Justify Your Alpha

In Press, Nature Human Behavior

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Acknowledgements: We'd like to thank Dale Barr, Felix Cheung, David Colquhoun, Hans IJzerman, Harvey Motulsky, and Richard Morey for helpful discussions while drafting this commentary. Daniel Lakens was supported by NWO VIDI 452-17-013. Federico G. Adolfi was supported by CONICET. Matthew Apps was funded by a Biotechnology and Biological Sciences Research Council AFL Fellowship (BB/M013596/1). Gary Collins was supported by the NIHR Biomedical Research Centre, Oxford. Zander Crook was supported by the Economic and Social Research Council [grant number C106891X]. Emily S. Cross was supported by the European Research Council (ERC-2015-StG-677270). Lisa DeBruine is supported by the European Research Council (ERC-2014-CoG-647910 KINSHIP). Anne-Laura van Harmelen is funded by a Royal Society Dorothy Hodgkin Fellowship (DH150176). Mark R. Hoffarth was supported by the National Science Foundation under grant SBE SPRF-FR 1714446. Junpeng Lao was supported by the SNSF grant 100014_156490/1. Cilene Lino de Oliveira was supported by AvH, Capes, CNPq. Andrea E. Martin was supported by the Economic and Social Research Council of the United Kingdom [grant number ES/K009095/1]. Jean-Jacques Orban de Xivry is supported by an internal grant from the KU Leuven (STG/14/054) and by the Fonds voor Wetenschappelijk Onderzoek (1519916N). Sam Parsons was supported by the European Research Council (FP7/2007–2013; ERC grant agreement no; 324176). Gerine Lodder was funded by NWO VICI 453-14-

016. Samuel Smith is supported by a Cancer Research UK Fellowship (C42785/A17965). Vishnu Sreekumar was supported by the NINDS Intramural Research Program (IRP). Miguel A. Vadillo was supported by Grant 2016-T1/SOC-1395 from Comunidad de Madrid. Tal Yarkoni was supported by NIH award R01MH109682.

Competing Interests: The authors declare no competing interests.

Abstract: In response to recommendations to redefine statistical significance to $p \leq .005$, we propose that researchers should transparently report and justify all choices they make when designing a study, including the alpha level.

Justify Your Alpha

Benjamin et al.[1] proposed changing the conventional "statistical significance" threshold (i.e., the alpha level) from $p \leq .05$ to $p \leq .005$ for all novel claims with relatively low prior odds. They provided two arguments for why lowering the significance threshold would "immediately improve the reproducibility of scientific research." First, a *p*-value near .05 provides weak evidence for the alternative hypothesis. Second, under certain assumptions, an alpha of .05 leads to high false positive report probabilities (FPRP[2]; the probability that a significant finding is a false positive).

We share their concerns regarding the apparent non-replicability of many scientific studies, and agree that a universal alpha of .05 is undesirable. However, redefining "statistical significance" to a lower, but equally arbitrary threshold, is inadvisable for three reasons: (1) there is insufficient evidence that the current standard is a "leading cause of non-reproducibility"[1]; (2) the arguments in favor of a blanket default of $p \leq .005$ do not warrant the immediate and widespread implementation of such a policy; and (3) a lower significance threshold will likely have negative consequences not discussed by Benjamin and colleagues. We conclude that the term "statistically significant" should no longer be used and suggest that researchers employing null hypothesis significance testing justify their choice for an alpha level before collecting the data, instead of adopting a new uniform standard.

Lack of evidence that $p \leq .005$ improves replicability

Benjamin et al.[1] claimed that the expected proportion of replicable studies should be considerably higher for studies observing $p \leq .005$ than for studies observing $.005 < p \leq .05$, due to a lower FPRP. *Theoretically*, replicability is related to the FPRP, and lower alpha levels will reduce false positive results in the literature. However, *in practice*, the impact of lowering alpha levels depends on several unknowns, such as the prior odds that the

examined hypotheses are true, the statistical power of studies, and the (change in) behavior of researchers in response to any modified standards.

An analysis of the results of the Reproducibility Project: Psychology[3] showed that 49% (23/47) of the original findings with *p*-values below .005 yielded $p \leq .05$ in the replication study, whereas only 24% (11/45) of the original studies with $.005 < p \leq .05$ yielded $p \leq .05$ ($\chi^2(1) = 5.92$, $p = .015$, $BF_{10} = 6.84$). Benjamin and colleagues presented this as evidence of "potential gains in reproducibility that would accrue from the new threshold." According to their own proposal, however, this evidence is only "suggestive" of such a conclusion, and there is considerable variation in replication rates across *p*-values (see Figure 1). Importantly, lower replication rates for *p*-values just below .05 are likely confounded by *p*-hacking (the practice of flexibly analyzing data until the *p*-value passes the "significance" threshold). Thus, the differences in replication rates between studies with $.005 < p \leq .05$ compared to those with $p \leq .005$ may not be entirely due to the level of evidence. Further analyses are needed to explain the low (49%) replication rate of studies with $p \leq .005$, before this alpha level is recommended as a new significance threshold for novel discoveries across scientific disciplines.

Weak justifications for the α = .005 threshold

We agree with Benjamin et al. that single *p*-values close to .05 never provide strong "evidence" against the null hypothesis. Nonetheless, the argument that *p*-values provide weak evidence based on Bayes factors has been questioned[4]. Given that the marginal likelihood is sensitive to different choices for the models being compared, redefining alpha levels as a function of the Bayes factor is undesirable. For instance, Benjamin and colleagues stated that *p*-values of .005 imply Bayes factors between 14 and 26. However, these upper bounds only hold for a Bayes factor based on a point null model and when the *p*-value is calculated for a two-sided test, whereas one-sided tests or Bayes factors for non-

point null models would imply different alpha thresholds. When a test yields BF = 25 the data are interpreted as strong relative evidence for a specific alternative (e.g., μ = 2.81), while a $p \leq .005$ only warrants the more modest rejection of a null effect without allowing one to reject even small positive effects with a reasonable error rate[5]. Benjamin et al. provided no rationale for why the new *p*-value threshold *should* align with equally arbitrary Bayes factor thresholds. We question the idea that the alpha level at which an error rate is controlled should be based on the amount of relative evidence indicated by Bayes factors.

The second argument for $\alpha = .005$ is that the FPRP can be high with $\alpha = .05$. Calculating the FPRP requires a definition of the alpha level, the power of the tests examining true effects, and the ratio of true to false hypotheses tested (the prior odds). Figure 2 in Benjamin et al. displays FPRPs for scenarios where most hypotheses are false, with prior odds of 1:5, 1:10, and 1:40. The recommended $p \leq .005$ threshold reduces the *minimum* FPRP to less than 5%, assuming 1:10 prior odds (the true FPRP might still be substantially higher in studies with very low power). This prior odds estimate is based on data from the Reproducibility Project: Psychology[3] using an analysis modelling publication bias for 73 studies[6]. Without stating the reference class for the "base-rate of true nulls" (e.g., does this refer to all hypotheses in science, in a discipline, or by a single researcher?), the concept of "prior odds that H1 is true" has little meaning. Furthermore, there is insufficient representative data to accurately estimate the prior odds that researchers examine a true hypothesis, and thus, there is currently no strong argument based on FPRP to redefine statistical significance.

How a threshold of p ≤ .005 might harm scientific practice

Benjamin et al. acknowledged that their proposal has strengths as well as weaknesses, but believe that its "efficacy gains would far outweigh losses." We are not convinced and see at least three likely negative consequences of adopting a lowered threshold.

Risk of fewer replication studies. All else being equal, lowering the alpha level requires larger sample sizes and creates an even greater strain on already limited resources. Achieving 80% power with α = .005, compared to α = .05, requires a 70% larger sample size for between-subjects designs with two-sided tests (88% for one-sided tests). While Benjamin et al. propose α = .005 exclusively for "new effects" (and not replications), designing larger original studies would leave fewer resources (i.e., time, money, participants) for replication studies, assuming fixed resources overall. At a time when replications are already relatively rare and unrewarded, lowering alpha to .005 might therefore reduce resources spent on replicating the work of others. More generally, recommendations for evidence thresholds need to carefully balance statistical and non-statistical considerations (e.g., the value of evidence for a novel claim vs. the value of independent replications).

Risk of reduced generalisability and breadth. Requiring larger sample sizes across scientific disciplines may exacerbate over-reliance on convenience samples (e.g., undergraduate students, online samples). Specifically, without (1) increased funding, (2) a reward system that values large-scale collaboration, and (3) clear recommendations for how to evaluate research with sample size constraints, lowering the significance threshold could adversely affect the breadth of research questions examined. Compared to studies that use convenience samples, studies with unique populations (e.g., people with rare genetic variants, patients with post-traumatic stress disorder) or with time- or resource-intensive data collection (e.g., longitudinal studies) require considerably more research funds and effort to increase the sample size. Thus, researchers may become less motivated to study unique populations or collect difficult-to-obtain data, reducing the generalisability and breadth of findings.

Risk of exaggerating the focus on single p-values. Benjamin et al.'s proposal risks (1) reinforcing the idea that relying on *p*-values is a sufficient, if imperfect, way to evaluate findings, and (2) discouraging opportunities for more fruitful changes in scientific practice

and education. Even though Benjamin et al. do not propose $p \leq .005$ as a publication threshold, some bias in favor of significant results will remain, in which case redefining $p \leq .005$ as "statistically significant" would result in greater upward bias in effect size estimates. Furthermore, it diverts attention from the cumulative evaluation of findings, such as converging results of multiple (replication) studies.

No one alpha to rule them all

We have two key recommendations. First, we recommend that the label “statistically significant” should no longer be used. Instead, researchers should provide more meaningful interpretations of the theoretical or practical relevance of their results. Second, authors should transparently specify—and justify—their design choices. Depending on their choice of statistical approach, these may include the alpha level, the null and alternative models, assumed prior odds, statistical power for a specified effect size of interest, the sample size, and/or the desired accuracy of estimation. We do not endorse a single value for any design parameter, but instead propose that authors justify their choices before data are collected. Fellow researchers can then evaluate these decisions, ideally also prior to data collection, for example, by reviewing a Registered Report submission[7]. Providing researchers (and reviewers) with accessible information about ways to justify (and evaluate) design choices, tailored to specific research areas, will improve current research practices.

Benjamin et al. noted that some fields, such as genomics and physics, have lowered the “default” alpha level. However, in genomics the overall false positive rate is still controlled at 5%; the lower alpha level is only used to correct for multiple comparisons. In physics, researchers have argued against a blanket rule, and for an alpha level based on factors such as the surprisingness of the predicted result and its practical or theoretical impact[8]. In non-human animal research, minimizing the number of animals used needs to be directly balanced against the probability and cost of false positives. Depending on these and other

considerations, the optimal alpha level for a given research question could be higher or lower than the current convention of .05[9,10,11].

Benjamin et al. stated that a “critical mass of researchers” endorse the standard of a $p \leq .005$ threshold for “statistical significance.” However, the presence of a critical mass can only be identified *after* a norm has been widely adopted, not *before*. Even if a $p \leq .005$ threshold were widely accepted, this would only reinforce the misconception that a single alpha level is universally applicable. Ideally, the alpha level is determined by comparing costs and benefits against a utility function using decision theory[12]. This cost-benefit analysis (and thus the alpha level)[13] differs when analyzing large existing datasets compared to collecting data from hard-to-obtain samples.

Conclusion

Science is diverse, and it is up to scientists to justify the alpha level they decide to use. As Fisher noted[14]: "...no scientific worker has a fixed level of significance at which, from year to year, and in all circumstances, he rejects hypotheses; he rather gives his mind to each particular case in the light of his evidence and his ideas." Research should be guided by principles of *rigorous science*[15], not by heuristics and arbitrary blanket thresholds. These principles include not only sound statistical analyses, but also experimental redundancy (e.g., replication, validation, and generalisation), avoidance of logical traps, intellectual honesty, research workflow transparency, and accounting for potential sources of error. Single studies, regardless of their *p*-value, are never enough to conclude that there is strong evidence for a substantive claim. We need to train researchers to assess cumulative evidence and work towards an unbiased scientific literature. We call for a broader mandate beyond *p*-value thresholds whereby all *justifications* of key choices in research design and statistical practice are transparently evaluated, fully accessible, and pre-registered whenever feasible.

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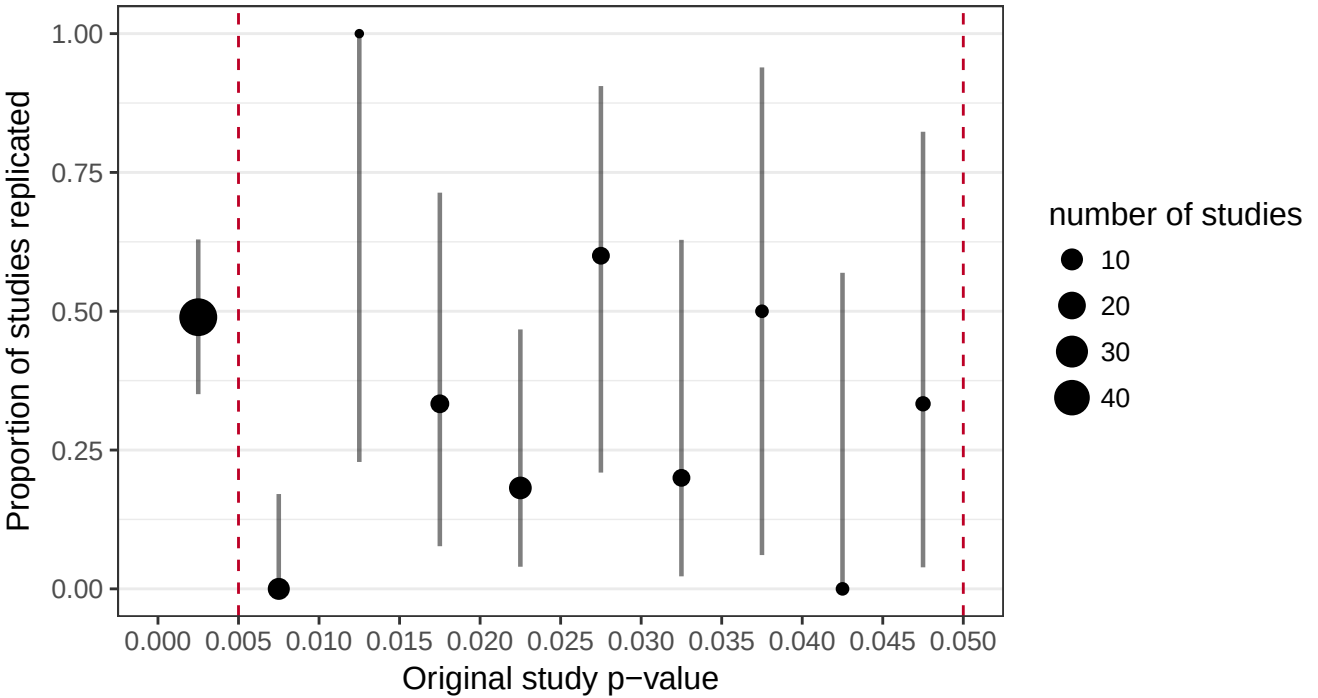
Figure Caption

Figure 1. The proportion of studies[3] replicated at α = .05 (with a bin width of .005). Window start and end positions are plotted on the horizontal axis. The error bars denote 95% Jeffreys confidence intervals. R code to reproduce Figure 1 is available from https://osf.io/by2kc/.


Proportion of studies replicated
1.00
0.75
0.50
0.25
0.00
0.000
0.005
0.010
0.015
0.020
0.025
0.030
0.035
0.040
0.045
0.050
Original study p−value
number of studies
10
20
30
40