Too Good to be False: Nonsignificant Results Revisited

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Author note

This paper is version controlled and all research files are publicly available at <https://osf.io/qpfnw/>. Analysis code was pre-registered.

Abstract

\*placeholder\*

*Keywords:* p-value, nhst, underpowered, effect size, fisher

Ha Chris

marcel, kun je je feedback op de methods section ietwat toelichten? Ik begrijp niet waarom je zegt dat dingen die in de methods section staan naar de methode sectie moeten. Ze staan daar immers al?

Je methodesectie bevat nu stukken die in drie verschillende secties thuishoren:

- theoriesectie

- methodesectie

- resultatensectie

Daar waar ik zeg 'dit hoort in methoden' hoort thuis in methodesectie

Hierdoor begrijp ik deze comment denk ik ook niet:"*Die mess moet je volgende keer echt voorkomen. Als je grotere projecten gaat doen, gaat dit een keer mis. Structureer! Eerst goed nadenken  structuur  doen, en af en toe wat opruimen."*(ik interpreteer dit nu voor de methode sectie, weet niet of dit ook voor intro geld)

Dit geldt niet voorde thesis maar meer als iets waar je in het vervolg, in je toekomstige carriere, rekening mee kunt proberen te houden. Als een project klein is behoud je het overzicht sowieso, maar als het groter wordt of je hebt er meerdere dan loont het georganiseerd te werken.

Ik heb wel een duidelijke structuur in gedachten voor de methode sectie namelijk, maar blijkbaar komt deze niet over?

Mijn commentaar ging vnl over het stuk. En daar moet je de gebruikelijke structuur aanhouden. En belangrijk, de onderzoeksvragen/hypothesen/doelstellingen moetn expliciet worden genoemd en structureren als het goed is je tekst.

Ik ga eerst op meta-niveau kijken (effect distributions) en vervolgens inzoomen op p-waarde verdeling binnen papers (micro-niveau; P-value distribution).

Waarom? Ik weet wel waarom, maar de lezer niet..

De laatste heeft dan weer paar subdelen, zoals de power simulaties van de toets en de toets uitgevoerd op de papers. A.d.h.v. de headings probeer ik deze scheiding duidelijk te maken. Ik begrijp dat er nog het een en ander te verbeteren valt, maar een mess is dit denk ik niet [ik kan niet goed plaatsen of je het echt een zooitje ongeregeld vindt of gewoonweg slordig]

Nee man, rustig. Die comment ging over toekomst...

Ik zou het fijn vinden om komende week de comments kort door te spreken, zodat ik alle nuances duidelijk meekrijg. De comments, zoals ze nu staan, missen wat context/gedachtegangen als ik ze lees (terwijl ze voor jou mogelijk wel duidelijk zijn), waardoor ik mij er niet altijd in kan vinden (dit geld alleen voor methode sectie, intro comments kan ik me prima in vinden).

Morgen plannen we hoe verder, en ik was van plan systematische bijeenkomsten tussen ons twee te plannen mbt schrijven. :-)

Groetjes,

Marcel

**Too Good to be False: Nonsignificant Results Revisited**

Popper's (1959/2005) falsificationism has become deeply rooted in current day philosophy of science, which, in tandem, has caused hypothesis testing to become ubiquitous in the social sciences. Falsification serves as one of the main demarcating criteria in the social sciences, which stipulates that a hypothesis is required to have the possibility of being proven false. This is the foundation of the theoretical logic of scientific hypothesis testing. Statistical hypothesis testing is an operationalization of scientific hypothesis testing, but these diverge in their logical rigor. Whereas outcomes of theoretical scientific hypothesis tests are deterministic, its operationalization makes outcomes probabilistic.

In quantitative research, statisticalhypothesis tests are often used. The most common statisticalhypothesis tests are Null Hypothesis Significance Tests (NHST), where a point hypothesis[[1]](#footnote-2) is tested against an alternative hypothesis. The probabilistic nature of specifically these types of statistical hypothesis tests yields individual results that are (1) inconclusive, (2) incomplete, and (3) error-prone. Inconclusive results can be seen in structural equation modeling, where different models yield identical results (Kline, 2011). Results are incomplete, as a *P*-value does not state the probability of a hypothesis (Cohen, 1994), and results are error-prone due to sampling fluctuations. Table 1 gives an overview of possible results based on NHST. These connotations do not invalidate the theoretical model of statistical hypothesis testing, but require attention as results are often misunderstood or misinterpreted (for a comprehensive review see Harlow, Mulaik, & Steiger, 1997). The errors that can result from statistical hypothesis testing are the focus of this paper, and can be categorized into false positives and false negatives. More specifically, the paper pertains to the prevalence rates of such errors.

False positives have been widely discussed in recent years, as events and papers have clearly indicated the plausibility of widespread false positive findings in the literature. Extreme and (hopefully) incidental fraud cases (e.g., Hauser, Stapel, Smeesters, Sanna, Ruggiero, and Förster) presented clearly false positive results; papers have also indicated that widespread questionable research practices (QRPs; John, Loewenstein, & Prelec, 2012) can have dramatic effects on false positive rates (Simmons, Nelson, & Simonsohn, 2011). Questionable editorial practices (QEPs; LeBel et al., 2013), publication bias (Francis, 2012a, 2013), systemic low power (Bakker, Van Dijk, & Wicherts, 2012), and excessive degrees of significance (Francis, 2012b, 2014; Schimmack, 2012; Sterling, 1959; Sterling, Rosenbaum, & Weinkam, 1995) are additional factors that could have led to high false positive rates in the academic literature. Failures to replicate effects, such as the elderly priming effect (original: Bargh, Chen, & Burrows, 1996; replication: Harris, Coburn, Rohrer, & Pashler, 2013), physical and moral cleansing effect (original: Zhong & Liljenquist, 2006; replications: Earp, Everett, Madva, & Hamlin, 2014; Fayard, Bernstein, & Roberts, 2009), and the ESP effect (original: Bem, 2011; replication: Ritchie, Wiseman, & French, 2012) cast serious doubt on the truth of these effects (e.g., Yong, 2012). Additionally, the need for innovative results in journals (Trafimow, 2013) could inflate the false positive rate in the literature further. It should be noted that the field of psychology took up these concerns, is open to changing its practices (Fuchs, Jenny, & Fiedler, 2012), is investigating remedies (e.g., Murayama, Pekrun, & Fiedler, 2013), and is implementing new standards for research practices (e.g., Brandt et al., 2013; Eich, 2013; Journal of Experimental Social Psychology, 2014).

False negatives have been discussed less explicitly, as the problems for false positives were more salient and more pressing. Nonetheless, power (i.e., true positive rate) has implicitly been part of the discussion, because replication requirements were being discussed for detecting possible false positives (e.g., Asendorpf et al., 2013; Brandt et al., 2013). Power is of importance, because the power for psychological studies has been estimated to be as low as 35% (Bakker et al., 2012), which implies a 65% false negative rate. This is substantial, given that this exceeds all but one of the false positive rates due to QRPs (81.5% in Table 1; Simmons et al., 2011). Systemic low power in psychology has been indicated before (e.g., Cohen, 1962), but this has had almost no effect on increasing power in the field (Sedlmeier & Gigerenzer, 1989). Discussing false negatives is of importance, because such false negatives are less easily discovered than false positives and causes research lines to be stopped prematurely, whereas false positives prolong research lines (Fiedler, Kutzner, & Krueger, 2012). Research has focused on the prevalence rate of false positives; less is known about the prevalence rate of false negatives.

This paper inspects the prevalence of false negatives, by focusing on null findings from statistical hypothesis tests. Null findings are often interpreted as the null hypothesis being true (Nickerson, 2000), which could be caused by unconscious psychological fallacies such as the law of small numbers (Tversky & Kahneman, 1971) or overconfidence about the power of a study (Bakker, 2014; Greenwald, 1975). Such fallacies increase the likelihood that null results are interpreted as true negatives, which is why we hypothesize to find widespread indication for false negative results in the literature. If null results are true negatives, *P*-values are uniformly distributed (Murdoch, Tsai, & Adcock, 2008; Sackrowitz & Samuel-Cahn, 1999). Our research investigates whether reported statistics from 8 flagship journals corroborates null distributions, or indicates possible false negative results.

**Methods**

**Data summary**

The dataset was retrieved from the Open Science Framework[[2]](#footnote-3), and includes APA style test statistics extracted from 8 journals. These test statistics were extracted with statcheck (Epskamp & Nuijten, 2013) and originally included a total of XXXX test results (*t*, *r, F, Z,* χ2 and Wald values). As only *t*, *r,* and *F-*values allow for direct and comparable effect size computation, these test results were selected (XXXX results; XX% of original). Table 2 summarizes the selected data used for the analyses in this paper. For a more extensive description of the sampling method underlying the dataset, see the Open Science Framework page (Footnote 2).

**Effect distribution**

The selected *t*, *F*, and *r*-values are readily computed into effect sizes, which form the observed effect distributions. The effect size metric used throughout the analyses is explained variance. For the selected *r*-values, this only requires taking the square (i.e., *r*2). Selected *F* and *t-*values are converted to effect sizes simultaneously, as squared *t-*values are *F-*values. The formula used to compute these effect sizes



where, for squared *t*-values, *df1* equals 1, and *df2* equals the original degrees of freedom from the *t*-test. Adjusted effect sizes are also computed (see Appendix for adjusted formula).

The theoretical null distribution across all test results was simulated. This was done by (1) randomly sampling 1,000,000 nonsignificant test results from the dataset with replacement, (2) sampling nonsignificant *P*-values uniformly between 0 and 0.95 (i.e., α = .05), representing the null, and (3) computing the effect size that accompanies the degrees of freedom for the simulated test results. Effect size computation is the same as computing the observed effect sizes, except no adjusted effect sizes are computed.

Subsequently, these observed distributions were compared with the theoretical null distribution, with the Kolmogorov-Smirnov test. The Kolmogorov-Smirnov test is a non-parametric goodness-of-fit test for distributions, which is based on the maximum absolute deviation between the independent distributions being compared (denoted D; Massey Jr., 1951). In this specific case, the fit of the observed effect size distributions (overall, and per journal) with the null effect distribution was inspected. Differences in distributions between journals were not subjected to inferential significance tests, as the data are the population of *t*, *r,* and *F*-values reported in the journals. These procedures will indicate whether the observed nonsignificant effects differ from the null distribution, indicating false negatives, and whether there are differences in effects reported across different journals.

***P*-value distribution**

Uniformity of *P*-values is tested with the Fisher method (Fisher, 1932), which only requires a set of *P*-values. This test is defined as



where *pi*is a vector of independent *P-*values, and *k* is the number of values in this vector. The resulting chi-square test statistic has twice the number of degrees of freedom as the number of *P*-values (i.e., 2*k*). Results of this test indicate the degree of deviation from uniformity. Related tests, which have been previously applied to test publication bias, are typically used with an alpha level of 10% (e.g., Francis, 2012b; Ioannidis & Trikalinos, 2007), and equivalent alpha will be used for the Fisher method throughout this paper.

In the current paper, we inspect whether nonsignificant *P*-values deviate from uniformity. As selecting nonsignificant *P*-values restricts the range, the selected *P*-values are transformed back into the original state space of [0; 1], by computing adjusted *P*-values with



where *pi* is the vector of selected *P*-values, and α is the selected significance threshold. Note this alpha is the significance threshold for the original test, not for the Fisher method, and was assumed to be 5% across all test results.

**Power simulations.** To simulate the power of the Fisher method, simulations were conducted across three factors (i.e., *N,* effect size, and the number of *P*-values). These factors were used to simulate *t*-test results, with 10,000 iterations for each condition. As mentioned, squared *t*-values are *F*-values, because of which results are readily generalizable to *F*-tests. We do not consider simulation results to be directly dependent on the number of groups, as power is a function of effect, *N*, and α — not group number.[[3]](#footnote-4)

Factorial specification resulted in 1600 conditions. Levels for *N* were set at 25, 50, 100, and 150 (total: 4 levels). Effect sizes in the form of eta-squared were specified at 0.00 and 0.01 through 0.95, in 0.01 steps (i.e., 0.01, 0.02, 0.03, …, 0.95; total: 100 levels). The number of *P*-values was specified at 2, 4, 7 and 10 (total: 4 levels). Per condition, 10,000 iterations were run, where each iteration yielded a *P*-value for the Fisher test. The proportion of significant iterations (α = .10) indicates sensitivity of the test for effect size zero, and power for all other effect sizes. If the resulting power in one condition was 99.5% or higher, power for subsequent effect sizes was automatically overridden to be 1 (given *N* and number of results), because larger effect sizes result in higher power (Aberson, 2010). This was done to counteract increases in runtime and decreases in precision for large non-centrality parameters (Lenth, 1989).

For each condition, 9 steps were necessary to simulate the result of the Fisher method. First, a critical value under the null distribution was needed (α = .05), which was readily computed by using the degrees of freedom (i.e., *N*-1). Second, the degrees of freedom and effect size were used to compute the non-centrality parameter. The non-centrality parameter (i.e., δ) is computed as (Smithson, 2001; Steiger & Fouladi, 1997)



where



This non-centrality parameter adjusts the null distribution for the population effect (e.g., shifts it to the right, see Figure 1). Third, for the population distribution, the area under the curve for a nonsignificant result was determined (i.e., β). Fourth, a value was uniformly drawn between 0 and the β value that resulted from step three. Fifth, the accompanying *t*-test-value was computed, which was, sixth, used to compute the *P-*value under the null distribution. Sixth, this nonsignificant *P*-value was transformed with Equation 3. Seventh, for each set of *P*-values per condition per iteration, a Fisher test was conducted and the resulting *P*-value saved. Finally, sensitivity/power was calculated as the proportion of significant results (α = 0.10).

**Effect estimation.** The simulation procedure can be used on observed results to estimate an effect size. Power estimates Summing the power values for each test statistic in the dataset, given an effect size, results in the expected value of significant Fisher tests for that effect size. Subsequently, the observed number of significant Fisher tests can be compared with the expected number of significant Fisher tests for certain effect sizes. Depending on the precision of the simulations, with regards to effect sizes, an effect estimate can be made in a certain

**Use case.**

**Results**

**Effect distribution**

***P*-value distribution**

**Power simulations.** Simulations indicated that the Fisher method is highly powerful when ordinally testing for the presence of an effect. Figure 3 visually summarizes the results, and clearly indicates that power increases as a function of the factors. In other words, the power of the Fisher method increases as *N* per individual result increases, as the number of results increases, and as the effect size increases. This is in line with power theory on an individual test level (Aberson, 2010).

**Effect estimation.**

**Use case.**

**Discussion**

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Footnotes

Table 1

|  |  |  |
| --- | --- | --- |
|  | H0 | H1 |
| ‘H0’ | 1-α  [0.95] | β  [0.20] |
|  | *True negative* | *Type II error* |
| ‘H1’ | α  [0.05] | 1-β  [0.80] |
|  | *Type I error* | *True positive* |

*Note.* Columns indicate the true situation in the population, rows indicate the statistical conclusion based on sample data. The true positive rate is also called power, and the true negative rate is also called XXXX. Values in square brackets are conventionally acceptable values.

Table 2

Table 3

*Note.* Alpha was assumed to be .05 to determine significant or not.

*Figure 1*

Visual depiction of steps 1 through 6 for simulation procedure of Fisher tests.

*Figure 2*

Observed effects versus simulated null effects.

*Figure 3*

Plots of the power (y-axis) across simulation results. Dashed lines indicate medium and large effect sizes, respectively.

1. This point hypothesis is commonly 0 (i.e., nil hypothesis), but can be any point value. [↑](#footnote-ref-2)
2. <https://osf.io/dzrtf/> [↑](#footnote-ref-3)
3. More groups does decrease power indirectly, because of decreased sample size per group, keeping all else constant. [↑](#footnote-ref-4)