Evaluating Evidence from Non-Significant Results: A Bayesian Perspective on Violent Games Research

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Abstract

Researchers in the psychological sciences often find themselves testing for invariances. This is a problem when the most commonly-used form of statistical inference, null-hypothesis significance testing (NHST), can only find evidence of variances, not invariances. Specific examples of this problem are apparent in pilot tests, in which researchers hope to demonstrate that two sets of stimuli do not differ on potential confounds, and disconfirmatory replications, in which experimenters argue that a previous finding was the result of Type I error or a confound later controlled for. We review why NHST cannot describe the strength of evidence for the null hypothesis and explain accessible and practical Bayesian alternatives. Research concerning the effects of violent games on aggressive behavior is used as an example throughout.

Despite more than two decades of research, scientific opinion on whether violent video games cause aggressive outcomes remains divided and the research literature controversial. To date, this relationship has been examined by four different meta-analytic teams, two of which argue that there is a meaningfully large effect (Anderson et al., 2010; Greietemeyer & Mugge, 2014) and two of which that argue there is no meaningful effect, especially after controlling for potential confounds (Ferguson; Sherry).

In this debate, a major point of contention has been the degree to which the observed effects instead may be caused by confounds. When stimuli are appropriately matched on these confounds, it is argued, the effect is eliminated. For example, Adachi and Willoughby (2011) argue that it is competition, not violence, which causes increases in aggressive behavior, and that matching game stimuli on competitive content eliminates the purported effect of violence. Similarly, research by Przybylski et al. (2014) indicates that changes in aggressive affect may be due to difficult, competence-impeding controls, rather than violent content. Finally, research by Elson [et al.] (2014) argues that changes in aggressive behavior are caused by games’ differences in pace of action, not violent content.

Each of these above arguments infers that, under certain conditions, the null hypothesis is true. However, this inference cannot be supported through the use of *p*-values and null hypothesis significance testing [NHST] using the nil hypothesis H0: δ = 0. This statistical approach can reject the null hypothesis in favor of an alternative hypothesis, thereby providing evidence for an effect, but it cannot reject the alternative hypothesis in favor of the null hypothesis. A *p*-value greater than .05 could reflect the truth of the null hypothesis, but it could also represent a true effect studied with insufficient power. The statistical analyses presented by the above studies, then, cannot quantify the accumulated evidence for the null hypothesis, if any.

Arguments for the null hypothesis are also common in this literature, even outside the above studies. A common scenario is pilot testing. The experimenter gathers ratings of stimuli from a (usually small) sample of subjects, hoping to find evidence in favor of the null hypothesis that the two stimuli do not differ on any confounding dimensions. This practice of pilot testing has been deemed a necessary criterion of best-practices studies in some meta-analyses (Anderson et al., 2010), despite the impossibility of concluding in favor of the null hypothesis on the basis of *p* > .05.

In the present manuscript, we reexamine select studies of the relationship between videogame violence and aggressive outcomes in order to better assess the degree of evidence for the null and alternative hypotheses. First we outline an approach for matching stimuli in pilot testing and evaluate the results of some previous pilot tests in the research literature. Then, we examine studies which have argued the truth of the null hypothesis, especially those studies in which improved experimental controls are thought to have eliminated the previously-observed effects of game violence on aggressive behavior.

**Providing evidence for the null hypothesis**

**Imperfect alternatives to nil-hypothesis NHST**

Two alternatives to nil-hypothesis NHST come to mind. First, one could perform a null hypothesis test against a second, non-nil null hypothesis. For example, when failing to detect an anticipated effect, one could test against the expected effect size δ with the secondary null hypothesis H02: µ1 - µ2 = δ. If the study retains H0 while rejecting H02, it could be argued that the study data are sufficiently unlikely given that the true effect size is δ (e.g., Simonsohn, Simmons, & Nelson, 2014). However, this approach does suffer from the typical NHST problem of dichotomous outcomes. Dichotomous NHST procedures cannot discriminate between “no evidence”, “a little evidence” and “a lot of evidence,” instead concluding simply either “there is evidence” or “there is not yet evidence.” This dichotomization is particularly troublesome when slight changes in *p*-value lead to opposite conclusions, such as how the null is rejected at *p* = .049 but the null is retained at *p* = .051. NHST also cannot handle small amounts of evidence well. Given slight evidence, either the null is retained and the slight evidence is mislabeled as “no evidence”, or the null is rejected and the effect size is grossly misestimated.

A second alternative is to instead quantify the effect size and its confidence interval [ESCI]. This does have the advantage relative to NHST of being continuous in quantification. However, ESCI provides neither quantifiable nor inferentially consistent statistics (see Morey, Hoekstra, Rouder, Lee, and Wagenmakers, submitted), and when making inferences using ESCI, researchers seem to mentally convert them to NHST anyway (Hoekstra, Morey, Rouder, & Wagenmakers, 2014). While it is true that values near the ends of the confidence interval are less likely, one cannot know exactly *how much less likely* they are. Similarly, a wide CI indicates that more samples would be necessary to provide a more precise estimate of the effect size, but at what point does the CI become *sufficiently precise* for inference? ESCI is, in our opinion, a useful descriptive tool, but does not permit inferences about the strength of evidence.

**Bayesian Statistics**

We propose Bayesian model comparison as the ideal inferential approach when studying and testing invariances. This statistical approach specifies an alternative hypothesis, then compares the probability of the data given the alternative against the probability of the data given the null. When the effect size is near zero, the data are more probable given the null hypothesis than they are given the alternative hypothesis. As the effect size moves away from zero, the data become less probable given the null and more probable given the alternative hypothesis. Increasing sample sizes yield a more precise estimate of the effect size and may exaggerate the difference in probabilities between the two hypotheses. Bayes factor describes the change between beliefs before and after observing data as articulated by Bayes’ theorem:

Pr(H0 | Data) / Pr(H1 | Data) = Pr(Data | H0) / Pr(Data | H1) \* Pr(H0) / Pr (H1)

The ratio of the probability of the data given the two hypotheses is called the Bayes factor and represents the weight of evidence for one hypothesis over the other. This is represented above by the term Pr(Data | H0) / Pr(Data | H1). The Bayes factor is in continuous odds units ranging from 0 (indicating perfect evidence for one hypothesis) to infinity (indicating perfect evidence for the other hypothesis). A Bayes factor of or near 1 indicates that the evidence are inconclusive and that either hypothesis predicts the data equally well. Since the emphasis of this manuscript is on providing evidence for the null, we will refer throughout this manuscript to the Bayes factor BF01, the strength of evidence for the null hypothesis over the alternative hypothesis. Thus, a BF01 greater than 1 indicates evidence for the null, a BF01 less than 1 indicates evidence for the alternative, and a BF01 near 1 indicates ambiguous evidence. Taking the reciprocal of this statistic gives the evidence for the alternative over the null, BF10 = 1 / BF01.

Bayesian statistics describe the change in beliefs as a function of observed evidence. Beliefs before seeing the data are called the “prior beliefs” or “prior odds”, and beliefs after seeing the data are called the “posterior beliefs” or “posterior odds”. To reach the posterior beliefs, Bayes theorem simply takes the prior beliefs and multiplies them by the Bayes factor. For example, if the null and alternative hypotheses initially seem equally probable (1:1 odds), and the Bayes factor indicates 3:1 evidence in favor of the null hypothesis, then the null hypothesis is now favored with 3:1 odds. If the null hypothesis seems, a priori, highly probable (say, 10:1 odds), and the Bayes factor is 2:1 in favor of the null, then the null hypothesis is now given 20:1 odds. When the data are incapable of discriminating the null from the alternative, the Bayes factor is 1, and the posterior odds are equal to the prior odds – the data have not changed beliefs. This is a substantial improvement over NHST, in which the same *p* > .05 test statistic could mean either than the null is true or that the data are insufficient, which prevents researchers from increasing their belief in the null hypothesis. This Bayesian approach is similarly an improvement over ESCI in that it describes precisely how much less likely values at the edge of a CI are, whether that constitutes evidence for or against a particular hypothesis, and if so, what quantity of evidence is provided.

**Specifying an Alternative Hypothesis.** The first, crucial step of Bayesian analysis is to *specify an alternative hypothesis.* To do this, one specifies a hypothesized distribution of the probable values of the effect size. For example, the effect size δ may be somewhere between 0 and 1, with smaller values more likely than larger values; the alternative hypothesis would then represent δ as the upper half of a normal distribution with mean 0 and standard deviation 1. Because several alternative hypotheses can be specified and compared, one could also investigate the possibility that the effect size δ is somewhere between -0.5 and 0.5, represented by a normal (or Cauchy) distribution centered at 0 with standard deviation (or scale) .5. This is what distinguishes the Bayesian approach from the frequentist: Frequentist statistics assume a single true effect size δ which is estimated in (hypothetical) repeated experiments, while Bayesian statistics allow a researcher to express beliefs about what are the probable values of the true effect size δ. In the typical approach of NHST, the alternative hypothesis is never specified. Because the alternative is never stated, it cannot be falsified in favor of the null. This specification of the alternative may seem challenging, but it is quite possible for anyone who consumes research with some attention to effect sizes. By proposing an alternative hypothesis, the researcher can perform a fair test between the two competing hypotheses. We briefly review methods and software tools for specifying alternative hypotheses and performing Bayesian model comparison.

***The JZS Default Prior.*** First, and most generally, one can specify a broad alternative hypothesis centered at zero by using a JZS Default Prior (Rouder, Morey, Speckman, & Province, 2012; Rouder & Morey, 2012). This prior can detect effects of either sign, representing the possible effect size as HA: δ ~ Cauchy(scale = *r*). (This parameter *r* defines the spread of the Cauchy distribution, much like the standard deviation defines the spread of a normal distribution; it is not to be confused with the Pearson correlation effect size *r*.) This Cauchy distribution is centered at zero and has broad tails, creating a general two-tailed hypothesis test. This model recognizes that small effect sizes are more probable than large effect sizes, and that the effect could be in either direction. Note the scale parameter *r* in the prior. The JZS Default Prior can be scaled to test effect sizes of various magnitudes. When scale *r* is large, effects are expected to be larger, and when scale *r* is small, effects are expected to be smaller in magnitude. Software tools exist for applying the JZS default prior in independent-group *t*-tests, paired-sample *t*-tests, regression, and ANOVA. This prior is easy to use due to its flexibility and its robust software tools, which include the R package “BayesFactor” (Morey, Rouder, & Jamil, 2014) as well as online calculators for t-tests and multiple regression at http://pcl.missouri.edu/bayesfactor.

***Normal, half-normal, and non-local normal priors.*** Alternatively, one can specify and test a more precise alternative hypothesis not centered at zero using the Bayes calculator developed by Dienes (2011, 2014). For example, one alternative hypothesis might describe the hypothesized effect as half a normal distribution, starting at 0, with standard deviation equal to .5. This hypothesis would represent a one-tailed test where the effect is predicted to be about δ = .5, with smaller values again more likely than larger values. In the most specific case, one could test an alternative hypothesis modeling the effect as a normal distribution with a non-zero mean, for example, δ ~ Normal(mean = .4, sd = .2). Such a hypothesis, then, describes the expected effect size as δ = .4 [0, .8], with values near .4 being most probable.

**Arguing the Null in Pilot Testing of Matched Stimuli**

Suppose that we intend to run a study to see whether violent content in games influences aggressive behavior. Participants will play one of two games (violent or nonviolent) and then have an opportunity to aggress against a confederate. In order to make a causal statement that the observed effects, if any, are specifically due to violence, it is useful to first make sure that the two games are alike in all dimensions save violence. We run a small pilot study (*n* = 20), asking each participant to rate each game for violence, difficulty, arousal, and enjoyment. Performing paired-samples t-tests on each outcome, only violence is found to significantly differ, *p* < .05. We might be tempted to conclude, then, that the two games are matched on the other outcomes. However, this conclusion does not follow on the basis of *p* > .05 alone.

In the research literature on violent games, advocates have suggested that this process of matching is one of the criteria that separate “best practices” studies that find larger effects from “not best practices” studies that find smaller effects (Anderson et al., 2010). At the same time, skeptics have suggested that matching games on certain dimensions eliminates the effect of violent games (Adachi & Willoughby, 2011). However, interpretation of these pilot tests has been improper and incoherent. For example, pilot tests in this research domain have sometimes estimated the differences between stimuli as being large, but because the results were not statistically significant, the null hypothesis was considered confirmed. In one particularly remarkable case, post-hoc Bonferroni correction for multiple comparisons was applied to control the Type I error rate across comparisons on 14 dimensions, lowering the critical value of *p* to .0036 (Arriaga, Esteves, Carneiro, & Monteiro, 2008). Differences as large as *r* = .53 were observed but not considered statistically significant due to the small sample size and harsh multiple comparison correction. To their credit, the authors acknowledge that the pilot sample was small, but still do not entertain the possibility that the pilot test provided evidence of differences; instead, they conclude that the pilot test indicates that the games are relatively well-matched.

Pilot tests using NHST in this way are constructed so that the researcher is on the wrong side of the null hypothesis. The researcher hopes not to find a significant effect; however, the more data he or she collects, the better the statistical power to detect a confound, and the more likely it becomes that one or more confounds will emerge as significant. This inferential approach, then, will reward researchers for collecting insufficient data and risks failing to detect substantial confounds. Indeed, with a sufficiently small pilot and harsh enough multiple comparison corrections, even large confounds will go undetected.

**Bayesian Analysis in Pilot Testing**

Bayesian analysis provides a proper approach to testing whether stimuli are matched. The researcher specifies a null hypothesis of no difference (H0: δ = 0) and an alternative hypothesis of a moderate difference (e.g., HA: δ ~ Cauchy(scale = .5)). If it is unreasonable to expect that the stimuli do not differ at all, a null hypothesis of minimal difference can be used instead (e.g., H0: δ = 0 or δ ~ Uniform(-.1, .1), see the nullInterval argument for the ttestBF function in the BayesFactor R package). Participants rate the stimuli, and the probabilities of the data given the null hypothesis and given the alternative hypothesis are compared. If the Bayes factor favors the null (BF01 > 1), the researcher has evidence that the two stimuli do not differ on the particular dimension. If the Bayes factor favors the alternative (BF01 < 1), this is evidence that the two stimuli do differ. Finally, if the Bayes factor favors neither hypothesis (BF01 ≈ 1), the data are not sufficient to discriminate between the two hypotheses.

This approach rewards researchers for collecting more, rather than less, pilot data. Because Bayes factors are insensitive to stopping rules (Rouder, 2014), the researcher may return to collect additional pilot data if the first wave of collection proves inconclusive. But how much evidence is needed? Recall that posterior beliefs are the product of prior beliefs and the Bayes factor. In the case that two stimuli seem to be obviously matched, it may not be necessary to provide a lot of evidence in a thorough pilot test; in the case that two stimuli would seem to be poorly matched, substantially more thorough pilot testing will be necessary to demonstrate their matchedness. There can be no objective threshold that separates “sufficient evidence” from “insufficient evidence”, as prior beliefs are inherently subjective. Thus, to the question “How much evidence do I need?” the answer is simply “Enough to convince your reviewers, readers, critics, and yourself.” Rouder, Morey, and Wagenmakers (submitted, p. XX) explain the value of evidence in the absence of a decision rule:

Finely graded evidence may be thought of as a quantity, say like the weight of some number of bananas. If one has a pound of bananas, there is no reason to make a decision whether a pound is a significant weight of bananas. We may all agree that it is what it is, a pound, even though it may have different meanings to differently sized monkeys, say gorillas and spider monkeys. For a pound will satiate a spider monkey but not a gorilla, and so it is with evidence. We may all have our own thresholds but still agree a Bayes factor of 5 is a Bayes factor of 5, and in all cases it is half as much as a Bayes factor of 10 and twice as much as a Bayes factor of 2.5.

We will caution that it can take a lot of data to provide evidence against the existence of very small effects, so it may not be appropriate to demonstrate that stimuli are matched to arbitrary precision via pilot testing. Researchers will need to consider the magnitude of potential confounds they intend to account for in pilot testing and balance that against the required sample sizes.

**Reanalysis of Select Pilot Tests in Violent Media Research**

As a demonstration, we apply this approach to several pilot tests from the violent games literature. We use the ttestBF function in the BayesFactor package (Morey et al., 2012) to calculate paired-sample or two-sample Bayesian *t*-tests with scale on effect size set to 0.5 and a null interval over [-0.1, 0.1]. That is, to compare the evidence for or against the null, we compare the null hypothesis H0: |δ| < 0.1 against the alternative hypothesis HA: δ ~ Cauchy(scale = 0.5). This choice of scale is subjective, but appropriate. Effects of violent games are expected to be small (e.g., *r* = .21, or about *d* = 0.43), so confounds should be controlled for on a similarly small scale. Increasing this scale variable will increase evidence for the null, while decreasing this scale variable will decrease the evidence for the null; this is because it is easy to demonstrate that there are not large effects, but difficult to demonstrate that there are not small effects. By entering the sample size and the obtained *t*-value of each test, we calculate a Bayes factor describing the strength of evidence for or against the null.

First, we re-examine pilot data from Arriaga et al. (2008). Results are summarized in Table 1. The pilot test, with its sample of *n* = 20 (within subjects), has not provided strong evidence of matching between stimuli on all dimensions. Bayes factors reveal that there is evidence that some dimensions do not differ, but evidence that other dimensions do. After the pilot test, the readers and researchers are forty times more confident that the two games do not differ in involvement and three times more confident they do not differ in presence, boredom, satisfaction, identification, or excitement. However, they should also be twice as concerned that the games differ in feelings of competence, and four times as concerned that they differ in difficulty. These conclusions are very different from those of the original authors, who interpret the nonsignificant results of the pilot test as indicating that the games are equivalent on all measures, or at worst, that the results might be merely inconclusive. Given that the two video games, *Unreal Tournament* (a first-person shooter game) and *Motocross Madness* (a racing game), come from very different game genres with very different rules of play, and that the evidence indicates differences between games in competence and difficulty, one might be concerned that the observed effects are due to differences in these confounds rather than the effects of violent game content alone.

One influential pilot test in this literature is found in Anderson et al., (2004, study 1), in which 120 subjects each played one of 10 games (i.e, *n* = 12 per cell). The games *Glider Pro* and *Marathon 2* were selected as differing in violent content but having nonsignificant differences in other matching variables. Our reanalysis is summarized in Table 1. Evidence for the null hypothesis is slight, and re-analysis indicates that the games may instead differ in the quantity of action. Because we obtain different *p*-values, it is possible that our re-analysis based on summary statistics is yielding slightly different *t*-values. For instance, a mean squared error is reported for all cells, rather than per-cell SDs, which may cause us to over-estimate or under-estimate the SD of a particular cell. In any condition, the Bayes factor is not likely to change by much, and at this small sample size per cell, will not strongly favor one hypothesis over the other. Further data collection would be necessary to demonstrate the equivalence of these two games on these dimensions.

Similarly, we re-evaluate the pilot test from Valadez and Ferguson (2010). Three game conditions were compared: a segment from the beginning of the open-world shooter game *Red Dead Redemption* (a control condition, argued to be a nonviolent portion of a violent game), a latter segment from that same game (the active condition, argued to be a violent portion of a violent game), and the soccer game *FIFA* (a second control condition, argued to be a nonviolent game). Only a small sample was collected (cell *n*s = 15, 10, and 15, respectively), and one-way ANOVAs were conducted to detect variance across conditions in ratings of difficulty, competitiveness, and pace of action. Differences in difficulty and competitiveness were reported as not significant, *F*(2,40) = 2.36, *p* > .05 and *F*(2, 40) = 3.09, *p* > .05, respectively, while differences in pace of action were significant *F*(2, 40) = 4.27, *p* = .02. This last variable was explored through Bonferroni post-hoc analysis, and it was decided that the two control conditions differed from each other but not from the active condition.

We perform all pairwise *t*-tests, then convert these into Bayes factors.[[1]](#footnote-2) Results are summarized in Table 2. Contrary to the author’s conclusions, the results of the pilot test indicate that the games are not well matched. Several Bayes factors strongly favor the alternative hypothesis: the two *Red Dead Redemption* conditions differ in Competitiveness, and the two control conditions differ in all dimensions. Most other comparisons are largely uninformative, as might be expected of the very small sample size. Given our prior beliefs that the early levels of a game are often rather easier than the latter levels, that *Red Dead Redemption* and *FIFA* are very different genres of game, and that the evidence indicates differences between the conditions, we are again not convinced that the stimuli are well-matched. Rather than demonstrate that the stimuli are matched, the pilot test has instead indicated that the games are probably quite different. Even large effect size estimates and modest amounts of evidence can result in nonsignificant *p*-values.

Some pilot studies are more successful in demonstrating invariances. Adachi & Willoughby (2011) report two pilot studies intended to demonstrate that the games used (*Conan,* an action-adventure combat game,and *Fuel,* a racing game) were matched on game characteristics but differed in violence. In the first pilot, *n* = 14 participants played each of two games (within-subjects). This pilot provided modest evidence that the two games did not differ in competition, difficulty, or pace of action, BF01s = 3.36, 3.12, and 2.68 in favor of the null, respectively. The subsequent Study 1 provided further slight evidence that the games did not differ, BF01s = 3.04, 1.07, and 2.24 in favor of the null, respectively. (BF01 = 1.07 is, of course, hardly any evidence at all.) Considering that the two games were, again, from very different genres of game, this might not be enough evidence to conclude that the games are matched stimuli; however, at least the data did not indicate that the games instead differed.

**Summary**

Because NHST cannot provide evidence in favor of the null hypothesis, it is inappropriate to argue that two experimental stimuli are matched on the basis of a non-significant test result. Through collection of an arbitrarily small sample size and application of post-hoc corrections for multiple comparisons, almost any difference could be presented as “not statistically significant”. Despite the inferential flaws of this approach, such pilot testing has been used in meta-analysis as a criterion separating “best-practices” from “not best-practices” research (Anderson et al., 2010). Indeed, the study by Arriaga et al. (2008), described above, was included as a best-practices study despite evidence that the games differed in feelings of competence and difficulty, confounds which can cause aggressive outcomes (Przybylski et al., 2014).

As an alternative to NHST, we advocate the use of Bayesian statistics. Evidence presented this way can favor the null hypothesis of no difference, an alternative hypothesis of a confounding difference, or indicate an absence of evidence for either hypothesis. Researchers are rewarded for more thorough pilot testing by larger Bayes factors for the correct inference. These principles apply also to tests of primary hypotheses, as we explore next.

**Arguing the Null in Demonstrating Boundaries of Effects**

**Interpreting Null Results in the Violent Games Literature**

We next consider null results in the research literature on violent video games. At present, the most-cited estimate of the effect size of violent games on aggressive behavior in best-practices lab experiments is *r* = .21 (Anderson et al., 2010), although other meta-analyses have argued smaller effect sizes (*r* = .08, Ferguson & Kilburn, 2009; *r* = .19, Greitemeyer & Mugge, 2014; *r* = .15, Sherry, 2001). It has often been suggested that this effect is not caused by violent game content itself, but rather caused by confounds such as competitive gameplay (Adachi & Willoughby, 2011), frustrated needs for competency (Przybylski et al., 2014), or pace of action (Elson, Breuer, Van Looy, Kneer, & Quandt, 2014). Research exploring these confounds attempt to demonstrate both an effect of the confound as well as an invariance with respect to violent content.

To date, sample sizes in some of these improved-control studies have been small. For example, two experiments are reported by Adachi & Willoughby (2011) with total samples of *n* = 40 and *n* = 60. Other experiments are reported by Ferguson and colleagues (2008), Ferguson and Rueda (2010), and Valadez and Ferguson (2012) with sample sizes of *n* = 50 (at least, for subjects randomly assigned), *n* = 77, and *n* = 100, respectively. Przybylski et al. (2014, Studies 1, 2, and 5) perform three experiments with *n* = 100, *n* = 100, and *n*= 109. Another study is reported by Elson et al. (2014) with a sample size of *n* = 80. Assuming that the true effect to be demonstrated or falsified is the *r* = .21 reported in meta-analysis, these studies would appear to be individually underpowered; sample sizes of 40, 60, 80, and 100 would yield one-tailed test power of 38%, 50%, 60%, and 69%, respectively (but note that for a larger effect, such as the expected effect on aggressive affect, *r* = .29, one-tailed power is 59%, 75%, 85%, and 91%). An ESCI inspection of these studies (Table 3) indicates that many CIs are quite broad, and that many enclose both *r* = 0 and *r* = .21, suggesting that the data are insufficiently precise to favor one hypothesis over the other. However, we nevertheless would like to understand just how much evidence is in each of these studies so that we can assess the validity of the arguments. Because few of these studies use the same paradigm, and many apply new paradigms argued to have eliminated the effect through innovations in experimental control, we cannot combine and meta-analyze studies for greater power. Thus, these single samples of <80% power each are all the evidence that is available for making an inference.

Because these samples are small and the tests underpowered, failure to reject the null may not provide evidence of the truth of the null. This possibility is sometimes dismissed out of hand by authors. For example, Adachi and Willoughby (2011) argue that sample size is not important, saying that “the effect size for game in the current study was zero (partial η2 = .000), and thus increasing the sample size would not have made the effect statistically significant.” (pp 266). On the contrary, the effect size is measured with error, especially in small samples; increasing the sample size would not only increase the precision of measurement, but also could cause the estimated effect size to change substantially. A similar argument is advanced by Ferguson et al. (2008) “Although the null hypothesis can not traditionally be accepted as ‘true,’ [Loftus (1996) presented that] if the 95% confidence interval in group difference scores (e.g., μ1 – μ2) is reasonably small, the null hypothesis can be effectively accepted as true. Similarly, [Cohen (1994) suggested examining the confidence interval around the effect size.] Effect-size confidence intervals that cross zero effect can be reasonably concluded to be ‘untrue’ and, thus, support the null.” This approaches an ESCI understanding of the null, arguing that as more data is collected, larger effect sizes can be excluded as being comparatively unlikely. However, given that the effect size confidence interval in that manuscript extended to values greater than the meta-analytic estimate (95% CI on *r* = [-.26, 30]), it does not appear that the 95% confidence interval is “reasonably small” enough to reject the alternative hypothesis in favor of the null.

There is also the problem of near misses in significance testing. For example, one of the study outcomes in Elson et al. (2014) only barely missed statistical significance, *p* = .073. Considering that the estimated effect size (*r* = .20) closely approximated that reported in meta-analysis (*r* = .21, Anderson et al., 2010), it does not seem appropriate to consider this a refutation of the effect. As the saying goes, “Surely God loves the .06 nearly as much as the .05” (Rosnow & Rosenthal, 1989). Instead, it is possible that this study provides some evidence for the effect, even if this evidence is not sufficiently strong to be considered “significant” by NHST.

**Bayesian Model Comparison and Hypothesis Formulation**

To assess the strength of evidence for or against the null hypothesis, we re-evaluate these null findings through Bayesian model comparison. We begin by using each study’s reported statistics to calculate a *t*-value, the effect size, and the standard error of the effect size.

Next, we specify two alternative hypotheses. First, the effect could be expected to be small-to-medium in magnitude, and a JZS Default Prior could be used to model this. We will refer to this minimally-informative alternate hypothesis as HA1, the first alternative hypothesis. HA1 summarizes this hypothesis’s predictions about the effect as a Cauchy distribution centered at 0 with a narrow width.

HA1: δ~ Cauchy(scale = .4)

By evaluating the probability of this hypothesis relative to the null hypothesis, we create Bayes factor BF01, the probability ratio of H0 as compared to HA1. As before, when effect sizes are large and have good precision, the data are increasingly improbable given the null relative to the, and the Bayes factor favors this alternative hypothesis, indicating evidence for an effect of small magnitude and nonspecific direction. When effect sizes are near zero, the data are relatively more probable given the null, and the Bayes factor favors the null over this alternative, indicating evidence for no effect.

However, suppose that we wish to test a more specific alternative hypothesis. Previous meta-analysis in this research literature provides a specific estimate of the effect as *r* = .21 [.17, .25] (Anderson et al., 2010), which could serve as our alternative hypothesis.[[2]](#footnote-3) We use the meta-analytic effect size estimate and standard error to derive our second alternative hypothesis, HA2:

HA2: ρ ~ Normal(mean=.21, sd=.02)

When the estimated effect size is close to this interval, the probability of the data given HA2 grows relative to the other two hypotheses. Again, this ratio of probability tends to grow with increasing statistical precision (i.e., data). When the estimated effect size is far from this interval, the probability of HA2 decreases. By comparing the probability of the data given H0 against the probability given HA2, we create Bayes factor BF02. BF02 gives the measure of evidence for the null hypothesis relative to the meta-analytic expectation of the effect size. (Note that the mean and standard deviation used in HA2 will vary depending on the particular outcome tested: aggressive cognition, aggressive behavior, and aggressive affect each have slightly different meta-analytic effect size estimates.) For a normally distributed effect, these Bayes factors can be easily calculated with the online calculator provided by Dienes (<http://www.lifesci.sussex.ac.uk/home/Zoltan_Dienes/inference/Bayes.htm>). For a Cauchy-distributed effect, one can instead adjust the values of µ and rscale in the BayesFactor package (Morey et al., 2014).

With these Bayes factors, researchers can now evaluate an experiment’s results as supporting either H0 or HA2. If BF02 < 1, the results replicate and support the meta-analytic findings. If BF02 > 1, the results provide evidence for the null hypothesis, indicating that the null is more likely than the meta-analytic alternative, given the observed data. Comparisons between HA1 and H0 or HA1 and HA2 could indicate evidence for an effect of a magnitude or direction not predicted by HA2. (See also Boekel et al. (in press) for a similar modeling approach testing both a minimally-informative and a more specific hypothesis.) This model comparison between the null and meta-analytic alternative is applicable in many research contexts in which researchers explore the mediators, boundaries, or potential confounds associated with a psychological phenomenon.

**Reanalysis of Null Findings in VVG Research**

We apply this approach to the current literature of studies claimed to have found the boundaries of the effect of violent video games on aggressive behavior. Each study has a confidence interval that overlaps with 0, which caused researchers to retain the null hypothesis and argue evidence for it. However, how much evidence do they provide for the null, if any?

Findings are summarized in Table 3. We find that, among these null findings, the strength of evidence for the null varies substantially. In studies with small sample sizes (Ferguson et al., Study 1; Adachi & Willoughby, 2011, Study 1 and 2), evidence for the null in each experiment is slight: BF02 ≈ 2.40. This indicates that the evidence provided by Adachi and Willoughby does favor the null, but that a third, larger experiment might be conducted before we conclude that there is no effect of violent content on aggressive behavior so long as competitive content is matched. In studies with larger sample sizes (Ivory & Kalyanaraman, 2007; Prybylski et al., 2014, Study 1, 2, and 5; Tear & Nielsen, 2014), evidence for the null is much stronger: Przybylski et al. find BF02s ranging from 7 to 62, Ivory and Kalyanaraman obtain BF02 = 125, and Tear and Nielsen obtain BF02 = 9.01 in favor of the null.

In cases where effect sizes were close to *r* = .21 but the confidence interval failed to exclude zero, we do not interpret the study as disproving HA2 in favor of H0. Bayes factors recognize that *r* = .20 much more closely resembles *r* = .21 than it does *r* = .00. Thus, re-examination of the effect of violent game content on noise intensity in Elson et al. indicates a moderately informative replication, BF02 = 0.20, or about 5 : 1 in favor of the alternative. The non-significant result has been misinterpreted as support for the null when instead support has been found for the alternative.

A similar phenomenon is observed in Valadez & Ferguson (2012). In this study, participants’ hostile feelings were measured (using the Social Hostility Scale; Anderson, Deuser, & DeNeve, 1995) before and after playing one of three games: a section from the beginning of *Red Dead Redemption,* a latter section of *Red Dead Redemption*, and *FIFA.* Participants played the game for either 15 or 45 minutes. The condition in which the participants played the beginning section of *Red Dead Redemption* was considered a nonviolent control condition, as was *FIFA*. Thus, the latter section of *Red Dead Redemption* was compared to the other two conditions, and with a time X group test statistic of *F*(1, 94) = 3.11, *p* = .09, *r* = .17, the authors argued positive evidence for the null hypothesis. On the contrary, compared to the meta-analytic estimate of the effects of violent games on aggressive affect (*r* = .29, [.25, .34], Anderson et al., 2010), the data slightly support the alternative hypothesis, not the null, BF02 = 0.52. Furthermore, it seems unlikely that the early section of *Red Dead Redemption* was truly nonviolent. Inspection of game footage indicates that the main character is shot in a cutscene within the first 15 minutes of play, much less the first 45 minutes of play (see <http://youtu.be/3lAB1JlbVIM?t=5m28s>). Thus, we performed the analysis again, this time comparing the two *Red Dead Redemption* conditions against the *FIFA* condition. This yields an effect size of *r* = .22, [.02, .39] with BF20 = 8.54, indicating moderately strong support for the alternative. There is one last wrinkle to this study: a main effect of time was observed such that Social Hostility Scale scores *decreased* from pretest to posttest, *F*(1, 94) = 8.15, *p* = .005, *r* = .277 [.078, .443], BF01 = 0.13, or about 7.7 : 1 in favor of the alternative. Thus, while this study provides evidence that violent games increase aggressive affect relative to nonviolent games, it also suggests that this observation is not due to increases in aggressive affect as a result of violent gameplay, but rather, smaller decreases in aggressive affect relative to those caused by nonviolent gameplay. (However, remember also that the conditions do not appear to be well-matched, and so this phenomenon could still be due to the same confounds suspected in other research.) Future research should explore this possibility through application of repeated measures designs.

In summary, while all nonsignificant findings receive the same decision in NHST, a Bayesian analysis provides a more nuanced perspective. Depending on the strength of evidence in a particular study, we might decide that the results reject the alternative hypothesis, in which case a boundary condition of the effect has been identified; the results support the alternative, in which case a boundary condition has not been identified, and the results seem to replicate the broader phenomenon; or the results are inconclusive, and further research would be necessary to determine whether one has found a boundary condition or not.

**Summary**

Clearly, *p* > .05 can describe a wide variety of situations, and thus, its inferential value is limited. Among the articles reviewed in this section, *p* > .05 applied to a range of all possible study results: some studies had strong evidence for the null, others had only slight evidence for the null, and still others actually supported the alternative. As in the pilot testing example above, failure to reject the null does not constitute evidence for the null; researchers hoping to retain the null can always manage to do so by collecting arbitrarily small sample sizes. While reviewers are becoming increasingly savvy to this problem, there still remains the issue of quantifying the evidence for or against the null, even in a sufficiently large sample. Thus, we advocate the application of Bayesian model comparison techniques presented by Rouder et al. (2012) and Dienes (2011, 2014).

Note that very few of the studies presented in Table 3 exclude *r* = .21 from their confidence interval. Applying a hypothesis test to see if the effect is significantly smaller than *r* = .21 would simply that the data were incapable of rejecting either hypothesis, even though, as our analyses demonstrate, there is at least some evidence in many of these studies. One could instead attempt to interpret the ESCI, arguing that, because *r* = .21 is nearer the extremes of the interval, perhaps some of these studies provide some evidence for the null. However, in the absence of an explicitly defined alternative hypothesis and a Bayesian analysis, it is not possible to know how much evidence the study provides, or even which hypothesis is supported.

We note also that the BayesFactor package for R allows comparison of ANOVA or multiple regression models. Thus, a researcher interested in whether competitiveness or violent content changes subsequent aggressive behavior can test models for a main effect of violence, a main effect of competitiveness, main effects of both violence and competitiveness, and even a full model with their interaction. Bayes factors can compare the strength of evidence between models, allowing researchers to argue, for example, that the data indicate evidence for a main effect of competitiveness and no effect of violent content. In this way, effects and invariances can be presented in a single unified analysis.

**Still No Replacement for Data Integrity**

Earlier in this manuscript, we described how Elson et al. (2013) seem to have found evidence for the theorized effect despite an original argument for the null based on *p* > .05. In correspondence with these authors, they asked that we consider their criticism that the CRTT measure is flexibly quantified, potentially allowing researchers to selectively report the quantification with the biggest effect size or the smallest *p*-value (Elson, Mohseni, Breuer, Scharkow, & Quandt, 2014). This criticism still holds for Bayesian analyses, as Bayes factors are still a function of the data, and thus, still sensitive to flexibility in quantification. These researchers demonstrated that the same experiment can yield substantially various effect sizes and p-values depending on which quantification strategy is used. In the same way, the obtained BF02 and varies substantially depending on the quantification: if mean intensity is used, BF02 = 0.20, a moderately informative replication, but if mean duration is used, BF20 = 0.94, indicating that the data favor neither H0 nor HA2. Bayes factors for a default alternative hypothesis (BF01) also vary dramatically by quantification strategy (Table 4). As Elson et al. (2014) had noticed, various quantification strategies yielded effect sizes ranging from ω = -.32 (count of low-volume trials, here reported as negative, as it is in the direction opposite to that hypothesized) to ω = .00 (first-trial volume) to ω = .39 (count of high-volume trials). Similarly, BF02 ranged from 1400 : 1 for the null (count of low-volume trials) to 3.52 : 1 for the null (first-trial volume) to 280 : 1 for the alternative (count of high-volume trials).

Bayes factor reports the strength of reported evidence. When evidence is selectively reported according to the hypothesis it supports, Bayes factor will be biased. We urge researchers to pre-register their hypotheses and analytic strategies, including method of CRTT quantification. We further urge researchers to attempt a thorough and systematic validation of the CRTT in an attempt to choose a limited number of methods which clearly measure a limited number of constructs. Like any other statistical analysis, Bayesian model comparison is still subject to the problem of “garbage in, garbage out.”

**Summary**

Making principled and coherent arguments for the null hypothesis is a crucial part of the scientific process. In this paper, we outlined two common situations in which researchers argue for the null: first, in matching stimulus materials in pilot testing, and second, in attempting to demonstrate the boundary conditions of an effect. The former is necessary for experimental design and precision, while the latter is an important part of determining the specific causal substrates of psychological phenomena and the discriminant validity of psychological measures. Despite the importance and frequency of these endeavors, traditional statistical practices cannot support these goals. *P*-values greater than a critical threshold do not have any interpretation as supporting the null hypothesis, as they only indicate an absence of evidence for an effect, not an evidence of absence of an effect.

As an alternative, we suggest previously-presented easy-to-use Bayesian alternatives to t-tests and ESCI. These Bayesian alternatives require the specification of a reasonable alternative hypothesis. Once researchers have specified an alternative hypothesis, this hypothesis can feasibly be falsified in favor of the null hypothesis. While specification of an alternative hypothesis may sound daunting, it is quite easy, and numerous resources exist to facilitate and evaluate the choice of an alternative hypothesis (e.g., Dienes, 2011, 2014; Rouder et al., 2012).

With regard to the specific research literature on violent video games’ effects on aggressive behavior, we find that researchers often intend to argue for the null, either to demonstrate that two games are equivalent in affective content or that a game has no effect on participants’ behaviors. However, studies arguing for the null vary substantially in their sample sizes and the strength of evidence for the null. In two cases, a *p*-value very close to the critical threshold was presented as a disconfirmatory finding; re-evaluation of this report indicates instead modest support for the alternative hypothesis. We applaud and encourage research efforts in this area which strive to test the boundaries and causal substrates of the effects (if any) of violent games on aggressive thoughts, feelings, and behavior. However, it is clear from this review that some arguments would benefit from greater evidence. Specifically, the evidence is strong that violent game contents do not seem to influence aggressive affect independently of player’s experienced competence (Przybylski et al., 2014). However, evidence that games matched for competitive content do not influence aggressive behavior (Adachi & Willoughby, 2011) is rather weaker, and further direct or conceptual replications may be necessary before the evidence is sufficiently persuasive. Researchers are again encouraged to collect larger samples to maximize evidentiary value, whether arguing for or against the null.

Another benefit of analysis with Bayes factors is that evidence is continuously quantified. This continuity allows researchers to understand when a little evidence or a lot of evidence is presented. This nuance is lost in NHST, which provides only dichotomous accept/reject decisions. It is perhaps this dichotomization of evidence which is, in part, responsible for the heated and sometimes acrimonious debate in the violent media literature, as each side may misunderstand their rejections or retentions of the null as decisive evidence for or against the effect. The re-analyses presented in this manuscript indicate that the evidence provided by individual experiments is often modest, perhaps in part because the anticipated effect is fairly small in magnitude. To provide evidence for or against very small effects will require large amounts of data.

Not only does Bayes factor alleviate psychology’s longstanding bias against the null, but it reduces the pressure on researchers to reach an arbitrary threshold of evidence by rejecting the null. Nobody wants to conduct a study and find that the results have no evidentiary value, and so publication pressures may encourage researchers to do what’s necessary to make “marginally significant” *p*-values into statistically significant values. However, when sample sizes are small, as they often are in clinical groups and other hard-to-recruit populations, statistical power is poor, and so finding statistical significance is unlikely. Researchers may find attaining this threshold to be an unattainable standard for evidence. Analysis with Bayes factors, being a continuous form of evidence, allows researchers to state what evidence they have, whether it is a little or a lot. Taking this perspective allows journals to publish according to sample size and the strength of evidence, rather than selecting publications according to whether they happened to attain an arbitrarily small *p*-value. Researchers can then be rewarded according to their methods, data collection, and analysis, rather than the significance of their results.

We urge researchers to adopt Bayesian techniques in pilot testing and hypothesis testing. Tools for these analyses are rapidly increasing in availability and ease of use. Adoption of these methods will allow researchers to understand how much or how little evidence they have, whether arguing for or against the null, thereby alleviating research controversy and more accurately representing research conclusions.

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*Table 1.* Pilot test results from Arriaga et al. (2008). Pilot data is largely agnostic between the null and alternative, and in fact indicates differences between stimuli in difficulty. BF10 ranges from 0 (perfect evidence for null) to infinity (perfect evidence for alternative). Contrary to the authors’ original conclusions, the pilot test has some evidence the games differ in feelings of competence, and fairly substantial evidence that they differ in difficulty. H0: δ = 0; H1: δ ~ Cauchy(scale = .5) and |δ| > .1.

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| Outcome | t | p | r | BF10 |
| difficulty | 2.63 | 0.017 | 0.53 | 3.982082 |
| competence | 2.27 | 0.035 | 0.47 | 2.191283 |
| discomfort | 1.67 | 0.11 | 0.37 | 0.883575 |
| realism | 1.56 | 0.135 | 0.35 | 0.758824 |
| frustration | 1.32 | 0.201 | 0.3 | 0.554741 |
| pleasure | 1.29 | 0.214 | 0.29 | 0.534506 |
| action | 1.24 | 0.229 | 0.28 | 0.502947 |
| disorientation | 1.14 | 0.267 | 0.26 | 0.447209 |
| excitement | 0.89 | 0.385 | 0.21 | 0.342904 |
| identification | 0.86 | 0.398 | 0.2 | 0.333156 |
| satisfaction | 0.83 | 0.419 | 0.19 | 0.323918 |
| boredom | 0.79 | 0.437 | 0.18 | 0.312355 |
| presence | 0.53 | 0.601 | 0.12 | 0.255489 |
| involvement | 0.48 | 0.634 | 0.11 | 0.024771 |

Table 2. Pilot test from Valadez & Ferguson, 2010. Pilot testing suggests that the conditions are different, not equivalent, on ratings. BF01 ranges from 0 (perfect evidence for alternative) to infinity (perfect evidence for null).

Table 3. Bayesian re-analysis of select studies claiming to find boundaries of violent game effects on affect, behavior, and cognition. Some studies present only modest evidence against the effect, and several indicate evidence for the effect despite nonsignificant *p*-values. BF02 ranges from 0 (perfect evidence for alternative) to infinity (perfect evidence for null).

Table 4. Bayes factors for each effect size calculated by Elson et al. (2014), study 2, table 2. Bayes factors vary dramatically by quantification method of the CRTT. BF01 ranges from 141 : 1 in favor of an increase in aggression to 2.78 : 1 in favor of the null. One quantification even finds a Bayes factor of 18 : 1 in favor of a decrease in aggression. Similarly, BF02 ranges from 279 : 1 in favor of an effect to 1418 : 1 in favor of the null. BF01 = evidence for H0: δ = 0 compared to HA1: δ ~ Cauchy(scale = 0.4). BF02 = evidence for H0: δ = 0 compared to HA2: *r* ~ *N*(.21, .02). BFs range from 0 (perfect evidence for alternative hypothesis) to infinity (perfect evidence for null).

|  |  |  |  |
| --- | --- | --- | --- |
|  | *r* | BF01 | BF02 |
| Mean volume | .196 | 0.831 | 0.209 |
| Mean volume after wins | .132 | 1.633 | 0.642 |
| Mean volume after losses | .219 | 0.603 | 0.138 |
| Mean duration | .107 | 1.970 | 0.989 |
| Mean duration after wins | .048 | 2.603 | 2.660 |
| Mean duration after losses | .144 | 1.473 | 0.523 |
| Mean volume x duration | .181 | 1.001 | 0.274 |
| Mean volume x sqrt(duration ) | .181 | 1.005 | 0.275 |
| Mean volume x ln(duration) | .161 | 1.256 | 0.393 |
| Count high volume settings | .404 | 0.007 | 0.004 |
| Count high duration settings | .045 | 2.622 | 2.777 |
| First trial volume | .031 | 2.710 | 3.516 |
| First trial duration | .011 | 2.780 | 4.901 |
| Count low volume settings | -.338 | 0.054 | 1418.827 |

1. While this would seem to invite a multiple comparisons problem, we remind that Bayes factor expresses evidence, and that multiple comparisons problems are a matter of interpretation, not evidence. “One should not confuse strength of evidence with the probability of obtaining it (Royall, 1997). Evidence is evidence even if, as one increases the circle of what tests are in the “family”, the probability that some of the evidence will be misleading increases.” (Dienes, 2011, pp 280, an excellent resource on this problem). [↑](#footnote-ref-2)
2. There exist other meta-analyses in this literature (Ferguson & Kilburn; Greitemeyer & Mugge; Sherry), but this is the most widely-cited of them. If the researcher is of the opinion that meta-analysis has failed to reveal an effect of violent content on aggressive behavior, he or she can use a JZS Bayes default prior, or, if testing for a hypothesized increase in aggressive behavior, a one-sided variant. One could also test against a less specific alternative hypothesis that incorporates the uncertainty about meta-analytic conclusions by expanding the variance around HA2’s effect size (e.g. HA2: r ~ N(mean = .20, sd = .1). [↑](#footnote-ref-3)