Responsible Product Design to Mitigate Excessive Gambling:
A Scoping Review

William H. B. McAuliffe, Ph.D.^{1,2,*}

Timothy C. Edson, Ph.D.^{1,2}

Eric R. Louderback, Ph.D.^{1,2}

Alexander LaRaja, B.S.¹

Debi A. LaPlante, Ph.D.^{1,2}

¹Division on Addiction, Cambridge Health Alliance, Malden, MA, USA

²Harvard Medical School, Boston, MA, USA

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Corresponding author: William H.B. McAuliffe, williamhbmcauliffe@gmail.com

Abstract

Researchers and gambling operators have developed *game-based responsible gambling tools and interventions* that are intended to mitigate risks for gambling-related harm. We conducted a scoping review of this literature, with a focus on replicability and transparency. We charted 86 studies on characteristics such as study design and sample size. No tools or interventions had unambiguous evidence of efficacy, but some show promise. Pre-registration of research hypotheses, methods, and analytic plans was absent until 2019, reflecting a more recent awareness of open science practices. Published studies also inconsistently reported effect sizes and power analyses. Finally, we observed that the replicability of the responsible product design literature is uncertain but could be low. Specifically, entering each study's primary result into a z-curve analysis—a tool for quantifying the replicability of a body of literature—revealed some evidence of publication bias based on statistical significance. Greater transparency and precision are paramount to improving the evidence base for responsible product design to mitigate gambling-related harm.

Introduction

Interventions and tools for the safe use of inherently risky consumer products take many forms. For example, cars have mandatory *structural* features that autonomously mitigate the effects of accidents (e.g., airbags and crumple zones), include optional *user-directed* tools that assist with safe driving practices (e.g., turn signals and seat belts), and are subject to *regulations* that further promote safe driving at large (e.g., minimum age of operator requirements and speed limits). Understanding the strength of evidence for various safety features and interventions can help stakeholders decide which to implement.

There is concern that popular gambling products, especially electronic gaming machines and internet gambling platforms, include features that increase risky gambling behavior (Lopez-Gonzalez, Estévez, & Griffiths, 2019; Yücel, Carter, Harrigan, van Holst, & Livingstone, 2018). For instance, the ability to prematurely stop the reels of a video slot machine that has predetermined outcomes gives users an illusion of control over the outcome, motivating them to play longer (Ladouceur & Sévigny, 2005). Researchers have called for a greater emphasis on implementing safety features and interventions for gambling products (e.g., Browne & Rockloff, 2018). Here, we report findings from a scoping review that quantitatively summarizes key features of existing research on game-based responsible gambling tools and interventions (Tricco et al., 2016). We identify trends in how studies are conducted, the state of knowledge about each type of tool, and whether a formal meta-analysis would be valuable. We also use the main results from each study to estimate the replicability of research on product safety in gambling.

Product Safety for Gamblers

Responsible product design encompasses the obligatory efforts of government and industry actors to protect consumers, as well as the empowerment of gamblers to make informed

decisions (Blaszczynski et al., 2011). Structural features might facilitate this goal by reducing the likelihood and impact of an inadvertent period of excessive gambling. Tools only are structural if they operate beyond the user's control. For example, Rockloff, Donaldson, and Browne (2015) introduced an automatic disqualification from jackpot wins after a fixed number of bets to disincentivize users from persisting in their play. Tools are user-directed if the user is not required to interact with them or can opt out of them. User-directed tools might empower gamblers by providing them with accurate information and concrete means of regulating their own temptations to gamble. For instance, pre-commitment systems for Electronic Gaming Machines (EGMs) allow users to voluntarily set limits for spending or losses, with mechanisms that interrupt play when reaching these limits (Thomas et al., 2016). Regulatory initiatives mandate the provision of accurate information and limit features that might facilitate excessive gambling. For example, Norway temporarily banned EGMs in 2006, later reintroducing new versions with less intense audio and visual stimuli, no banknote acceptors or payouts, and limits for daily and monthly losses (Rossow and Hansen, 2016).

Decisions to implement product designs intended to facilitate responsible gambling should be based on scientific assessment rather than intuitions or anecdotal evidence (Ladouceur, Blaszczynski, Shaffer, & Fong, 2016). Interventions that have not been thoroughly vetted might have harmful effects that outweigh any intended benefits (Lilienfeld, 2007). Furthermore, the implementation of tools that appear effective but are not could provide industry actors with unwarranted moral cover from advocates of more invasive interventions (Ivanova, Magnusson, & Carlbring, 2019).

Existing Reviews of Responsible Product Design for Gambling

There have been several qualitative reviews of the empirical evidence for responsible gambling interventions as of 2015 (for an umbrella review, see McMahon, Thomson, Kaner, & Bamber, 2019). These include reviews of structural features in electronic gambling (Harris & Griffiths, 2017), user-directed tools, such as self-exclusion (Drawson, Tanner, Mushquash, Mushquash, and Mazmanian, 2017), government and industry initiatives (Tanner, Drawson, Mushquash, Mushquash, & Mazmanian, 2017), product safety tools within real gambling environments (Ladouceur, Shaffer, Blaszczynski, & Shaffer, 2017), and EGM warning messaging (Ginley, Whelan, Pfund, Peter and Meyers, 2017). Although the reviews vary in their takeaway messages, they all stress that existing studies are limited by (a) relying on retrospective self-report, (b) using observational methods without incorporating features that address threats to causal inference, or (c) studying non-gamblers in laboratory settings. However, a more basic desideratum is whether published studies have yielded *replicable* findings. In addition to systematically charting the characteristics of available product safety evaluations, our review makes a unique contribution by focusing on replicability. Because opacity in how studies were conducted and analyzed undermines replicability, we also quantify the *transparency* of the responsible product design literature.

Replicability of Responsible Product Design Research

Collaborative efforts to estimate the replicability of studies published in eminent journals (e.g., Camerer et al., 2018; Open Science Collaboration, 2015), as well as the increasing number of individual replication attempts (e.g., Nature Human Behaviour, 2020), have undermined confidence in numerous foundational findings in social science. Central reasons for poor replicability include low statistical power (Button et al., 2013), undisclosed methodological

decisions that artificially inflate type I error rates (often called “questionable research practices”; Simmons, Nelson, & Simonsohn, 2011; or “researcher degrees of freedom”; Wicherts et al., 2016), and publication bias abetted by incentives for novel, positive findings (Smaldino & McElreath, 2016).

The best statistical tool available to estimate a literature’s replicability is z-curve (Brunner & Schimmack, 2019). Researchers use z-curve to estimate the mean power of a set of studies with significant effects. Because we do not know which studies test true alternative hypotheses, “power” here does *not* refer to probability of obtaining a significant result conditional on the null hypothesis being false. Instead, power is the *unconditional* probability of obtaining a significant result. As Schimmack (2018) explains, “... if a set of studies with mean power of 60% were replicated exactly (including sample sizes), we would expect that 60% of the replication studies produce a significant result again.” To our knowledge, researchers have not yet applied z-curve analyses, or any other estimator of replicability, to any segment of the gambling literature.

Transparency of Responsible Product Design Research

Because many replicability issues are due to a lack of transparency about analytic decisions, we also coded the transparency of each study along several dimensions. First, we employ a broad conceptualization of potential conflicts of interest by coding not only for funding sources, but also for the presence of a conflict of interest statement and a disclosure of funding sources from the past five years. Second, we code for whether the study was pre-registered and, if so, whether a link to the pre-registration is available in the manuscript. Pre-registration is the process by which researchers preempt questionable research practices by publicly document their hypotheses, methods, and analytic strategies prior to commencing a study (Nosek et al., 2019;

Wicherts et al., 2016). Third, we code for whether the study contains a power analysis and, if so, whether it was computed *a priori* or *post-hoc*. Conducting and reporting an *a priori* power analysis incentivizes researchers to conduct adequately powered studies, which in turn increases the likelihood that significant effects reflect true effects (Button et al., 2013; Loken & Gelman, 2017). Fourth, we code for whether the study accompanies the test statistics with effect sizes and, if so, whether such effects were unstandardized or standardized. Effect sizes help readers understand whether authors' qualitative description of an effect's practical importance is consistent with its actual magnitude (Pek & Flora, 2018). Effect sizes also can contain information about (a) replicability, because tests of larger effects more often yield significant results, and (b) fidelity of the research process, because honestly reported tests of non-trivial hypotheses typically yield medium-to-small effect sizes (Funder & Ozer, 2019; Hilgard, 2019).

Methods

We drafted our research protocol using the Preferred Reporting Items for Systematic reviews and Meta-Analyses extension for Scoping Reviews (PRISMA-ScR; Tricco et al., 2018). The pre-registration for this project, as well as transparent changes to the pre-registration, are available on the Open Science Framework (osf.io/m3nju/files/).

Study Inclusion Criteria

We included studies if they (1) were peer-reviewed, (2) were published at any point prior to our search, (3) were written in English, and (4) describe a quantitative empirical assessment of a *game-based* (i.e., intrinsic to a specific gambling product, rather than restrictions or tools that are intended to reduce gambling harm across any and all gambling products) structural feature, user-directed tool, or regulatory initiative to promote responsible gambling. We specified the

first three criteria during our sample acquisition and the fourth criterion during our title and abstract inspection and full-text inspection.

Information Sources and Search Strategy

A PRISMA diagram (see Figure 1) displays a summary of our search for studies that meet the inclusion criteria. To identify potentially relevant studies, on February 5, 2020, we searched the following bibliographic databases covering a variety of scientific disciplines: Medline, Embase (medicine); PsycARTICLES, PsycINFO (psychology); Global Health (public health); the Education Resources Information Center [ERIC] (education); and the Social Science Premium Collection. We exported the results into EndNote and removed duplicates.

Figure 1.

PRISMA diagram of study selection and coding process

We selected search terms by reviewing search terms used in published reviews of gambling-related research. Specifically, we searched abstracts for the following keywords: gambl*, betting, wager*, responsib*, regulat*, protect*, warn*, structural, and product safety. We used the following search combinations: (gambl* OR betting OR wager*) AND (responsib* OR regulat* OR protect* OR warn* OR structural OR "product safety").

Once we specified our initial sample of studies by employing the first three inclusion criteria during a database search, we eliminated duplicates resulting from databases containing overlap in their results. Then, three research assistants screened the titles and abstracts of 10% of all retrieved studies to assess their relevance with respect to the fourth criterion (i.e., Title & Abstract Inspection; Krippendorff's $\alpha = .77$). Research assistants deemed studies as relevant when they described an empirical test of a game-based intervention. Vague study titles and abstracts were retained for full text inspection. Afterwards, they resolved disagreements through discussion with the first author.

Next, research assistants divided the remaining retrieved studies into three groups and screened their titles and abstracts independently. Vague study titles and abstracts were retained for full-text inspection. After reading the full texts, the first author deemed 11 studies as irrelevant that research assistants had flagged as meeting inclusion criteria. Our analytic sample consisted of all of the eligible studies from the database search ($N = 43$), as well as ($N = 23$) studies that the first author found after examining the reference lists of previous reviews, the studies that met inclusion criteria, and studies that cited the included studies according to Google Scholar. As a final quality check, the first author examined the abstracts that the research assistants indicated were irrelevant and found 12 that in fact met inclusion criteria. The final

sample consisted of 78 journal articles with 86 relevant studies. See Table S1 at <https://osf.io/nkapj/> for how each full text was evaluated.

Data Charting Process

Raters charted the studies on the data items listed in Table 1 using Google Forms. We used an iterative process to determine the reliability of our charting. Two raters independently charted the data from a randomly selected subset of articles representing 10% of our eligible studies from the database search. Because the two coders' interrater reliability did not meet our standard (i.e., Cohen's κ is at least 0.70 and percentage agreement is at least 80%; McHugh, 2012) after four iterations, we amended our pre-registration such that both raters charted all studies. The basis on which the first author resolved each discrepancy is available at <https://osf.io/fa539/>. We also created a Characteristics of Included Studies (COIS) table that summarizes each study in terms of the charted items (see <https://osf.io/k9sbq/>).

Data Charting Process for Study Funder

Following Gray, Edson, Nelson, Grossman, and LaPlante (2020), we used funding sources' websites to code whether studies had ties to the gambling industry. We counted a study as having direct gambling industry funding if any of the listed funding sources were directly part of the gambling industry (e.g., private companies such as Aristocrat Leisure Industries, and nationalized companies such as Loto-Q) or were nonprofits funded by the gambling industry (e.g., International Center for Responsible Gaming). Non-industry funders included government agencies, universities, and private foundations. The websites on which we based our categorization decisions are available at <https://osf.io/6sg9d/>.

Table 1. Data items, response format and response options

Data item	Response format	Response options, if applicable
Study ID	Fill-in-the-blank	e.g., Nelson, S. E., LaPlante, D. A., Peller, A. J., Schumann, A., LaBrie, R. A., & Shaffer, H. J. (2008). Real limits in the virtual world: Self-limiting behavior of Internet gamblers. <i>Journal of gambling Studies</i> , 24(4), 463-477.
First Author Last Name	Fill-in-the-blank	e.g., Nelson
Study Funder	Fill-in-the-blank	e.g., International Center for Responsible Gaming
General Past 5-year Author Funding Statement	Select one	Yes, no
Conflict of Interest Statement	Select one	Conflicts reported, No Conflicts reported, No Conflict of Interest Statement
Month and year of earliest study publication	Fill-in-the-blank	e.g., March, 2016 Online First
Power analysis reported	Select one	Yes, <i>a priori</i> , Yes, <i>post-hoc</i> , Yes, timing unclear, No
Sample size (final analytic sample size reported after missing data is accounted for)	Fill-in-the-blank	e.g., N=300

Specific sample description	Fill-in-the-blank	e.g., “gambling treatment clients recruited from 2 treatment sites”
Study design	Gated question, Select one	(1) Experimental or Observational (2a) Experimental: RCT, Non-randomized trial, or other (2b) Observational: Cross-sectional, Prospective Cohort, Retrospective Cohort, or Case Series/Case Study, or other
Gambling concept(s) measures	Select all that apply / Fill-in-the-blank	Gambling participation / involvement (e.g., frequency, money spent, type of gambling activity, age of onset of gambling) regardless of gambling problem, Presence / severity of problem gambling (includes gambling-related consequences, age of onset of gambling problem), and Other (specify)
Game-based tool or intervention type	Gated question, Select all that apply / Fill-in-the-blank	(1) User-directed tool, Structural feature tool, and/or Regulatory tool to promote responsible gambling, Other (specify) (2) Regulatory tool: Game-based tool or Other regulation (specify)
Duration of follow up phase, if applicable	Fill-in-the-blank	e.g., 3 months

Source of gambling concept(s) measured	Select all that apply	Self-report, Proxy report, Gambling records, Financial records, Other (specify)
Did the authors statistically test the association between the game-based tool or intervention type and a gambling outcome?	Select one	Yes, no (i.e., descriptive only)
Did the results of the statistical test include an effect size measure?	Select one	Yes, standardized and unstandardized; Yes, standardized; Yes, unstandardized; No
Registration status	Select one	Pre-registration available, Pre-registration and registered report available, Pre-registration not available, No Pre-registration
Major finding(s)	Fill-in-the-blank (narrative summary; 2-3 sentences)	
Notes	Fill-in-the-blank (optional)	

Analytic Strategy and Synthesis of Evidence

We examined separate cross-tabulations of intervention type (i.e., structural feature, user-directed tool, or regulatory initiative) with study funder, study design, and registration status. We also provided a narrative summary of the major findings related to the effectiveness of identified safety characteristics, organized by intervention type. Finally, we calculated a year-by-

year summary count of the number of publications by intervention type. See

<https://osf.io/76wm4/> for the syntax to conduct these analyses.

Z-Curve

We used the *zcurve* package in R to conduct z-curve analyses (Bartoš & Schimmack, 2020b). The *zcurve* function uses the *z*-scores or *p*-values of significant findings to estimate the distribution of the population values that should be represented by the significant studies (see our pre-registration amendment at <https://osf.io/zcegu/> or Bartoš & Schimmack, 2020b for technical details). The *z*-scores or *p*-values for all studies composes the Observed Discovery Rate, or the proportion of coded tests that had significant results in the hypothesized direction. Information from the estimated distribution of *z*-scores underlies the Estimated Discovery Rate, or the estimated proportion of all studies that have been conducted that had significant results. Unambiguous evidence for publication bias exists if the Observed Discovery Rate is higher than the upper confidence limit of the Estimated Discovery Rate.

In addition to these statistics, the File-Drawer Ratio estimates how many non-significant results there might be for each reported significant result. The File-Drawer ratio provides information to compute the False Discovery Risk, or the maximum proportion of significant studies that could represent false positives. Finally, the Expected Replication Rate is the estimated proportion of significant studies that would yield another significant effect if subjected to a direct replication. However, commentators frequently point to small differences between original studies and replication studies that could explain why the former yield larger effect sizes than the latter (e.g., Noah, Schul, & Mayo, 2018; Schwarz & Clore, 2016). Consequently, the Expected Discovery Rate should more accurately predict the outcome of actual replication efforts than the Expected Replication Rate.

We included all 78 studies that contained inferential tests of their key hypothesis in the z-curve analysis. Two articles used the same dataset; we included only the first publication in z-curve, as the second article examined moderators of the findings reported in the first article.

If studies did not report exact p -values for significant effects, we computed them based on the sample size and either the descriptive statistics or test statistic using either base packages functions (e.g., the *pf* function for the F distribution) or the *compute.es* package in R (Del Re, 2013). We contacted authors for this information when they did not include the minimally sufficient information in the paper. Two studies reported at least one p -value as less than .001, and we were unable to manually compute an exact value. After failing to receive clarification from the original authors, we treated these p -values as .0009 in the analysis.

We assigned studies with non-significant results a p -value of .300 if they did not report exact value and we could not reconstruct the exact p -value from the results reported in the paper ($n = 8$). The value of non-significant results does not impact the outcome of z-curve. We also computed exact p -values for studies that reported p -values to fewer than three decimal places. We recorded the exact p -values of significant effects in the opposite direction of what was hypothesized ($n = 3$) but treated them as non-significant (arbitrarily assigning them $p = .300$ for the purposes of the z-curve analysis). In a sensitivity analysis, we excluded studies that used a significance criterion other than a two-tailed alpha of .05 to evaluate the p -value of interest ($n = 8$), as z-curve's model of publication bias is based on censoring z-scores smaller than 1.96.

The *zcurve* function assumes that it is possible to identify a single key hypothesis test in each study. We anticipated that many studies in our review would regard multiple hypotheses as of equal importance or report multiple tests of the same hypothesis (e.g., using slightly different measures to represent the same dependent variable). After consulting with one of the developers

of the *zcurve* package (U. Schimmack, personal communication, February 26, 2020), we used the following strategies to select the “most focal hypothesis test”: (a) In cases where authors tested the effect of interventions of varying dosage, we treated the test of the strongest intervention vs. the control condition as the most focal hypothesis test (e.g., if the effect of a short break and the effect of a long break are each compared to a no-break control condition, we would regard the comparison between taking a long break and taking no break as most focal); (b) When there were multiple dependent variables that were equally relevant to the central hypothesis, we randomly chose which test to regard as most focal; (c) When not all hypotheses were relevant to promoting responsible gambling, we only treated the hypotheses relevant to responsible gambling as candidates for the most focal hypothesis.

We also conducted a sensitivity analysis in which we repeated the analysis ten times, each time *randomly* selecting which test from each study to regard as focal by using a different seed number in *R*. After discovering that a small number of studies had a very large number of focal tests, we limited the number of potentially focal tests to six per study. To estimate upper- and lower-limits of replicability, we also re-ran *z-curve* once using the highest *p*-value from each study, and once more using the smallest *p*-value available from each study. The *z-curve* dataset is available at <https://osf.io/acf3r/>; the syntax we used to manually compute *p*-values and conduct *z-curve* is available at <https://osf.io/aj6eu/>.

Results

Characteristics of Included Studies

Study Design

Of the 86 included studies, 97.7% ($n = 84$) of studies included at least one statistical test of the association between the game-based tool or intervention type and a gambling outcome.

We observed that 69.8% ($n = 60$) of all studies were experimental, 91.7% ($n = 55$) of which randomly assigned participants to condition. Also, 30.2% ($n = 26$) of all studies were observational. Among these observational studies, 34.6% ($n = 9$) were cross-sectional, 46.2% ($n = 12$) were retrospective cohort studies, 7.7% ($n = 2$) were prospective cohort studies, and 11.5% ($n = 3$) were case series.

Sample

Sample sizes varied widely, ranging from 10 to 200,000. The median sample size was 136. Authors most commonly sampled people who gamble in their everyday life (70.9%; $n = 61$). 68.9% of these studies recruited gamblers from a convenience population ($n = 42$) — that is, from a nearby casino, advertisement in a local newspaper, online panel pre-screened for gamblers, gamblers in an undergraduate psychology student pool, etc. The rest (31.1%; $n = 19$) sampled gamblers from a gambling platform or casino loyalty program.

Among the 29.4% ($n = 25$) of studies that did not exclusively sample gamblers, 48% ($n = 12$) sampled community members and 52% ($n = 13$) sampled university students. Across all studies, 8.1% ($n = 7$) of studies used screening tools during enrollment to sample at-risk gamblers, and 11.6% ($n = 10$) used screening tools during enrollment to exclude at-risk gamblers from participation.

Gambling Concepts

Of the 86 included studies, 94.2% ($n = 81$) of studies measured gambling participation. 73.3% ($n = 63$) of studies measured the presence or severity of problem gambling. Included in this count of problem gambling are measures of problem gambling specifically, as well as measures of constructs that might be symptomatic of problem gambling, such as impulsivity.

One study measured recall of the content of gambling messages, which we did not count as a measure of either gambling participation or problem gambling.

Measurement Method

Many studies relied on multiple measurement methods. 76.7% ($n = 66$) of studies used at least one self-report measure. 61.6% ($n = 53$) of studies used gambling records to measure at least one construct. Two studies used proxy reports (in these cases, by trained observers), and one used financial records.

Exploratory tetrachoric correlations revealed that studies featuring self-reports had a very strong positive association with studies of problem gambling (.95) and a moderately negative association with studies of gambling participation (-.29). Similarly, studies drawing on gambling records were often the same studies that measured gambling participation (.68), and less likely to be studies that measured problem gambling (-.46).

Follow-Up Period

Of the 86 included studies, 29.1% ($n = 25$) of studies included a “follow-up” component (i.e., at least one measurement occasion beyond the day on which the intervention was implemented). The median follow-up length among studies that had a follow-up component was 60 days (minimum = 1 day, maximum = 1 year).

Current and Past Funding Sources

Some articles reported multiple sources of funding. Of all 78 included articles, 26.9% ($n = 21$) had direct funding from the gambling industry, 10.4% ($n = 8$) had university funding, 3.8% ($n = 3$) had funding from a private foundation, 39.7% ($n = 31$) had funding from a government agency, 10.3% ($n = 5$) received no funding, and 25.6% ($n = 20$) did not provide enough

information about funding source to code. Of government-funded articles, 25.8% ($n = 8$) were from an agency that is funded by revenue from gambling.

Conflict of Interest Statement

Of the 78 articles, 20.5% ($n = 16$) reported conflicts of interest, 30.8% ($n = 24$) reported that they had no conflicts of interest, and 48.7% ($n = 38$) did not include a conflict of interest statement. Only two studies provided an explicit statement about all sources of funding that authors had received in the past five years.

Pre-Registration Status

Of the 86 included studies, 93% ($n = 80$) of studies made no mention of a pre-registration. 5.8% ($n = 5$) of studies had a pre-registration that we were able to access. An additional study mentioned pre-registrations, but the hyperlink did not work. All six studies that mentioned a pre-registration were published in either 2019 or 2020.

Power Analysis

Of the 84 studies that included a statistical test, 7.1% of studies ($n = 6$) reported an *a priori* power analysis. Another 6.0% ($n = 5$) reported a *post hoc* power analysis. 87.2% ($n = 75$) of studies did not report a power analysis justifying sample size.

Effect Size

Of the 84 studies that included a statistical test of an intervention on gambling, 64.3% ($n = 54$) reported at least one effect size. Of those, 37.0% ($n = 20$) reported at least one unstandardized effect size and 77.8% ($n = 42$) reported at least one standardized effect size. Because of the very large number of tests reported in several studies, many of which were not related to the evaluation of responsible product design per se, we abandoned our pre-registered

plan to “report what percentage of the test statistics we transcribe are accompanied by an effect size.”

Unplanned Coding of Sampled Populations

While charting studies, we noticed that most studies were conducted in a small number of countries. To follow up on these anecdotal observations, we charted the country or countries from which each study sampled. The most commonly sampled countries were Australia (22.1%, $n = 19$), Canada (20.9%; $n = 18$), and the United States (10.5%, $n = 9$). There were six studies (6.9%) that sampled several countries, usually all from Europe but in some cases from multiple continents. There were eight studies (9.3%) where authors did not specify the country(ies) where the research was conducted. There were no studies that primarily sampled from Asia, Africa, or South America.

We also formed the impression that most studies did not discuss threats to generalizability based on participant characteristics. Consequently, we transcribed statements in each article’s discussion section about potential limitations based on the sampled population. 15.1% of studies ($n = 13$) explicitly mentioned limitations based on country or cultural milieu (e.g., ethnic group, socioeconomic status, generation of birth, urbanicity, etc.). Many discussion sections that did not mention cultural constraints did discuss how university students might differ from the general population, how sampling players who favor a certain game might have impacted results, or how results from low-risk gamblers might not extend to high-risk gamblers.

Game-Based Tool or Interaction Type

Some studies ($n = 7$) tested multiple types of responsible product designs. Of the 86 studies, 61.6% of studies ($n = 53$) tested structural tools, 41.9% ($n = 36$) tested user-directed tools, and 4.7% tested ($n = 4$) regulations. We used chi-square tests of independence to examine

whether there is a relationship between intervention type and study funder, study design, or registration status. We excluded studies that investigated multiple types of tools ($n = 7$) from this analysis.

Intervention type had a marginally significant association with pre-registration status, $\chi^2(4) = 9.20, p = .056, \phi_c = .24$. The test was significant when using Fisher's exact test to account for low cell counts, $p = .027$. This potential effect was driven by all pre-registered studies testing user-directed tools.

Intervention type significantly varied by study design, $\chi^2(2) = 13.73, p = .001, \phi_c = .42$. Standardized residuals were significant (greater than 2; Agresti, 2007) for structural features and regulations, but not user-directed tools. Structural tools were tested more often via experiments ($n = 37$) than by observational methods ($n = 9$). User-directed tools were tested by observational methods ($n = 13$) almost as often as by experiments ($n = 16$). Regulations were tested only in observational studies.

Last, we found that intervention type did not vary by industry-funded research status, $\chi^2(2) = 1.69, p = .429, \phi_c = .15$. The non-significance of this pattern remained when we ran an unplanned analysis that counted studies ($n = 9$) sponsored by government agencies that are funded by earmarked tax revenue from gambling operators (e.g., research funded by Gambling Research Exchange Ontario) as industry-funded research.

Temporal Trends

Included studies were published between 2001 and 2020. See Figure 2 for the frequency of studies on structural features, user-directed tools, and regulations by year (the total count is more than 86 because some studies examined more than one type of tool). Researchers have published about two to five evaluations of structural feature tools annually since 2003, with a

spike at 13 studies in 2019. Studies of user-directed tools have continued at a steady rate from 2007 to the present, with a peak at 10 in 2019. Regulations were studied only between 2008 and 2011.

Figure 2.

Yearly counts of studies on structural features, user-directed tools, and regulations

Note: 2020 only includes studies through May.

Narrative Review

Structural Feature Tools

Our charting suggested that the gambling research literature has examined three main structural feature tools: (1) pop-up messages, (2) breaks in play, and (3) covert structural tools.

Pop-up Messages

In all, 41 of 53 structural feature tool studies (77%) examined pop-up messages. Perhaps the most well-studied pop-up warning message seeks to educate participants of the statistical principles that explain why the expected value of gambling is negative. However, studies of so-called self-appraisal messages that encourage gamblers to reflect on whether their current gambling behavior is consistent with their goals are also common.

There were 20 pop-up message studies (49%) suggesting that pop-up messages had a favorable responsible gambling impact. For example, Jardin and Wulfert (2009, 2012) compared pop-ups that remind participants about the chance-based nature of gambling to pop-ups with pop-up trivia and a control condition with no pop-ups. Participants who received reminder messages lost less money and made fewer bets than those who read trivia or did not see a pop-up.

We observed that 7 pop-up message studies (17%) suggested pop-up messages had no responsible gambling impact. For example, Lavoie and Main (2019) presented pop-ups just before playing slots which warned that gambling can produce a state of immersion that can cause excessive spending. In a second study, they presented pop-ups in the middle of blackjack to inform the user how long he or she had been playing. The pop-ups did not reduce immersion or time or money spent gambling in either study.

Finally, our charting indicated that 14 pop-up message studies (34%) suggested pop-up messages had mixed impact. For example, Tabri, Hollingshead, and Wohl (2019) had participants set money limits, and varied whether participants (a) received a single warning message asking whether they would like to continue when the limit was reached, or (b) also received a message when they were close to reaching their limit. Messages about approaching limits increased stopping of play before participants reached their limits. This effect was strong (i.e., an odds ratio above 31) for participants who did not have a financially focused self-concept (i.e., those who do not define themselves in terms of financial success), but was non-significant for those with a financially focused self-concept (i.e., those whose self-worth is tied up in financial success).

Breaks in Play

In all, 7 of 53 structural feature tool studies (13%) examined breaks in play. The typical justification for mandatory pauses in between rounds or after playing for a certain duration is that gambling induces in at-risk players a dissociative state that undermines rational decision-making (Diskin & Hodgins, 1999; Oakes, Pols, Lawn, & Battersby, 2020). In games of chance, the break in play would purportedly mitigate excessive gambling by lifting players out of their trance. In games of skill, a forced pause would give losing players the time to reevaluate their strategy.

There were 3 breaks in play studies (43%) suggesting favorable responsible gambling impact. For example, Thompson and Corr had both problem gamblers (Thompson & Corr, 2013) and non-problem gamblers (Corr & Thompson, 2014) from Wales play a card game in which winning became less likely over time. They found that imposing a 5-second pause between bets reduced the number of rounds played and monetary losses.

We observed that 3 breaks in play studies (43%) had no or unfavorable responsible gambling impact. For example, Blaszczynski, Cowley, Anthony, and Hinsley (2016) varied whether Australian university students received no break, a 3-minute break, or 8-minute break from blackjack. The results indicated that both 3-minute and 8-minute breaks increased cravings to gamble and did not decrease dissociative feelings.

Finally, one breaks in play study (14%) suggested mixed responsible gambling impact. Parke, Dickinson, O'Hare, Wilson, Westerman-Hughes, and Gerling (2019) found that a three-minute break did increase the response latency between rounds for EGM players in the face of consistent losses. However, this slowed play did not translate into playing fewer trials.

Covert Structural Tools

In all, 6 of 53 structural feature tool studies (11%) examined covert structural tools. Covert interventions are intended to affect the proximate causes of excessive gambling without requiring buy-in from the gambler. One covert intervention study (17%) reported no responsible gambling impact (Armstrong, Rockloff, Browne, and Blaszczynski, 2019). The authors examined whether an implicit prime of analytic thinking, or a stimulus that is designed to induce a reflective mindset without the participant realizing that the stimulus has this effect, would attenuate erroneous gambling beliefs and gambling intensity in a sample of EGM players. Randomly assigning participants to unscramble sentences that either did or did not include words related to rationality had no significant effects on gambling beliefs or behavior.

Our charting indicated that 5 covert intervention studies (83%) yielded mixed responsible gambling impact. For example, Sharpe, Walker, Coughlan, Enersen, and Blaszczynski (2005) modified EGMs in Australian venues with varying levels of reel speeds, maximum bet restrictions, and restrictions on the maximum bank note accepted. Limiting the maximum bet to \$1 reduced the number of bets and losses relative to EGMs with \$10 bet maximums but modifying note acceptors and reel speeds did not reduce gambling activity.

User-Directed Tools

Our charting suggested that the gambling research literature has examined two main user-directed feature tools: (1) precommitment and (2) information aids.

Precommitment

In all, 16 of 36 user-directed tool studies (44%) examined precommitment. Precommitment involves prospectively planning to restrict one's own ability to gamble

excessively. Its efficacy is premised on players recognizing that they have difficulty exercising self-control “in the heat of the moment.”

Our charting revealed 6 precommitment studies (38%) finding that precommitment had a favorable responsible gambling impact. One example was Brevers, Noel, Clark, Zyuzin, Park and Bechara (2016), who presented participants with a gambling task with four options that varied in reward and risk. During “precommitment” trials, there was a preliminary step in which participants could remove the high-risk, high reward options from the trial. In the control trials, all four options were always available. Participants eliminated the high-risk options in about half of precommitment trials, resulting in lower-risk decisions in the precommitment condition.

We observed 4 precommitment studies (25%) suggesting that precommitment had no or unfavorable responsible gambling impact. In arguably the best-designed study that met inclusion criteria, Ivanova et al. (2019) manipulated whether Finnish online gamblers were presented with a prompt to consider setting a deposit limit. Deposit limits control how much users can wager over a certain period. The prompts greatly increased limit-setting relative to a no-prompt control condition. But this increased limit-setting did not reduce net loss, total number of days gambled, or amount of money deposited in the following 90 days.

Finally, 6 precommitment studies (38%) reported that precommitment had mixed responsible gambling impact. To illustrate, Caillon et al. (2019) randomly assigned users to self-exclude from an online gambling platform for a week. Self-exclusion prevents the user from using a certain gambling platform at all. Self-exclusion did not cause significant differences in money or time spent gambling fifteen days or two months after the self-exclusion began. There were, however, decreases in gambling illusions and desire to gamble two months later.

Information Aids

In all, 24 of 36 user-directed tool studies (67%) examined information aids. Information aids provide the user with facts about a game's payout structure. They should reduce gambling to the extent that they correct misbeliefs that cause excessive play and should minimize losses to the extent that they help users make statistically optimal decisions.

There were 8 information aid studies (33%) suggesting that information aids had a favorable responsible gambling impact. For example, Walker, Stange, Dixon, Koehler, and Fugelsang (2019) found that when scratch card players had access to both the number of unclaimed prizes left and the payback percentage (the ratio of unclaimed prizes to total scratch cards remaining) in a numerical format, participants were influenced by the former even though it is only the latter that is diagnostic. In a follow-up experiment, Walker et al. (2019) presented the same payback percentage information using a visual (a five-star rating system). Participants now more often relied on the payback percentage to make decisions about scratch cards, even though they did not understand the concept of payback percentage any better than participants in the first experiment. The visual rating system might have been interpreted by participants as recommendations for or against a given scratch card. So long as participants trusted the testimony of the experimenters, there was no need for participants to understand why payback percentages are more germane than counts of unclaimed prizes.

We observed that 6 information aid studies (25%) suggested information aids had no or unfavorable responsible gambling impact. For example, Beresford and Blaszczynski (2020) tested multiple formats to improve understanding of return-to-player percentage. The concept refers to the percentage of money that a game returns to players in the long run, but players often believe that it approximates the percentage of stakes that remain with the average player at the

end of individual sessions. This belief is incorrect because EGM winnings are designed to vary substantially in the short-term, and reinvesting wins tends to reduce earnings to zero. The authors reported that neither an infographic, a vignette, or a brochure increased understanding of return-to-player percentage relative to the mandatory signage on EGMs in South Australia.

Finally, our charting indicated that 10 information aid studies (42%) reported mixed responsible gambling impact. For instance, “Basic strategy” is a decision tool for reducing the house edge in blackjack to less than one percent (Thorp, 1966). Phillips and colleagues conducted experiments where the blackjack program recommended the decision that was in accordance with Basic strategy to the user. The authors found that the presence of recommendations increased adherence to statistically optimal play, but also increased participants’ willingness to make risky bets (Phillips & Ogeil, 2007).

Regulatory Initiatives

Our charting suggested that the gambling research literature has empirically examined two main regulatory initiatives: restricting the supply of EGMs ($n = 2$) and restricting EGM features ($n = 2$). The supply reduction studies did not show favorable impact. Delfabbro (2008) observed how restrictions in the number of EGMs allowed in venues in South Australia *increased* gambling revenues. A follow-up survey revealed that a majority of gamblers in the region had noticed that the number of EGMs had fallen, but only a minority had reported gambling less as a result.

The feature restriction studies did show evidence of efficacy. Hansen and Rossow (2010) found that adolescent problem gambling declined in Norway after the government removed banknote acceptors from slot machines. Participants reported having gambled fewer times in the past year after the ban on banknote acceptors. In a follow-up analysis, Hansen and Rossow

(2012) reported that this decrease held across all levels of gambling, although the decrease in the proportion of participants gambling at least 80 times a year was about three times larger than the decrease in participants gambling at least 20 times a year.

Replicability: z-curve

The point estimates and 95% confidence intervals for the z-curve of the most focal hypothesis tests, as well as our 12 sensitivity tests (i.e., the highest p -values, the lowest p -values, and 10 iterations in which p -values from each study were randomly selected), are presented in Table 2. Because choosing the single most focal hypothesis test was difficult in many cases, we summarize the range of point estimates across the 13 iterations rather than focusing just on the single z-curve composed of p -values that we deemed most focal.

The Observed Discovery Rate ranged from .50 to .74 across our tests, indicating that most studies in the responsible product design literature report significant findings. The Expected Discovery Rate ranged from .10 to .58, suggesting that at least 42% of studies that have been conducted on responsible product design had null results. In 8 of 13 iterations of z-curve (e.g., Most Focal and Smallest p), the lower confidence limit for the Observed Discovery Rate was higher than the point estimate for the Expected Discovery Rate, but the upper confidence limit for the Expected Discovery Rate was higher than the point estimate of the Observed Discovery Rate. This pattern is suggestive but inconclusive evidence of publication bias. In 2 cases (i.e., Largest p and Random 10), the two confidence intervals did not overlap, which is clear evidence of publication bias. In two cases (i.e., Random 2 and Random 6), the confidence intervals for the two estimates overlapped, but the point estimate for the Observed Discovery Rate was higher than the upper confidence limit of the Expected Discovery Rate. This also is evidence of publication bias. In one case (i.e., Random 4), the point estimate for the Observed Discovery

Rate was well within the confidence interval of the Expected Discovery Rate and vice versa, providing some evidence against publication bias.

Table 2. z-curve Estimates and 95% Confidence Intervals

Test	ODR	EDR	ERR	Max FDR	File-Drawer
Most Focal	.64 [.52, .74]	.31 [.05, .75]	.65 [.47, .84]	.12 [.02, .94]	2.28 [.34, 17.86]
Smallest <i>p</i>	.74 [.63, .83]	.48 [.11, .95]	.79 [.63, .95]	.06 [.00, .42]	1.10 [.06, 8.06]
Largest <i>p</i>	.50 [.39, .61]	.10 [.05, .21]	.56 [.35, .74]	.46 [.20, 1.0]	8.73 [3.84, 19.00]
Random 1	.65 [.54, .76]	.36 [.06, .78]	.63 [.43, .83]	.09 [.02, .81]	1.77 [.28, 15.30]
Random 2	.64 [.52, .74]	.17 [.05, .60]	.65 [.47, .85]	.26 [.04, 1.00]	4.89 [.67, 19.00]
Random 3	.56 [.45, .67]	.18 [.06, .73]	.73 [.55, .90]	.24 [.02, .81]	4.64 [.37, 15.35]
Random 4	.60 [.49, .71]	.58 [.24, .88]	.75 [.54, .90]	.04 [.01, .17]	0.72 [.14, 3.15]
Random 5	.58 [.46, .69]	.18 [.06, .82]	.74 [.55, .92]	.24 [.01, .77]	4.62 [.22, 14.67]
Random 6	.65 [.54, .76]	.15 [.05, .57]	.60 [.42, .79]	.31 [.04, 1.0]	5.85 [.75, 19.00]
Random 7	.60 [.49, .71]	.24 [.06, .68]	.67 [.49, .85]	.16 [.03, .91]	3.11 [.48, 17.23]
Random 8	.62 [.50, .72]	.15 [.05, .59]	.69 [.50, .86]	.30 [.04, 1.00]	5.72 [.69, 18.99]
Random 9	.60 [.49, .71]	.24 [.06, .68]	.67 [.49, .85]	.16 [.03, .91]	3.11 [.48, 17.23]
Random 10	.64 [.52, .74]	.14 [.05, .32]	.67 [.50, .85]	.34 [.11, 1.0]	6.36 [2.13, 19.00]

Notes. All tests were calculated using a two-tailed alpha of .05. Test = Dataset on which z-curve was computed. ODR = Observed Discovery Rate. EDR = Expected Discovery Rate. ERR =

Expected Replication Rate. Max FDR: Maximum False Discovery Rate; File-Drawer = Ratio of studies conducted to published significant findings.

The Expected Replication Rate ranged from .60 to .79. The maximum false discovery rate ranged from .04 to .46. However, the confidence intervals for these estimates are so wide that they preclude our ability to make a general statement about the maximum proportion of significant findings that could be false positives. The file-drawer ratio ranged from 0.72 to 8.73. Similarly, the associated confidence intervals were too wide to allow us to draw conclusions about how many studies are conducted for each significant result that is published.

Re-running these 13 tests without studies that used a significance criterion other than a two-tailed alpha of .05 yielded a similar pattern of results (see Table S2 at <https://osf.io/8merf/>).

Figure 3 presents a histogram of z -scores from the most focal hypothesis tests, as well the estimates of replicability based on the z -curve of these values.

Figure 3.

z -curve of most focal hypothesis tests

Notes. The vertical red line refers to a z-score of 1.96, the critical value for statistical significance when using a two-tailed alpha of .05. The dark blue line is the density distribution for the inputted *p*-values (represented in the histogram as z-scores). The dotted lines represent the 95% confidence interval for the density distribution.

Discussion

We conducted a scoping review of 86 studies evaluating game-based responsible gambling tools that were published between 2001 and 2020 to better understand the current state of the literature in this area. Several general trends in study design were apparent in charting the included studies. First, studies were most likely to involve structural tools, followed by user-directed tools, and then game-specific regulations. (Of course, in practice some jurisdictions allow players to opt in or opt out of tools that we categorized as structural because the authors conceptualized and implemented them as involuntary. We only categorized tools as user-directed if participants could choose whether to use them or the authors noted that they would be implemented as user-directed.) Researchers were more likely to test structural features by experiments, about equally likely to use experiments or observational methods to test user-directed tools, and used only observational methods to study regulations.

The median sample size was 136. This is higher than in social and personality psychology (median = 104; Fraley & Vazire, 2014) but worse than in clinical psychology (median = 179; Reardon, Smack, Herzhoff, & Tackett, 2019) during comparable periods. Most studies did sample actual gamblers, and even those that did not sampled as often from the community as they did from university participant pools, the latter of which is likely to be the least representative of individuals who are at risk of gambling harm (Gainsbury, Russell, & Blaszczynski, 2014). Most studies did not include follow-up periods, but some of those that did measured gambling months later.

Most studies included self-reports, usually of constructs related to problem gambling. However, most studies (61.6 %) also included behavioral measures that were captured by gambling records. Given the importance of measuring behavior in order to reveal actual preferences, responsible product design research is faring much better than social psychology, where only about 6%-12% of the empirical papers in the field's premiere journal (*Journal of Personality and Social Psychology*) feature behavioral measures (Doliński, 2018).

It was rare in the responsible product design literature or in narrative reviews thereof to find a discussion of cultural moderators of a given intervention's efficacy. Most interventions so far have been tested in a small number of highly developed countries, such as Canada and Australia. There were no studies which primarily sampled populations in Asia, Africa, or South America. These results are consistent with general trends in social science more broadly, and pose similar risks for overgeneralization (Henrich, Heine, & Norenzayan, 2010). Ideally, researchers would articulate the specific characteristics of their sample that theory would predict to constrain the generality of results (Simons, Shoda, & Lindsay, 2017).

Preliminary Conclusions about Efficacy

Convergence in results across studies licenses some preliminary recommendations about which kinds of responsible product design are promising. Unfortunately, our review of the literature is consistent with earlier reviews: none of the game-based intervention tool types provide strong evidence for a particular strategy. Setting that reality aside, for structural feature tools, the best evidence supports pop-ups that encourage self-appraisal rather than pop-ups that attempt to rein in the influence of cognitive distortions. These pop-ups likely work in part because they create a brief break in play between trials. Imposing breaks long enough to effectively end a session, by contrast, increases craving and is irrelevant in settings where

customers can easily switch to a different game. For covert interventions, modifications that undo features of games that promote excessive gambling likely have efficacy.

There are evidence-based reasons to doubt that user-directed tools are sufficient to prevent risky gambling. Many gamblers view pre-commitment tools as relevant only to problem gamblers (Gainsbury, Jakob, & Aro, 2018), and a common feature of problem gambling is denial of maladaptive behavior patterns (Shah et al., 2019). Moreover, precommitment tools such as limit-setting and self-exclusion do not reliably reduce time or money spent gambling. On the other hand, ruling out riskier bets from the start (Brevers et al., 2016) is a novel idea with some support. About a third of information aid studies indicated that they appear to have positive short-term effects, but long-term effects still require examination. Finally, with only four studies on regulatory initiatives, it is premature to draw conclusions about efficacy.

Replicability and Transparency

We tested whether the responsible product design literature contains publication bias using z-curve. Point estimates for the Expected Discovery Rate were very low and almost always below the lower confidence limit of the Observed Discovery Rate. Thus, there is probably some publication bias based on statistical significance in the responsible product design literature. Bias based on statistical significance could manifest through not publishing null results or using questionable research practices to obtain significant results. However, the confidence intervals for the Expected Discovery Rate were very wide, leaving the magnitude of publication bias in this literature unclear.

To the extent that direct replications inevitably differ in some respects from the original studies, the point estimates of the Expected Discovery Rate suggests that most replications of significant findings in the responsible product design literature would fail. Conditions appear

more favorable, however, if exact replications could be conducted, as the Expected Replication Rate ranged from .61 to .79. These estimates are in line with large-scale replication efforts in experimental philosophy (Cova et al., 2018), experimental economics (Camerer et al., 2016), and social science experiments published in *Science* and *Nature* (Camerer et al., 2018), but lower than research on associations between the five-factor model of personality and consequential life outcomes (Soto, 2019).

How do our estimates of replicability compare with that of the Open Science Collaborative (OSC, 2015), which attempted to replicate 100 studies published in high-impact psychology journals in 2008 and has played a large role in generating concerns about low replicability in social science? Bartoš and Schimmack (2020b) constructed a z-curve on the original studies that were subjected to replication in the OSC, recovering an Observed Discovery Rate of .94 (95% CI [.87, .98]), an Expected Discovery Rate of .39 (95% CI [.07, .70]), and an Expected Replication Rate of .62 (95% CI [.46, .75]). The responsible product design literature has a greater tolerance for null results (or tests more true negatives), a slightly lower expected discovery rate (though the confidence intervals have considerable overlap), and a similar expected replicability rate. These observations suggest that the responsible product design gambling literature is less insistent on the inclusion of significant findings than publications in eminent psychology journals were in 2008 but is not more insistent on replicable findings.

An exploratory analysis of z-curve within game-based intervention type did not lead to greater clarity. In fact, the z-curve would only run for studies of pop-up messages and information aids due to there being too few significant effects for other types of interventions. For pop-up messages, the results were very similar to the overall dataset. For information aids, the extant studies had a slightly lower Observed Discovery Rate, a lower Expected Discovery

Rate, but very similar confidence interval, and a slightly lower Expected Replicability Rate. Hence, our appraisal of the replicability of responsible product design appears to generalize across intervention types, though this could be an artifact of pop-up messages making up the majority of interventions studied.

Effect size magnitude also can speak to replicability, as studies of large effects (all else equal) have more statistical power, but very large observed effects can be symptomatic of infidelities in the research process. In the responsible product design literature, researchers most often report only standardized effect sizes. We recommend that researchers prioritize unstandardized effect sizes because they typically frame research questions in unstandardized terms (Pek & Flora, 2018), such as whether a certain tool will reduce money or time spent gambling. Even dependent variables that use rating scales, such as problem gambling screeners, can be imbued with meaning because they often have validated thresholds based on harm severity (e.g., Currie, Hodgins, & Casey, 2013). Standardized effect sizes are appealing in large part because they put effects composed of different variables on the same metric. Nevertheless, standardized effect sizes do not directly illuminate the relative importance of different predictors because they are influenced by the variance of the predictors and the outcome (Lenth, 2001; Pek & Flora, 2018).

About one third of studies did not report any effect sizes. Journals could incentivize effect size reporting by insisting that researchers conduct power analyses. Few studies incorporated a power analysis, and about half of those that did reported a post-hoc power analysis, which is redundant with the p -value (Lenth, 2001). All included studies that did conduct an *a priori* power analysis used conventional benchmarks for what constitutes a medium or large effect (Cohen, 1992). Basing sample size on effect size conventions is dubious because

it ignores the influence of measurement error (Lenth, 2001). Furthermore, small standardized effects may have large practical effects in the long-term or when scaled to a large population (Funder & Ozer, 2019). That said, we concur that power analysis should be based on the minimum effect size necessary to justify implementation. The smallest effect size of interest could be defined by the smallest change in the dependent variable that causes gamblers to report that they are experiencing less harm (Anvari & Lakens, 2019).

A lack of transparency undermines the benefit of adopting practices that increase replicability. Pre-registration limits obfuscation of which analyses were planned versus exploratory (Nosek et al., 2019). We found that only a handful of studies in the responsible product design literature were pre-registered, and all of them were published in 2019 or 2020. These findings are consistent with trends in psychological science, where uptake of pre-registration between 2014 and 2017 was rare (Hardwicke, Thibault, Kosie, Wallach, Kidwell, & Ioannidis, 2019).

Similarly, researchers must become more routine about disclosing their potential conflicts of interests and funding sources. About half of the included articles did not include a conflict of interest statement. This might be because no such conflicts exist, but an explicit statement reinforces a norm of transparency and neutralizes concerns about impartiality.

Study Limitations

The primary limitation of the present scoping review is its scope. Official reports and internal government studies on product safety in gambling were not included. We also did not include unpublished studies, some of which may have been high quality. However, this method follows that of previous scoping reviews (e.g., Gray et al., 2020), and is grounded in the conservative approach of keeping the review limited to research that has undergone some form

of scrutiny (i.e., independent peer review). Second, the keywords and inclusion criteria may have led us to exclude or overlook studies that would have changed our conclusions about efficacy or replicability. Last, we could not include all eligible studies in z-curve because they did not include an inferential test of their most focal hypothesis.

Conclusion

The responsible product design literature has several reassuring trends, such as widespread use of experimental methods and behavioral measures. But uncertainty about the literature's overall methodological rigor, replicability, and transparency precludes any strong recommendations about which interventions stakeholders should promote and implement. Ignoring these important factors, currently the product safety literature provides the best evidence, albeit limited evidence for pop-ups with self-appraisal messaging, breaks in between rounds of play, precommitment to less risky bets, undoing EGM features that promote excessive gambling, providing recommendations that minimize house edge, and removing bank note acceptors. Because the literature remains premature, we do not think that a meta-analysis on the responsible product design literature would settle this matter. For scientific and policy, confident evidence-based product safety recommendations will remain elusive until there are a much larger number of high-powered, transparently reported studies.

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