Ignored evident multiplicity harms replicability - adjusting for it offers a remedy
Yoav Zeevi$^{1,2}$, Sofi Astashenko$^2$, Yoav Benjamini$^{1,2}$
1. The Sagol School for Neurosciences, Tel Aviv University, Israel
2. Department of Statistics and Operations Research, Tel Aviv University, Israel

Abstract

It is a central dogma in science that a result of a study should be replicable. Only 90 of the 190 replications attempts were successful. We attribute a substantial part of the problem to selective inference evident in the paper, which is the practice of selecting some of the results from the many. 100 papers in the Reproducibility Project in Psychology were analyzed. It was evident that the reporting of many results is common (77.7 per paper on average). It was further found that the selection from those multiple results is not adjusted for. We propose to account for selection using the hierarchical false discovery rate (FDR) controlling procedure TreeBH of Bogomolov et al. (2020), which exploits hierarchical structures to gain power. Results that were statistically significant after adjustment were 97% of the replicable results (31 of 32). Additionally, only 1 of the 21 non-significant results after adjustment was replicated. Given the easy deployment of adjustment tools and the minor loss of power involved, we argue that addressing multiplicity is an essential missing component in experimental psychology. It should become a required component in the arsenal of replicability enhancing methodologies in the field.
1 Introduction

1.1 Replicability crisis
In recent years we have witnessed a dramatic increase in the number of papers addressing the problem of replicability in a larger number of scientific fields, Biology, Medicine, Social Sciences, and even Computer Sciences, being just a handful of examples (Fanelli 2009; Peng 2011; Prinz et al. 2011; Gelman & Loken 2013; Achenbach 2015; Baker 2016; Lithgow et al. 2017; Frank et al. 2017; Nosek & Errington, 2017). Psychology is one of the first research fields where the replicability problem has received significant attention (Smith 1970). This could possibly be attributed to the echo psychological discoveries have in the general public (Vanpaemel et al. 2015; Gertler et al. 2018; Anvari & Lakens, 2019).

It is a central dogma in science that a result of a study should be replicable, in the sense that other investigators using a similar setup and methods get essentially the same result. Nevertheless, the way to assess whether a result is replicable is unequivocal. It was Fisher (1935) who suggested that replicability is established when the p-value of a result is less than a threshold both in the original study and in the replication effort. In fact, he suggested the use of a significance threshold because the p-value itself will never be the same in two experiments. Adding the obvious requirement that the direction will be the same, this has become the most common way of assessing replicability, usually with .05 as a practice-reflecting threshold (Goodman et al. 2016). Other definitions of successful replication have been offered (Valentine et al. 2011; van Aert & van Assen, 2017, 2018), including Hung & Fithian (2019) that make use of a conditional argument. Our primary goal in this study is to offer a way to enhance the replicability of the original study results. Therefore, the variations presented in the assessment of replicability are not that important. We use the arguably most common definition for a successful replication, that both the replication study’s and the original study’s p-values are smaller than 0.05, with an effect in the same direction. According to this criterion, in six of the major replication projects undertaken in psychology in recent years, only 90 of the 190 replication attempts had a p-value smaller than 0.05, as the original study declared. (Klein et al. 2014, 2018; Open Science Collaboration, 2015; Ebersole et al. 2016; Camerer et al. 2016, 2018). In view of that, no wonder that the replicability problem in experimental psychology is sometimes referred to as a crisis.
1.2 Selective inference

Selective inference is to select whether to publish the study and what results to include and highlight among the reported results after looking at the results of the analysis. As such, selective inference has long been identified as a key problem in getting replicable results, though it is not necessarily recognized by this term. The appreciation of the problem holds mainly for selective inference which is not evident in the published work, a practice which is briefly discussed below. The effect on replicability of selective inference which is evident in the published work is less appreciated and mostly ignored. It is the topic of this work.

1.2.1 Selective inference not evident in the paper

Selective inference not evident in the paper is known mostly as publication bias and General Questionable Research practices. Publication bias (also termed the file-drawer problem, Rosenthal, 1979) results from the practice to publish only statistically significant results. Studies that do not reach statistical significance are discarded and are therefore not evident in the scientific literature. It is impossible to know that these studies were conducted and failed in the above sense. Hence, when viewing a statistically significant published result, one can never know how many failed attempts were at achieving significance. Another effect of this publication practice is the chase for results that pass the threshold by general Questionable Research practices (QRPs). Indeed, the flexibility researchers might perceive they have with the data analysis, while in truth it must be accounted for, is a major playground for selective inference. This includes research practices such as p-hacking (Simmons et al. 2011), hypothesizing after the results are known (HARKing, Kerr 1998), using unfit statistical tests, optional stopping of data collection, manipulation of outliers, malicious exclusion of participants, post-hoc analysis, and use of covariates while modeling, etc. (John et al. 2012; Button et al. 2013). Many of these are different names for the issue we are concerned with, selective inference.

Much of these and other efforts in understanding the causes of replicability problems and the possible remedies are summarized in the “Reproducibility and Replicability in science” report (R&R report) published by the National Academies of Sciences, Engineering, and Medicine (2019). The R&R report clarifies the distinction between reproducibility, which a property of the study, and replicability, which the property of its results, and emphasizes the importance of replicability efforts by others in science. The focus of that report is on improving the reproducibility of the study by increasing the transparency of the study design, data, and computations, under the umbrella of Open Science. In a more extreme step, the pre-registration movement tries to prevent publication bias by pre-approving the paper (before the data is collected), and QRP’s by pre-registering the statistical analysis to limit the degrees of freedom of the analysis.
A full reporting of all statistical tests is also recommended in the R&R report and Open Science initiative, accepting that a single study cannot be designed and end with a single result. This leads to the next type of selective inference, selecting from the multiple hypotheses evident in the published paper.

1.2.2 Selective inference evident in the paper
Selective inference evident in the paper is the practice of selecting some of the results from the many reported in the paper for highlighting. The common situation is that some results of multiple statistical tests and confidence intervals are reported in the published work and in the supplementary material. Other results are not reported but are evident from the text, tables, and figures (and their captions and footnotes). Researchers emphasize a selected subset of the results by publishing them in full detail, starring those passing a statistical significance threshold (*,**, etc.), or highlighting them in the Abstract and Discussion sections, while relaying others to supplementary material. It is quite common in psychological research to find that those highlighted are the statistically significant ones at some level. Still, these may also be the ones with the largest effect sizes or confidence intervals that do not cover some relevant value.

Such evident selective inference is unavoidable in the current 'industrialized' way science is conducted, where new technologies for data generation (computerized tracking system, fMRI, genomics, etc.), data storage, and computing software increases the number of statistical tests and inferences conducted in a single study. Moreover, it may be argued that part of the responsibility of a researcher is to highlight the important findings and summarize them for the casual reader. However, doing that in a study without adjustment for the selection increases the risk of making false-positive discoveries (Simmons et al., 2011). Unless adjusted for the selection, the selected p-values and confidence intervals lose their probabilistic meaning (Mayo 2018.)

Schnall et al. (2008), "With a clean conscience," serves as an example for evident selective inference, where some results are reported, and others are not yet are evident from the material in the paper. This study aimed to answer the question: "does priming for cleanliness affect response?". Subjects were randomly divided into a primed for cleanliness group and a control group, presented with six moral dilemmas, and asked to grade how these dilemmas made them feel with respect to 10 different emotions. A group mean score was also calculated for each of the 10 emotions across all 6 dilemmas. The researchers used two priming methods: verbal & physical, in two separate experiments. Thus, the study had 2 X 10 X 7 = 140 evident comparisons (two priming methods, ten emotions, six moral dilemmas, and the group mean score). The authors reported only six p-values, all six statistically significant at the 0.05 level without selective inference adjustment. The study concluded that the findings support the idea that moral judgment is affected by priming for cleanliness. Given the 140 evident comparisons, the expected number of falsely rejected hypotheses (if there is no effect
at all) was 7. Even if there were some real effects, the statistical strength should be much higher (the p-value much smaller) after such selection, so doubt should be cast on these results' statistical significance. The RPP replication attempt of this study failed. A second attempt with a larger number of subjects (126 as opposed to the original 43, Johnson et al. 2014) also failed. Let us emphasize: All information appears in the paper, so it was a transparent study. It could have been pre-registered, and the methods of analysis could have been detailed there as well. Notice that specifying an adjustment for the many inferences conducted is usually not required, not even by the Open Science Framework.

1.3 Our goals
The goals of this paper are to (i) Assess the level of multiplicity that is evident in psychological papers; (ii) Find to what extent the effect of such selection is being addressed; (iii) Observe how much can be gained in the replicability of the results by addressing properly the effect of selection which is evident in the published work.

We propose to account for selection when facing multiple tests by controlling the False Discovery Rate (FDR). Procedures such as the well-known BH procedure (Benjamini & Hochberg, 1995), account for selective inference by keeping the expected number of false-positive discoveries as a proportion of all discoveries under a pre-specified desired level. We shall explain, demonstrate, and make use of the relatively new hierarchical FDR controlling procedure TreeBH (Bogomolov et al. 2020), which exploits hierarchical structures that are common in experimental psychological studies, to gain power and relevance while maintaining a desired FDR. We assess the potential replicability benefits of using the TreeBH methodology in the context of Reproducibility Project in Psychology (RPP, Open Science Collaboration, 2015) where the replicability results are available.

Specifically, we analyzed all 100 papers in the RPP, and adjusted for selection where it was needed. We show that many non-replicable results would not have passed the 0.05 significance threshold used for discovery, while missing one replicable result.
2 Methods

2.1 The False Discovery Rate (FDR)

The selective inference concern when facing multiple comparisons is not foreign to experimental psychologists who are well trained in the use of post-hoc analysis or pairwise comparisons. A common and often required practice is that when comparing all levels of a factor, a protecting methodology should be used, so that the probability of making even one type I error is controlled. However, outside this context, multiplicity is usually ignored. For example, when two aspects are compared between levels, such as speed and accuracy, or the levels of more than one factor are compared, the fact that this increases the severity of the selection is usually ignored. In fact, some scientists believe that if you declare your hypotheses in advance, there is no need to adjust for multiplicity, regardless of the number of tests performed. Not a trivial underlying reason is that controlling a large ensemble of inferences for family-wise error-rate imposes a large penalty in terms of power, a penalty that increases as multiplicity increases.

A different approach for selective inference adjustment has been offered by Benjamini and Hochberg (1995), who proposed the FDR as an appropriate yet more lenient goal when encountering multiple inferences. The false discovery proportion is the proportion of errors among the discoveries made (0 if none is made), and the FDR is its expectation. In the context of testing, controlling the FDR at the 0.05 level maintains that at most, 5% of results deemed “statistically significant” will be false positives on average. Regular unadjusted testing fails to guarantee it, so Benjamini & Hochberg have introduced the BH procedure for controlling the FDR at a desired level q (also known as \( \alpha \), usually 0.05. For full description, see supplementary materials A). If many true results are evident (i.e., many p-values are smaller than q), the BH method is almost as unadjusted testing; if merely one exists, the BH method yield results identical to the traditional Bonferroni. This makes sense since while inspecting 100 features, two false discoveries among 50 discovered is bearable. Two false ones out of four is unbearable. Furthermore, similarly to the p-value, one can define the FDR-adjusted p-value (sometimes called q-value), which can be compared with any desired significance level. With the many variations introduced later, the FDR approach and the BH procedure have gained extensive usage in many scientific fields. Later work has further extended this approach to confidence intervals that control the false coverage-statement rate (FCR, Benjamini and Yekutieli, 2003). As Shaffer (2005) noted, the availability of such dual confidence intervals means that the BH procedure further controls not only the type-I errors but also the false directional errors (called type-S errors by Gelman & Tuerlinckx, 2000.)
2.2 Hierarchical FDR
However, the BH procedure does not exploit any inferential structure among the tested hypotheses in more complex studies. For example, consider Goschke & Dreisbach (2008), which was replicated in the RPP, where subjects had to notice rare prospective memory cues. Figure 1 illustrates the structure of the inference in the paper. There were three 3-way ANOVAs, each designed to support a different hypothesis (not completely de-similar from one another, but rather interchangeable in meaning. For example: incompatible regular trials in one 3-way ANOVA and incompatible trials on which the stimulus was semantically related to the Prospective memory cue in another). Each of the 3-way ANOVAs tested in principle three main effects, three 2-way interactions, and one 3-way interaction. Seven hypotheses in total for each of the 3-way ANOVAs. As the study reports, rare prospective memory cues were overlooked more often on non-compatible trials than on compatible trials, and between task relevance and irrelevance. The replication target was a 2-way interaction between the two, $F(1, 38) = 6.21, p=0.0172$. In this case, using the plain BH procedure (i.e., adjusting for the 21 tests done in this paper) would not yield a significant result. Traditional post-hoc methods would also not yield a significant result, as they are more conservative.

Benjamini & Bogomolov (2014) proposed a two-level hierarchical testing procedure, later generalized to three or more levels by Bogomolov et al. (2020), that controls a relevant generalization of the FDR and follows the structure of the inference, such as in Fig 1. Starting at the first level, these three 3-way ANOVAs are tested as one family controlling the FDR at some level $q$ (usually .05). Each of the $k$ rejected ANOVAs leads to testing a family of seven hypotheses at the second level. These are tested at a $q$ level lowered by the factor of $k/3$. In our example, all three ANOVAs at the first level were statistically significant, so no attenuation of the $q$ level was needed at the family below (a short TreeBH tutorial can be found in the supplementary materials). The hierarchical FDR adjusted $p$-value of the targeted interaction was, therefore, the same as the BH adjusted $p$-value for the family of 7, which ended being $p_{adj} = 0.0172*7/3 = 0.0401$. Reassuringly, the result was replicated successfully in the RPP.

![Figure 1: FDR tree schematic for the case of Goschke & Dreisbach (2008). Significant results at the 0.05 level after hierarchical BH adjustment are Bolded. The interaction between Prospective Memory and Compatibility (PM X Comp) was the target for replication in the RPP.](image-url)
The main advantage of such hierarchical methods is the flexibility of the statistical analysis without increasing the rate of false positives among the discoveries. As you analyze your data, you can “turn off” branches of the study that did not yield positive results. By that, you can focus your adjustment on the statistically interesting branches of the study without them being affected by the details of other branches of the study. For example, if we analyze a 3 by 3 2-way Anova, each main effect has 3 pairwise comparisons while the interaction has 18 (24 possible pairwise comparisons overall, and even more contrasts.) Still, they can be sorted in a hierarchical structure that reflects the interest of the researcher and may drastically reduce the number actually tested. Suppose the interaction effect is statistically not significant, a researcher can investigate down the two significant main effects at the $\frac{2}{3} \cdot 0.05$ level and “turn-off” 18 post-hoc pairwise comparisons of the interaction, which would have joined as noise to the three pairwise comparisons per factor. This is especially useful if the true effects are clustered at few families.

It is very common to “turn-off” branches in ANOVA when one or more main effects or interaction are not statistically significant, without any adjustments for the other effects. However, that method of turning off branches does not maintain a .05 type I error level (Homack 2001) nor .05 FDR level.

2.3 The data: The Reproducibility Project in Psychology (RPP)

We make use of the data of the RPP by the Open Science Collaboration (2015). In this study, several research groups have attempted to replicate one result from each of 100 papers published in 2008 in three Psychological journals: Psychological Science (PSCI), Journal of Personality and Social Psychology (JPSP), and Journal of Experimental Psychology: Learning, Memory, and Cognition (JEP: LMC). We note that some criticisms have been leveled at the RPP regarding the extent to which the original experiments’ methods were followed (Stroebe 2016, Gilbert et al. 2016).

For each of the 100 original papers, we estimated the number of evident comparisons and the hierarchical structure of the hypotheses as implied from how they were presented. The original p-value was adjusted for multiplicity using the TreeBH method of Bogomolov et al. (2020). The supplementary material details the hierarchical BH analysis for each experiment in each paper. Out of the 100 replication attempts, two studies did not publish the statistical results, one original result was replicated twice, three replications were attempts to establish non-significance (achieving $p > 0.05$), and six studies had an original result of $p > 0.05$ upon our recalculation (although reported as statistically significant results and therefore exposed to replication attempts). All 12 were set aside (as detailed in the supplementary materials). The remaining 88 original results, 32 of whom were replicated successfully (36%), were utilized to assess the effect of addressing selection.
3 Results

3.1 The number of evident statistical tests in the RPP papers
For each paper in the RPP, we estimated the number of evident tests in the paper (see Schnall example above). The number of evident tests ranged from a minimum of 5 to a maximum of 730, averaging 77.7 with a SD of 95.7. There was some difference in the number of evident tests between papers dealing with Cognitive Psychology ($M=61$, $Md=26.5$, $SD=72$, $N=26$) and papers dealing with Social Psychology ($M=100$, $Md=69$, $SD=123$, $N=44$; $t_{(df=53)} = 1.95$, $p = 0.057$, $p_{adj} = 0.114$).

Figure 2: Notched boxplot of the number of evident comparisons per paper (log scale) in Cognitive and Social Psychology.

3.2 Addressing selective inference in RPP
Unfortunately, selective inference received truly little attention. There were few to no selective inference adjustments or any text dedicated to discussing this problem in most papers. Not a single paper adjusted for selective inference appropriately. That is, no paper encompassed all its statistical tests into the selective inference adjustment framework. Only 8 of the papers in the RPP reported any selective inference adjustments.

The possibly surprising result here is that the papers that made a partial adjustment for selective inference had fewer comparisons ($M=40$, $Md=24$, $SD=34.5$, $N=8$) than those who did not adjust at all ($M=83$, $Md=58$, $SD=102$, $N=85$; $t_{(df=10)} = 1.55$, $p = 0.15$, $p_{adj} = 0.15$).

This might be due to the high cost of power in the familywise error rate. For the rarely used FDR controlling methods, this need not be the case (Table 1). However, the number of papers adjusting for selection was too small to draw any clear conclusion.

3.3 The effect of Selective inference adjustment on replicability
Each of the 88 papers was read and analyzed in the above way by two of the authors, and by all three when in doubt. Hierarchical models were built based on the apparent structure of the studies, as reported in the original papers. Next, adjustment for selective inference using TreeBH was made on the original results in the focus of the replication attempts. The results per paper are given in Supplementary Material.
Summarizing them, results that were statistically significant at the .05 level after selective inference adjustments with the TreeBH were 97% of the replicable results (31 out of 32 results) while maintaining a replicability rate of about 46% (31 out of 67 were replicated). On the other hand, only 1 out of 21 results (5%) were not statistically significant after selective inference adjustments yet replicated (Table 1).

| Selective inference adjustment | Replication’s p-value | Total |
|-------------------------------|------------------------|-------|
|                               | p ≤ .05 | p > .05 |       |
| Original result after TreeBH adjustment | p_{adj} ≤ .05 | 31 | 36 | 67 |
|                               | p_{adj} > .05 | 1   | 20  | 21  |
| Total                         | 32   | 56   | 88  |

Table 1: Replication before and after FDR adjustment for selective inference. \( \chi^2(df = 1, N = 88) = 11.9, p < 0.0001, p_{adj} < 0.0001 \).

A further examination found that papers that did not replicate suffered more from multiplicity than those who did replicate (fig 3).

**Figure 2:** Scatter plots of the TreeBH adjusted versus the original P-values. Red circles depict failed replications while Green triangles represent replicated ones. (Left) The dashed line depicts the significance threshold for the BH procedure, while the black line represents no change in p-value due to multiplicity. (Right) A scatter plot of the effect of multiplicity adjustment on the p-value (TreeBH adjusted p-value difference from the original p-value) presented on the square root scale. Horizontal black lines represent no change in p-value after multiplicity adjustment.
4. Discussion

4.1 the importance of selective inference adjustment in practice
Although the papers in RPP had on average 77.7 evident statistical tests, only 8 found it necessary enough to address selective inference at any level. Moreover, even those 8 did not consider the full effect of selective inference on the interpretation of their results. Here, we showed that publication of "significant" results that are statistically not significant after full adjustment for multiplicity is deceiving as they rarely get replicated (1 in 21, incidentally, the type I error rate). Moreover, by adjusting for false discovery rate with the hierarchical approach, one can enhance the replicability of results without giving up much on power (one out of the 31 replicated results was not significant after the adjustment, table 1). Interestingly, ten of the failed replication protocols were “not endorsed” by the original. New replication efforts with revised protocols have been planned and pre-registered to test if any of the 10 failed replications can be explained to poor replication design. All 10 original TreeBH adjusted p-values were statistically significant. 2 of those revised replications suggested a replicated effect (Ebersole et al. 2020), implying that the conclusions from Table 1 are even stronger.

In view of the above results, we argue that reporting unadjusted “significant” p-values that would not pass the significance threshold if they were adjusted, is not only fallacious but amounts to deceiving the readers, as adjusted statistical results that are not significant can rarely be replicated. Therefore, we recommend that the usual way of reporting p-values should always be amended with adjusted p-values. Since the multiplicity adjustment should consider the entire body of results, the method chosen (e.g., BH, TreeBH, Bonferroni, or other methods) can be specified once in the method section, and only “p_{adj} = .012” should be added to the common way of reporting of statistical tests results.

4.2 Isn’t it the p-value’s fault?
Most scientific research have been using significance testing (based on P-value) as a decision tool to separate the highlighted experimental results from those prone to be generated by pure chance. Therefore, it is natural that general questionable research practices (QRPs) that are blamed for the crisis were mostly related to the use of p-values, which, in turn, led to an attack on the concept of the p-value itself. The attack, stirred by some psychological journals (Trafimow & Marks, 2015; Lee 2016; Amrhein et al. 2019) and conducted in leading scientific and non-scientific venues, gave rise to a formal statement by the American Statistical Association (Wasserstein & Lazar, 2016). Although it did not rule out the use of p-value, it was cautious about its use and discussed alternative measures, including confidence intervals, likelihood ratios, and Bayesian methods. Unfortunately, alternative methods do not offer an advantage over
the p-value in minimizing this problem (Savalei & Dunn, 2015). In particular, to reach a conclusion about an effect, checking whether a 95% confidence interval covers the null or not is equivalent to the use of p-value ≤ 0.05, although it is proposed as an “alternative” (Benjamini 2016). Hence, the proper use of CI requires it to be adjusted for selection as well (Benjamini & Yekutieli, 2005). However, the use of multiple CI adjustment is almost non-existance in the field of psychology. Hence, the move toward unadjusted CI instead of the p-value might increase the replicability problem as it does not allow even an informal scrutinizing of the multiplicity effect as p-values allow. For example, one can assess the strength of a specific p-value given the number of p-value calculated (using the Bonferroni method, for example). However, CI cannot be informally assessed without further computations.

The p-value should remain as a central inference tool, as it is the most assumptions-free statistical tool, and these minimal assumptions hold for well-designed experiments. This recognition is evident even among the participants of a follow-up meeting convened by the American Statistical Association (ASA) under the heading “Statistics in the 21st century: a world beyond p ≤ 0.05.” The editorial summarizing the conference (Wasserstein et al. 2019) was perceived as a second and more definitive ASA call against the use of p-values and “bright line” thresholds in general – even though it did not carry ASA stamp of approval. However, about half of the 43 papers did find a role for the p-value, and some even emphasized its vital role. Interestingly, the p-values and the p-values adjusted for selection presented in this paper require no pre-assigned threshold, and they can be compared with any level the reader wishes to use. In contrast, to construct a confidence interval (whether frequentist or Bayesian), one must specify a threshold (say 95% confidence), and this threshold bounds the displayed interval. Offering confidence intervals instead of p-values as a way of avoiding thresholds is illogical. Using CI’s in tandem with the p-value is always preferable when relevant and feasible. Finally, it is important to emphasize that questionable research practices in general and those that concern selection after viewing the data are not limited to the p-value. For example, a move to a Bayesian-based decision making will ultimately lead to the same multiplicity problems as explained extensively in this paper regarding the p-value (de Jong 2019), Bayes-hacking, and Bayesian hypothesizing after the results are known (BARKing).
4.3 Multiple replications
We have used Fisher’s formulation for assessing replicability in a follow-up study, where results are required to statistically significant at some prescribed level and consistently in the same direction in both studies. A generalization to more than two studies can be found in Benjamini & Heller (2008), and a call for multiple replications rather than one large and definitive one has been voiced in Schooler (2014) and embodied in the multi-lab projects. Multiple replication studies allow the use of a different approach, treating the effect of the studies as random. Then, either the lack of significance of the Study-by-Effect interaction is considered as an evidence of replicability (e.g., Crabbe et al. 1999), or the significance of the effect while taking into consideration the random interaction is considered as evidence of replicability (Kafkafi et al. 2005). However, these approaches address replicability only after replication studies were conducted, and do not carry different implications about enhancing the replicability of an original stand-alone study (which is the target of this work). The only exception is the proposal by Kafkafi et al. (2017), relying on the laboratory (study) as a random effect. It uses a database to estimate the laboratory-by-effect interaction, incorporating it in a corrected test and CI for the stand-alone study. The original suggestion was made in the field of animal behavior, and in our future work, we explore whether a similar approach is relevant in experimental psychology, using the data generated by the multi-lab projects.

4.5 Conclusion
The general goal remains to improve the replicability rate, decrease the rate of false discoveries, and focus the replication attempts. While it is commonly accepted that addressing selective inference not evident in the paper is crucial, the results here show that addressing selective inference from the many evident in the published work is essential as well. Adjusting for it is an important tool that can sort out many false positives without missing true effects. Therefore, we propose to report exact selective inference adjusted p-values (and adjusted CIs, which we only mentioned in passing). We claim that reporting only unadjusted results is negligent, especially given the easy deployment of FDR controlling adjustment tools and the minor loss of power. We believe that addressing evident selective inference is an essential missing piece in the replicability puzzle, and should be a required way of enhancing replicability in experimental psychology.
Acknowledgment

This work was supported by H2020 European Research Council through Human Brain Project and US–Israel Binational Science Foundation grant 0604317633.

References

Achenbach, J. (2015). Why do many reasonable people doubt science. National Geographic, 14(5), 2-8.

Amrhein, V., Greenland, S., & McShane, B. (2019). Scientists rise up against statistical significance.

Anvari, F., & Lakens, D. (2019). The Replicability Crisis and Public Trust in Psychological Science: REPLICABILITY CRISIS AND PUBLIC TRUST.

Baker, M. (2016). 1,500 scientists lift the lid on reproducibility. Nature News, 533(7604), 452.

Benjamini, Y. (2016). It’s not the p-values’ fault. The ASA’s statement on p-values: context, process, and purpose. The American Statistician, 70(2), 129-133.

Benjamini, Y., & Bogomolov, M. (2014). Selective inference on multiple families of hypotheses. Journal of the Royal Statistical Society: Series B (Statistical Methodology), 76(1), 297-318.

Benjamini, Y., & Heller, R. (2008). Screening for partial conjunction hypotheses. Biometrics, 64(4), 1215-1222.

Benjamini, Y., & Hochberg, Y. (1995). Controlling the false discovery rate: a practical and powerful approach to multiple testing. Journal of the Royal statistical society: series B (Methodological), 57(1), 289-300.

Benjamini, Y., & Yekutieli, D. (2005). False discovery rate–adjusted multiple confidence intervals for selected parameters. Journal of the American Statistical Association, 100(469), 71-81.

Bogomolov, W., Peterson, C. B., Benjamini, Y., & Sabatti, C. (2020). Hypotheses on a tree: new error rates and testing strategies. Biometrika.
Button, K. S., Ioannidis, J. P., Mokrysz, C., Nosek, B. A., Flint, J., Robinson, E. S., & Munafò, M. R. (2013). Power failure: why small sample size undermines the reliability of neuroscience. *Nature Reviews Neuroscience, 14*(5), 365.

Camerer, C. F., Dreber, A., Forsell, E., Ho, T. H., Huber, J., Johannesson, M., … & Heikensten, E. (2016). Evaluating replicability of laboratory experiments in economics. *Science, 351*(6280), 1433-1436.

Camerer, C. F., Dreber, A., Holzmeister, F., Ho, T. H., Huber, J., Johannesson, M., … & Altmejd, A. (2018). Evaluating the replicability of social science experiments in Nature and Science between 2010 and 2015. *Nature Human Behaviour, 2*(9), 637.

Crabbe, J. C., Wahlsten, D., & Dudek, B. C. (1999). Genetics of mouse behavior: interactions with laboratory environment. *Science, 284*(5420), 1670-1672.

de Jong, T. (2019). A Bayesian Approach to the Correction for Multiplicity.

Ebersole, C. R., Atherton, O. E., Belanger, A. L., Skulborstad, H. M., Allen, J. M., Banks, J. B., … & Brown, E. R. (2016). Many Labs 3: Evaluating participant pool quality across the academic semester via replication. *Journal of Experimental Social Psychology, 67*, 68-82.

Ebersole, C. R., Nosek, B. A., Kidwell, M. C., Buttrick, N., Baranski, E., & Hartshorne, J. Many Labs 5: Testing pre-data collection peer review as an intervention to increase replicability.

Fanelli, D. (2009). How many scientists fabricate and falsify research? A systematic review and meta-analysis of survey data. *PloS one, 4*(5), e5738.

Fisher, R. A. (1935). The fiducial argument in statistical inference. *Annals of eugenics, 6*(4), 391-398.

Fisher, R. A. (1935). The logic of inductive inference. *Journal of the royal statistical society, 98*(1), 39-82.

Frank, M. C., Bergelson, E., Bergmann, C., Cristia, A., Floccia, C., Gervain, J., … & Lew-Williams, C. (2017). A collaborative approach to infant research: Promoting reproducibility, best practices, and theory-building. *Infancy, 22*(4), 421-435.

Gelman, A., & Loken, E. (2013). The garden of forking paths: Why multiple comparisons can be a problem, even when there is no “fishing expedition” or “p-hacking” and the research hypothesis was posited ahead of time. *Department of Statistics, Columbia University.*
Gelman, A., & Tuerlinckx, F. (2000). Type S error rates for classical and Bayesian single and multiple comparison procedures. *Computational Statistics, 15*(3), 373-390.

Gertler, P., Galiani, S., & Romero, M. (2018). How to make replication the norm.

Gilbert, D. T., King, G., Pettigrew, S., & Wilson, T. D. (2016). Comment on “Estimating the reproducibility of psychological science”. *Science, 351*(6277), 1037-1037.

Goodman, S. N., Fanelli, D., & Ioannidis, J. P. (2016). What does research reproducibility mean?. *Science translational medicine, 8*(341), 341ps12-341ps12.

Goschke, T., & Dreisbach, G. (2008). Conflict-triggered goal shielding: Response conflicts attenuate background monitoring for prospective memory cues. *Psychological Science, 19*(1), 25-32.

Homack, S. R. (2001). Understanding What ANOVA Post Hoc Tests Are, Really.

Hung, K., & Fithian, W. (2019). Statistical Methods for Replicability Assessment. *arXiv preprint arXiv:1903.08747.*

Iyengar, S., & Greenhouse, J. B. (1988). Selection models and the file drawer problem. *Statistical Science, 109*-117.

John, L. K., Loewenstein, G., & Prelec, D. (2012). Measuring the prevalence of questionable research practices with incentives for truth telling. *Psychological science, 23*(5), 524-532.

Johnson, D. J., Cheung, F., & Donnellan, M. B. (2014). Does cleanliness influence moral judgments?. *Social Psychology.

Kafkafi, N., Benjamini, Y., Sakov, A., Elmer, G. I., & Golani, I. (2005). Genotype–environment interactions in mouse behavior: a way out of the problem. *Proceedings of the National Academy of Sciences, 102*(12), 4619-4624.

Kafkafi, N., Golani, I., Jaljuli, I., Morgan, H., Sarig, T., Würbel, H., ... & Benjamini, Y. (2017). Addressing reproducibility in single-laboratory phenotyping experiments. *Nature methods, 14*(5), 462.

Kerr, N. L. (1998). HARKing: Hypothesizing after the results are known. *Personality and Social Psychology Review, 2*(3), 196-217.

Klein, R. A., Ratliff, K. A., Vianello, M., Adams Jr, R. B., Bahník, Š., Bernstein, M. J., ... & Cemalcilar, Z. (2014). Investigating variation in replicability. *Social psychology.
Klein, R. A., Vianello, M., Hasselman, F., Adams, B. G., Adams Jr, R. B., Alper, S., ... & Batra, R. (2018). Many Labs 2: Investigating variation in replicability across samples and settings. Advances in Methods and Practices in Psychological Science, 1(4), 443-490.

Lee, D. K. (2016). Alternatives to P value: confidence interval and effect size. Korean journal of anesthesiology, 69(6), 555.

Lithgow, G. J., Driscoll, M., & Phillips, P. (2017). A long journey to reproducible results. Nature News, 548(7668), 387.

Mayo, D. G. (2018). Statistical inference as severe testing. Cambridge: Cambridge University Press.

National Academies of Sciences, Engineering, and Medicine. (2019). Reproducibility and replicability in science. National Academies Press.

Nosek, B. A., & Errington, T. M. (2017). Reproducibility in cancer biology: Making sense of replications. Elife, 6, e23383.

Open Science Collaboration. (2015). Estimating the reproducibility of psychological science. Science, 349(6251), aac4716.

Peng, R. D. (2011). Reproducible research in computational science. Science, 334(6060), 1226-1227.

Prinz, F., Sch Lange, T., & Asadullah, K. (2011). Believe it or not: how much can we rely on published data on potential drug targets?. Nature reviews Drug discovery, 10(9), 712.

Reiner, A., Yekutieli, D., & Benjamini, Y. (2003). Identifying differentially expressed genes using false discovery rate controlling procedures. Bioinformatics, 19(3), 368-375.

Rosenthal, R. (1979). The file drawer problem and tolerance for null results. Psychological bulletin, 86(3), 638.

Savalei, V., & Dunn, E. (2015). Is the call to abandon p-values the red herring of the replicability crisis?. Frontiers in psychology, 6, 245.

Schnall, S., Benton, J., & Harvey, S. (2008). With a clean conscience: Cleanliness reduces the severity of moral judgments. Psychological science, 19(12), 1219-1222.

Schooler, J. W. (2014). Metascience could rescue the ‘replication crisis’. Nature News, 515(7525), 9.

Schooler, JW (2014) Metascience could rescue the replication crisis. Nature, 515, 9.
Simes, R. J. (1986). An improved Bonferroni procedure for multiple tests of significance. *Biometrika*, 73(3), 751-754.

Simmons, J. P., Nelson, L. D., & Simonsohn, U. (2011). False-positive psychology: Undisclosed flexibility in data collection and analysis allows presenting anything as significant. *Psychological science*, 22(11), 1359-1366.

Smith, N. C. (1970). Replication studies: A neglected aspect of psychological research. *American Psychologist*, 25(10), 970.

Stroebel, W. (2016). Are most published social psychological findings false?. *Journal of Experimental Social Psychology*, 66, 134-144.

Trafimow, D., & Marks, M. (2015). Editorial in Basic and Applied Social Psychology. *Basic and Applied Social Psychology*, 37, 1-2.

Valentine, J. C., Biglan, A., Boruch, R. F., Castro, F. G., Collins, L. M., Flay, B. R., ... & Schinke, S. P. (2011). Replication in prevention science. *Prevention Science*, 12(2), 103.

van Aert, R. C., & Van Assen, M. A. (2017). Bayesian evaluation of effect size after replicating an original study. *PloS one*, 12(4), e0175302.

van Aert, R. C., & van Assen, M. A. (2018). Examining reproducibility in psychology: A hybrid method for combining a statistically significant original study and a replication. *Behavior research methods*, 50(4), 1515-1539.

Vanpaemel, W., Vermorgen, M., Deriemaecker, L., & Storms, G. (2015). Are we wasting a good crisis? The availability of psychological research data after the storm. *Collabra: Psychology*, 1(1).

Wasserstein, R. L., & Lazar, N. A. (2016). The ASA’s statement on p-values: context, process, and purpose. *The American Statistician*, 70(2), 129-133.

Wasserstein, R. L., Schirm, A. L., & Lazar, N. A. (2019). Moving to a world beyond “p<0.05”.