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Treatment effects of therapeutic interventions for gaming disorder: A systematic review and meta-analysis

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ABSTRACT

The prevalence of gaming disorder is assumed to be between 2%−5%. The treatment effect of different therapeutic interventions of gaming disorder has not been studied extensively. This systematic review and meta-analysis sought to identify all intervention studies on gaming disorder with a control group, determine the effect of the interventions, and examine moderators. Studies applying a therapeutic intervention and using an appropriate comparison group were identified by searching electronic databases, previous reviews, and reference lists. Data on type of treatment, name of outcome measurement, symptom level and other study characteristics were extracted and analyzed using meta-analysis and meta-regression. A total of 38 studies and 76 effect sizes, originating from 9524 participants were included. RoB2 and ROBINS-I risk of bias tools were used to assess within-study risk of bias. Correlational hierarchical models with robust variance estimation were fitted to effect size data and yielded a moderate summary estimate. Egger's sandwich test, funnel plot inspections, and other tests were conducted to assess risk of bias between studies. Results indicate that there may be an overall effect of therapeutic interventions for gaming disorder, but confidence in these findings is compromised by small-study effects, possible publication bias, a limited study pool, and a lack of standardization. The field needs more higher quality studies before the evidence-base can support reliable meta-analytic estimates.

1. Introduction

Recently, observations of gaming behavior resulting in significant distress or impairment for the individual have been reported (Petry et al., 2014; Saunders et al., 2017). In 2013, the American Psychiatric Association announced internet gaming disorder as a new diagnosis to be included in the fifth edition of the Diagnostic and Statistical Manual of Mental Disorders (American Psychiatric Association, 2013). The 11th edition of the International Classification of Diseases included gaming disorder as a diagnosis as of 2022 with a similar definition (World Health Organization, 2019). The literature uses both internet gaming disorder and gaming disorder to denote the phenomenon, with an ongoing debate about consensus of the concept. Both diagnoses include both on-line and off-line gaming.

Excessive gaming behavior has been associated with several health issues, such as eye problems (Gillespie, 2002), musculoskeletal problems (Zapata et al., 2006), tendinosis (Macgregor, 2000), increased body mass (Ballard et al., 2009), high blood pressure in overweight and obese adolescents (Goldfield et al., 2011) and depression (Brunborg et al., 2014). Global prevalence of gaming disorder has been estimated to around 3 % (Anthony et al., 2022; Stevens et al., 2021), although corrections for biases may lower these estimates (Kim et al., 2022). On the other hand, prevalence may be higher for some populations (Chiang et al., 2022). A meta-analysis by Stevens et al. (2019) examined the treatment effects of cognitive-behavioral therapy interventions on patients with internet gaming disorder and found a substantial treatment effect overall. Several systematic reviews looking at treatment effects on adolescents (Gentile et al., 2017; Paulus et al., 2018) and in general (King & Delfabbro, 2014; King et al., 2017; Zajac et al., 2020) found similar results. However, no meta-analysis has been conducted on treatment effects of other interventions besides cognitive-behavioral therapy approaches.

1.1. Characteristics of gaming disorder

Gaming disorder is observed mainly in young male adults and

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adolescents (Kim et al., 2022). The symptomatology of gaming disorder shares some core features with other addiction disorders such as alcohol use disorder (Karim & Chaudhri, 2012; Na, Lee, et al., 2017). Among them are loss of control and compulsive engagement in the substance or behavior. While most clinical studies have been conducted in Asian countries like South Korea and China (Costa & Kuss, 2019), gaming disorder is a global phenomenon. Costa and Kuss (2019) found that diagnostic criteria varied among studies. While most studies used severe impairments such as jeopardizing work, education, or relationships as a criterion, other criteria varied between studies. Some used DSM-5 criteria, some DSM-IV-TR, and some added game time as a criterion. Thus, in the research literature, there seems to be a lack of general consensus on how to identify patients with gaming disorder, despite the existence of established criteria in both ICD-11 and DSM-5.

First-person shooter (FPS) and Massive-Multiplayer Online Role-Playing Game (MMORPG) players more frequently meet the criteria for internet gaming disorder, indicating that the type of game genre could require different therapeutic approaches (Na, Choi, et al., 2017). Altered neurobiological structures could explain internet gaming disorder (Şalvarlı & Griffiths, 2022). Weinstein and Lejoyeux (2022) concludes that patients with internet gaming disorder showed less grey-matter volume and white-matter density, reward deficits and impaired inhibition. Implications for therapeutic outcomes are, however, unclear.

1.2. Therapeutic interventions for gaming disorder

Research literature on therapeutic interventions for gaming disorder is not yet very sizeable, but nevertheless varied. Studies of cognitive-behavioral therapy interventions seem to be the most numerous (King et al., 2017; Zajac et al., 2020). Pharmacological interventions have been studied with two RCT studies on escitalopram and bupropion (Zajac et al., 2020). Other innovative therapeutic treatment approaches have been attempted and studied, such as transcranial stimulation (Jeong et al., 2020), treatment camps (Sakuma et al., 2017) and family therapy (Nielsen et al., 2021).

Both reviews by King et al. (2017) and Zajac et al. (2020) conclude that the current issues in the field of treatment of gaming disorder is a consensus of the construct, as well as a need for well-designed treatment studies. Most of the studies investigating cognitive-behavioral therapy have a combination of small sample sizes, active control groups, or combining cognitive-behavioral therapy with medications. The two RCT studies on psychopharmacological interventions had less than 50 participants in each group. Currently, no attempt has been made to investigate variation in treatment effect among different therapeutic interventions using state-of-the-art meta-analytic methods.

1.3. Measurement of gaming disorder

A wide range of measurement tools for assessing internet gaming disorder and gaming disorder has been developed, leading to a lack of standardization and diverse psychometric qualities (King et al., 2013). Self-report questionnaires are most common, with alternatives ranging from brain imaging (Meng et al., 2015) to parental reports (Wartberg et al., 2019). One of the first used assessment tools (Moon et al., 2018) is Young's Internet Addiction Test, but this seems to have been used by a relatively small proportion overall. A newly updated review by King et al. (2020) indicated that 23 other measurement tools across several hundreds of studies have been used. Add to this the fact that the concept of gaming disorder itself is construed differently in different studies, uncertainty of results increases even further. Since it seems quite possible that effects sizes in intervention studies could vary as a function of the type of measurement used, an attempt to assess whether this is the case seems warranted.

1.4. Objectives of the present study

The aim of this systematic review and meta-analysis is to evaluate the effectiveness of different therapeutic interventions of both adolescent and adult participants with gaming disorder. The proposed systematic review and meta-analysis will attempt to answer the following questions:

Do therapeutic interventions decrease symptoms in patients with gaming disorder?
Does effectiveness of different therapeutic interventions vary in patients with gaming disorder?
Does the effect size vary by how the outcome is measured?

2. Method

The effects of different therapeutic interventions on gaming disorder were evaluated through a systematic review and meta-analysis. This report follows the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines (Page et al., 2021).

2.1. Protocol and registration

A protocol (CRD42022338931) was submitted prior to the study to the Prospective Register of Systematic Reviews (PROSPERO) in August 2022. Deviations from the protocol were as follows; 1) studies of preventive interventions were included, and 2) some changes to the assessment of publication bias, sensitivity analyses and additional analyses were made after seeking advice from Dr. Pustejovsky and comments from reviewers.

2.2. Eligibility criteria

2.2.1. Participants

Studies with participants within the age range of 10 to 65 years, diagnosed internet gaming disorder or gaming disorder only or having comorbid psychiatric illnesses were included. Participants not diagnosed with gaming disorder, but being measured with appropriate measurements for the condition were included. This is justified by an acceptable inter-validity between gaming disorder and internet gaming disorder (Jo et al., 2019). Studies with samples with comorbid addiction conditions were excluded. Participants characterized or diagnosed with Internet Addiction were included if the gaming disorder participants were separated from subjects with other sub-groups of internet addiction.

2.2.2. Interventions

Studies with a psychological, behavioral, pharmacological, or medical intervention targeting gaming disorder were eligible. Interventions indirectly treating gaming disorder, such as parental guidance to manage children's gaming behavior, were also included. No restrictions on the setting for the intervention were applied.

2.2.3. Comparison groups

All studies with a control group were included. In the interest of homogeneity, a passive control group, such as a no treatment, waiting list, sham, placebo or treatment as usual (TAU), would be preferable, but control groups exposed to a different type of treatment were also included in order to increase the sample of studies. Studies using the same sample as control group with repeated measures, those using healthy participants or a different kind of clinical population (e.g., ADHD, Depression) as a control group were excluded.

2.2.4. Outcome measurement

Studies using a measure of gaming disorder or internet gaming disorder based on the diagnostic criteria of DSM-5 or ICD-11 were included

(e.g., GASA, IGD9-SF, CGAS). Studies using a measurement of the more general condition internet addiction (e.g., IAT, YIAS) were included if the measurement was adapted to gaming, or participants were screened for internet gaming disorder / gaming disorder and time on internet was pre-dominantly spent on gaming. Studies only measuring symptoms associated with gaming disorder (e.g., time spent, craving, impulsivity) were excluded. Other secondary outcomes were not coded nor considered eligible.

2.3. Search strategy

We searched the electronic databases PsycINFO, MEDLINE, EMBASE (all on Ovid), CINAHL (on Ebsco Host), Web of Science, Scopus, BASE (Bielefeldt Academic Search Engine), and the Cochrane Library, with no restrictions on dates. The last search in the electronic databases was conducted on august 13th 2022. The methodological trial filters used in these searches were all adaptations of the three versions of the filters for all clinical trials provided by Canada's Drug and Health Technology Agency (CADTH, 2022). Search results were uploaded to a reference manager for deduplication. The complete search strategies for all the electronic databases are provided in Appendix A in the OSF repository linked to in the Data availability, Code, and other Supplementary Materials section at the end of the paper.

Manual searches were conducted by tracing references in selected previous reviews. These reviews were identified via searches on Google Scholar and PsycINFO, from the pool of results from the search for eligible studies in electronic databases, and from reference lists of relevant studies. The complete list of the ten review articles with description of potential eligible studies is provided in Appendix B in the OSF repository linked to under Data availability, Code, and other Supplementary Materials.

2.4. Data management

2.4.1. Selection

Study selection was performed according to recommendations of Polanin et al. (2019). The deduplicated search results were uploaded to Abstrackr (Wallace et al., 2012) for screening. An abstract screening tool was designed and tested by two of the authors (PASD and TL) independently on a random sample of 5 % of the search results (n = 143). The screening tool was adjusted after discussing the results of the pilot screening. The same authors continued to screen independently and met 3 times to reach consensus on divergent screening decisions and prevent drift.

Full texts for the complete list of screened articles were retrieved using the in-built function in reference management tool or accessed manually. The corresponding author was contacted where data was missing, the study trial was not yet completed, or the full report not published. The abstract screening tool and the full list of excluded articles with notes is found in Appendix C in the OSF repository linked to under Data availability, Code, and other Supplementary Materials.

2.4.2. Extraction

Two authors (PASD and TL) independently coded on a test sample of seven studies. Comparison of the coding was done, and on this basis, a code sheet with instructions was designed with a data input form in Excel. The data input form was coded with visual basic in Excel to reduce risk of error during coding, reduce variance between coders, and to make coding more efficient (Li et al., 2022). Effect size calculations were based on unadjusted means and standard deviations from post-tests, even if the design included baseline measures. If means and standard deviations were not provided, effect sizes were computed from other statistics, if given (e.g., test statistics from mean comparisons), or requested from authors. Effect sizes were calculated with an effect size calculator from the Campbell collaboration (Wilson, 2022), implementing the formulas given in Lipsey and Wilson's (2001) book. Data from other languages than English, German or Scandinavian were extracted with Google translate. The complete final code sheet, including details on effect size calculations, is found as Appendix D in the OSF repository linked to under Data availability, Code, and other Supplementary Materials.

2.4.3. Data items

The data extracted were categorized into two categories, paper description and data description. For paper description, items such as short citation and publication year was coded. For data description, items such as effect size and demographics of participants were coded. Data items were extracted and coded according to the protocol, with a few exceptions. Title was not coded into the coding sheet but rather stored in the reference management tool. The setting of the study, level/mode of intervention and reviewer conclusion was not coded into the final data sheet.

2.5. Risk of bias assessment

2.5.1. Within studies

Risk of bias was assessed according to guidelines in the Cochrane Handbook of Systematic Reviews (Higgins et al., 2019). The effect of interest was the intention-to-treat (ITT). The tools used for assessment was RoB2 (Sterne et al., 2019) and ROBINS-I (Sterne et al., 2016) for RCT and nRCT studies, respectively. For RCT studies, the domains of interest were 1) bias arising from the randomization process; 2) bias due to deviations from intended interventions; 3) bias due to missing outcome data; 4) bias in measurement of the outcome; and 5) bias in selection of the reported result. For nRCT studies, the domains of interest were 1) Bias due to confounding, 2) Bias in selection of participants into the study, 3) Bias in classification of interventions, 4) Bias due to deviations from intended interventions, 5) Bias due to missing data, 6) Bias in measurement of outcomes, and 7) Bias in selection of the reported result. The risk of bias judgments for each domain and an overall judgement are illustrated with 'traffic light' plots and bar plots, respectively. The code sheet for RoB2 and the coding sheets for each study with ROBINS-I is found in the folder "Analysis" in the OSF repository, linked to under the Data availability, Code, and other Supplementary Materials section.

2.5.2. Between studies

To detect and adjust for possible publication bias and small-study bias, several different methods were used in a sensitivity analysis approach (Vevea et al., 2019). Contour-enhanced funnel plots were visually inspected, and asymmetry judgements supported by Egger Sandwich Test using the RMLA estimator (Rodgers & Pustejovsky, 2021). A fixed-random trim and fill (Duval, 2005), using the L0 estimator was performed on two different data sets. The first included all the effect sizes from every study, treating them as independent when in fact they are not. The second included one effect size from each study, aggregating within a study. A PET-PEESE analysis was also conducted following the tutorial by Bartoš et al. (2022) and by using the regtest() function within 'metafor', setting the model to "lm" (Sterne & Egger, 2005) and the predictor to standard errors and standard errors squared for PET and PEESE, respectively.

Additionally, we ran an intercept only model on effect sizes from studies with approximate statistical power of at least 80 % to detect a 0.4 standardized mean difference, by filtering out effect sizes from studies with less than 200 participants total.

To include some tests that were not asymmetry based, we ran a 3-parameter selection model (Hedges & Vevea, 2005) on the dataset containing one effect size from each included study, with these being aggregated in the case of studies providing more than one. Also, a test of excess significance was conducted with the function tes() from 'metafor' R package fitted specifically to meta-analyses. This test evaluates whether the observed number of statistically significant effects in a body

of evidence is too high compared to their expected number, as estimated from the power of included studies and an assumed population effect size (Ioannidis & Trikalinos, 2007).

2.6. Analysis and synthesis

The methodology was guided by the Cochrane handbook of systematic reviews, the methodological guidance paper by Pigott and Polanin (2020), and *Doing meta-analysis with R: A hands-on guide* (Harrer et al., 2021). All statistical analyses were conducted using RStudio 4.2.1 (RStudioTeam, 2020) with the R packages *metafor* (Viechtbauer, 2010), *metameta* (Quintana, 2022) and *clubSandwich* (Pustejovsky, 2022). The correlated hierarchical effects (CHE) working model without additional levels was selected based on the flow chart provided by Pustejovsky and Tipton (2022). Using robust variance estimation (RVE) with CHE allows us to examine relationships between variables while considering the hierarchical nature of the data, i.e., more than one effect size from each study. RVE provides more reliable estimates by accounting for potential violations of assumptions, i.e., independence between effect sizes. An intercept-only model and a model for each of the two main individual moderators were fitted to the data. Both the between-study (τ^2) and within-study (ω) variance estimates are provided to describe heterogeneity of the effect sizes using variance decomposition. The result of the meta-analysis is presented in a forest plot and tables. Each of the included studies are presented in a table with a selection of data items.

The main moderators of interest were according to our protocol in prioritized order: 1) type of intervention and 2) type of outcome measure. 3) The study characteristics weeks of follow up and male percentage were used as control variables. The moderating data items were clustered to achieve higher statistical power and a more reasonable comparison of different effect sizes in preparing the dataset. We grouped the type of intervention and name of outcomes into a higher-order categorization, e.g., any intervention which had components of typical talk-therapies were grouped into “psychotherapy”.

2.7. Additional analyses

A meta-regression model including the moderators and the control variables were fitted to the effect size data. Sensitivity analyses for various magnitudes of the assumed correlations between effects sizes from the same study were performed on the intercept-only model by varying this parameter between 0.0 and 0.95 with 0.05 steps. A power analysis of the individual studies was visualized in a sunset plot.

3. Results

Nine studies were excluded due to no appropriate outcome measurement (Afriwilda & Mulawarman, 2021; Babic et al., 2015; Delpazirian, 2017; Drks, 2012; Lu-lu et al., 2021; Nct, 2016, 2018, 2019; Wu et al., 2020). 17 studies were excluded due to no appropriate control group (Chang et al., 2020; González-Bueso et al., 2018; Han et al., 2010; Han et al., 2012, 2018; Liu et al., 2018; Park et al., 2017, 2018; Yao et al., 2017; Young, 2013) or no control group at all (Han et al., 2009; Mannikko et al., 2022; Pallesen et al., 2015; Sakuma et al., 2017; Szasz-Janocha et al., 2020; Thana-Ariyapaisan et al., 2018). 23 studies were excluded because participants were primarily diagnosed with internet addiction (Jeong, 2012; Liu et al., 2015; Mun & Lee, 2015; Orzack et al., 2006; Park et al., 2014; Santos et al., 2016; Shek et al., 2009; Shin et al., 2015; Su et al., 2011; Wölfling et al., 2014; Yang et al., 2017; Zhu et al., 2012) or another sub-group of internet addiction (Bong et al., 2021; Lee et al., 2016; Twohig & Crosby, 2010). Eight out of the 23 studies did not have a specific outcome measurement for internet gaming disorder or gaming disorder to justify inclusion (Bai & Fan, 1991; Bipeta et al., 2015; Cao et al., 2007; Dell’Osso et al., 2008; Du et al., 2010; Fu & Liu, 2016; Hui et al., 2017; Kim, 2008). Ten studies were either non-clinical papers (Huang et al., 2010; jRCTs, 2021; Lee et al., 2014; Nielsen & Rigter, 2018; Poddar et al., 2015; Thorens et al., 2014; Van Rooij et al., 2012), or meta-analyses on internet addiction (Liu et al., 2017; Winkler et al., 2013; Yeun & Han, 2016).

Two studies were case-reports (Torres-Rodríguez et al., 2019; Vasiliu & Vasile, 2017). Three otherwise eligible studies were excluded because it was not possible to extract effect sizes from the papers, nor were they obtained by contacting corresponding authors (Li & Wang, 2013; Torres-Rodríguez et al., 2018; Young, 2013). For two studies, pertinent statistics not reported in the papers were supplied by corresponding authors, one by calculating the outcome measure at post-intervention only (Evans et al., 2018) and the other by descriptive statistics for both intervention and control groups for all timepoints (Walther et al., 2014). The process of study selection is illustrated in Fig. 1. The full list of the 38 included studies with study characteristics is presented in Table 1. A narrative synthesis of study characteristics is presented in the following paragraphs.

3.1. Study characteristics

Out of the included studies, most of them were RCTs ($k = 33$). Five studies were nRCTs. A few studies were written in a non-English language ($k = 4$); two studies in Korean, one study in Chinese, and one study in Spanish. No studies reported conflicts of interest, and all of them were peer-reviewed. For the full coding sheet with all data items, see the data sheet “Data” in Appendix D, linked to in Data availability, Code, and other Supplementary Materials.

3.2. Participants

Of the total number of participants (N = 9524), 5223 participants were in treatment and 4301 in control. The mean number of participants in a unique treatment per study was 113, group sizes varying from 12 to 931. For control, the average number was 113 and group sizes varying between 10 and 1221. The mean age of treatment and control ranged from 9.77 to 26.21 and from 9.97 to 27.80, respectively. The range of male percentage across studies for each treatment–control group pair was 38.2 % to 100 %, where k = 14 studies had males only. Due to a lot of missing data and variations in reporting for both level of education and income level across studies, these statistics are not reported in this paper.

3.3. Intervention

The most frequent type of intervention was behavioral (k = 12), which consisted of abstinence, craving behavioral intervention (CBI), or a kind of response inhibition training using computer tasks. Eight studies used psychotherapy, where most of them were a variety of cognitive-behavioral therapy (k = 5). Other types of treatments were pharmacological (k = 5), prevention programs (k = 3) or school-based prevention programs (k = 5), physical exercise (k = 3), and transcranial direct current stimulation (tDCS) (k = 2). Remaining studies had a unique kind of treatment either designed by the study authors or a modified type of treatment of those mentioned above (See Table 3).

3.4. Comparison group

The comparison groups were mostly a no treatment group or participants on a waiting list. For pharmacological, brain stimulation and some of the behavioral type of interventions, a sham or placebo group was used. Five studies used a treatment-as-usual as control. Five studies used another kind of treatment as control which was not categorized as TAU by the authors. These active controls were bupropion (Kim et al., 2012), escitalopram (Nam et al., 2017), atomoxetine (Park, Lee, et al., 2016), CBT (Park, Kim, et al., 2016) and physical exercise (Lee & Son, 2008).

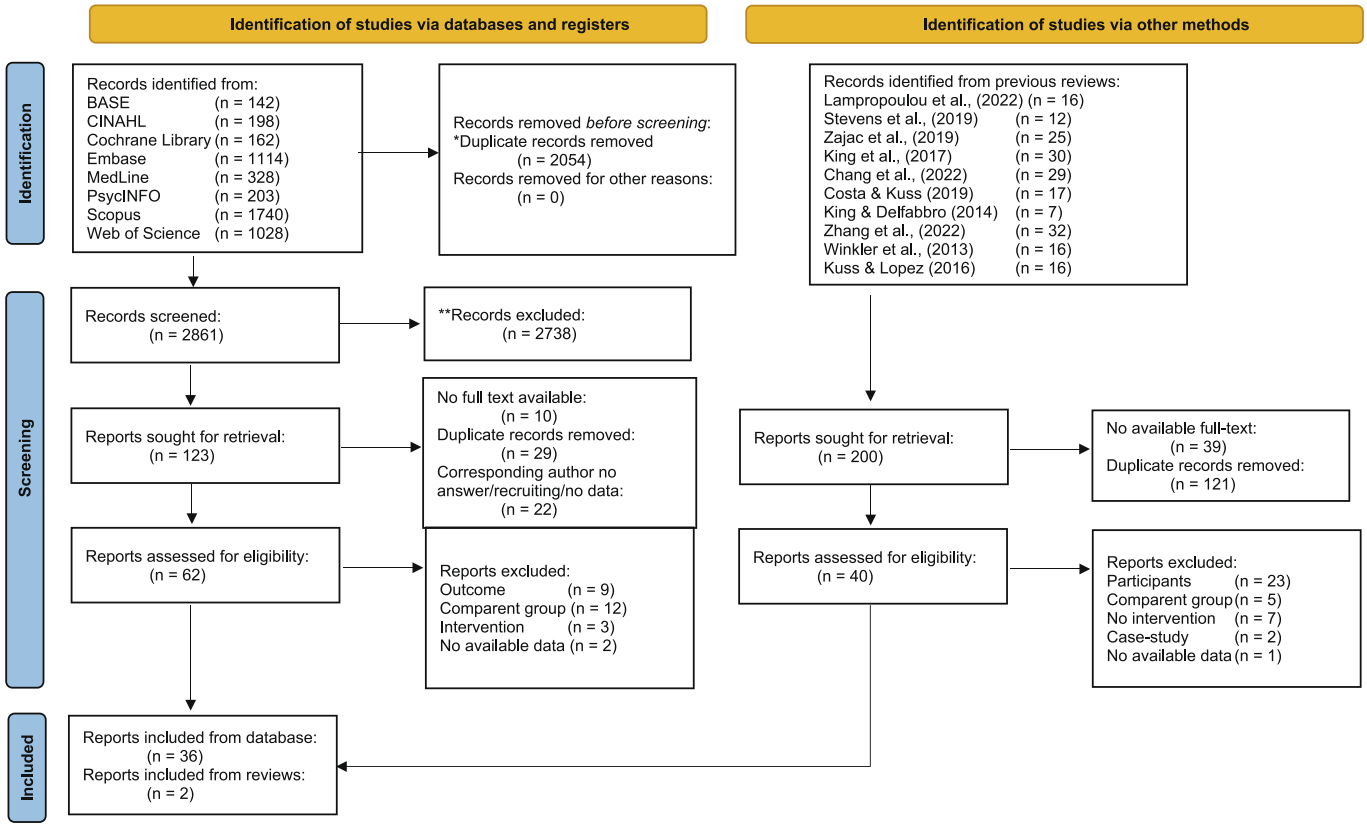

Fig. 1. Flow of studies diagram. *Records de-duplicated manually. **All manually screened independently by the first and third authors (PAD and TL).

3.5. Outcome

All studies used self-report questionnaires to assess the outcome of gaming disorder, except for one study which used parental reports (Krossbakken et al., 2018). The most frequent tool was Young's Internet Addiction Scale, used by eight studies. Five studies used IGDS9-SF (Pontes et al., 2014), three studies used Internet Addiction Test, two studies used Chen Internet Addiction Scale, and two used Gaming Addiction Screening Test. The rest of the studies used a unique measurement not used by any other study, though still being a type of self-report GD assessment (See Table 4).

3.6. Risk of bias within studies

Risk of bias within studies was assessed for the outcome of gaming disorder. An equal weight was applied to all assessments. The Intention-To-Treat was assumed for all studies. The overall risk of bias for RCT and nRCT studies are illustrated as a bar chart in Figs. 2a and 2b, respectively. An overview of individual studies risk of bias judgements for each domain are illustrated in traffic light plots in Fig. 3a for RCT and Fig. 3b for nRCT. The plots were made with the online tool robvis (McGuinness & Higgins, 2021).

Out of the RoB2 assessments, only two studies were judged to have a low risk of bias (Li et al., 2018; Wölfling et al., 2019). Most studies received the "some concern" level of risk of bias, and five studies received an overall "high" risk of bias judgement. The main contributing domain in percentile was in domain 2 with a lack of single and double blinding for most studies. Most studies had self-report questionnaires as outcome measure increasing the risk of bias in domain 4. Only a few studies had a pre-registration or clinical trial registration available, either by search or mentioned in the paper, impacting domain 3 and 5. Most studies also lacked a detailed description of the randomization process.

Of the ROBINS-I assessment, only one study received a moderate overall risk of bias judgement (Deng et al., 2017). Three studies had a serious overall risk of bias (Han et al., 2020; Ortega-Barón et al., 2021; Pornnoppadol et al., 2020) and one study at critical risk of bias (Zhang et al., 2016). The reasoning behind judgements was similar to the RCT studies. A lack of preregistrations and of information about measurement of confounding variables were main concerns. The similarity between treatment and control groups in terms of sampling size and demographics did not indicate a bias in selection of participants.

3.7. Results of individual studies and synthesis

A total of 38 studies reporting 76 effect sizes ranging between 1 and 9 effect sizes and a median of 2 effect sizes per study were synthesized. The overall pooled treatment effect size across the variety of treatments on gaming disorder was estimated to 0.63 (95 % CI [0.43, 0.83], $p < .001$). The 95 % prediction interval estimate indicates a single observation is somewhere between −0.555 and 1.815. The level of heterogeneity was significant, tau = 0.576, $p < .001$. The I^2 was at 94.03 %, where 81.29 % was between-study heterogeneity and 12.74 % was within-study heterogeneity, meaning the biggest proportion of the variance is explained by difference between the studies. The Q-statistic of heterogeneity was significant, $Q(75) = 516.51$, $p < .001$, indicating a heterogeneous study

Table 1
Included studies with description of authors, male percentage, outcome measurement, and number of participants, mean age and treatment name for the intervention and control group.

Study	Treatment			Control			% Male	Outcome
	N	Age	Treatment. [*Group*]	N	Age	Treatment		
Apisitwasana et al. (2018)	151	9,8	Participatory-learning and family-based intervention program for preventing game addiction by developing self-regulation. [*Prevention*]	159	10,1	No treatment	53,6	GAST
Bonnaire et al. (2019)	228	–	single session prevention intervention. [*Prevention*]	209	–	No treatment	–	GAS
Brailovskaia et al. (2022)	143	26,2	Abstinence from gaming for 14 days. [*Behavioral*]	149	25,1	Control group	71,6	IGD-scale
Deng et al. (2017)	44	21,9	CBI. [*Behavioral*]	19	22,1	Waiting list	100,0	POGUS
Evans et al. (2018)	19	14,3	Abstinence/withdrawal. [*Behavioral*]	18	15,2	No treatment	91,9	IGD criteria checklist
Han and Renshaw (2012)	29	21,2	Bupropion + education. [*Other*]	28	19,1	Placebo + education	100,0	YIAS
Han et al. (2020)	101	25,9	CBT. [*Psychotherapy*]	104	26,5	Supportive therapy	100,0	YIAS
Hong et al. (2020)	27	15,4	CBT + PE. [*Other*]	27	16,0	CBT + counseling	100,0	YIAS
Huang et al. (2010)	17	–	Interpersonal group counseling. [*Psychotherapy*]	10	–	No treatment	–	Computer Gaming Addiction Invention
Jeong et al. (2020)	13	22,2	tDCS. [*Other*]	13	23,2	Sham tDCS	57,7	IAT
Joo and Park (2010)	24	nr	Empowerment education program. [*Prevention*]	24	nr	No treatment	56,3	Internet addiction selfdiagnosis test
Kim et al. (2012)	35	16,2	CBT + bupropion. [*Other*]	37	15,9	Bupropion	100,0	YIAS
Kochuchakkalackal Kuriala and Reyes (2020)	20	16–19	ACRIP. [*Behavioral*]	20	1–19	No treatment	nr	IGDS9-SF
Krossbakken et al. (2018)	831	10,1	Parental educational program. [*Prevention*]	826	10,1	No treatment	nr	Video game problems (DSM-5)
Lee and Son (2008)	13	nr	CBT-group. [*Psychotherapy*]	16	nr	Sports excercise group	nr	Internet game addiction tool (YIAS-K)
Lee et al. (2021)	31	23,1	tDCS. [*Other*]	31	25,3	Sham tDCS	100,0	YIAT
Li et al. (2019)	163	10,2	Game over intervention. [*Prevention*]	199	10,0	Effective learning forchildren	61,9	K-IAT for Adolescents, modified to gaming
Li et al. (2018)	15	22,2	Mindfulness-Oriented Recovery Enhancement. [*psychotherapy*]	15	27,8	Social support	80,0	DSM-5 criteria
Lindenberg et al. (2022)	167	14,6	PROTECT CBT-group. [*Psychotherapy*]	255	15,4	No treatment	45,7	CSAS
Maden et al. (2022)	15	23,8	Virtual Reality-based Training, Aerobic Training. [*Other*]	15	22,2	No treatment	100,0	IGDS9-SF
Marco and Choliz (2017)	612	12,2	Traditional program, traditional program + impulse control. [*Prevention*]	471	12,3	Waiting list	46,3	Video game dependence test (TDV)
Mumcu et al. (2021)	40	12,6	School-based recreational exercise. [*Prevention*]	40	11,6	No excercise	100,0	Digital Game Addiction scale (DGA-SF)
Nam et al. (2017)	17	22,9	Bupropion + education. [*Other*]	17	23,9	Escitalopram + education	nr	YIAS
Nielsen et al. (2021)	12	14,9	Multidimensional family therapy. [*Other*]	30	14,9	Family therapy as usual	97,6	DSM-5 criteria
Ortega-Barón et al. (2021)	120	12,2	Safety.net. [*Prevention*]	45	11,9	No treatment	38,2	IGDS9-SF
Park, Lee et al. (2016)	44	16,9	MPH. [*Other*]	40	17,1	ATM	100,0	YIAS
Park, Kim et al. (2016)	12	24,2	CBT. [*Psychotherapy*]	12	23,6	VRT	100,0	YIAS
Pornnoppadol et al. (2020)	24	14,6	S-TRC, PMT-G, S-TRC + PMT-G. [*Other*]	30	14,3	Waiting list	75,0	GAST
Song et al. (2016)	44	20,0	Bupropion, Escitalopram. [*Other*]	36	19,6	No treatment	100,0	YIAS
Walther et al. (2014)	995	11,8	School-based media literacy program. [*Prevention*]	1308	12,1	No treatment	47,5	KFN-CSAS-II
Wang et al. (2022)	23	21,9	CBI. [*Behavioral*]	17	22,0	No treatment	100,0	CIAS
Wu et al. (2022)	45	20,6	EABM. [*Behavioral*]	45	20,6	EABM sham	77,8	IAT
Wölfling et al. (2019)	74	26,2	STICA (CBT). [Psychotherapy]	75	26,2	Waiting list	100,0	AICA Self-Report
Zamanian et al. (2020)	36	13,8	The theory of planned behavior. [*Behavioral*]	36	13,8	No treatment	na	Game dependecy
Zhang et al. (2016)	20	21,8	CBI. [*Behavioral*]	16	22,4	No treatment	100,0	CIAS
Zheng, He, Fan and Qiu (2022)	20	14,8	Approach Bias Modification training, Response inhibition Training group, RT + ApBM training. [*Behavioral*]	20	14,7	No treatment	100,0	OGAS
Zheng, He, Nie et al. (2022)	25	21,4	Abstinence. [*Behavioral*]	25	21,0	No treatment	nr	IAT, DSM-5 score

Note. ACRIP = Acceptance and Cognitive Restructuring Intervention Program, AICA = Assessment of Internet and Computer Game Addiction, ATM = Atomoxetine, CBI = Craving-Behavioral intervention, CBT = Cognitive-Behavioral Therapy, CIAS = Chen Internet Addiction Scale, EABM = Emotional Association Biases Modification, GAS = Game Addiction Scale, GAST = Game Addiction Screening Test, IAS = Internet Addiction Scale, IAT = Internet Addiction Test, IGDS9-SF = Internet Gaming Disorder Scale – short form, KFN-CSAS-II = Video Game Dependency Scale, K-IAT = Korean Internet Addiction Test, MPH = Methylphenidate, Nr = Not reported, OGAS = Online Game Addiction Scale, PE = Physical Exercise, PMT-G = Parent Management Training for Game Addiction, S-TRC = Siriraj Therapeutic Residential Camp, tDCS = transcranial Direct Current Stimulation, VRT = Virtual Reality Therapy, YIAS = Young's Internet Addiction Scale.

sample where each study might not be measuring the exact same effect size.

A Cook's distance diagnostic (Cook & Weisberg, 1982; Viechtbauer & Cheung, 2010) was run on the intercept model to check for outliers. By visually scanning the plot (Fig. 4), we detected and excluded the outlier which had an effect size of 6.30 (Kochuchakkalackal Kuriala & Reyes, 2020). A re-fitted intercept-only model was estimated to 0.56 (95 % CI [0.40, 0.71], p <.001), see Table 2. A forest plot with the new total of 37 studies and 75 effect sizes is provided in Fig. 5. The robust confidence intervals were 0.39 to 0.72. The 95 % prediction interval estimate

Table 2
Estimates with Robust 95% CI for Intercept, Treatment and Outcome from the CHE-Working Wodels.

Level	Groups	Est. [95 % CI]*	Within-study Variance	Between-study Variance	Meta-regression Est./β [95 % CI]**
Intercept			19.67 %	70.54 %	
		0.555 [0.392, 0.718]			0.285 [0.070, 0.499]
Treatment Type			22.85 %	66.70 %	
	Behavioral	0.546 [0.201, 0.890]			0.197 [-0.204, 0.597]
	Other	0.630 [0.326, 0.934]			0.242 [-0.141, 0.624]
	Prevention	0.398 [0.067, 0.729]			0.313 [0.083, 0.544]
	Psychotherapy	0.675 [0.153, 1.197]			0.179 [-0.403, 0.761]
	% Male				0.013 [0.004, 0.022]
	Follow-up				0.001 [-0.009, 0.012]
Outcome Measure			17.87 %	72.07 %	
	DSM-5	0.256 [-0.214, 0.726]			0.151 [-0.283, 0.584]
	IAT	0.493 [-0.229, 1.215]			−0,178 [-0.645, 0.288]
	Other	0.648 [0.372, 0.924]			0.373 [0.139, 0.608]
	YIAS	0.710 [0.369, 1.052]			−0.047 [-0.529, 0.434]
	% Male				0.015 [0.008, 0.022]
	Follow-up				0.000 [-0.010, 0.010]

Note. *37 studies and 75 effect sizes. ** 30 studies and 62 effect sizes using percentage male and follow-up weeks as control variables.

Table 3
Number of Studies and Effect Sizes for Each Type of Treatment Within Each of the Groups.

Group variable	Type of treatment	Studies	Effects
Behavioral	Behavioral	10	25
Psychotherapy	Psychotherapy	8	13
	Familiy therapy	1	2
Prevention	School-based prevention program	5	7
	Prevention	3	6
	Parental program	1	3
Other	Brain stimulation	2	2
	Pharmacological	4	6
	Therapeutic camp + Parental program	1	3
	Physical exercise	3	4
	Psychotherapy + Pharmacological	1	2
	Therapeutic camp	1	3

indicates a single observation is somewhere between −0.347 and 1.458. The level of heterogeneity was significant, tau = 0.438, p <.001. The I^2 was at 90.21 %, where 70.54 % was between-study heterogeneity and 19.67 % was within-study heterogeneity. The Q-statistic of heterogeneity was significant, $Q(74) = 455.45$, p <.001.

Table 4
Number of Studies and Effect Sizes for Each Type of Outcome Within Each of the Groups.

Grouping	Name of outcome measurement	Studies	Effects
IAT	IAT	3	5
	YIAT	1	1
	K-IAT	1	2
YIAS	YIAS	8	11
	YIAS-K	1	2
DSM-5	DSM-5 score	1	2
	Video game problems (DSM-5)	1	2
	DSM-5 criteria	1	2
	IGDS9-SF	5	9
	French version of Petry's 2014 IGD-scale	1	2
	IGD criteria checklist	1	1
Other	CIAS	2	2
	GAST	2	11
	OGAS	1	6
	CSAS	1	3
	POGUS	1	3
	DGA-SF	1	1
	Internet addiction self-diagnosis test	1	1
	number of addictive gamers	1	1
	AICA self-report	1	2
	Game dependency	1	2
	KFN-CSAS-II	1	2
	TDV	1	2
	Computer Gaming Addiction Invention	1	1

3.7.1. *Type of treatment*

Psychotherapy had the highest significant effect size, $g = 0.68$, 95 % CI [0.34, 1.01], p <.001. Behavioral ($g = 0.55$, 95 % CI [0.25, 0.84], p <.001), Prevention ($g = 0.40$, 95 % CI [0.15, 0.65], p <.01) and Other ($g = 0.63$, 95 % CI [0.37, 0.89], p <.001) were all significantly different from null in the naïve model. Furthermore, none of the robust confidence intervals overlapped the null effect. See Table 2. The robust Wald test indicates that we cannot rule out that the average effects are equal across the types of treatment categories, $F(17.4, 1) = 0.03$, p =.864.

3.7.2. *Outcome measurement tools*

Young's Internet Addiction Scale had the highest effect size estimate, $g = 0.71$, 95 % CI [0.38, 1.04], p <.001). Estimates for the Internet Addiction Test ($g = 0.49$, 95 % CI [0.12, 0.86], p <.01) and Other group of measurement ($g = 0.65$, 95 % CI [0.41, 0.89], p <.001) were significantly different from null in the naïve model. DSM-5 criteria as outcome measurement was non-significant, $g = 0.26$, 95 % CI [-0.04, 0.55], p <.1. The estimated robust confidence intervals for both DSM-5 and Internet Addiction Test overlapped the null-effect, see Table 2. The robust Wald test indicates that we cannot rule out that the average effects are equal across dependent variables, $F(1, 17.3) = 2.66$, p =.121.

3.8. *Risk of bias between studies*

The Egger Sandwich test for our intercept model was statistically significant, $t = 6.26$, p <.001, indicating a small-study effect or publication bias. The result confirms the visual interpretation of the contour-enhanced funnel plot (Fig. 6) as being clearly asymmetric, with a cluster of effect sizes from smaller studies on the lower right side of the observed effect, and the larger, more precise studies tending to be to the left of the average. For the first trim and fill analysis, including all effect sizes and ignoring their statistical dependency, the results revealed an estimated 20 missing studies on the left side, $SE = 5.632$. The random-effects model with L0 estimator ($k = 95$) revealed an adjusted effect size of $g = 0.37$, 95 % CI [0.234, 0.490], p <.001. The heterogeneity was large, $I^2 = 95.2$ %, $\tau^2 = 0.331$ ($SE = 0.059$). For the second analysis, using aggregated effect sizes, the results revealed an estimated 11

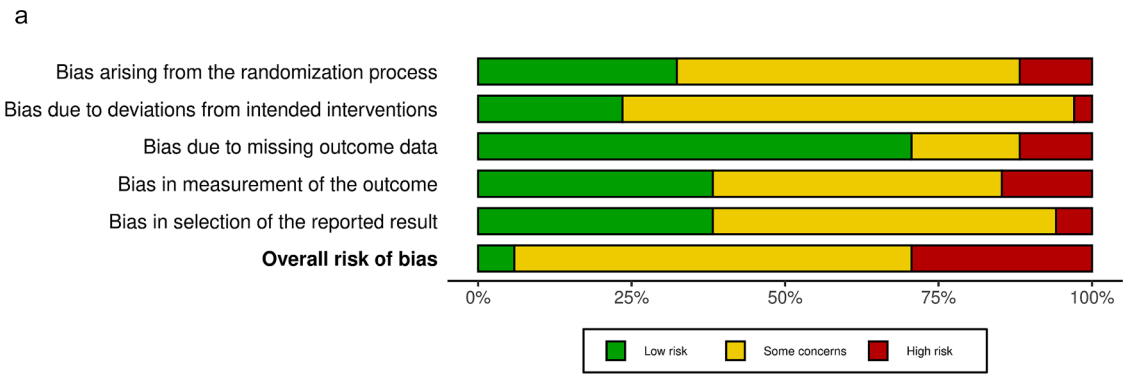

Fig. 2a. Overall judgements of RCT-studies using RoB2 plotted in a bar chart.

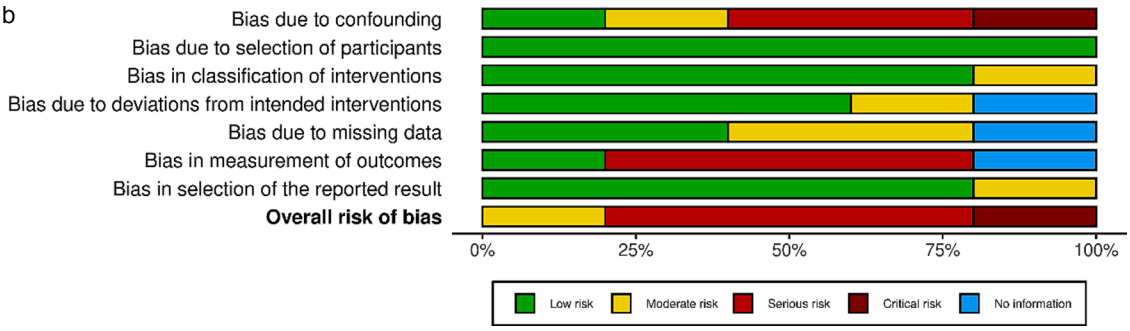

Fig. 2b. Overall judgements of nRCT-studies using ROBINS-I plotted in a bar chart.

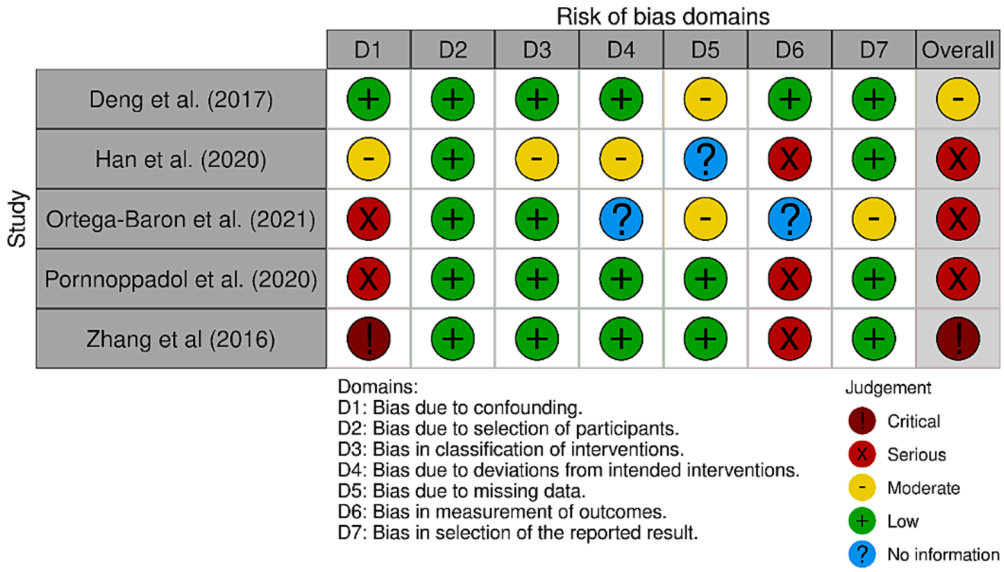

Fig. 3a. Risk of bias judgements of RCT studies for each domain.

missing studies on the left side, $SE = 3.947$. The random-effects model with L0 estimator ($k = 48$) revealed an adjusted effect size of $g = 0.33$, 95 % CI [0.160, 0.506], $p < .001$. The heterogeneity was large, $I^2 = 91.5$ %, $\tau^2 = 0.293$ ($SE = 0.076$).

As a part of the sensitivity analysis for publication bias, we repeated the intercept only model on effect sizes from studies with statistical power of at least 80 % to detect a 0.4 standardized mean difference, by filtering out effect sizes from studies with less than 200 participants total. This resulted in a summary estimate of $g = 0.215$, 95 % CI [0.04, 0.39], $p < .05$ for 19 effect sizes from 9 studies.

For the PET-PEESE analysis, the mean-effect-size estimate adjusted for correlation with standard errors from the PET model was non-significant, $\beta = 0.009$, 95 % CI [-0.11, 0.13], $p = .887$. For the PEESE model it was significant, $\beta = 0.15$, 95 % CI [0.06, 0.24], $p = .002$, but substantially lower than the naïve model estimate. Since the PET model using standard errors as predictor was non-significant, we choose the PET-model for interpretation. The likelihood test for the 3-parameter selection model was not quite significant ($\chi^2 = 2.78$, $p = 0.095$), and the model produced an adjusted summary estimate at 0.40. The test of excess significance observed 53 significant out of 75 total effect sizes, expected number at 22.59, giving a ratio of 2.34. The test was significant, $\chi^2 = 58.59$, $p < .001$. The theta limit estimate was 0.595.

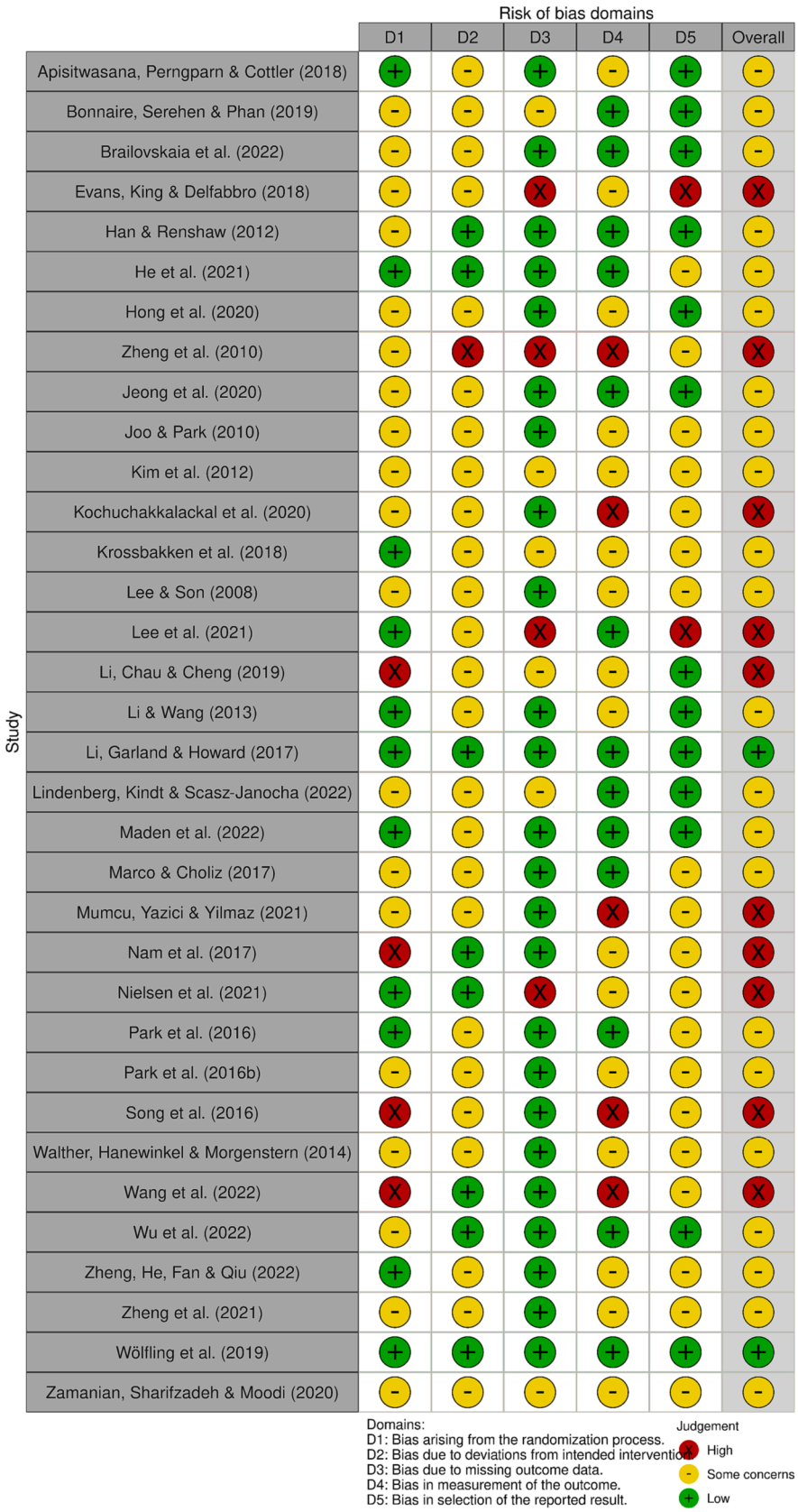

Fig. 3b. Risk of bias judgements of nRCT studies for each domain.

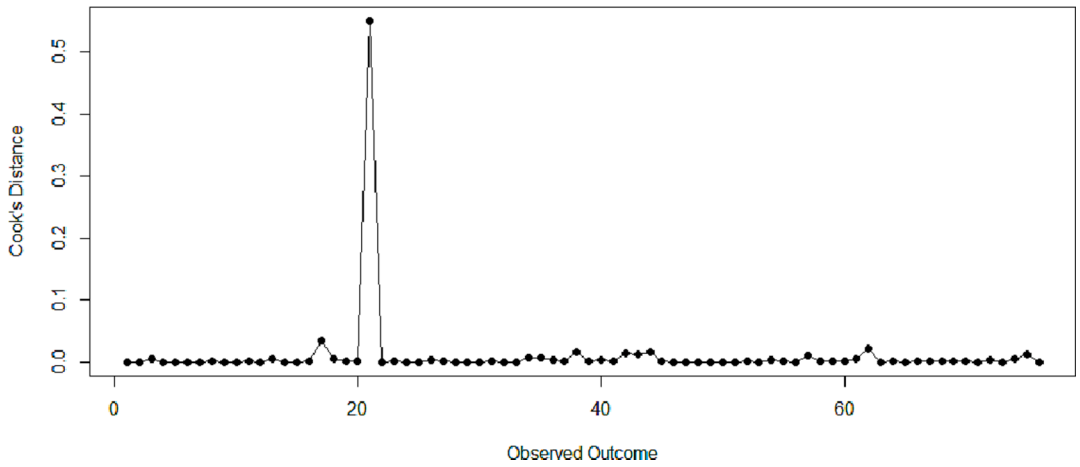

Fig. 4. Cooks distance diagnostic of effect sizes from included studies.

3.9. Additional analyses

Meta-regression models were fitted for each of the two main moderators, both with and without the centered control variables male percentage and follow-up in weeks. The control variables male percentage and follow-up in weeks was centered and added to the model. The number of effect sizes were reduced from 76 to 62 due to NAs in follow-up and percentage male. The number of studies was reduced from 37 to 30. For outcome measurements, only the Other group of outcome measurement was statistically significant with control variables included in the model, $g = 0.37$, 95 % CI [0.17, 0.58], $p < .001$. The robust CI was from 0.139 to 0.608. See Table 2. Percentage male and follow-up control variables are reported with beta-coefficients.

For type of treatment, only the prevention type of treatment was statistically significant with control variables included in the model, $g = 0.32$, 95 % CI [0.07, 0.56], $p < .05$. The robust CI was from 0.083 to 0.544. See Table 2.

A sensitivity analysis by adjusting the assumed correlation coefficient (rho) for the association between effect sizes within-studies was conducted, and the results plotted in Fig. 7. The effect size estimate differs with a non-correlated assumption from 0.568, to an effect size estimation of 0.547 assuming rho = 0.95.

An estimation of the statistical power of each study was conducted and visualized in a sunset plot (Fig. 8). A visual inspection indicates that half the studies reach less than 80 % statistical power, assuming the observed effect size estimate of 0.55.

Following a reviewer comment, an additional post-hoc analysis fitting a correlational hierarchical working model to the same dataset, but without k = 5 alternative types of control groups (i.e. pharmacological control groups). This yielded a summary estimate close to the original one, $g = 0.659$, 95 % CI [0.43, 0.89], $p < .001$.

4. Discussion

The main aims of our study were to investigate the effectiveness of therapeutic interventions on gaming disorder, if type of treatment mattered and if observed effect sizes varied according to how treatment outcome was measured. An overall moderate to strong effect size estimate (a standardized mean difference of 0.56) on average across a variety of therapeutic treatments for gaming disorder was found. Further investigations indicated that psychotherapeutic, behavioral, preventive, and other types of therapeutic treatments all had effects. There was a significant amount heterogeneity, where the majority of variance was found between studies.

Importantly, though, there were clear indications from publication and small study bias tests that these initial estimates are substantially inflated by effect sizes contributed by the smaller, imprecise studies. Adjusted estimates obtained in these analyses span a range from virtually zero to 0.40. Even taking these estimates with a large pinch of salt, they collectively suggest that a true weighted average effect size for this population of studies is substantially smaller than the naïve model estimate. Most of the included studies had a statistical power well below 80 %. Add to this the fact that most of them also had sources of bias within the study, this highlights the need for extreme caution in drawing conclusions from our analyses.

What seems clear from our study is that, while therapeutic interventions may decrease symptoms of gaming disorder (even the downward adjusted estimates are significantly different from a null effect), the field is characterized by small, statistically weak studies, whose validity is threatened by less than stringent (or not properly reported) randomization, incomplete reporting in general, and few preregistered protocols. Additionally, and notably, there is huge diversity in terms of how gaming disorder is measured. This raises concerns about the validity of meta-analytic estimates, just as it does for prevalence estimates (Anthony et al., 2022). All of these factors make it difficult to hold strong opinions about which treatment types are most effective and also what the outcomes of treatment can reasonably be expected to be. Thus, perhaps the only conclusion to be made is that the research base is not sufficiently mature at present to support a reliable meta-analytic evaluation of the effectiveness of therapeutic treatments for gaming disorder, and certainly not to allow reliable results from exploring sources of heterogeneity.

4.1. Implications for future research

Though the failure of clear and strong results to appear from our study is disappointing, we would nonetheless like to tentatively suggest a few potentially important implications. First, we suggest that the somewhat optimistic interpretations of previous meta-analysis results (Stevens et al., 2019; Winkler et al., 2013), to the effect that cognitive-behavioral therapies are highly effective in reducing internet gaming disorder symptoms short-term, should be reconsidered. These previous meta-analyses included studies without control groups, had relatively small samples of studies, did not adequately assess or adjust for small-study effects, and did not consider within-study risks of bias. A marked strength of our approach is that all these are accounted for. Our results do, therefore, lend further support to the more cautious observations of other reviewers, that the evidence base it too underdeveloped to reach firm conclusions, (Stevens et al., 2019, p. 200; Zajac et al., 2020, p. 92).

Second, it seems to us that in order for any substantial conclusions about interventions for gaming disorder to be reached, the field needs

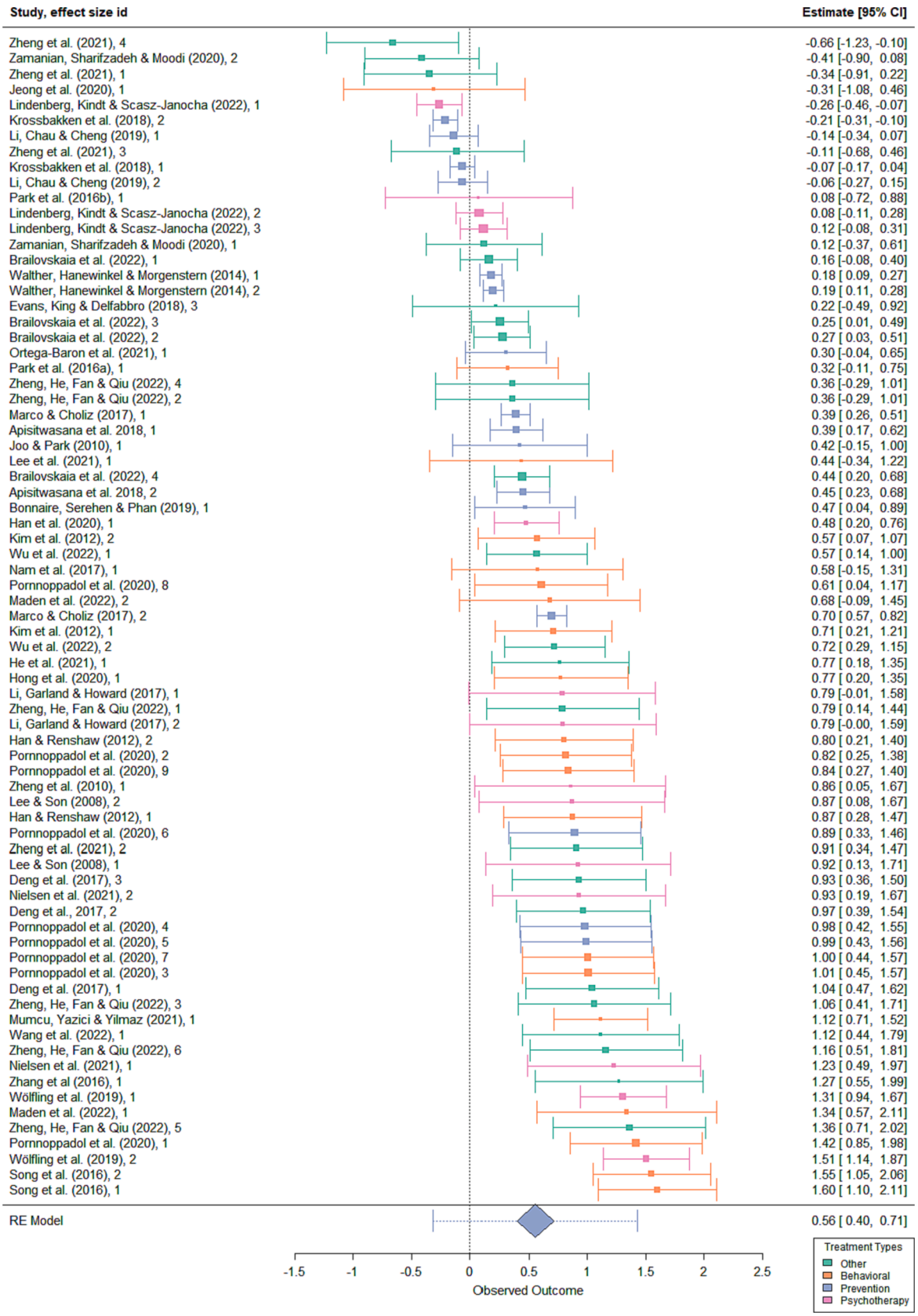

Fig. 5. Forest plot of the included studies' effect size estimates with 95% robust confidence intervals.

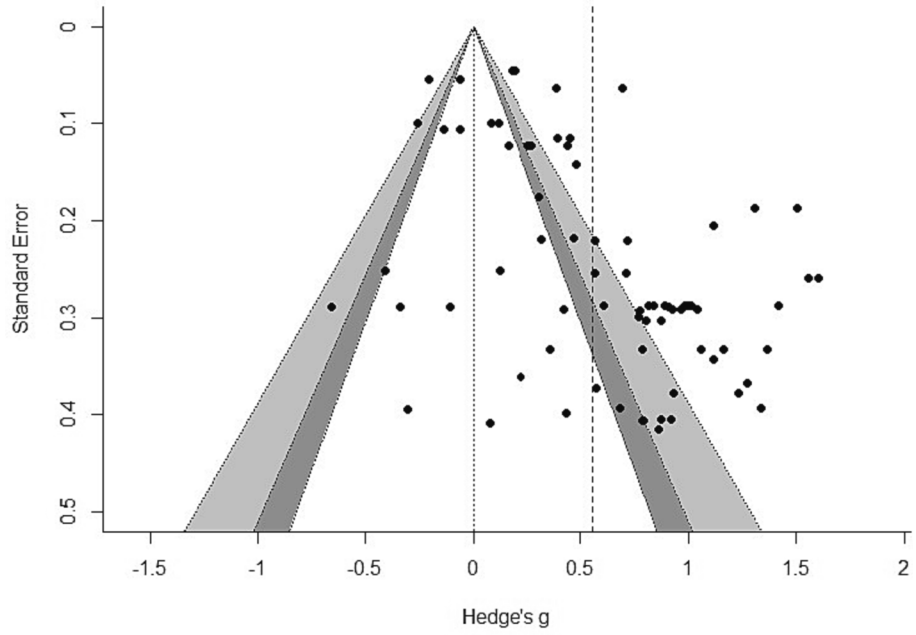

Fig. 6. Contour-Enhanced Funnel Plot with reference lines at null and observed effect.

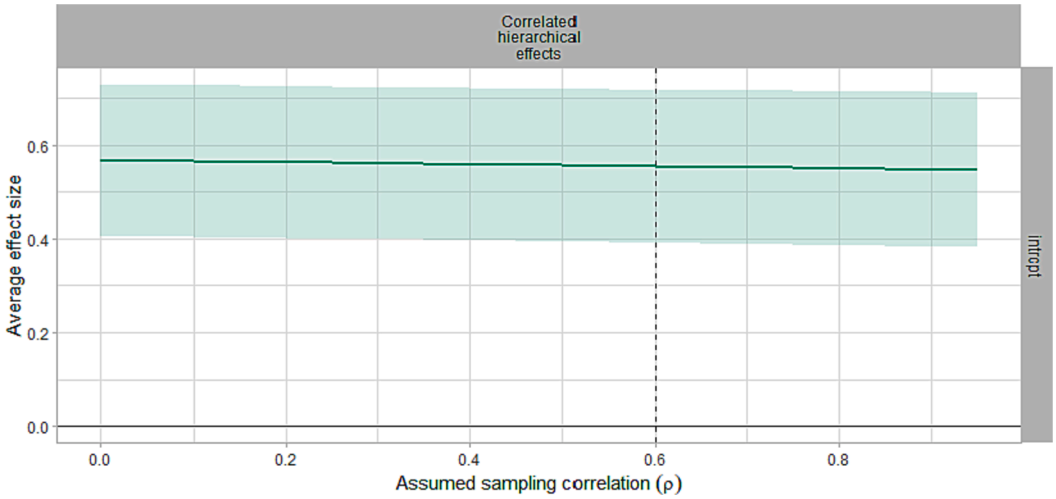

Fig. 7. Estimated average effect sizes as a function of assumed sampling correlation.

larger, more precise, preregistered, well-designed, and more completely reported trials. Interest in treatments for gaming disorder is on the rise, as evidenced by an increasing number of studies included in the reviews that have been published over the years. Despite this, recent reviews (e.g., Zajac et al., 2020), including this one, are still unable to provide clear guidance. Underpowered and possibly biased studies do little to advance the field. Furthermore, it seems clear that the research field would benefit greatly from a standardization of, in particular, the measurement of gaming disorder. Identifying a set of core outcome measures (Prinsen et al., 2016) would allow future meta-analysts to better compare treatments. Ideally, such a set should include a structured clinical interview, such as The Clinical Video game Addiction Test 2.0 (Van Rooij et al., 2017), given that self-report instruments tend to introduce bias in studies where participants cannot be blinded.

4.2. Further limitations of the evidence

Our study did not achieve the recommended number of studies (k = 55) to reach a statistical power of 80 % in a random-effects model assuming a summary effect size of 0.15 and a within-study sample size of 20 per cell (Valentine et al., 2010). However, this is not accounting for the larger amount of effect sizes extracted. The range of different therapeutic treatments further decrease the amount of studies for each type of treatment (Fu et al. (2011), and our grouping barely achieves the number of studies recommended. Several of the confidence intervals vary by a value of 1.

The results from our within-study risk of bias assessments indicate a moderate to high risk of bias in many studies. The main contribution to higher risk comes from a lack of pre-registered protocols, analysis plans, or other sources to support the judgement. Within-study risk of bias levels were therefore not included as a moderator in the meta-analyses but is left as a qualitative judgement of study quality only. This limits the current study in terms of moderating or excluding studies which could be biased. All of this further accentuates the need for caution in placing confidence in our estimates.

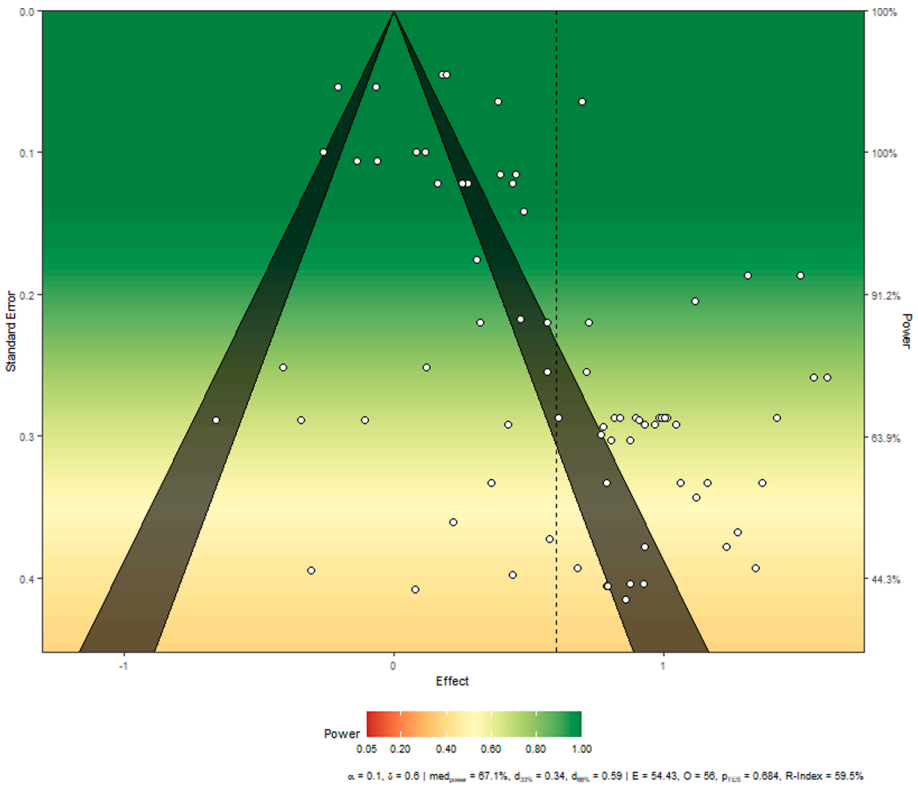

Fig. 8. A sunset plot of statistical power of the included studies effect size estimates.

4.3. Conclusion

While the main finding of an observed effect of therapeutic interventions on gaming disorder is promising, the number of large, robust, low-risk clinical trials in the field of gaming disorder treatment or prevention is still limited. The most important message to be derived from our analysis is the underdeveloped state of the field and the need for higher quality, pre-registered and replication studies with sufficient power and more homogenous outcome measurement, to provide a stronger empirical evidence base. A further recommendation for future research is to follow open-science guidelines, keep data in open repositories, and more closely follow reporting standards.

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Data availability, Code, and other Supplementary Materials

Appendices and other supplementary materials are to be found on an OSF repository, the hyperlink is https://osf.io/kb7f6/.

Author contribution statement

PD conceived the project and planned it with some input from TL and RM. PD and TL wrote the protocol. TL developed strategies for searching the electronic databases. PD and TL screened and coded eligible studies. PD drafted the first manuscript. TL read and provided feedback on the manuscript and provided methodological guidance and recommendations. RM has read the protocol, provided guidance during the planning phase, especially regarding the theoretical background and research status, and has read and provided feedback on the manuscript. PD researched the literature on both gaming disorder and interventions, prepared data for analysis and wrote the R-scripts and performed statistical analyses and data visualizations. PM, TL, and RM, all revised the manuscript and approved the final version.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

All data collected is available in Open Science Framework.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.addbeh.2023.107887.

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