1.000**
1.0	**0.997**	**0.997**	**1.000**	**0.879**	**0.968**	**0.999**	**1.000**
1.5	0.175	0.503	**0.962**	0.505	0.733	**0.958**	**0.994**
Het	0.000	**0.994**	0.007	0.797	**0.942**	**0.997**	**0.999**
N500/K100							
0.0	**0.958**	0.000	**1.000**	0.211	0.394	0.659	0.769
0.5	0.806	0.437	**0.843**	0.112	0.285	0.602	0.784
1.0	0.000	0.066	0.028	0.000	0.001	0.013	0.046
1.5	0.001	0.058	0.000	0.000	0.000	0.000	0.000
Het	0.000	0.408	0.000	0.015	0.067	0.233	0.383
N500/K1000							
0.0	**1.000**	0.000	**1.000**	**0.995**	**1.000**	**1.000**	**1.000**
0.5	**1.000**	**0.999**	**1.000**	**0.966**	**0.999**	**1.000**	**1.000**
1.0	0.004	0.159	0.775	0.116	0.271	0.676	**0.908**
1.5	0.000	0.046	0.012	0.002	0.002	0.007	0.026
Het	0.000	**0.997**	0.000	0.772	**0.935**	**0.998**	**1.000**
Best / Satisfactory	6 / 7	4 / 5	7 / 8	0 / 4	1 / 7	3 / 8	11 / 8

Bold numbers: > 0.8 $p < 0.05$. Underlined: best estimator. Grey shaded: inflated false positive rate, cf. Table 6

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Table 10 (on next page)

Statistical power for 100% p-hacking

100% PH	PU	FAT	TES	3% CT	5% CT	10% CT	15% CT
N100/K100							
0.0	**0.999**	0.808	0.212	0.203	0.331	0.477	0.497
0.5	**1.000**	**0.997**	**0.992**	0.481	0.727	**0.918**	**0.964**
1.0	**0.854**	**0.916**	**0.985**	0.286	0.518	**0.835**	**0.947**
1.5	0.089	0.293	0.679	0.051	0.165	0.443	0.648
Het	0.000	**0.903**	0.390	0.235	0.436	0.724	**0.847**
N100/K1000							
0.0	**1.000**	**1.000**	**0.999**	**0.984**	**0.999**	**1.000**	**1.000**
0.5	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**
1.0	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**
1.5	**0.887**	**0.976**	**1.000**	**0.957**	**0.997**	**1.000**	**1.000**
Het	0.000	**1.000**	**0.999**	**0.997**	**1.000**	**1.000**	**1.000**
N500/K100							
0.0	**1.000**	**0.979**	**1.000**	0.561	0.791	**0.977**	**0.994**
0.5	**0.999**	**0.997**	**1.000**	0.525	**0.847**	**0.995**	**1.000**
1.0	0.001	0.106	0.138	0.000	0.002	0.036	0.119
1.5	0.000	0.051	0.000	0.000	0.000	0.000	0.000
Het	0.000	**0.916**	0.003	0.099	0.328	0.758	**0.917**
N500/K1000							
0.0	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**
0.5	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**	**1.000**
1.0	0.028	0.405	**1.000**	0.438	0.804	**0.996**	**1.000**
1.5	0.000	0.061	0.028	0.000	0.002	0.022	0.072
Het	0.000	**1.000**	0.000	**0.998**	**1.000**	**1.000**	**1.000**
Best / Satisfactory	8 / 11	7 / 14	9 / 12	4 / 8	6 / 9	8 / 13	12 / 15

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Table 11(on next page)

Regression analysis false positive rate

	PU	FAT	TES	3% CT	5% CT	10% CT	15% CT
K = 1.000	-0.005***	0.000	0.006***	0.026***	0.023***	0.026***	0.050***
(ref. K = 100)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
I^2 [+10 percentage points]	-0.003***	-0.001***	-0.001***	0.000	0.000	-0.001***	-0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant [a]	0.023***	0.049	0.010***	0.003***	0.008***	0.019***	0.028***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	73,960	74,560	74,560	62,644	66,546	69,718	70,936
R^2	0.005	0.000	0.002	0.010	0.007	0.006	0.017

[a] Test H0: constant = 0.05; Standard errors in parentheses; *** p<0.01. ** p<0.05. * p<0.1

Table 12 (on next page)

Regression analysis statistical power

	PU	FAT	TES	3% CT	5% CT	10% CT	15% CT
K = 1.000	0.165***	0.244***	0.238***	0.573***	0.513***	0.382***	0.307***
(ref. K = 100)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
I^2 [+10 percentage points]	-0.065***	0.001**	-0.064***	0.006***	0.005***	0.008***	0.010***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
p-hacking	0.048***	-0.110***	0.075***	0.179***	0.187***	0.186***	0.177***
(ref. file-drawer)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Comitted PB [+10ppts]	0.051***	0.030***	-0.065***	-0.035***	-0.053***	-0.073***	-0.084***
(ref. mean = 32.5%)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Successful PB [+10ppts]	0.103***	0.099***	0.221***	0.162***	0.193***	0.224***	0.234***
(ref. mean = 18.8%)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant [a]	0.483***	0.569***	0.515***	-0.002***	0.125***	0.300***	0.386***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	123,520	123,520	123,520	107,736	111,315	115,243	117,207
R^2	0.572	0.306	0.473	0.497	0.483	0.457	0.446

[a] Test H0: constant = 0.05; Standard errors in parentheses; *** p<0.01. ** p<0.05. * p<0.1

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Table 13 (on next page)

Recommended tests for different conditions

	Directional		Non-directional	
	Small *K*	Large *K*	Small *K*	Large *K*
Homogenous	FAT	FAT / TES	TES	TES
Heterogenous	FAT	FAT	CT	CT

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