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# Does sexualization in video games cause harm in players? A meta-analytic examination

Christopher J. Ferguson [a,*], James D. Sauer [b], Aaron Drummond [c], Julia Kneer [d], Emily Lowe-Calverley [b]

[a] Stetson University, United States
[b] University of Tasmania, Australia
[c] Massey University, New Zealand
[d] Erasmus University Rotterdam, The Netherlands

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ABSTRACT

Whether video games with sexualized content do or do not relate to mental health and body image problems in players, and/or sexualization and hostility toward women, is an issue of broad public interest. However, evidence from empirical studies has generally been mixed. To examine this issue, we explored the degree to which sexualization in games was related to both well-being/body dissatisfaction and sexism/misogyny among players in two separate meta-analyses. Results revealed that sexualization in games was neither related to well-being/body dissatisfaction ($r = 0.082$, $k = 10$, $n = 2{,}010$, $p = .066$) nor sexism/misogyny ($r = 0.040$, $k = 15$, $n = 15{,}938$, $p = .070$). Better designed studies, and those that showed less evidence for researcher expectancy effects (for sexism/misogyny outcomes), tended to find less evidence for effects. As appears commonly in other realms of media effects, the evidence is weak that sexualized games influence player attitudes and behavior.

## 1. Introduction

Historically, many games have included sexualized content, particularly related to how women and female characters are presented in games, ranging from damsels in distress (e.g., Zelda) to prostitution (the *Grand Theft Auto* series), (Williams et al., 2009). Evidence suggests that this state of affairs may have improved in more recent years (Lynch et al., 2016). Nonetheless, significant concerns persist about sexualization in games and, in particular, the impact of this content on gamers. Concerns relate to both potential mental health impacts including (but not limited to) body dissatisfaction in players, and whether players (both male and female) will develop more hostile views toward women as a consequence of playing such games (Begue et al., 2017; Dill et al., 2008). Due to controversies and inconsistencies in the literature (Breuer et al., 2015; Ferguson & Donnellan, 2017), to date, no consensus has been achieved. The present meta-analyses seek to examine the degree to which evidence supports causal effects of sexualized content on players' well-being and misogyny/sexism, and how methodological considerations between studies may explain inconsistent results in this field.

### 1.1. A brief review of prior research

Sexualization in games may take several forms. In most studies it appears to be defined as presenting characters (particularly, though not exclusively, female characters) in ways which define them by their appearance, or as objects of sexual attraction. This may include presenting characters in skimpy clothing or nude, through to having characters engaged in actual sexual situations (such as the infamous prostitutes in the *Grand Theft Auto* series). As a related issue, misogynistic content could also include explicit violence toward women, though it is important to distinguish between games that include lead female characters actively participating as strong characters in action games (e.g., *Last of Us*, *Horizon Zero Dawn*, *Alice: Madness Returns*) and games in which characters are explicitly targeted for violence for being female.

Across the research areas of well-being and sexism there are perhaps two dozen or so relevant published studies. However, both the methodologies used to examine these constructs, and the outcomes yielded by the studies have tended to be inconsistent. For example, Dill et al. (2008) compared exposure to sexualized game content on judgements of a

* Corresponding author. Department of Psychology, 421 N. Woodland Blvd., DeLand, FL, 32729, United States.
*E-mail address:* cjferguson1111@aol.com (C.J. Ferguson).

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sexual harassment scenario and rape myth acceptance, finding inconsistent effects (i.e., significant results for the former but not the latter). However, the exposure involved PowerPoint slides of video game content, and not actual gameplay, making it difficult to know how well these results generalize. By contrast, Fox et al. (2013) examined the experimental effects of avatar sexualization on rape myth acceptance in female players. They found some unusual results, wherein sexualization of avatars with the players' own faces was related to increased rape myth acceptance, while sexualization of avatars with others' faces was related to decreased rape myth acceptance as compared to control groups. Results such as these can be difficult to interpret in light of existing media effects theories.

Some studies have been clearer in not finding evidence for sexualization effects either longitudinally (Breuer et al., 2015) or experimentally (Beck & Rose, 2018; Read et al., 2018), whereas other studies have suggested effects might exist (Beck et al., 2012). For other studies, results may be "statistically significant", but effect sizes are so small that it is unclear whether they should be considered hypothesis-supportive. For example, Begue et al., (2017) found a correlation between games and sexism that was statistically significant but below $r = 0.10$ ($\beta = 0.07$). Some authors argue that, below this threshold, the imprecision of psychological measures leaves researchers unable to distinguish real effects from noise (Ferguson & Heene, in press).

### 1.2. Methodological issues that can influence outcomes

The field concerned with sexualization in games has many ostensible similarities with the larger field of violence in video games, including the mission to examine whether content considered to be morally objectionable (sexualization or violence) is linked to negative outcomes. The methods used in such studies, whether experimental randomization of different types of games, or survey studies regarding self-reported exposure to games, are similar. Thus, methodological issues that have been prevalent in the violent game literature may similarly be an issue for studies of sexualization in games.

In morally-valanced research fields, there may be pressure for scholars to return research results that support moral narratives. This can lead to publication bias issues, particularly for smaller experimental samples (see for instance, Hilgard et al., 2017) and researcher expectancy effects which can often be identified via citation bias, or the tendency for researchers to cite only studies supporting their hypotheses (de Vries et al., 2016). Consistent with this idea, a recent meta-analysis of violent game studies (Drummond et al., 2020) found that citation bias, as an index of researcher expectancy effects, was associated with higher effect sizes. Further, Drummond et al.'s meta-analysis identified several best practices for studies, ranging from the use of preregistration (publishing data-analysis plans in advance of data collection to cut down on researcher expectancy effects), the use of standardized outcome measures, careful matching of game conditions in experiments, use of distractor tasks to reduce demand characteristics, controlling for gender, age and preexisting aggression in correlational and longitudinal studies, and independent ratings of game content that did not rely on participant ratings. Best practice studies were found to present less evidence for links between gaming and aggression than those studies adhering to fewer best practices. Overall, the results suggested that problematic methodologies and researcher expectancy effects can have non-trivial impacts on outcomes.

There has, as of yet, been no meta-analysis of studies of sexualization in games. We, thus, conducted this meta-analysis with several research questions in mind, namely:

RQ1: Overall, what are the effect sizes for game sexualization and player well-being and sexism/misogyny?
RQ2: Is there publication bias in this literature?
RQ3: Are issues related to citation bias and methodological best practices related to effect sizes in this domain?

In summary, this meta-analysis is testing several related hypotheses. First, that exposure to sexualized games will be associated with decreased player well-being and increased sexism/misogyny. Second, that publication bias will be positively associated with an inflation of effect sizes. Third, that effect sizes will be higher in studies with (a) citation bias and (b) fewer best practices.[1]

## 2. Method

### 2.1. Pre-registration

We have preregistered the methodology of this meta-analysis prior to data collection and this is available here: https://osf.io/pj7f9/

### 2.2. Inclusion criteria

As per our preregistration, studies had to include a measure of exposure to general or sexualized video games in survey form or an experimental manipulation of sexualized game content. Because there were so few survey studies, studies that examined general game exposure were included along with those that examined sexualization specifically. Studies that sexualize both males and females were considered. For the meta-analysis of sexism/misogyny outcomes, only behavioral outcomes related to aggression toward women, or sexist or misogynistic attitudes were looked at. For the well-being meta-analysis we included a broad range of outcomes related to depression, body image, or anxiety (e.g., self-esteem, self-objectification, etc.). One modification of our preregistration was that our preregistration stated our meta-analysis would be conducted as long as study areas included at least 10 studies. Studies of well-being fell short of this when only female samples were considered (as stated in the preregistration[2]), so we amended this to include mixed samples as well as female only samples. To be included, the studies also must have included enough information to calculate an effect size *r*.

### 2.3. Selection of studies

We undertook a search on PsycINFO and Medline using the terms "sexuali* OR misogynist* OR sexist" AND "video games" OR videogames OR "computer games" OR "digital games". These searches were made in the SUBJECT search field. This search yielded 50 results. Removing duplicates, and articles which did not meet the inclusion criteria, resulted in the inclusion of 18 articles which included a total of 25 relevant independent studies (some articles reported more than a single study). In a few cases, authors were unable to provide data that would have allowed for the calculation of an effect size. A PRISMA diagram is included as Appendix A and available at: https://osf.io/hxvae/. The list of studies with effect sizes and coding is available at: https://osf.io/7gbhx/. A list of studies is presented as Appendix B.

Some datasets have produced multiple articles. Only one study from each dataset was included in the final analysis. As per the preregistration, in the event of competing publications, we included those that used standardized assessments, included the most theoretically relevant controls (i.e., gender, age, time 1 outcome variables, family environment, mental health, peer environment), or were preregistered. In the event the dataset was available, we independently calculated effect sizes (rather than relying on published effects sizes).

[1] We did not pre-register hypotheses for the current meta-analysis. However, these have been included at the request of a reviewer.

[2] Studies with female only samples were: Fox et al., 2013; Fox & Tang, 2014; Lindner et al., 2020; Showronski et al., 2021.

### 2.4. Analysis plan

For experimental results, effect sizes were calculated in terms of "*r*" either from published means and standard deviations where available or from *F* or *t* statistics. As per our preregistration, for correlational and longitudinal studies, the main effect size was standardized regression coefficients (betas) which were calculated from the effect size employing the greatest degree of theoretically relevant controls in each study. Theoretically relevant variables have been identified for media research generally, including gender, family environment, mental health, personality and, for longitudinal studies, time 1 outcome variables (Savage & Yancey, 2008). Two authors extracted effect sizes from each article. We calculated interrater reliability for this to be $\alpha = 0.99$. Where there were disputes regarding effect sizes, these were addressed via discussion and agreement.

Initial results were calculated using Comprehensive Meta-Analysis (CMA). CMA was used to calculate random effects weighted mean effect sizes and conduct moderator analyses. Publication bias was assessed using the shinyapps meta-analysis calculator. Publication bias was assessed with tools including basic funnel plot analysis, Trim and Fill, PET/PEESE, and *p*-curve. *P*-curve analysis was pre-registered to only be used if more than 20% of *p*-values were marginal, (that is, between 0.01 and 0.05). Our purpose for potentially incorporating a *p*-curve analysis was to correct for an overabundance of marginal *p*-values which, if one existed, may indicate *p*-hacking or other QRPs. However, we assumed that we should normally expect a number of marginal *p*-values by chance and thus did not intend to run a *p*-curve analysis if there did not appear to be an overabundance of marginal *p*-values. We recognize this is not the only reason for undertaking a *p*-curve analysis, but it was our intent in using it, hence our preregistration.

Given the high power of meta-analysis, almost all meta-analyses return "statistically significant" effects. Consistent with recommendations of Drummond et al. (2020) and Ferguson & Heene, (in press), and as per our preregistration, we considered an effect size of $r = .10$ the minimum for practical significance in order to avoid false positives due to noise effects (see those papers for full discussion of the use of this cut-off value). A copy of all data is available at https://osf.io/7gbhx/.

### 2.5. Best practices analysis

We coded studies for employing current best practices (e.g., preregistration) to determine whether using such practices affected the effect sizes reported. Studies were given a point each for the inclusion of a number of different best practices (see below), resulting in a numeric score that ranged from 0 to 5. This score was used as a moderator variable to determine the effect of employing best practices on effect size. Studies were given credit (1 point each) for the following best practices:

1. Using a standardized outcome measure for the outcome variable (either well-being or sexism/misogyny).
2. Not relying on respondents to rate the violence in video games but rather using independent ratings such as scholars or ratings boards (e.g., ESRB, PEGI). Such measures have been found to be valid indices of potentially objectionable content when compared to the ratings of trained, blinded raters and correlate highly with other approaches and demonstrate good construct validity (Fikkers et al., 2017). Applicable to correlational/Longitudinal studies only.
3. Games were matched carefully on variables other than sexualization (e.g., competitiveness, difficulty, etc.) in experimental conditions.
4. Distractor surveys or other tasks were used to reduce demand characteristics.
5. Controlled, at minimum, gender and T1 aggression. Applicable to correlational/Longitudinal studies only.
6. Experimental studies used actual game playing, not mere watching of games or game clips in both conditions.
7. Preregistration of analysis plan.

### 2.6. Citation bias

Papers were assessed for citation bias. To determine if a paper suffered from citation bias, we examined the literature review. If the literature review included no citations to papers with conclusions that conflicted with the authors' hypotheses, they were coded as having citation bias. Papers that acknowledged at least one research study or paper conflicting with the authors' hypotheses, were coded as not having citation bias.

### 2.7. Moderator analyses

The following pre-registered variables were included in moderator analyses to determine whether they influenced reported effect sizes: age of the sample, year of the study, best practices, type of study and citation bias. For continuous moderator analyses, meta-regression was used.

## 3. Results

### 3.1. Mental well-being

For mental well-being a random effect meta-analysis found an overall effect size or $r = 0.082$, which was both below our effect size cut-off of $r = 0.10$ and non-significant $p = .066$ (see Fig. 1). As noted above, different from our preregistration, this included a number of mixed samples (i.e., including male and female participants), so reanalyzing without those samples provided an effect size that was slightly larger $r = 0.122$, but still non-significant $p = .118$. As between study heterogeneity was significant ($Q = 29.76$, $p < .001$), moderator analysis is warranted (see Fig. 2).

#### 3.1.1. Publication bias

For this group of studies, Trim and Fill suggested the presence of slight publication bias (adjusted $r = .040$). This was particularly true for experimental studies which had a point estimate of $r = 0.111$ but which was adjusted down to $r = 0.048$ using Trim and Fill. However, neither PET/PEESE nor *p*-curve indicated bias. When two non-experimental studies were removed from the analysis PET/PESE indicated significant publication bias.

#### 3.1.2. Moderator analysis

Using meta-regression, neither age nor year of the sample was a significant moderator. However, best practices was a significant moderator, with better practice studies finding weaker effects than studies with fewer best practices ($Q = 7.72$, $Z = -2.78$, $p = .005$). As per our preregistration, we then conducted a median split of best practice studies to examine effect sizes of studies above and below the split. Fixed effects analysis for moderator effects revealed that non-best practice studies had higher effect sizes ($r = 0.116$) than did best practice studies ($r = 0.006$), and this difference was significant ($Q = 6.052$, $p = .014$). Using mixed effects analysis, however, the difference was non-significant ($p = .111$). Mixed effects are a bit more conservative and may tend to be less likely to overestimate the impact of moderator effects. Samples with mixed or female-only participants did not substantially differ aside from that studies by one research group (Fox et al., 2013; Fox & Tang, 2014) were higher in regard to effect size than all other samples.

Likewise, our analysis of citation bias as an index of researcher expectancy effects yielded no significant differences between studies with citation bias ($r = 0.117$) and studies which avoided citation bias ($r = 0.035$), for either fixed ($p = .082$) or mixed analysis ($p = .149$).

All results for the mental well-being meta-analysis are presented in Table 1.

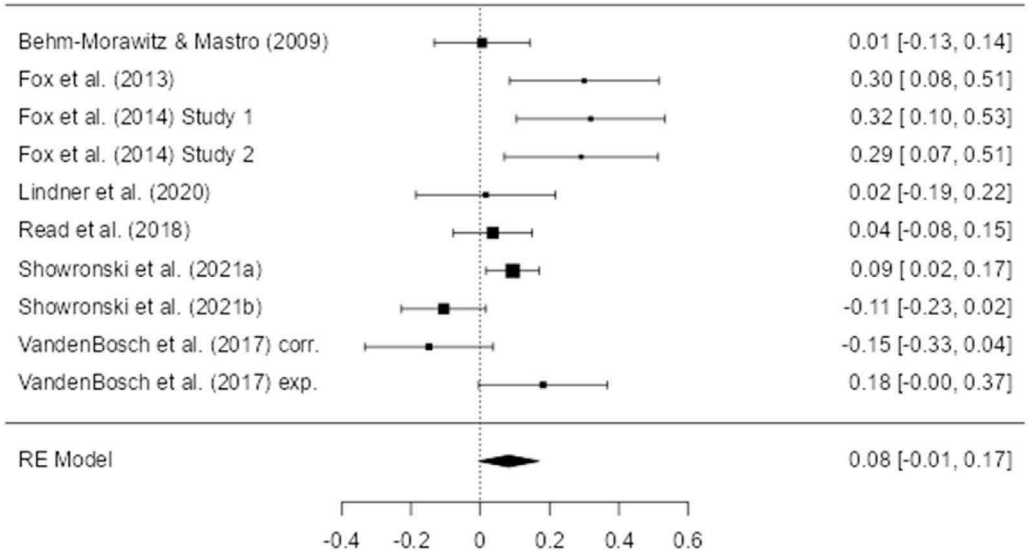


**Fig. 1.** *A Forest Plot of the effect sizes for effects on mental well-being.*

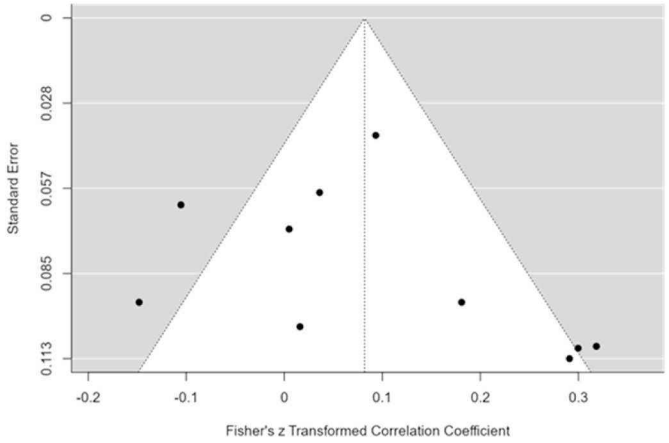


**Fig. 2.** Funnel plot of studies included in the analysis of effects on well-being.

### 3.2. Sexism misogyny

For sexism/misogyny, a random effect meta-analysis found an overall effect size of $r = 0.040$, which was both below our effect size cut-off of $r = 0.10$ and non-significant $p = .070$. In this case, heterogeneity was non-significant ($Q = 22.44$, $p = .070$). Thus, moderator analyses will be interpreted with caution.

#### 3.2.1. Publication bias

For this group of studies, Trim and Fill suggested an absence of publication bias. Likewise, neither PET/PEESE nor *p*-curve indicated publication bias.

#### 3.2.2. Moderator analysis

Using meta-regression, as for analyses on well-being, neither age nor year of the sample was a significant moderator. However, again, best practices was a significant moderator, with better practice studies finding weaker effects than studies with fewer best practices ($Q = 6.54$, $Z = -2.56$, $p = .011$). Using fixed effects analysis for moderator effects

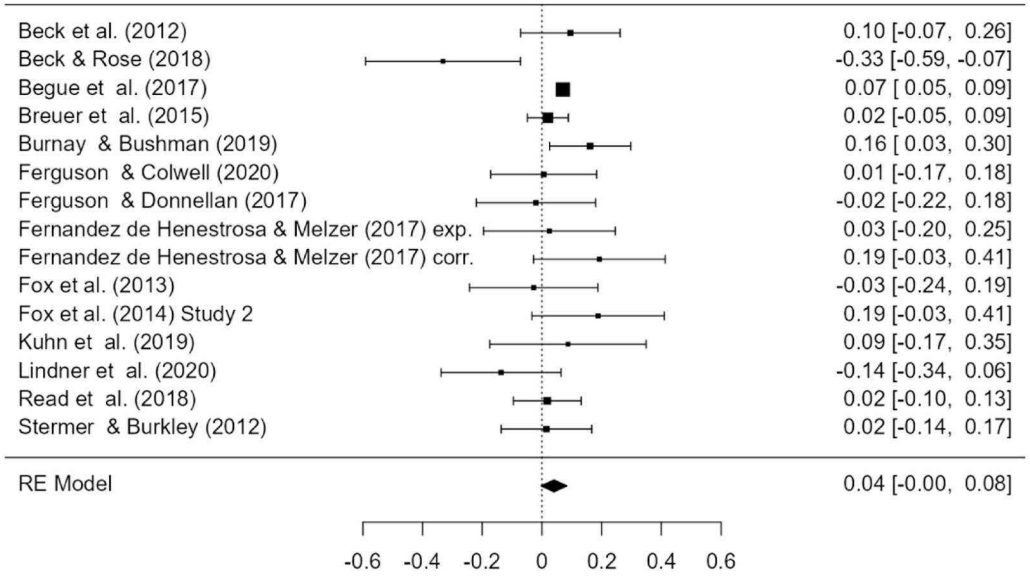


**Fig. 3.** *A Forest Plot of the effect sizes for effects on sexism/misogyny.*

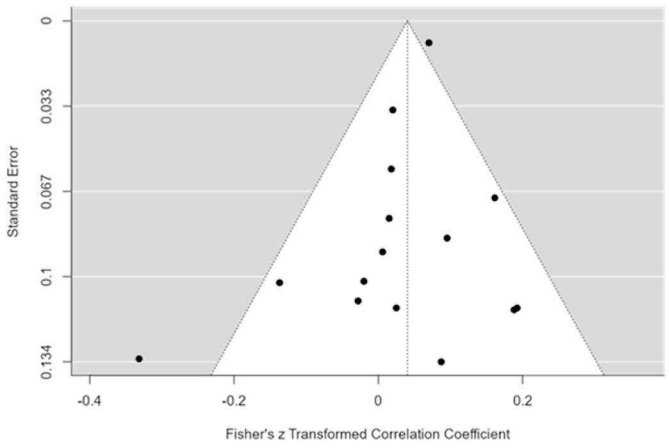


**Fig. 4.** Funnel plot of studies included in the analysis of effects on sexism/misogyny.

**Table 1**
Effect sizes for mental well-being outcomes.

| Grouping Variable | *k* | Effect Size | 95% CI | $I^2$ | tau |
|---|---|---|---|---|---|
| Total Effect | 10 | .082 | -.005, .167 | 69.76 | .113 |
| Study Type | | | | | |
| Experimental | 10 | .031 | -.026, .088 | 46.97 | .088 |
| Non-experimental | 5 | .067 | .051, .083 | 2.21 | .006 |
| Best Practices | | | | | |
| No | 8 | .116 | .055, .177 | 75.18 | .152 |
| Yes | 7 | .006 | -.057, .069 | 42.19 | .063 |
| Citation Bias | | | | | |
| Yes | 7 | .071 | .055, .088 | 0.00 | .000 |
| No | 8 | .004 | -.044, .052 | 31.05 | .051 |

Note: $k$ = number of studies, $I^2$ = estimate of heterogeneity, tau = estimated standard deviation of effects across studies.

**Table 2**
Effect sizes from sexism/misogyny outcomes.

| Grouping Variable | K | Effect Size | 95% CI | $I^2$ | tau |
|---|---|---|---|---|---|
| Total Effect | 15 | .040 | -.003, .084 | 37.61 | .004 |
| Study Type | | | | | |
| Experimental | 8 | .111 | -.005, .167 | 71.04 | .130 |
| Non-experimental | Not enough studies for meta-analysis | | | | |
| Best Practices | | | | | |
| No | 8 | .068 | .052, .084 | 13.49 | .020 |
| Yes | 7 | -.007 | -.074, .060 | 38.88 | .075 |
| Citation Bias | | | | | |
| Yes | 6 | .117 | .003, .231 | 74.24 | .163 |
| No | 4 | .035 | -.019, .089 | 59.08 | .071 |

Note: $k$ = number of studies, $I^2$ = estimate of heterogeneity, tau = estimated standard deviation of effects across studies.

revealed that non-best practice studies had higher effect sizes ($r$ = 0.068) than did best practice studies ($r$ = −0.007) and this effect was significant ($Q$ = 4.534, $p$ = .033). However, again, using mixed effects analysis, the difference was non-significant ($p$ = .100).

Likewise, our analysis of citation bias as an index of researcher expectancy effects found that studies with citation bias had higher effect sizes ($r$ = 0.071) than did studies which had avoided citation bias ($r$ = 0.0004) and this effect was significant ($Q$ = 6.842, $p$ = .009). Using mixed effects analysis, the difference remained significant ($p$ = .025). This indicates that researcher expectancy effects appear to be related to increased effect sizes. Thus, where researcher expectancy effects are not controlled, effect sizes may be spuriously high.

We wished to determine if effect sizes varied according to study type. However, there were not enough longitudinal ($k$ = 2) or correlational ($k$ = 3) studies to examine these separately. As their effect sizes were similar ($r$ = 0.067 and 0.050 for longitudinal and correlational studies, respectively), they were combined into a non-experimental category. Results indicated that study type was not a significant moderator ($p$ = .742 in mixed effects analysis, 0.476 in fixed effects analysis).

In conclusion, the evidence does not indicate a link between sexualized games and either well-being or misogyny. Although publication bias does not appear common in this research field, both citation bias and lower quality studies predicted higher effect sizes.

## 6. Discussion

The issue of whether sexualization in games influences either well-being or sexism/misogyny among players remains hotly contested among both the general public and scholars. With this meta-analysis, we sought to examine whether the current state of evidence could support concerns that sexualized game content negatively affects players' well-being or is positively associated sexist/misogynistic attitudes and behaviors. On balance, the current state of the data could not support beliefs that sexualized game content influences either players' mental well-being or sexist attitudes and behaviors.

Effect sizes for sexualization effects in video games were both non-significant and below the $r$ = 0.10 threshold we used for interpreting results as hypothesis supportive. Additionally, results indicated two important influences on effect sizes. First, effect sizes were lower in best practices studies. Second for studies of sexism/misogyny, effect sizes were lower in studies with balanced literature reviews, compared to studies which evidenced citation bias. Taken together, these results suggest that effect sizes may be spuriously inflated by both less well-controlled studies, and potentially by researchers' expectancy effects. Efforts such as preregistration that would reduce these influences are likely to produce more reliable and valid results. Preregistration can help reduce researcher expectancy effects and reduce false positive results, which were demonstrated to be an issue for this field. Without preregistration, effect size estimates may be spuriously high, not a reflection of true effects. For instance, Simmons et al. (2011) estimate that without pre-registration, combinations of researcher degrees of freedom can increase the false-positive rate to over 60%. Increasing pre-registration may lead to an increase in the reporting of null results, but if this represents the actual state of the field this is a good outcome. It is important that the reported data accurately represents the true effect. Thus, we call upon researchers in this area to more comprehensively employ preregistration, standardized measures, and open science principles.

One possibility is that effect sizes for short-term exposure to sexualized games may be small, but effects may accumulate over time, becoming more substantial (for discussion about accumulating effects see Funder & Ozer, 2019; and Sauer & Drummond, 2020). This can be tested with longitudinal studies. Unfortunately, there are relatively few longitudinal studies in this area. However, the few available (e.g., Bègue et al., 2017; Breuer et al., 2015) had among the weakest effect sizes, suggesting that the accumulation hypothesis is not currently supported from the available data. This fits also with meta-analyses of longitudinal studies in the related area of video game violence which, likewise, found no evidence of an accumulation effect (Drummond et al., 2020). Similarly, evidence from experimental studies suggest longer exposure times are associated with smaller effect sizes in video game effects (Sherry, 2001). This may be because short term exposures do not facilitate player competence, leading to frustration. In contrast, longer exposures may facilitate competence; increase enjoyment and reducing aggression.

Though concerns about sexualization in video games are reasonable from a moral and ethical perspective, our results suggest that advocates and policy makers may do well to steer clear of making causal implications that may not be supportable by current data. Indeed, the authors of this paper personally applaud efforts to improve representations of women and female characters in games. However, arguments that such

cultural changes may result in benefits such as improved well-being or reduced sexism appear to be, at present, unsupported by the data. Again, this does not mean such efforts are without merit in their own right. Rather, that advocates must take care not to imply that such changes will produce benefits for which there is little empirical support.

Our results appear to fit with increased scrutiny in other areas of media research such as in video game violence (Hilgard et al., 2017), thin ideal media (Want & Saiphoo, 2017; Whyte et al., 2016) or movie smoking and teen smoking (Ferguson et al., 2020). For instance, meta-analyses in other areas have found similar concerns regarding publication bias in video game research (Hilgard et al., 2017), issues related to citation bias in media sexualization research (Ferguson et al., 2017), and quality issues that influence effect sizes in media violence research (Savage & Yancey, 2008) and in thin ideal/body image research (Holmstrom, 2004). This may fit well with the general replication crisis in psychology wherein social cognitive theories, particularly those based in assumptions of automaticity, may prove to be a poor fit to the data. A growing awareness of these trends in the literature, and of the tendency for better-conducted research to produce weaker evidence of problematic relationships, may warrant a review of public statements by professional guilds that state or imply strong and stable evidence for problematic media effects.

As we indicated earlier, this is the first meta-analysis to consider this research field. Some studies have found some evidence for effects (Beck et al., 2012), whereas others have not (e.g., Breuer et al., 2015). Meta-analysis can help us to understand why inconsistencies occur. In this case, discrepancies appear to be consistently related to quality issues and researcher expectancy effects. We also note that some studies purport to find evidence for effects (e.g., Bègue et al., 2017) yet the effect sizes are so small, they may be better explained as noise effects Ferguson & Heene, (2021) than real effects. Over-interpretation of these effects may be contributing to misunderstandings in this field. In other words, there simply does not appear to be much impact of sexualized game content on either mental health or misogyny outcomes, and some studies may have misinterpreted noise effects as supportive of their hypotheses (see Figs. 3 and 4).

One likelihood is that family environment or peers may influence issues related to sexualization, body dissatisfaction, misogyny or mental health (Sandoval-Obando et al., 2022). For instance, lack of parental warmth is cross-culturally related to youth psychological well-being (Garcia et al., 2020). Some evidence from body dissatisfaction, for instance, finds that peers but not media, are associated with teen body dissatisfaction (Ferguson & Colwell, 2020).

## 7. Limitations

As with all studies, our study has limitations that are worth acknowledging. First, these research realms are currently rather small, necessitating that relatively few studies were included in our analyses. Consequently, several categories in the present meta-analyses were a bit broader than would be ideal. For instance, in survey studies both measures of general and sexualized game content were included, so long as the outcome variables involved sexism or mental well-being. Our observation of the effect sizes did not suggest that studies of sexualized game content specifically produced higher effect. Nonetheless, it would be ideal to have a larger pool of studies specifically examining sexualized content. Similarly, there were so few survey studies overall, it was difficult to examine these comprehensively. Preregistered studies were very rare in this realm, making it impossible to examine the impact of preregistration on effect sizes. An increase in pre-registration would be especially beneficial to this research domain. Meta-analyses are also only as good as the quality of the studies included within. Although we conducted best practices analyses to attempt to assess for quality issues, it is possible that the quality of different game exposures could explain inconsistent results.

## 8. Conclusions

Whether sexualized game content adversely affects mental health (including body dissatisfaction) or promotes sexism and misogyny in players is likely to be a matter of debate for the foreseeable future. Our analyses find that the current evidence base does not support the conclusion that sexualized games, however offensive they may be to many individuals, are adversely affecting players' well-being or contributing to sexism/misogyny. We hope that our article is a constructive contribution to debates in this area.

## Statement of relevance

Policy makers, scholars and the public continue to express concerns about sexualization in media, including video games. Understanding what impact sexualized games have on players can help us understand what policies, whether by industries or through government regulation, may be helpful. It can also help advocates for better representation of women in games and in the gaming community know how to best frame their efforts. The current analysis suggests that sexualized content in games has little direct impact on players, whether male or female. Advocates for women in games are advised to focus messages on the moral value of better representation rather than implying causal impacts.

## Funding

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## Data sharing

All data are available at: https://osf.io/7gbhx/

## Preregistration

Preregistration available at: https://osf.io/pj7f9/

## Credit author statement

CJF conceived of the study and worked on main analyses, and wrote the first draft. AD and JS worked on main analyses and helped write all drafts. JK and ELC helped write all drafts.

## Declaration of competing interest

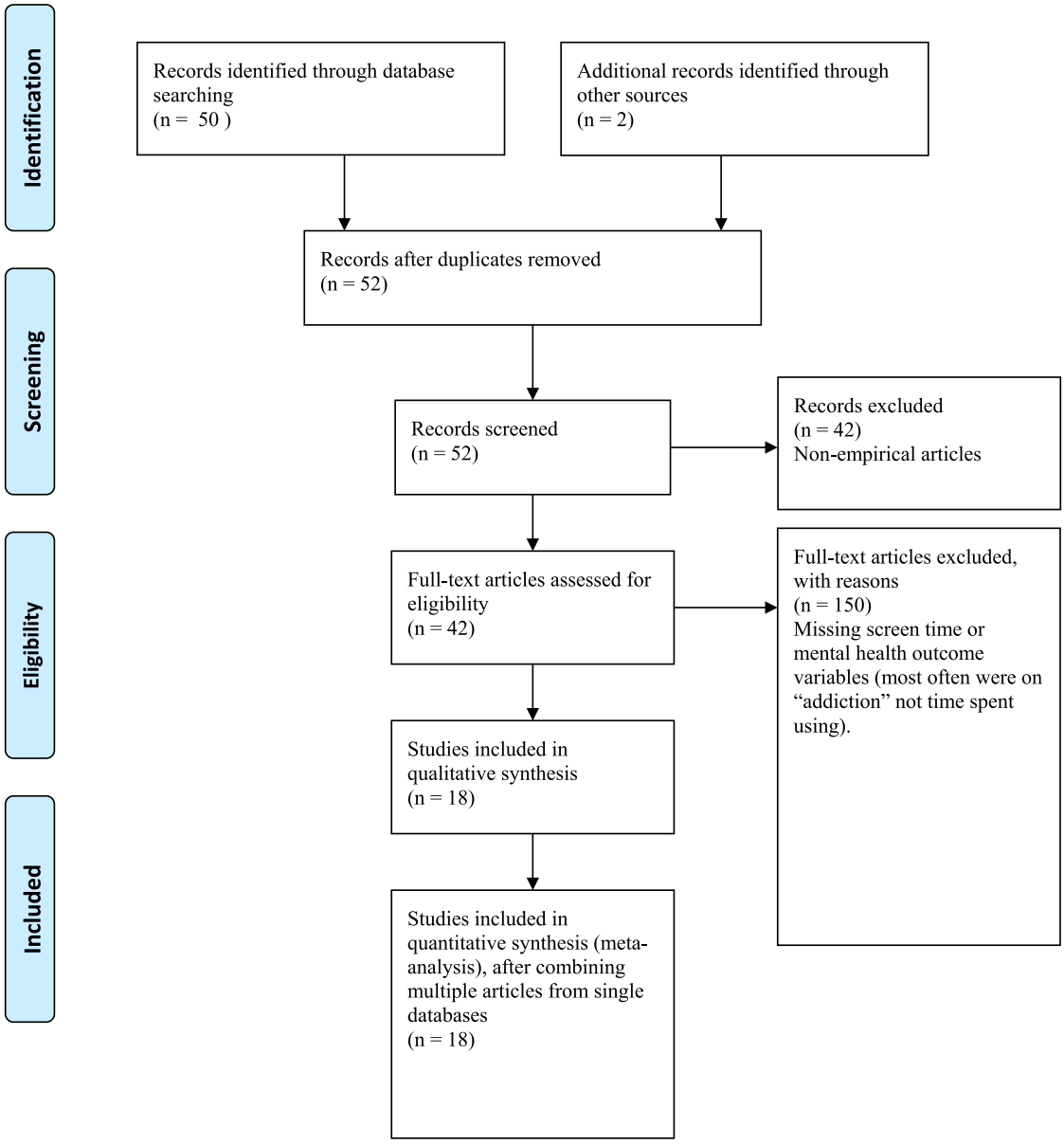
There are no conflicts of interest to report.

## Appendix B. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.chb.2022.107341.

## Appendix A

**RISMA 2009 Flow Diagram**

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