1   Beyond p-values: Utilizing Multiple Methods to Evaluate Evidence

Abstract

Null hypothesis significance testing (NSHT) is cited as a threat to validity and reproducibility. While many individuals suggest we focus on altering the *p*-value at which we deem an effect significant, we believe this suggestion is short-sighted. Alternative procedures (i.e., Bayesian analyses and Observation Oriented Modeling; OOM) can be more powerful and meaningful to our discipline. However, these methodologies are less frequently utilized and are rarely discussed in combination with NHST. Herein, we discuss three methodologies (NHST, Bayesian Model comparison, and OOM), then compare the possible interpretations of three analyses (ANOVA, Bayes Factor, and an Ordinal Pattern Analysis) in various data environments using a frequentist simulation study. We find that changing significance thresholds has little effect on conclusions. We find that evaluating multiple estimates as evidence of an effect allows for more robust and nuanced reports of findings. These findings suggest the need to redefine evidentiary value and reporting practices.

Keywords: null hypothesis testing, p-values, Bayes Factors, Observation Oriented Modeling, evidence

₁₇   Beyond *p*-values: Utilizing Multiple Methods to Evaluate Evidence

₁₈   Recent events in psychological science have prompted concerns within the discipline

₁₉ regarding research practices and ultimately the validity and reproducibility of published

₂₀ reports (Etz & Vandekerckhove, 2016; Lindsay, 2015; Open Science Collaboration, 2015; van

₂₁ Elk et al., 2015). One often discussed matter is over-reliance, abuse, and potential hacking of

₂₂ *p*-values produced by frequentist null hypothesis significance testing (NHST), as well as

₂₃ misinterpretations of NHST results (Gigerenzer, 2004; Ioannidis, 2005; Simmons, Nelson, &

₂₄ Simonsohn, 2011). We agree with these concerns and believe that many before us have

₂₅ voiced sound, generally accepted opinions on potential remedies, such as an increased focus

₂₆ on effect sizes (Cumming, 2008; Lakens, 2013; Maxwell, Lau, & Howard, 2015; Nosek, Spies,

₂₇ & Motyl, 2012). However, other suggestions have been met with less enthusiasm, including

₂₈ an article by Benjamin et al. (2018) advocating that researchers should begin thinking only

₂₉ of *p*-values less than .005 as "statistically significant", thus changing α levels to control Type

₃₀ I error rates. In another recent article, Trafimow et al. (2018) critiques this suggestion to

₃₁ broadly lower the α level to .005, and alternatively suggest that findings should be weighted

₃₂ on the basis of evidence accumulation from multiple studies. Additionally, Pericchi and

₃₃ Pereira (2016) promote the use of fluctuating α levels as a function of sample size to assist

₃₄ with these errors. Given this unsettled notion with significance testing, We argue it is not

₃₅ the threshold, or critical *p*, that needs to be rethought when seeking evidence, but rather if a

₃₆ *p*-value should be utilized at all, and, if so, what that *p*-value can tell you in relation to other

₃₇ indicators. While NHST and *p*-values may have merit, researchers have a wealth of other

₃₈ statistical tools available to them. We believe that improvements may be made to the

₃₉ sciences as a whole when individuals become aware of the tools available to them and how

₄₀ these methods may be used, either alone or in combination, to strengthen understanding and

₄₁ conclusions. These sentiments have been shared by the American Statistical Association who

₄₂ recently held a conference focusing on going beyond NHST, expanding their previous stance

⁴³ *p*-values (Wasserstein & Lazar, 2016).

⁴⁴   Therefore, we undertook this project to show researchers how two alternative ⁴⁵ paradigms compare to NHST in terms of their methodological design, statistical ⁴⁶ interpretations, and comparative robustness. Herein, we will discuss the following ⁴⁷ methodologies: NHST, Bayes Factor comparisons, and Observation Oriented Modeling. The ⁴⁸ three approaches will be compared via simulated data using a three timepoint repeated ⁴⁹ measures design with a Likert-type scale as the outcome variable. One goal of this study is ⁵⁰ to introduce social scientists to Observation Oriented Modeling (OOM), as it is a relatively ⁵¹ new paradigm that is readily interpretable and, as we will show, useful in these contexts. ⁵² Additionally, we aim to discuss the conclusions these three methods reach given the same ⁵³ data, and to compare how often these methodologies agree within different data ⁵⁴ environments (i.e., given varying sample sizes and effect sizes). We hope that by discussing ⁵⁵ these methodologies in terms of a simple statistical analysis researchers will be able to easily ⁵⁶ compare and contrast methodologies. For this discussion, it is important to understand their ⁵⁷ historical background, procedural steps, and limitations, which are outlined below. After this ⁵⁸ discussion, we describe a simulation study comparing methodologies and α criteria, and end ⁵⁹ with potential implications for researchers.

<p style="text-align:center;">⁶⁰ Null Hypothesis Significance Testing</p>

⁶¹ **History**

⁶²   Many attribute the frequentist NHST procedure to Ronald A. Fisher (Fisher, 1932). ⁶³ However, Fisher's ideas are a far cry from the NHST procedure implemented today. Fisher ⁶⁴ believed in creating one "null" hypothesis, which he described as a hypothesis to be ⁶⁵ "nullified", or shown incorrect, not as a zero-difference hypothesis (Lehmann, 2011). He also ⁶⁶ believed that the use of any omnibus level of significance showed a "lack of statistical

⁶⁷ thinking" (Gigerenzer, Krauss, & Vitouch, 2004). He instead believed we should report the

⁶⁸ exact significance value of a test and let others make their own decision about the claims,

⁶⁹ which is more in line with the typical reporting recommendations provided by the American

⁷⁰ Psychological Association (American Psychological Association, 2010). Fisher spoke of this

⁷¹ work to William Gosset, the man who created the Student's t-test and contributed work on

⁷² the correlation coefficient (Lehmann, 2011). Gosset in turn discussed the idea of an

⁷³ alternative hypothesis, a piece not included in Fisher's procedure, with decision theorist

⁷⁴ Egon Pearson.

⁷⁵   From this discussion, Egon Pearson and Jerzy Neyman created Neyman-Pearson

⁷⁶ decision theory. This theory consists of two hypotheses (i.e., null and alternative) and a

⁷⁷ binary decision criteria (i.e., significant or not, Lehmann, 1993). However, this procedure

⁷⁸ created the possibility of researcher decision errors (Dienes, 2008). A researcher may falsely

⁷⁹ reject the null hypothesis (Probability of Type I error, α) or falsely fail to reject the null

⁸⁰ (Probability of Type II error, β). α levels set the binary decision criteria, which are used as

⁸¹ the critical p-value for hypothesis testing (i.e., $p < .05$), and are thus seen as evidence to

⁸² reject the null hypothesis. β and power are inherently linked (Power = 1-β), so as the

⁸³ likelihood of finding a true effect increases beta decreases (Maxwell & Delaney, 2004).

⁸⁴ Although α values can be chosen to be quite small, and methods (such as decreasing error

⁸⁵ variance or using a one-tailed test in contrast to a two-tailed test) can decrease β values as

⁸⁶ well, a researcher can never know if they have made the correct decision, or a decision error.

⁸⁷ Thus, Neyman and Pearson clearly state that a hypothesis should not be blindly supported

⁸⁸ based solely on the estimates of one statistical test, and that replication and reproduction of

⁸⁹ results are imperative. The recent work of the Open Science Collaboration (2015) has also

⁹⁰ highlighted the need for replication studies and interpretation of results in an appropriate

⁹¹ context. Additionally, Neyman and Pearson emphasized that use of set αs and βs is illogical

⁹² and sought instead for researchers to adjust their analysis to the needs of the particular task

⁹³ at hand (Gigerenzer, 2004).

Typical NHST Procedure

Neither Fisher's hypothesis testing, nor Neyman-Pearson decision theory quite match the NSHT procedure as it is taught and applied today. Psychologists have largely adopted an amalgamation of the two approaches. Here, we attempt to outline what we believe is the most appropriate way to carry out the traditional NHST procedure in the context of a repeated measures ANOVA with three levels, although we note that this set of steps is not necessarily how researchers carry out the procedure in practice:

1) Create two hypotheses, one to be "nullified" and one "alternative" hypothesis. Within this repeated measures framework, most researchers would define a null hypothesis (H_0) that indicates of all three time point population means are equal. The alternative hypothesis (H_A) would then be that not all of the population means are equal. These can be operationalized in our example data as follows (note that for H_A, we use a common short hand to denote the model wherein any difference is hypothesized across the possible combinations of [in]equality):

$$H_0 : \mu_1 = \mu_2 = \mu_3$$

$$H_A : \mu_1 \neq \mu_2 \neq \mu_3$$

2) Select an α level that is appropriate given the context of your research, your analysis plan, and your research question, and do not blindly adopt an omnibus critical p-value (Lakens et al., 2018; Lehmann, 2011). Again, we reiterate that α justification and selection is not necessarily how all researchers approach these tests.

3) Compute your given analysis and identify the corresponding p-value. If your p-value is less than the chosen α, reject the null hypothesis and state that there appear to be differences between some of your population means; however, if your p-value is greater than or equal to the value selected, do not reject the null hypothesis, and state that a

116   difference between the population means could not be supported.

117   While the NHST procedure itself gives us testable models, the specific analysis used to

118 test these models here, the repeated measures ANOVA with three levels, requires some

119 additional assumptions that must be met before an analysis is begun (Tabachnick & Fidell,

120 2012). Data need to have no outlying or influential observations. Data must have a normal

121 sampling distribution, be linearly related, and have independent errors. Depending on the

122 statistical test, data must also be checked for equal variances, sphericity, and additivity.

123 These assumptions can be checked and, if necessary, corrected for; however, violations of

124 these assumptions can lead to inaccurate decisions and attenuated power. Further, with

125 many analysis programs, data are required to have no missing values.

126   While this approach is widely used, there are many limitations associated with it.

127 First, this method can be sensitive to violations of the stated assumptions, and especially, if

128 the sample size is not large enough for the sampling distribution to approximate a normal

129 distribution (Tabachnick & Fidell, 2012). Even if assumptions are met, or nonparametric

130 tests are implemented (e.g., for situations where a normal distribution assumption cannot be

131 met), this methodology does not allow a researcher to state anything about the absence of

132 an effect (i.e., no true differences). Through traditional NHST, one can only discuss evidence

133 regarding the alternative hypothesis; one can never support the null hypothesis through this

134 procedure. Given the recent findings regarding reproducibility, showing support for the

135 absence of an effect can be even more crucial than showing support for the presence of an

136 effect (Bakker, van Dijk, & Wicherts, 2012; Lakens, 2017).

<div style="text-align:center">

Bayes Factors

</div>

History

Thomas Bayes was a statistician and Presbyterian minister whose works are still influential today (Bellhouse, 2004). Bayes' theorem solved the inverse probability problem, namely that through the frequentist approach, one can only know the probability of data existing given a hypothesis being true, never the probability of a hypothesis being true given that the data exist (Dienes, 2008). Bayes' theorem allows one to calculate the probability of a hypothesis given some data (posterior belief) by using how probable one believes the hypothesis to be before data was collected (prior belief) and how probable one believes the data to be given one's hypothesis (likelihood). Thus, with his theorem, researchers are able to update (through the use of the likelihood) our initial beliefs (our prior) given some data (Gelman, Carlin, Stern, & Rubin, 2013). Pierre-Simon Laplace pioneered Bayesianism and advocated for a broader interpretation of this theorem (De Laplace, 1774). The use of Bayesian statistics has been suggested as an NHST alternative (Dienes, 2014; Wagenmakers, 2007), but this approach has largely been undervalued in favor of frequentist methods as, until recently, Bayesian analysis required considerable computational effort. However, today we possess the technology necessary to efficiently conduct Bayesian analyses. While open source software, such as R and JASP, require minimal learning to be able to effectively operate (Morey & Rouder, 2015), researchers will need to invest more effort to understand the focus and interpretation of Bayes Factor (BF) comparisons as they differ from traditional NHST.

The Bayesian framework can be viewed as a continuum, with objective Bayesian analyses on one end, and subjective Bayesian analyses on the other (Press, 2002). While this topic could lend itself to its own manuscript, here we will simply summarize the two endpoints, and discuss where our analysis may be perceived to fall on the line. Objective

Bayesian analysis is closest to frequentist theory, as the aim is to minimize the influence of priors through the use of non-informative priors (such as Jefferys priors that are designed to be invariant under reparameterization Datta & Ghosh, 1996); thus, the data are allowed to maximally effect the posterior distribution. Further, objective Bayesian methods are influenced by the same quality criteria that frequentist methods used, including Type I error rate and power (Sellke, Bayarri, & Berger, 2001). On the other end, subjective Bayes analyses include rigorously informed priors so that current knowledge can play a large role in the posterior. Our current analysis splits these two; we do not utilize completely uniformed (objective) priors, as we can adjust for basic knowledge of the constraints of our data type. Given the usual lack of information about underlying distributions, a wider band of inclusion was used for prior information. The *BayesFactor* package (Morey & Rouder, 2015) assists greatly in the choice of prior and is especially user-friendly for applied researchers, as it makes use of recommended default priors that have been chosen to be safe to assume under a broad range of data and topics (Rouder, Morey, Speckman, & Province, 2012; Rouder, Speckman, Sun, Morey, & Iverson, 2009). Instead of conventional *F*, *t*, and *p*-values, a ratio of the likelihood of the alternative model to the null is report, usually BF_{10}. For instance, $BF_{10} = 20$ would indicate that the effects model is favored 20 to 1 over the null model. Conversely, if the BF_{10} were 0.10, the null model is favored 10 to 1 over the effects model.

Typical Procedure

The procedure behind BF comparisons requires two steps.

1) One must design two models for the data. For our purposes, the first of these models will be the null model, which states that there are no differences between means (μ; i.e., all of our observed values X_i, regardless of which time point they were assessed at X_{ij}, arise from a normal distribution N with some mean μ and variance σ^2). The second model for these analyses is the effects model, which states that each mean (μ) is

187   allowed to be different from the grand mean by some amount (δ; as we now have

188   observations being drawn from three potential normal distributions, all of which may

189   have a different mean value, but the same variance). These can be operationalized as

190   follows:

$$H_0 : X_{ij} \sim N(\mu, \sigma^2)$$

$$H_A : X_{ij} \sim N(\mu + \delta_i, \sigma^2)$$

191   In designing these models, one must choose the prior distributions that are believed to

192   describe the data. Reasonable expectancies of where the data lie should be incorporated in

193   this decision based on previous research into the studied phenomena (Rouder et al., 2012).

194   2) Analyze the data given the selected priors and models. Consider the BF and use the

195   BF_{10} as evidence of how one should update their beliefs about the models.

196   Based on the flexibility of the analysis, the only assumption that needs to be made is

197   that data exists such that two competing, plausible models with different constraints may be

198   specified.

199   Bayesian inference improves upon the traditional frequentist point of view by allowing

200   not only a clear interpretation of the evidence provided by the data, but also the ability to

201   speak in favor of the null hypothesis. It is important to note that while previous work has

202   indicated that p-values and BF largely agree on which hypothesis should be supported, they

203   differ in the strength of that conclusion, especially when p-values were slightly lower than α

204   (i.e., .05 to .01; Wetzels et al., 2011). However, some limitations do arise in this paradigm.

205   Bayesian analyses require the researcher to take an active role in the choice of prior

206   distributions for the phenomenon they are modeling, and this decision can take some effort

207   to fully understand; however, in the meantime, there are packages such as *BayesFactor* that

208   provide the researcher simple default options that can readily lend themselves to many

research areas with little fear of being outrageous specifications. Further, unlike NHST, Bayesian analyses do not necessarily control long-run error rates, as the focus is on updating current model beliefs. Another concern that many researchers have is that these analyses are necessarily sensitive to prior choice. However, research has shown that the choice of priors has essentially no effect on conclusions when sufficient data has been collected as the priors give way to the weight of the data (Klugkist & Hoijtink, 2007; Rouder et al., 2012) and when reasonable priors are considered, data are only mildly sensitive to these (Haaf & Rouder, 2017). Finally, many believe Bayesian analysis to be too computationally intensive to complete. However, many simple programs, packages, and tutorials exist to help ease the transition from frequentist to Bayesian analysis (JASP Team, 2017; Kruschke, 2014; Morey & Rouder, 2015).

Observation Oriented Modeling

History

James Grice argues that our problems as a science go beyond use of NHST and extend into the philosophical ideas underpinning our research. Therefore, he developed a new paradigm called Observation Oriented Modeling (OOM, Grice, 2011, 2014; Grice, Barrett, Schlimgen, & Abramson, 2012). He reasons that by viewing psychology through the lens of philosophical realism, instead of positivism, we should be able to properly and effectively conduct research and analyze data. In contrast to positivism (i.e., which is solely concerned with finding an effect, not with how the effect occurred), philosophical realism holds that the causal structure of nature can be understood through scientific investigation. The goal is then to understand the causal mechanisms that give rise to the patterns observed in a given data set. Switching to this philosophy allows for techniques that match the daily activities of social scientists in their endeavors to unravel the story of how humans operate. OOM pushes the researcher to seek an inference to best explanation (Grice et al., 2017). This causal

234 inference procedure differs from both NHST and Bayes yet are often conflated, which is

235 concerned with inferences to population parameters and their various assumptions

236 underlying statistical tests (e.g., random sampling, normality, homogeneity of population

237 treatment differences, etc.).

238   Generally speaking, this approach can handle any type of data, including ordinal

239 rankings and frequency counts, as all analyses are calculated in the same general fashion (see

240 Valentine & Buchanan, 2013 for an example). This simplicity occurs because OOM works on

241 the deep structure of the data which is a binary coding technique similar to dummy and

242 effect coding. Deep structures are matrices of zeros and ones which can be efficiently

243 manipulated to investigate patterns within the data. The most important values from any

244 OOM analysis are the PCC (percent correct classification) values. These values represent the

245 summation of how well an individual's responses matched the stated or expected pattern or,

246 in the case of causal modeling, how many of the individuals conformed to a given cause.

247 Complete matches are the proportion of observations that match the researcher-designated

248 pattern on all dimensions. For example, in a three-level Ordinal Pattern Analysis (OPA), a

249 person would be tallied as a "complete match" if the ordinal pattern of his/her data matched

250 the expected ordinal pattern across all three levels. For example, imagine we have set a

251 pattern that designates Time 1 < Time 2 < Time 3. Person A has values of 3, 4, and 5 at

252 timepoints 1, 2, and 3, respectively, while person B has values of 4, 5, and 2. Person A

253 matches the pattern completely, and is therefore counted in the PCC value. Person B,

254 however, matches only the first part of the pattern (time 1 less than time 2) and is not

255 counted in the PCC value. However, while person B matched the first part of our pattern

256 (time 1 less than time 2), they did not match on the third point of our pattern (time 2 less

257 than time 3); thus, they would not be counted in the PCC value. As the PCC is simply the

258 percentage of individuals in a sample whose responses match the expected ordinal pattern

259 perfectly, its computation is therefore not based on means or variances, but on the basis of

260 the observations themselves. The PCC value replaces all of the conventional values for effect

261 size used in statistical analyses.

262   The analysis we focus on here (OPA) does not form any type of linear or nonlinear

263 equation or regression, but simply looks for those individuals who match the expected

264 ordinal pattern (Grice, Craig, & Abramson, 2015). The main point of the analysis, then, is

265 to see how many people fit the expected pattern which is based on a causal theory. If all

266 causes are accounted for in the study and observations have been made with sufficient

267 precision and accuracy, then 100% of the persons should fit the expected pattern; otherwise,

268 a lower PCC value will be expected and it is up to the researcher to determine how high a

269 PCC must be in order to support an inference to the causal mechanism.

270   In OOM, traditional p-values are no longer utilized (Grice, 2011). As a secondary form

271 of reference value, a chance value (c-value) is obtained, which is a type of randomization test

272 in which the researcher determines the number of randomized trials for the test (e.g. 1,000 or

273 5,000 randomized versions of actual observations). This procedure is akin to permutation

274 tests, where PCCs are computed for the randomized data to form a distribution. The

275 observed PCC is then compared to these values, and the c-value (which is an empirical

276 probability) is determined. If the randomized data sets fit the pattern as well as or better

277 than the actual data does, the c-value will be high (close to 1). Low c-values (close to 0)

278 indicate a pattern of observations that is improbable (i.e., unlikely produced by chance)

279 when compared to randomized versions of the same data. Although low c-values, like low

280 p-values, are desirable, c-values do not adhere to a strict cut-off and should be considered a

281 secondary form of confirmation for the researcher that their results are distinct.

282 **Typical Procedure**

283   OPA is analogous to repeated measures ANOVA and contains two steps.

284   1) Designate the expected ranked pattern: each variable as being higher, lower, or equal

to the other variables. For instance, for our analyses we defined the following pattern
of individual responses X_i, whereby the first time point should be less than the second
time point which should be less than the third time point. This pattern can be
operationalized as follows:

$$X_{i_1} < X_{i_2} < X_{i_3}$$

2) Analyze the data using OPA. Consider the PCC (the percentage of individuals
matching the ordinal hypothesis) and c-values in light of the data and use your best
judgment as to whether or not the data conform to the expected pattern. This analysis
only requires the assumption that the data exists such that a pattern may be designed.

As with all of these methodologies, limitations do exist. This approach is largely
concerned with patterns of responses, not with magnitudes of differences, which may be an
integral piece of information to some researchers. Unlike all approaches mentioned before, we
do not discuss the probability of some data given our hypothesis here, and instead focus on
the observed responses of the individual and how it may or may not behave as expected.
Finally, similar to the Bayesian analysis, long-run error rates are not discussed in this
methodology.

A Simulation Study

Simulated Data

In this study, we generated 20,000 datasets by manipulating sample size and effect size
for a repeated measures design with three levels. A repeated measures design was chosen as
it is widely used across many disciplines of psychology. These datasets were created using
the *mvtnorm* package in R (Genz et al., 2017), and all code for simulations can be found at

https://osf.io/u9hf4/?view_only=1caa9092868b4d7aadb9a83a31a979cd. Interested readers

can easily adapt the *R* code to incorporate different research designs. Likert data, ranging

from 1 to 7, was created by rounding *mvtnorm* estimates to whole numbers and truncating

any data points out side of the appropriate range (i.e., values < 1 were rounded to 1, and

values > 7 were rounded to 7). We specifically chose Likert-type data as this data type is

one of the most common data types utilized by most social scientists. Additionally, we add

to the literature as other simulations have chosen to use completely continuous data (i.e.,

simulated numbers are often precise to 10+ decimals, which is unlikely for traditional

sampling). The simulated data did increase in skew with this procedure from approximately

no skew (i.e., <0.01) to approximately 0.40 for the smallest and no effect conditions;

however, these values closely resembled a normal distribution with the use of *mvtnorm*. The

population means for each level were set to 2.5, 3.0, and 3.5, and pairwise effect sizes (e.g.,

the comparison between time 1 v. time 2 and time 2 v. time 3) were manipulated by

adjusting the standard deviation to create negligible effects ($SD = 3.39$, $d = 0.10$), small

effects ($SD = 3.00$, $d = 0.20$), medium effects ($SD = 0.50$, $d = 0.50$), and large effects ($SD = 0.10$, $d = 0.80$) using Cohen (1992)'s traditional guidelines for d interpretation. The smallest

effect size was set such that Likert style data could still be retained with the smallest

possible effect size. Sample size was manipulated at 10, 30, 100, 500, and 1,000 data points.

All combinations of the five sample sizes and four effect sizes were created, and each dataset

was simulated 1,000 times, totaling 20,000 datasets.

     The advantage of using *mvtnorm* and set *SDs* for each group was the ability to

approximate the assumptions of normality by randomly generating from a multivariate

normal distribution, and homogeneity by setting equal *SDs* for each group. In a repeated

measures design, the assumption of sphericity was met by setting the correlations between

levels in *mvtnorm* to zero. By maintaining the lowest level of relationship between levels, we

additionally controlled for power and examined situations of significance given the lowest

power scenario. During the data simulation, the standard deviation of the difference scores

333 was examined to maintain differences greater than zero, especially for low N simulations.

Analyses Performed

335 **Descriptive Statistics.** Means, mean differences between levels, and the confidence

336 intervals for each mean can be found in the complete dataset online,

337 https://osf.io/u9hf4/?view_only=1caa9092868b4d7aadb9a83a31a979cd. For each simulation,

338 we also calculated d values using the standard deviation of the difference score as the

339 denominator (d_z, Lakens, 2013). The *MOTE* library was used to calculate the non-central

340 confidence interval for each d value as well (Buchanan, Valentine, & Scofield, 2017;

341 Cumming, 2014). This data was mainly used to determine if simulations were meeting

342 expected values overall.

343 **Parametric NHST - Repeated Measures ANOVA.** Repeated measures

344 ANOVA using the *ezANOVA()* function in the *ez* library was utilized with type three sum of

345 squares (Lawrence, 2017). This style of ANOVA is used to compare the same individuals

346 across multiple or all conditions in an experiment. The null hypothesis states that there are

347 no significant differences between population means, and the research hypothesis posits that

348 there are differences between some population means, but does not specify which population

349 means may differ, just that one or more will differ as the alternative. This test uses the F

350 distribution focusing on p values.

351 To determine where differences may exist, *post hoc* dependent t-tests are normally

352 analyzed in the event of a significant F-ratio. We did not run all pairwise comparisons,

353 instead focusing on the linear trend simulated by comparing level one to two and level two to

354 three. This set of comparisons also controlled the effect size between comparisons, as

355 comparing level one to three would have doubled the effect size. However, we assumed that

356 typical researchers might compare all three pairwise combinations in practice and used a

357 Bonferroni correction across all three possible pairwise combinations to calculate p values for

358 *post hoc* tests. Therefore, while we only discuss the two comparisons, we utilized the more

359 stringent cutoff of the Bonferroni correction as we believe this procedure would be how the

360 majority of researchers would handle the data. Interested readers can find all three

361 comparison values in the complete dataset online. Following traditional usage, a p-value of

362 less than .05 was binned as significant, whereas p-values ranging from .10 to .05 were binned

363 as marginally significant. Any p-values larger than .10 were binned as non-significant. A

364 second set of p-value comparisons was calculated given Benjamin et al. (2018)'s suggestion

365 to change α criterion to less than .005. Any p-value less than .005 was binned as significant,

366 while data ranging from .005 to .10 was marginal or suggestive, and $p > .10$ was

367 non-significant.

368 **Bayesian Analysis: Bayes Factor.**   We compared a null model with one grand

369 mean for all three levels to an effects model wherein means were allowed to differ using the

370 *BayesFactor* package (Morey & Rouder, 2015). The default in this package is a Jeffreys prior

371 with a fixed rscale (0.5) and random rscale (1.0). BF were calculated, and follow up t-test

372 BFs were computed for the same two comparisons as in the previous models using default

373 priors from the *BayesFactor* package (e.g., Jeffreys prior for population variance, Cauchy

374 prior for standardized effect size). To compare Bayesian results to other statistical methods,

375 we used recommendations from Kass and Raftery (1995) to bin results into weak evidence

376 (BFs < 3), positive evidence (e.g., akin to marginal p-values, BFs $= 3$-20), and strong

377 evidence (BFs > 20). We must stress here that BF interpretation should focus on

378 understanding the odds of model ratios, not necessarily the presence or absence of an effect.

379 However, given that we wanted to compare the conclusions one would reach given this data

380 in a Bayesian paradigm to that of a frequentist paradigm, these bins are used as a convenient

381 comparison to the frequentist procedures using set criteria for interpretation (Morey, 2015).

382 Should any reader become curious how a different set of binning values affect our analyses,

383 all code and data are at their disposal at

384 https://osf.io/u9hf4/?view_only=1caa9092868b4d7aadb9a83a31a979cd, and this manuscript

385 was written with the *papaja* package allowing one to view the code inline with this text

386 (Aust & Barth, 2017).

387   **OOM: Ordinal Pattern Analysis.** An *R* script of the Ordinal Pattern Analysis

388 from Grice et al. (2015)'s OOM program was provided from Sauer and Luebke (2016). We

389 set the expected ranked pattern as level one less than level two less than level three. Once

390 this pattern was defined, we then analyzed the data to see if each individual's set of

391 observations matched this expected ordinal pattern. PCC values were generated, and

392 *c*-values were computed by randomizing the data 1,000 times. Solely for purposes of

393 comparison, we used the following significance coding schema: significant studies had a high

394 PCC value ($.50 < \mathrm{PCC} < 1.00$) and a low *c*-value ($c < .05$), marginal studies had a high

395 PCC value and a moderate *c*-value ($.05 < c < .10$), and non-significant studies had low PCC

396 values ($\mathrm{PCC} < .50$), regardless of their *c*-values. Again, we must stress that this paradigm

397 eschews binning estimates and that our use of bins was a) discussed and decided upon before

398 data analysis, and b) created only for the purposes of comparing this new methodology's

399 possible conclusions to that of a frequentist framework. We welcome interested readers to

400 explore the data more, defining their own bins and viewing the affects, by viewing and

401 editing our code online.

Results

Percent of Estimates

404   For all simulations, we first binned the estimates into significant, marginal, and

405 non-significant effect categories as described in the Analyses Performed section above. Next,

406 we calculated the percentage of these analyses that would be classified into each of these

407 categories, separated about by statistical analysis, sample size, and effect size. These

estimates were binned across both the overall and follow up *post hoc* tests, and the combined

data are presented for this analysis. Since all three categories of binning total to 100%, we

present only the significant and non-significant results. Significant critical omnibus estimates

are presented in Figure 1. All figures discussed in this manuscript may be viewed as

interactive graphics on our OSF page through a provided Shiny app. In Figures with sample

size on the axes, we log transformed N to allow for visual distinction between sample sizes,

as smaller N values were compressed when using the $N = 10$ to 1000 on the axis. Both N

and $\log(N)$ can be found in the Shiny app, along with the ability to zoom in to specific

ranges of sample size.

For negligible effects at $p < .05$ (solid lines), we found that NSHT analyses showed a

predictable Type I error bias, in that they detected significant estimates with extremely

small d values as sample size increased. Binned BF values showed a similar pattern, but

were more conservative with less percent significant estimates. OOM analyses were the most

conservative, essentially never detecting an estimate in the negligible effect simulations.

Small effect sizes showed the same pattern for NHST, BF, and OOM results, with the

proportion of significant estimates increasing more rapidly and asymptoting at a smaller

sample size than negligible effects. At medium effect sizes, NHST analyses nearly always

detected significant estimates, while BF and OOM analyses would have been considered

significant around 75% of the time. Interestingly, with large effect sizes, OOM analyses

mirrored NHST by always detecting estimates, and BF analyses were generally more

conservative except at the largest sample size. Figure 1's dashed lines indicate the results if

values were binned at $p < .005$, and the differences between these results were very subtle.

Lowering α reduced the number of significant estimates at small N values for all four effect

sizes, with more pronounced differences at negligible and small effect sizes. However, the

graphs converged to the same conclusion that large enough sample sizes could produce

significant results at negligible and small effect sizes.

434   Figure 2 portrays the results for non-significant binned simulations, which were the

435 same for both α criterion. Across all effect sizes, BF and NHST showed similar results,

436 where non-significant estimates were detected at lower sample sizes for negligible and small

437 effect size simulations. At medium and large effect sizes, almost all estimates would have

438 been considered significant, therefore, detection rates for non-significant estimates were

439 around zero. OOM displayed a conservative set of findings, showing nearly 100%

440 non-significant estimates at negligible and small effect sizes (mirroring results from Figure 1).

441 At medium effect sizes, approximately a quarter of estimates were non-significant,

442 illustrating the conservative nature of OOM interpretations.

443   Figure 3 depicts the relationship between the effect size of time 1 minus time 2 and the

444 corresponding PCC values. These metrics appear to represent different concepts where effect

445 size measures the magnitude of the difference between two data points while PCC disregards

446 magnitude and represents the proportion of the sample following the given ordinal pattern

447 across all three time points. Given these differences, it is interesting how well these two

448 measures converge together. As sample size increases, estimates for both d and PCC become

449 more precise (i.e., smaller range, closer to the simulated effect size). We believe that PCC

450 offers researchers the ability not only to confirm that their effect size is reasonable, but also

451 to better understand the pattern their data are following, especially if an observed effect size

452 contradicts previous literature. For example, let us assume there is previous literature that

453 states that a small positive effect exists, such that responses should increase from time 1 to

454 time 2. Under conditions of a true small effect ($d= -0.20$) and sample size of 30, our graph

455 shows us that it is possible to obtain a positive medium effect size ($d = 0.50$; indicating the

456 time 1 is more extreme than time 2). Upon finding these contradicting results, the researcher

457 could further seek to understand the pattern their data are following by computing the PCC

458 value for the experiment. The PCC value for this example would be above .50, indicating

459 that, in over half of respondents the values for time 1 are less than time 2 (in turn less than

460 time 3, as it measures the entire pattern), even though magnitude of change suggests that

time 1 is larger than time 2. This gives the researcher a richer piece of information, which can help to describe their results in a more nuanced fashion.

Percent Agreement

A goal of this project was to expand the toolbox of options for researchers to determine what evidence supports their hypotheses by examining multiple methodologies. We calculated the percent of time that all analyses agreed across overall and *post hoc* comparison estimates. Figure 4 illustrates the pattern of 100% agreement on effects for critical omnibus tests only at each sample size and effect size. Figure 5 portrays the results for *post hoc* tests, which only uses NHST and Bayes Factor analyses, as OOM does not have a *post hoc* test (i.e., the test is a pattern analysis that presupposes the expected direction of *post hoc* tests).

When effect sizes were negligible and for small effects, agreement was best across small samples and decreased across sample size, as NHST was overly biased to report significant estimates and OOM and BF were less likely to do so. For medium and large effect sizes, 50-75% agreement was found, usually regardless of sample size. Additionally, we found that for negligible, small, and medium effects, agreement for *post hoc* tests was higher than agreement for overall comparisons. The *post hoc* comparisons for levels 1 to 2 and levels 2 to 3 were less likely to be binned as significant across negligible and small effects, so the agreement levels were higher for these individual comparisons due to non-significant follow up tests. The critical omnibus test was more likely to be significant due to the inclusion of effect of comparisons between level 1 and 3, which were double the effect size. However, these *post hoc* comparisons do not include the conservative significant binning from OOM, which decreased critical omnibus 100% agreement seen in Figure 4. Again, the differences between $p < .05$ and $p < .005$ were minimal. Complete tables of percentages of binning across critical omnibus and *post hoc* tests, along with agreement percentages broken down by bins can be found at https://osf.io/u9hf4/?view_only=1caa9092868b4d7aadb9a83a31a979cd.

Criterion Comparison

As the relationship between BF and *p*-values is already well documented, we will not discuss them here beyond stating that we found the expected pattern shown in previous work (Rouder et al., 2012), and that individuals who wish to view this comparison, as well as all the other comparisons discussed here should visit our interactive Shiny application at our OSF page. Of interest was the comparison of OOM indices to traditional NHST and Bayesian indices. First, in Figure 6, PCC values are plotted against log BF values and *p*-values. The log of BF was taken to include all values on a viewable axis, and all infinity values were windsorized to the next highest point. Increasing sample size is shown by increasing point size and lighter colors. Additionally, since OOM values are a combination of PCC and *c*-values, *c*-values below .05 are shown as Xs instead of dots. Therefore, all values PCC >= .50 that are also denoted as Xs would be considered significant in this example. The provided Shiny application uses color to distinguish sample size differences, as well as includes options to create each combination effect size and criterion individually. Only two graphs are provided here to save space.

In Figure 6, the left hand column portrays the relationship between log BF values and PCC values in negligible and medium effect sizes. With negligible effect sizes, we found large variability in PCC values across a small span of BF values while sample sizes remained low, but as *N* increased, we saw that the range of PCC values narrowed considerably with increasing BF values. Therefore, as sample size increased, the PCC values constricted, while BF values expanded. A similar pattern appeared when viewing the medium sample size graph, as again PCC values became less variable with increased sample size, and BF tended to increase both in variability and in value as the sample size grew. Here, we can see a benefit of PCC, along with *c*-values, as increasing sample size portrayed more precision in PCC, instead of the increased variability found in BF.

It is also important to note that within the negligible effects graph, while many of these PCC values reached high values, that these values did not denote patterns that would necessarily be seen as unique. *c*-values were a secondary measure of evaluation that eliminated a number of these matches from being considered meaningful. A large majority of points with larger sample sizes on the figure included low chance values, however, the PCC values for these simulations were lower than a meaningful percent used for cutoff criterion. This two-step process helped to weed out effects that were negligible, especially at larger sample sizes.

Additionally, we compared *p*-values and PCC values, which are illustrated on the right hand side of Figure 6. Again, PCC values showed far more variability with small sample sizes, and the *p*-values associated with these smaller sample sizes were also quite variable. Importantly, even when an effect was negligible, PCC values become less variable with increasing sample size. PCC values also indicated that there was little evidence of the hypothesized pattern by shifting toward zero. *p*-values decreased in variability at high sample sizes and shifted toward minuscule values, thus, pointing toward rejecting the null hypothesis. With medium effect sizes, both *p*-values and PCC values were variable at small sample sizes. At larger sample sizes, *p*-values decreased towards floor effects (i.e., closer to zero), while PCC values simply narrowed in range shifting slight above .50. The benefit of multiple criteria evaluation here was clear, as *p*-values increasingly indicated significance as sample size increased, PCC values were not effected in this way and thus presented a more stable picture of the presence of an effect. While multiple criteria may not completely reduce the interpretation of false positives in the literature, the relationship between these values illustrated that multiple indices can provided a clearer picture of the evidentiary value available in a study.

Limitations

Within any study a number of limitations exist. The largest limitation of our study is that we chose to focus on a simple three level repeated measures ANOVA design. The benefit to this focus is the simplicity of understanding the relationship between analyses, while also using a well understood NHST procedure. However, is possible that these same relationships may or may not exist in alternative design contexts. Additionally, our choices for classification of significant effects for p-values, BF, PCC, and c-values was based on what we believe a reasonable researcher may designate; however, these classifications may vary in the real world. We provide open access to our simulations and code so that an interested party can tinker with these choices. We believe the global conclusions would likely be similar across changes, however, the specific percentages and patterns would likely differ. Finally, due to the specification of our simulation we did not violate any statistical assumptions. It is possible that the violation of these assumptions may cause changes in the relationships we see here.

Discussion

This manuscript was designed to showcase two alternative paradigms to NHST researchers and to compare the conclusions these alternative methodologies might make in a given data environment to those NHST would make. We believe that the awareness of multiple methodologies might assist in strengthening our conclusions and improving reproducibility by giving researchers the ability to identify an optimal method given the question at hand. Further, we believe that should a researcher utilize multiple methodologies (e.g., analyzing and reporting both a NHST p-value as well as an OOM PCC value) that these estimates in tandem can help readers to weight these various forms of evidence and arrive at a more robust conclusion. We found that changing the threshold at which p-values are deemed significant had little to no effect on conclusions, especially at large sample sizes,

560 regardless of effect size. This finding is notable as the article by Benjamin et al. (2018)

561 states that an increase in sample size is likely to decrease false positives "by factors greater

562 than two" (p. 10), and work by Pericchi and Pereira (2016) state that an adaptive level of

563 significance would be beneficial in these circumstances, neither of which are supported by our

564 simulations. Our science will not grow by moving the significance line in the sand, as this line

565 has already been shown to have "no ontological basis" (Rosnow & Rosenthal, 1989, p. 1277).

566   Instead, we need to embrace the multitude of perspectives available to us and to begin

567 to employ these diverse approaches. While NHST can still serve us well when properly

568 utilized, it is important for researchers to understand that different methods seek to answer

569 different questions, and that we need to ensure that we are using the right method to answer

570 a given question. When evaluating evidence in order to answer these questions we must be

571 wary of looking for significant differences and focus instead on finding meaningful differences.

572 By combining these approaches we may be better able to qualify the strength of our evidence

573 and discuss a more nuanced version of our data. Additionally, while all of these methods

574 have drawbacks, when used in combination these methods can begin to overcome many of

575 these limitations. For instance, given a large sample size, we would expect BF values to be

576 very large and p-values to be very small, both indicating that the null model/hypothesis

577 should not be supported. However, if we also have a PCC value of .30, we may decide that it

578 is possible that this effect is very small and possibly negligible. This multifaceted approach

579 can help to curb our enthusiasm over small or negligible effects that may not be practically

580 meaningful and possibly may not replicate. Regardless if analyses agree or disagree on the

581 presence of an effect, a researcher can investigate the direction and size of the effect, the

582 proportion of data that agrees or disagrees with the direction of the effect, and discuss

583 conclusions accordingly. Each methodology behaves slightly differently in given data

584 environments, which might begin to highlight meaningful differences when discussed together.

585   Some may contest that all of these analyses are capable of being hacked, like p-values,

through researcher degrees of freedom, choice of priors, or pattern choice, among other actions (Simmons et al., 2011). Transparency throughout the research process is key to eliminating these issues, as α changes may only encourage bad research practices with the current incentive structure on publishing. Although we have the capability to share research across the world, research often still occurs behind closed doors. The Open Science Framework grants insight into research processes, allowing researchers to share their methodologies, code, design, and other important components of their projects. In addition to posting materials for projects, pre-registration of hypotheses and methodology will be an important facet in scientific accountability. Further, with increased transparency editors and other researchers can weigh the evidence presented according to their own beliefs.

Our key suggestion in this project is the redefinition of evidentiary value. The current focus on p-values has shown to be problematic, as many of the studies from the Open Science Collaboration (2015) do not replicate at $p < .05$ or $p < .005$ (Lakens et al., 2018). With the change in transparency mentioned above, publishing research with solid research designs and statistics, regardless of p-values, will allow for a broader range of evidence to become available. Publishing null findings is critical in replication and extension for discovering the limits and settings necessary for phenomena. Registered replications and reports will allow studies to be accepted prior to results being known, thus allowing researchers to focus on experimental design and hypotheses *apriori* instead of p-values *post hoc*. Reports should describe multiple indicators of evidence, such as effect sizes, confidence intervals, power analyses, Bayes Factors, and other descriptive statistics (Finkel, Eastwick, & Reis, 2015; Nosek & Lakens, 2014; van't Veer & Giner-Sorolla, 2016).

A misunderstanding of statistical power still plagues psychological sciences (Bakker, Hartgerink, Wicherts, & van der Maas, 2016), and the effect of sample size, especially small ones, was shown here by comparing the criterion available in these analyses. Often, individual research labs may not have the means to adequately power a proposed study.

Multilab studies and collaboration with other scientists is fundamental to alleviating these issues, while encouraging interdisciplinary science. Collaboration increases our statistical abilities, as every researcher cannot be expected to be proficient in all methods and analyses, but teams of researchers can be assembled to cover a wider range of statistical skills to provide adequate estimates of evidence in their reports. We understand that there may be resistance to the implementation of multiple methodologies as these new methodologies take time and effort to learn. However, through the use of free programs (JASP, R, OOM, Shiny) and tutorials (YouTube, Coursera, http://www.statstools.com), we believe all researchers are capable of learning these analyses. We believe that through the expansion of our analytical knowledge and application of these new methodologies, we can begin to attenuate some of the strain currently placed on psychological science and to increase the strength of evidence in our discipline.

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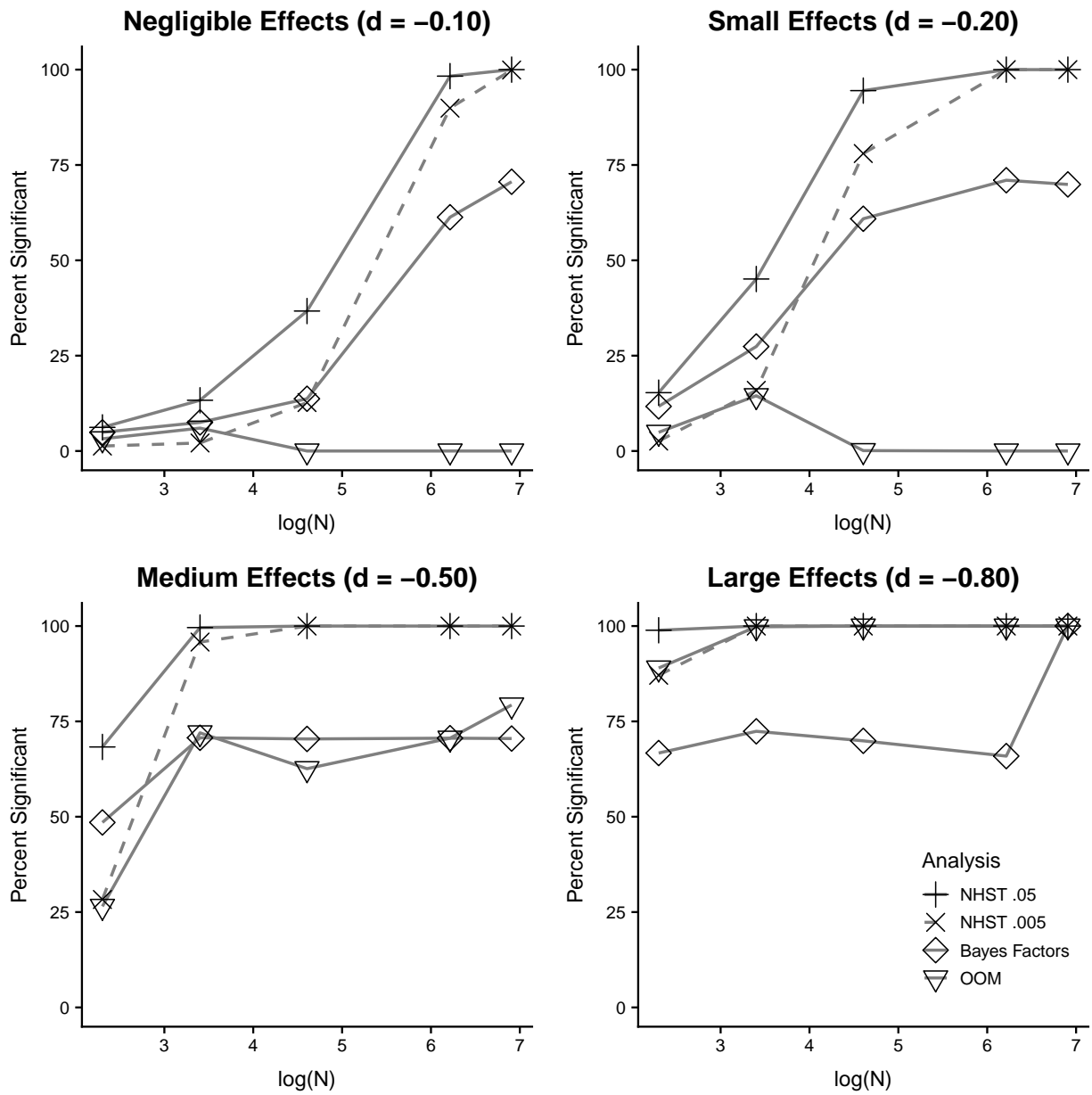
Figure 1. For NHST analyses only, percent of significant estimates at $p < .05$ (solid) and $p <$ .005 (dashed) for each analysis given effect size and sample size.
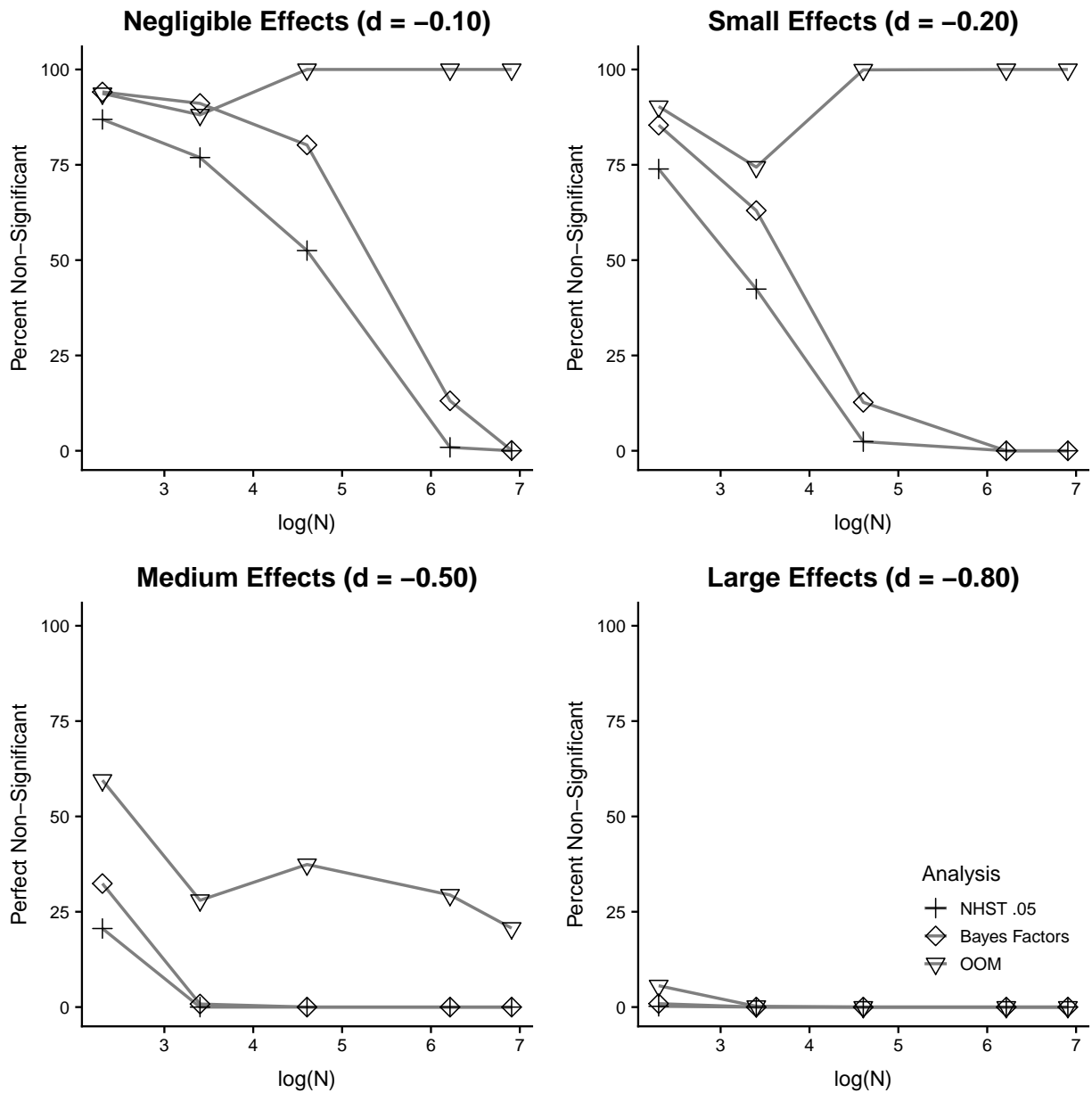
Figure 2. Percent of non-significant effects for each analysis given effect size and sample size.
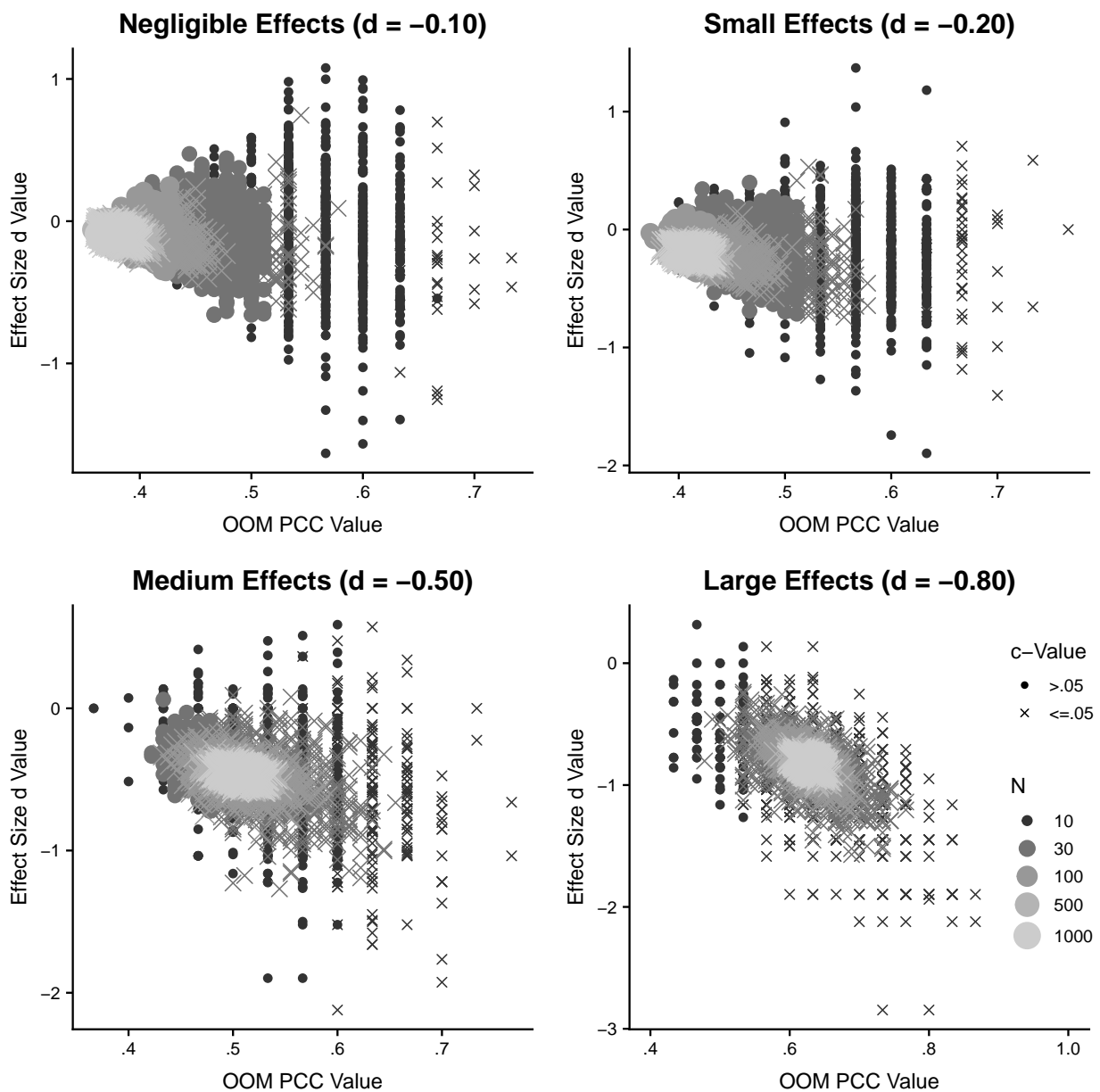
Figure 3. PCC and c-values plotted against observed effect size (*d*-values) given effect size and sample size conditions. Xs indicate simulations with c-values < .05,which were binned as significant if they were found in conjunction with PCC values over .50. Point size and color indicates sample size wherein larger samples are lighter colors. Please note that the key for the entire set of graphics is in the bottom right hand corner.
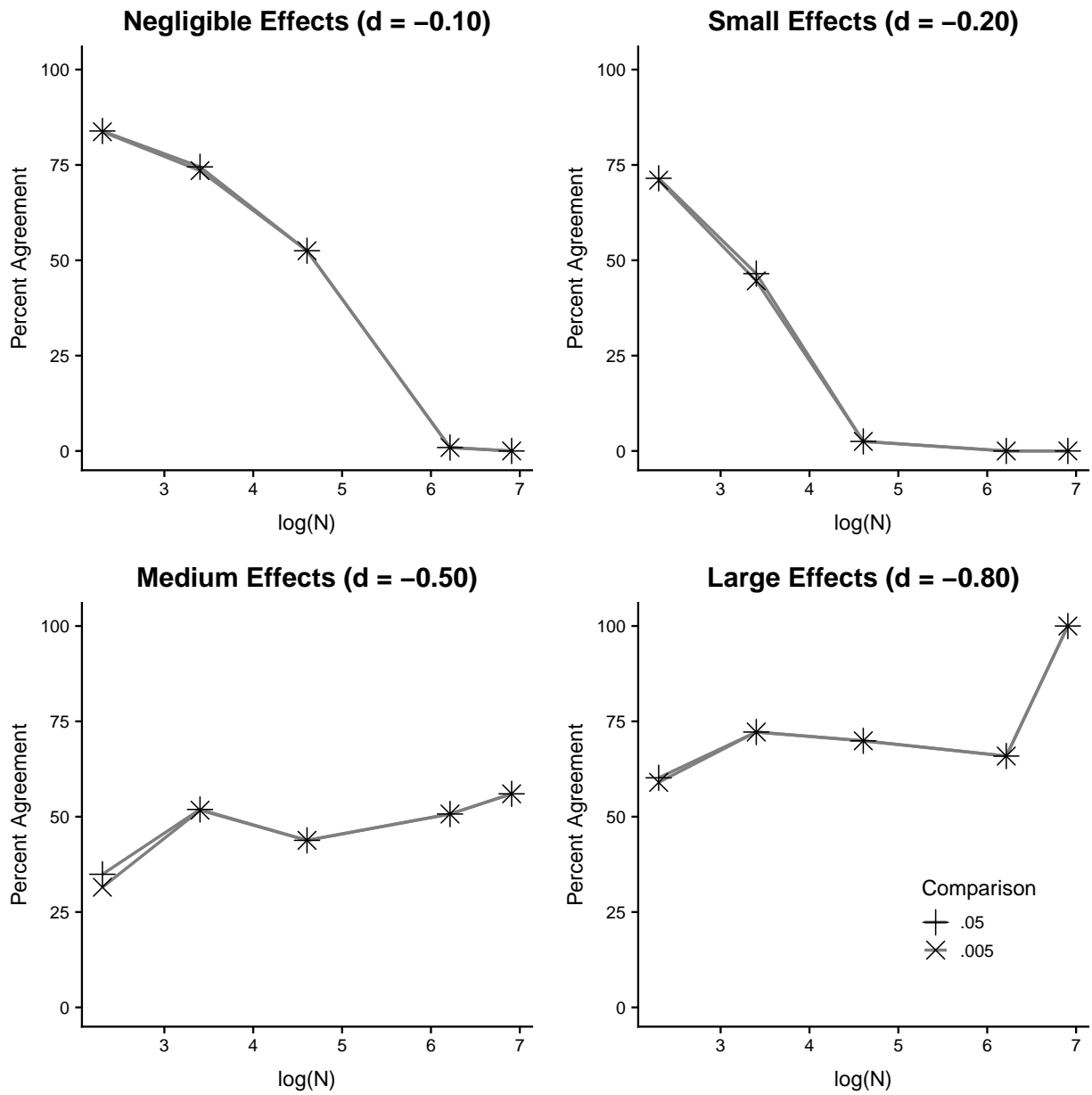
Figure 4. Percent of agreement across all analyses given effect size and sample size for omnnibus tests.
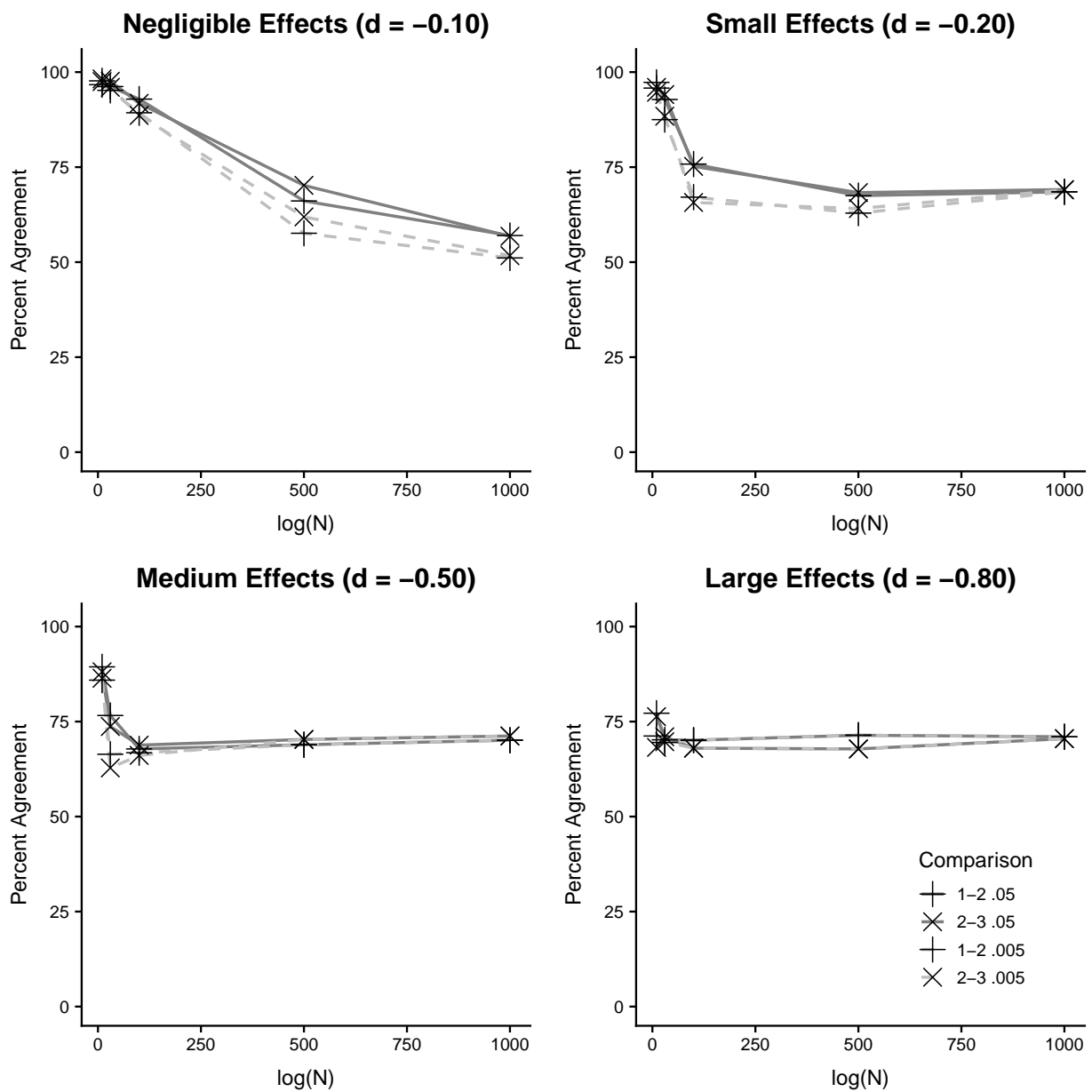
Figure 5. Percent of agreement across each analysis given effect size and sample size *posthoc* tests with $p < .05$ (solid) and $p < .005$ (dashed). Note that this graph only compares the NHST and BF conclusions.
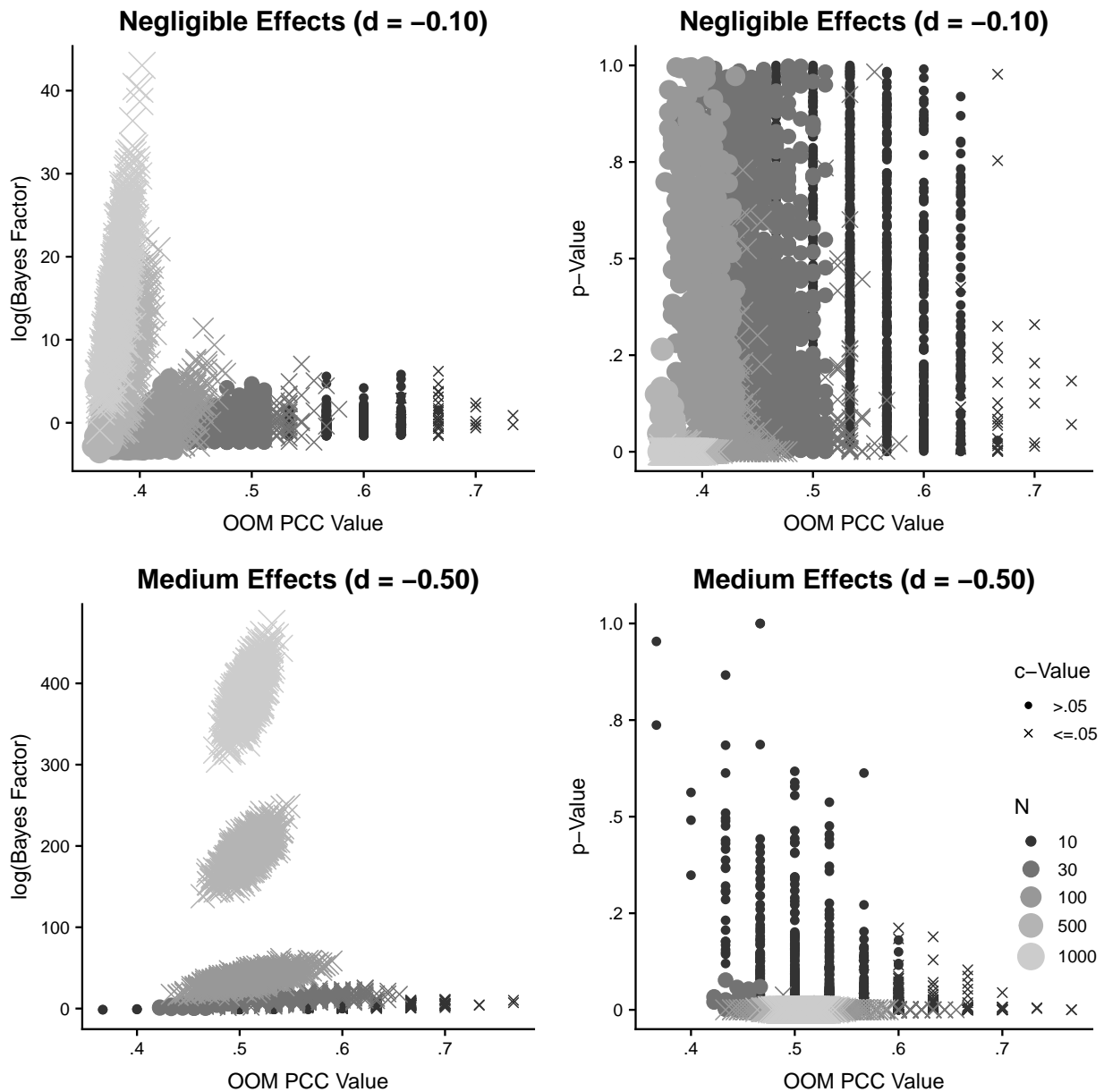
Figure 6. PCC and c-values plotted against *p* and BF values for negligible and medium effect size conditions. Xs indicate simulations with *c*-values < .05,which were binned as significant if they were found in conjunction with PCC values over .50. Point size and color indicates sample size wherein larger samples are lighter colors. Please note that the key for the entire set of graphics is in the bottom right hand corner.