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Treatment Effects of Therapeutic Interventions for Gaming Disorder: A Systematic Review and Meta-Analysis

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Preface

This thesis is a part of my independent research requirement for the cand.psychol. degree at the University of Tromsø – The Artic University of Norway. The topic of therapeutic interventions and gaming disorder was of great interest to me and allowed me to combine psychological theory on addiction and advanced meta-analytic methods. My aim with the thesis is to contribute to the scientific literature and produce a paper for clinicians and policy makers as an introduction and guidance on the topic.

My supervisors have provided great guidance during the review process. Torstein Låg has helped with the planning and editing of the protocol, with developing strategies for searching the electronic databases, been the second screener and coder of eligible studies, read and provided feedback on the manuscript, and provided methodological guidance and recommendations. Rune Mentzoni 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.

I conceived the project myself and planned it with some input from the supervisors. I researched the literature on both gaming disorder and interventions, and I independently acquired the knowledge and skills needed to prepare and meta-analyse effect size data using the most recent models and statistical techniques. I wrote the R-script.

The thesis has followed the author guidelines from the journal *Psychology of Addictive Behavior* (https://www.apa.org/pubs/journals/adb?tab=1) with one exception: the maximum number of pages of 40 has been exceeded, but held within the guidelines on word limit of 12500 words without preface, title page, references, figures and tables from the department of Psychology.

I wish to thank both of my supervisors, especially Torstein Låg for encouraging me throughout the process of writing my thesis.

Abstract

The prevalence of gaming disorder (GD) is assumed to be between 2% - 5%. The treatment effect of different therapeutic interventions of GD has not been studied extensively. This systematic review and meta-analysis sought to identify all clinical GD-studies with a control group, determine the effect of the interventions and examine moderators. Clinical studies applying a form of therapeutic intervention on participants with GD using an appropriate comparison group was searched using electronic databases, previous reviews and reference lists. Data on type of treatment, name of outcome measurement, symptom level and other study characteristics was extracted and analysed using meta-analysis and meta-regression. A total of 38 studies, 76 effect sizes, and 9524 participants were included in this meta-analysis. RoB2 and ROBINS-I risk of bias tools were used to assess within-study bias. A correlational hierarchical (CHE) working model with robust variance estimation (RVE) of the overall effect on symptom level yielded a moderate to large summary estimate (g = 0.56, 95% CI [0.40, 0.71], $p < .001$, $k = 37$). Egger's sandwich test, funnel plot inspections, and sensitivity analysis was conducted to assess risk of bias between studies. The results of this study indicate that there is an overall effect using a variety of therapeutic interventions on GD. However, the results are weakened by moderators, a probable small-study effect or publication bias, and a small number of studies. The field needs more higher quality studies for different therapeutic interventions.

Keywords: Gaming disorder, therapeutic intervention, meta-analysis, systematic review, Internet gaming disorder, treatment effect, treatment.

Gaming as an industry has evolved rapidly over the last decades, resulting in a global revenue of 178 billion U.S. Dollars in 2021, with projections up to 268.8 billion U.S. dollars by 2025 (Clement, 2021b). While people from all demographic categories play video games, males and young adults most often engage in this pastime (Clement, 2021a). 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 (IGD) 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 (GD) as a diagnosis as of 2022 with a similar definition (World Health Organization, 2019). The literature uses both IGD and GD to denote the phenomenon, with an ongoing debate about consensus of the concept. Both diagnoses include both on-line and off-line gaming.

The literature on GD

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). A systematic review and meta-analysis by Stevens et al. (2021) found a global gaming disorder prevalence of 2.38% - 3.91%. Kim et al. (2022) found similar results, an estimated prevalence of 2.6% - 4.0%. A meta-analysis by Stevens et al. (2019) examined the treatment effects of CBT interventions on patients with IGD and found a significant treatment effect overall. There have been several 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) with similar conclusions. However, no meta-analysis has been conducted on treatment effects of other interventions besides CBT approaches.

Characteristics of GD

GD is observed mainly in young male adults and adolescents (Kim et al., 2022). The symptomatology of GD 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 the majority of clinical studies have been conducted in Asian countries, more specifically South Korea and China (Costa & Kuss, 2019), GD is a global phenomenon. Costa and Kuss (2019) also found the diagnostic criteria varied among studies. Most studies used severe impairments as criteria, such as jeopardizing work, education or relationships. Other criteria varied between studies however, some using DSM-5, some DSM-IV-TR, or adding game time as a criterion. Costa and Kuss conclude there is a lack of general guidelines to identify patients for GD despite having established criteria in both ICD-11 and DSM-5.

The type of game genre seems to be a factor in diagnosis of GD, with first-person shooter (FPS) and Massive-Multiplayer Online Role-Playing Game (MMORPG) players more frequently meeting the criteria for IGD, indicating that the type of game genre could require different therapeutic approaches (Na, Choi, et al., 2017).

The Big-Five model (McCrae & Costa Jr, 2008) has been assessed among GD patients, showing a negative correlation of the traits Conscientiousness, Agreeableness and Extroversion, and a positive correlation of Neuroticism (Chew, 2022). The author concludes it corresponds with the DSM-5 IGD criteria. These findings were moderated by age, however, and it is not clear how the interactions appear, or even whether such findings have any clear implications for the treatment of GD.

Reviews of association between IGD and impulsivity has also been investigated (Şalvarlı & Griffiths, 2022), with conclusion of some explanation is due to altered neurobiological structures. A review by Weinstein and Lejoyeux (2022) on neurobiological mechanisms conclude that patients with IGD showed less grey-matter volume and white-matter density, reward deficits and impaired inhibition. The results imply there is some neurobiological risk factors for developing symptoms of GD.

There are several characteristics of GD such as age and gender, type of games, personality traits and neurobiological structures. There is evidence that these factors have an association with GD, however the interactions are not clear whether it impacts therapeutic outcomes of treating GD.

Therapeutic Interventions for GD

Cognitive-behavioral therapy (CBT) seems to have the most empirical evidence of efficacy in treatment of GD (King et al., 2017; Zajac et al., 2020). Although CBT has the most promising empirical evidence, the few RCT studies investigating CBT have a combination of small sample sizes, active control groups or combining CBT with medications.

Pharmacological interventions have been studied with two RCT studies (Zajac et al., 2020). These two studies had small sample sizes, below 50 participants in each group. Both studies indicated an effect using bupropion and escitalopram, medications commonly used to treat depression. Other innovative therapeutic treatment approaches have been attempted and studied, such as transcranial stimulation, treatment camps and family therapy.

Both reviews by King et al. (2017) and Zajac et al. (2020) conclude that the current issue in the field of treatment of GD is a consensus of the construct, as well as a need for well-designed treatment studies. Currently, no systematic review using modern meta-analytic methods has investigated questions of effects of different therapeutic treatments.

Measurement of GD

Probably due to theoretical inconsistency, a variety of measurement tools for assessing IGD/GD has been developed (King et al., 2013). King and colleagues found that the variety in psychometric instruments creates difficulties in the treatment literature, due to lack of standardization and other psychometric qualities. The most common type of measurement approach seem to be by self-report questionnaire, but other types, such as brain imaging (Meng et al., 2015) and parental reports (Wartberg et al., 2019), have occasionally been used. One of the first and most frequently used assessment tools (Moon et al., 2018) is Young's Internet Addiction Test (Young, 2009). YIAT is by no means dominant, though. It seems to be used by a relatively small proportion overall, as indicated by a newly updated review by King et al. (2020), who compared the use of 23 other measurement tools across several hundreds of studies.

Most of the measurement tools for IGD/GD assumes a reflective construct. That is, the abstract concept of GD is assumed to be the cause of psychological and behavioral symptoms. This implies several characteristics of the scale such as 1) items covariate and therefore internal consistency (Chronbach's alpha) tests goodness-of-fit and 2) dropping items because of low correlation is desired and do not disrupt the validity of the concept. However, the concept of GD is debated, and one issue is whether it is reflective or formative. The latter conceptualization assumes a reversal of causation, where symptoms or groups of symptoms causes GD.

Clearly, there is a wide range of different measurement tools used to assess GD. The tools vary also in how it is measured, using self-report, brain imaging or observational data such as game time. The construct itself is also up for debate, increasing the uncertainty of interpretation of the individual studies even further. Investigating if measurement tools differentially impact outcome effect sizes in intervention studies is therefore of interest.

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:

1.0 Do therapeutic interventions decrease symptoms in patients with GD?

1.1 Does effectiveness of different therapeutic interventions vary in patients with GD?

1.2 Does the effect size vary by how the outcome is measured?

1.3 Does effectiveness of therapeutic interventions vary by intervention characteristics?

2.0 What is the overall quality of the studies included?

Method

The effects of different therapeutic intervention on GD will be evaluated through a systematic review and meta-analysis. The report will follow the Preferred Reporting Items of Systematic Review and Meta-Analysis (PRISMA) guidelines (Page et al., 2021).

Protocol and Registration

A protocol (CRD42022338931) was drafted and 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, 2) treatment types and dependent variable names were grouped, and 3) changes to the assessment of publication bias, to sensitivity analyses and additional analyses were made after correspondence with Dr. Pustejovsky and further studies of the literature.

Eligibility Criteria

Participants

Studies with participants within the age range of 10 to 65 years were included. Studies with samples with comorbid addiction conditions were excluded. Other comorbid psychiatric

conditions were included. Participants diagnosed with either IGD or GD were included. This is justified by an acceptable inter-validity between GD and IGD (Jo et al., 2019). Participants characterized or diagnosed with Internet Addiction (IA) were included if the authors separated GD participants from the pool of subjects with other sub-groups of IA.

Interventions

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

Comparison group

All studies with a control group were included. The control group would preferably be a no treatment, sham, placebo or treatment as usual (TAU). Studies where the control group was exposed to a different type or variant of a treatment were also included. All studies using the same sample as control group with repeated measures or using healthy participants or a different kind of clinical population (e.g., ADHD, Depression) as a control group were excluded.

Outcome measurement

Studies using a measure of GD or IGD 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 IA (e.g., IAT, YIAS) were included if the measurement was adapted to gaming, or participants were screened for IGD/GD and time on internet was predominantly spent on gaming. Studies only measuring symptoms associated with GD (e.g., time spent, craving, impulsivity) were excluded. Other secondary outcomes were not coded nor relevant for eligibility.

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 management tool for deduplication. The complete search strategies for all the electronic databases are found in Appendix A linked to OSF under section Data availability, Code, and other Supplementary Materials 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 was recorded in a separate document found in Appendix B linked to OSF under section Data availability, Code, and other Supplementary Materials.

Data Management

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 the author and one of the supervisors (TL) independently on a random sample of 5% of the studies (n = 143). The screening tool was adjusted after discussing the results of the pilot screening. The author and one supervisor continued to screen independently and met 3 times to reach consensus on divergent screening decisions and prevent drift.

The reviews were searched by the author for studies according to stated inclusion criteria and the abstract screening tool. The selected articles were uploaded to a reference management tool for deduplication against the set of studies identified through the electronic database searches.

Full-texts for the complete list of screened articles were retrieved using the in-built function in reference management tool. For studies where the automation tool did not retrieve full-text, it was attempted to get access manually. The selected studies were further full-text screened with the abstract screening tool. The corresponding author was contacted where data was missing, the study trial was not yet completed, or the full report not published. A full list of excluded articles with notes is found in Appendix C linked to OSF under section Data availability, Code, and other Supplementary Materials. The abstract screening tool is found in the same OSF link under the folder "search and screen".

Extraction

The author and one of the supervisors (TL) independently coded on a test sample of seven studies. Comparison of the coding was done, and 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 sizes were calculated with an effect size calculator from Campbell collaboration (Wilson, 2022). Data from other languages than English, German or Scandinavian was extracted with Google translate. The code sheet is found in Appendix D under the data sheet "Kodebok".

Data Items

The data extracted was 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 demographic of participants was

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.

Risk Of Bias Assessment

Within Studies

The risk of bias was assessed according to 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” with the link under section Data availability, Code, and other Supplementary Materials.

Between studies

To account for 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 using RMLA estimator (Rodgers & Pustejovsky, 2021).

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). A traditional meta-analysis, either using the fixed-effects or random-effects model, assumes that each effect size is independent, i.e., each effect size is from a separate sample. Instead of pooling effect sizes from the same study, the robust variance estimation (RVE) divides the combined study weight evenly among effect sizes from the same sample. A working model most fitted to the data was selected based on the flow chart provided by Pustejovsky and Tipton (2022), which proved to be the correlated hierarchical effects (CHE) model without additional levels. Using the CHE-working model, an intercept-only model and a model for each of the two main individual moderators was fitted to the data. Both the between-study (τ^2) and within-study (ω) variance estimations 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) name 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 preparation of 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”.

Additional analyses

A meta-regression model was fitted to the moderators with each of the control variables male percentage and follow-up time measurement. Sensitivity analyses for various magnitudes of the assumed correlations between effects sizes from the same study was performed on the intercept-only model by varying this parameter between .0 and .95 with .05 steps. A power analysis of the individual studies was visualized in a sunset plot.

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; Han et al., 2018; Liu et al., 2018; Park et al., 2017; Park et al., 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 IA (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 IA (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 IGD/GD 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 (X. Q. 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 IA (Liu et al., 2017; Winkler et al., 2013; Yeun & Han, 2016).

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	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	159	10,1	no treatment	53,6	GAST
Bonnaire et al. (2019)	228	-	single session prevention intervention	209	-	no treatment	-	GAS
Brailovskaia et al. (2022)	143	26,2	Abstinence from gaming for 14 days	149	25,1	control group	71,6	IGD-scale
Deng et al. (2017)	44	21,9	CBI	19	22,1	waiting list	100,0	POGUS
Evans et al. (2018)	19	14,3	Abstinence/withdrawal	18	15,2	No treatment	91,9	IGD criteria checklist
Han and Renshaw (2012)	29	21,2	bupropion + education	28	19,1	placebo + education	100,0	YIAS
Han et al. (2020)	101	25,9	CBT	104	26,5	Supportive therapy	100,0	YIAS
Hong et al. (2020)	27	15,4	CBT + PE	27	16,0	CBT + counseling	100,0	YIAS
Z. Huang et al. (2010)	17	-	interpersonal group counseling	10	-	no treatment	-	Computer Gaming Addiction Invention
Jeong et al. (2020)	13	22,2	tDCS	13	23,2	sham tDCS	57,7	IAT
Joo and Park (2010)	24	nr	empowerment education program	24	nr	No treatment	56,3	internet addiction selfdiagnosis test
Kim et al. (2012)	35	16,2	CBT + bupropion	37	15,9	bupropion	100,0	YIAS
Kochuchakkalackal Kuriala and Reyes (2020)	20	16-19	ACRIP	20	1-19	no treatment	nr	IGDS9-SF

Krossbakken et al. (2018)	831	10,1	parental educational program	826	10,1	no treatment	nr	Video game problems (DSM-5)
Lee and Son (2008)	13	nr	CBT-group	16	nr	sports excercise group	nr	internet game addiction tool (YIAS-K)
Lee et al. (2021)	31	23,1	tDCS	31	25,3	sham tDCS	100,0	YIAT
Li et al. (2019)	163	10,2	Game over intervention	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	15	27,8	social support	80,0	DSM-5 criteria
Lindenberg et al. (2022)	167	14,6	PROTECT CBT-group	255	15,4	No treatment	45,7	CSAS
Maden et al. (2022)	15	23,8	Virtual Reality-based Training, Aerobic Training	15	22,2	no treatment	100,0	IGDS9-SF
Marco and Choliz (2017)	612	12,2	traditional program, traditional program + impulse control	471	12,3	waiting list	46,3	Video game dependence test (TDV)
Mumcu et al. (2021)	40	12,6	School-based recreational exercise	40	11,6	no excercise	100,0	Digital Game Addiction scale (DGA-SF)
Nam et al. (2017)	17	22,9	bupropion+education	17	23,9	Escitalopram + education	nr	YIAS
Nielsen et al. (2021)	12	14,9	Multidimensional family therapy	30	14,9	Family therapy as usual	97,6	DSM-5 criteria
Ortega-Barón et al. (2021)	120	12,2	safety.net	45	11,9	no treatment	38,2	IGDS9-SF
J. H. Park et al. (2016)	44	16,9	MPH	40	17,1	ATM	100,0	YIAS
S. Y. Park et al. (2016)	12	24,2	CBT	12	23,6	VRT	100,0	YIAS
Pornnoppadol et al. (2020)	24	14,6	S-TRC, PMT-G, S-TRC + PMT-G	30	14,3	waiting list	75,0	GAST
Song et al. (2016)	44	20,0	Bupropion, Escitalopram	36	19,6	no treatment	100,0	YIAS
Walther et al. (2014)	995	11,8	school-based media literacy program	1308	12,1	no treatment	47,5	KFN-CSAS-II
Wang et al. (2022)	23	21,9	CBI	17	22,0	No treatment	100,0	CIAS

Wu et al. (2022)	45	20,6	EABM	45	20,6	EABM sham	77,8	IAT
Wölfling et al. (2019)	74	26,2	STICA (CBT)	75	26,2	waiting list	100,0	AICA Self-Report
Zamanian et al. (2020)	36	13,8	The theory of planned behavior	36	13,8	no treatment	na	Game dependecy
Zhang et al. (2016)	20	21,8	CBI	16	22,4	no treatment	100,0	CIAS
Zheng, He, Fan, et al. (2022)	20	14,8	Approach Bias Modification training, Response inhibition Training group, RT + ApBM training	20	14,7	no treatment	100,0	OGAS
Zheng, He, Nie, et al. (2022)	25	21,4	Abstinence	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, 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

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, and 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 Figure 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.

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 under section Data availability, Code, and other Supplementary Materials.

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 varied from 12 to 931. For control, the average number was 113 and group sizes varied between 10 to 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 a variety of reporting for both level of education and income level across studies, these statistics are not reported in this paper.

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 had psychotherapy as a type of treatment, where most of them were a variety of CBT (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.

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 TAU as control (Han et al., 2020; Hong et al., 2020; Li et al., 2019; Li et al., 2018; Nielsen et al., 2021). Five studies used another kind of treatment as control which was not categorized as TAU by the authors, such as bupropion (Kim et al., 2012), escitalopram (Nam et al., 2017), atomoxetine (J. H. Park et al., 2016), CBT (S. Y. Park et al., 2016) and physical exercise (Lee & Son, 2008).

Outcome

All studies used self-report questionnaires to assess the outcome of GD, except for one study which used parental report (Krossbakken et al., 2018). There was a substantial variety of self-report outcome measurement tools. The most frequent tool was YIAS, used by eight studies. Five studies used IGDS9-SF (Pontes et al., 2014), three studies used IAT, two studies used CIAS, and two used GAST. The rest of the studies used a unique kind of measurement.

Risk of Bias Within Studies

Risk of bias within studies was assessed by the outcome of GD. An equal weight was applied to all assessments. The ITT was assumed for all studies. The overall risk of bias for RCT and nRCT studies are illustrated as a bar chart in Figures 2a and 2b, respectively. An overview of individual studies risk of bias judgements for each domain are illustrated in traffic light plots in Figure 3a for RCT and Figure 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 (bias due to deviations from intended intervention). A lack of single and double-blinding was evident for most studies, due to the fact of the natural design in psychotherapy studies. Most studies had self-report questionnaires

as outcome measure, which in most cases contributed to a risk of bias in domain 4 (bias in measurement of the outcome).

Figure 2a

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

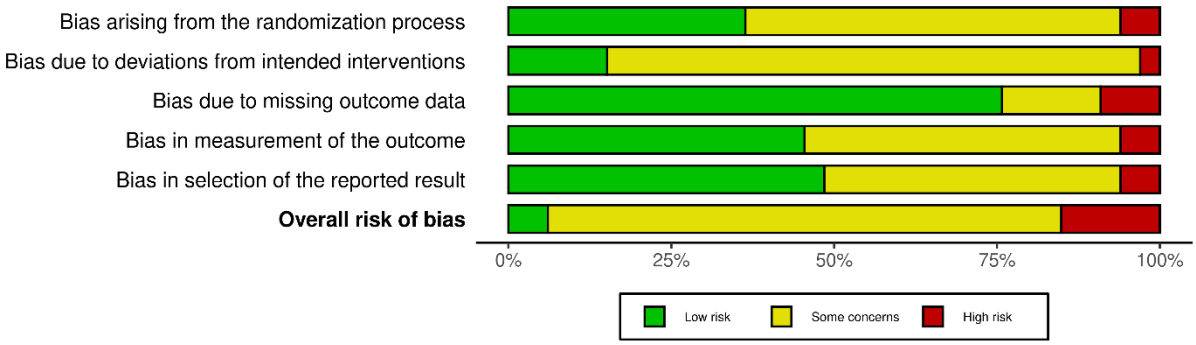
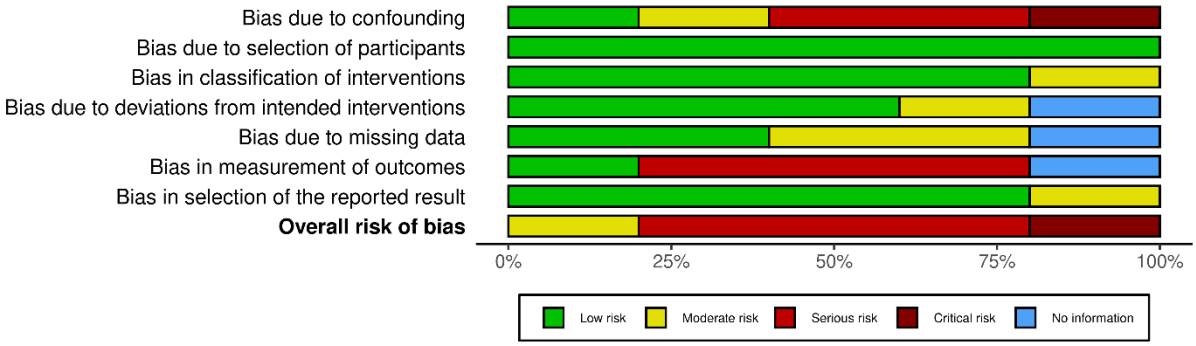


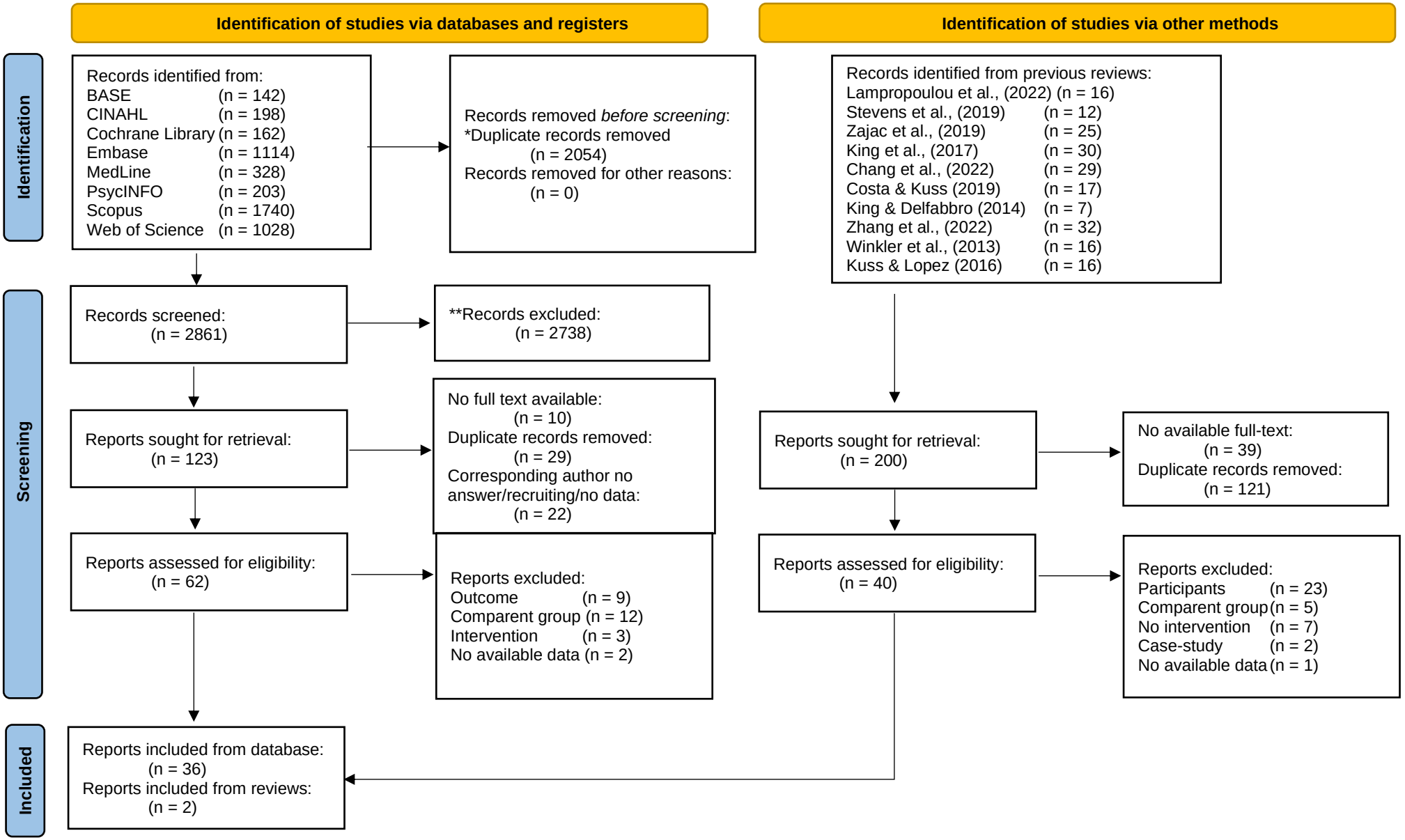
Figure 2b

Overall judgements of nRCT-studies using ROBINS-I plotted in a bar chart



Only a few studies had a pre-registration or clinical trial registration available, either by search or mentioned in the paper. This was problematic in assessing the analysis of both domain 3 (bias due to missing outcome data) and domain 5 (bias in selection of the reported results) where a pre-planned analysis was required. Most studies also lacked a detailed description of the randomization process. This was, however, not assessed too strictly as the

Figure 1. *PRISMA flowchart of the search and screening process*



*Records de-duplicated manually. **All manually screened independently by the author and one of the supervisors (TL). Template from Page et al. (2021)

protocol of RoB2 states that if a paper had to be shortened for publication, it is enough if the authors mention participants were randomized (Sterne et al., 2019). Since studies were peer-reviewed, this may explain a somewhat a higher percentage of low-risk judgements in domain 1 (bias arising from 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 of the judgements was similar to the RCT studies. Bias due to confounding (domain 1) and bias in measurement of outcomes (domain 6) were the main contributors of the overall judgement. The lack of information about measurement of confounding variables and pre-registration of the analysis plan was the main concern. All studies had a low risk in bias due to selection of participants (domain 2). The similarity between treatment and control groups in terms of sampling size and demographics did not indicate a bias in selection of participants.

Figure 3b

Risk of bias judgements of nRCT studies for each domain

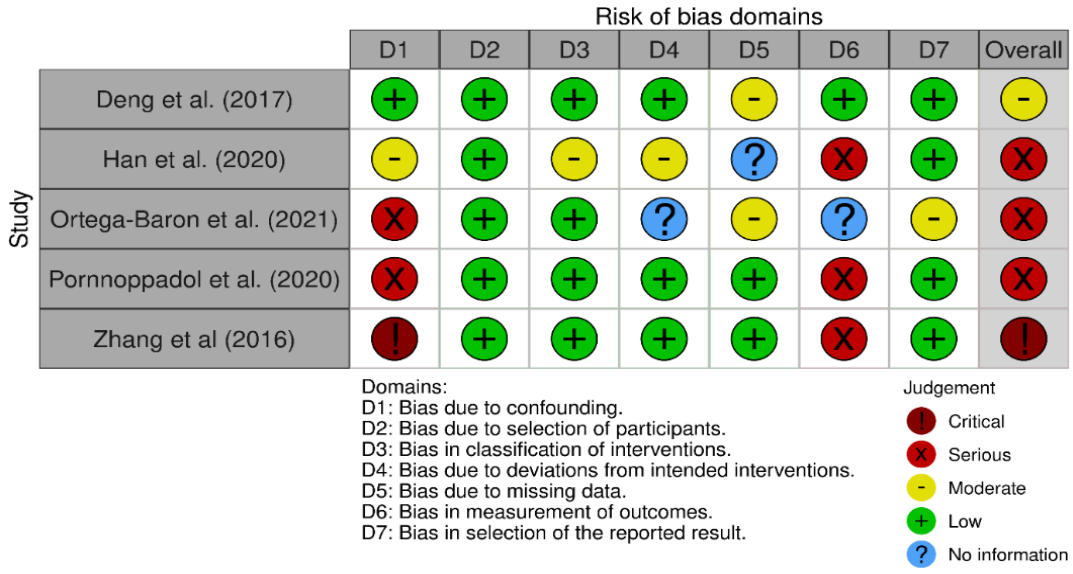
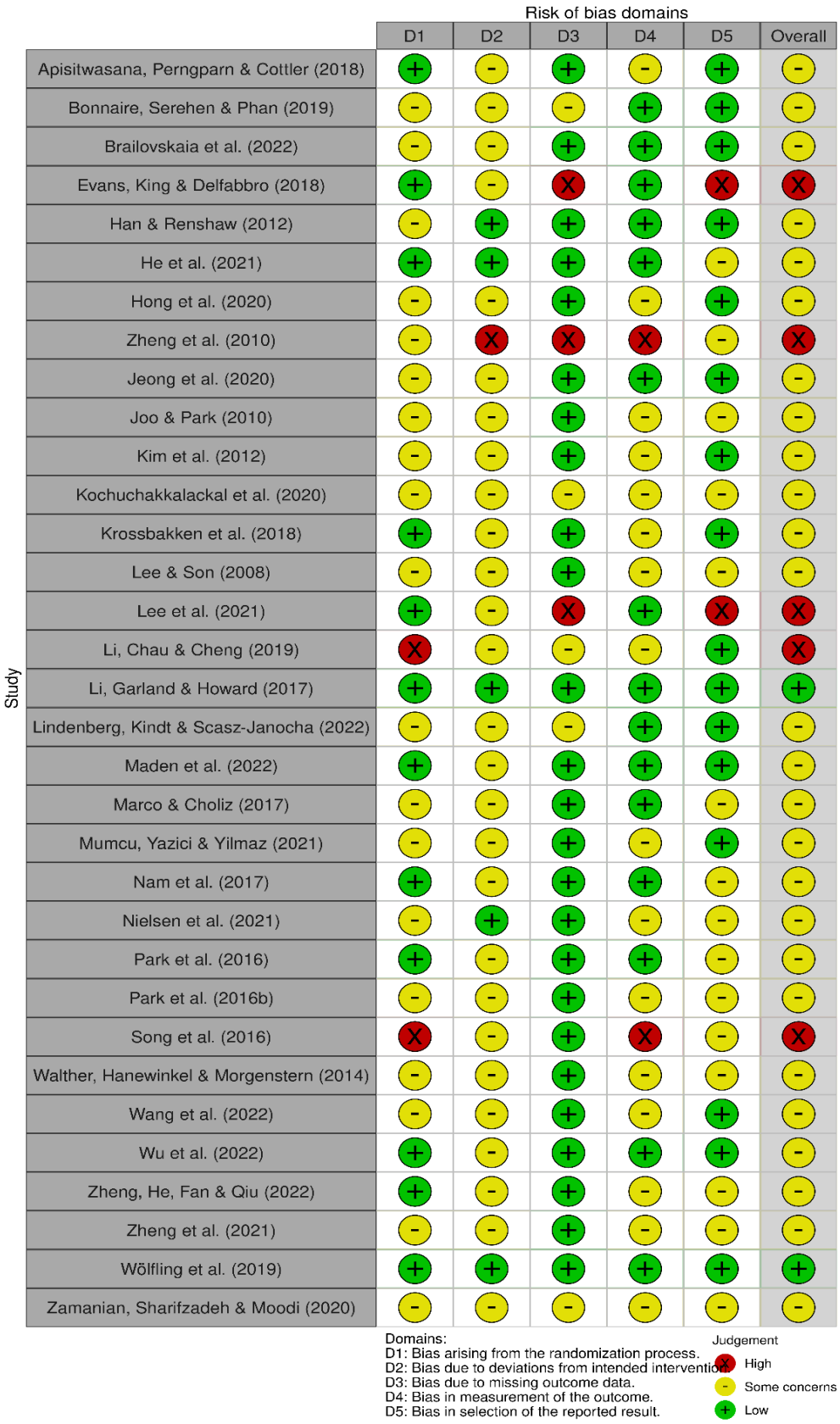


Figure 3a

Risk of bias judgements of RCT studies for each domain



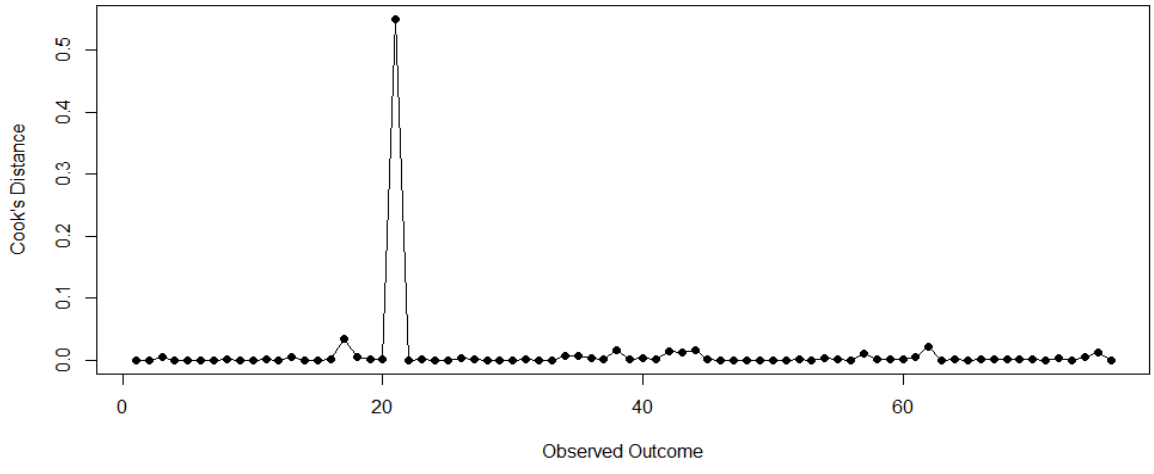
Results of Individual Studies and Synthesis

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 and was synthesized. The overall pooled treatment effect size across the variety of treatments on GD was estimated to 0.63 (95% CI [0.43, 0.83], z = 6.24, $p < .001$). The 95% prediction interval estimate indicates a single observation is somewhere between -0.555 and 1.815. The level of heterogeneity was significant, tau = .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 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 (Figure 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 was plotted, see figure 5. The robust confidence intervals were 0.39 to 0.72. The 95%

Figure 4

A plot of Cook's distance diagnostic of the included studies effect sizes



prediction interval estimate indicates a single observation is somewhere between -0.347 and 1.458. The level of heterogeneity was significant, tau = .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, $Q(74) = 455.45$, $p < .001$.

Type of treatment

For 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. 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, $F(17.4, 1) = 0.03$, $p = .864$.

Table 2

Estimates with Robust 95% CI for Intercept, Treatment and Outcome 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.

Figure 5

Forest plot of the included studies effect size estimates with 95% Robust CI

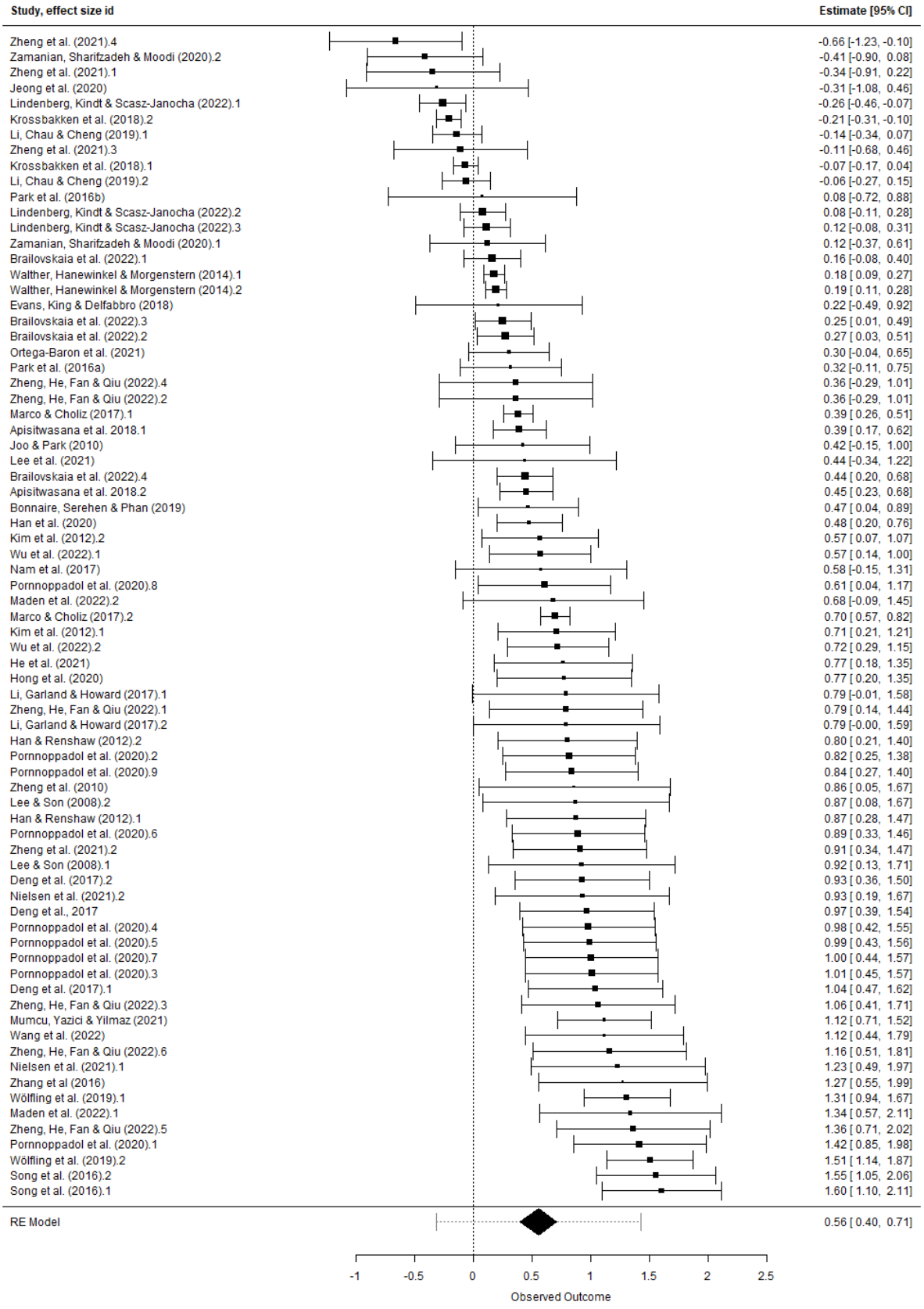


Table 3

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

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

Outcome measurement tools

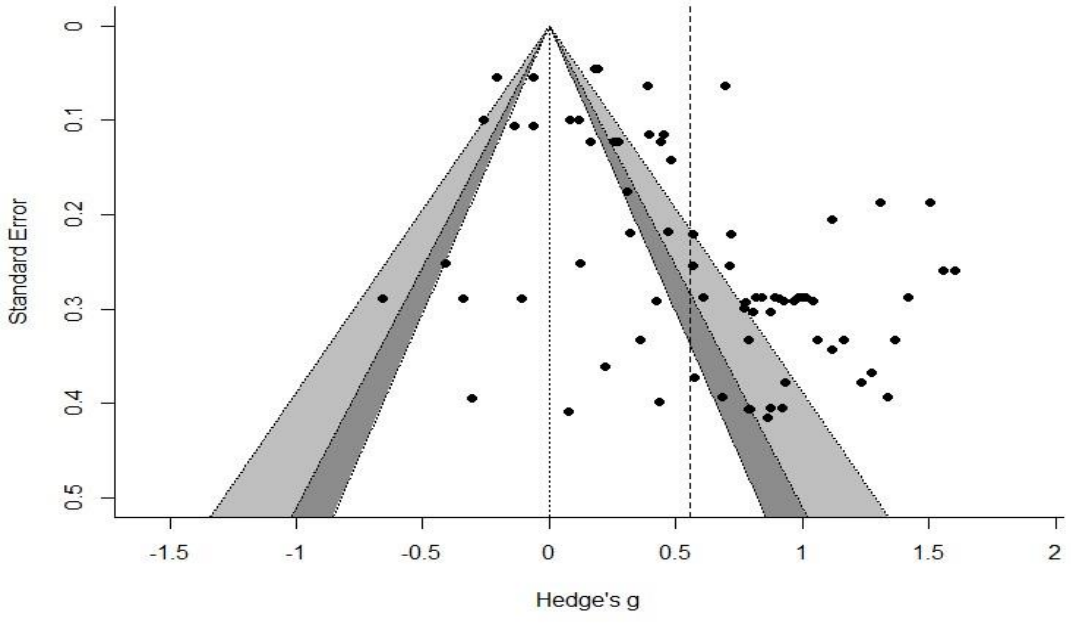
For the different measurement tools, YIAS had the highest effect size estimate, g = 0.71, 95% CI [0.38, 1.04], $p < .001$). For IAT (g = 0.49, 95% CI [0.12, 0.86], $p < .01$) and Other group of measurement (g = 0.65, 95% CI [0.41, 0.89], $p < .001$) were significantly different from null in the naïve 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 IAT 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, $F(1, 17.3) = 2.66$, $p = .121$.

Risk of Bias Between Studies

The Egger Sandwich test for our intercept model is 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 (Figure 6) as showing a skewed distribution, with a cluster of effect sizes on the lower right side of the observed effect, indicating a small-study effect or publication bias.

Figure 6

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



Additional analyses

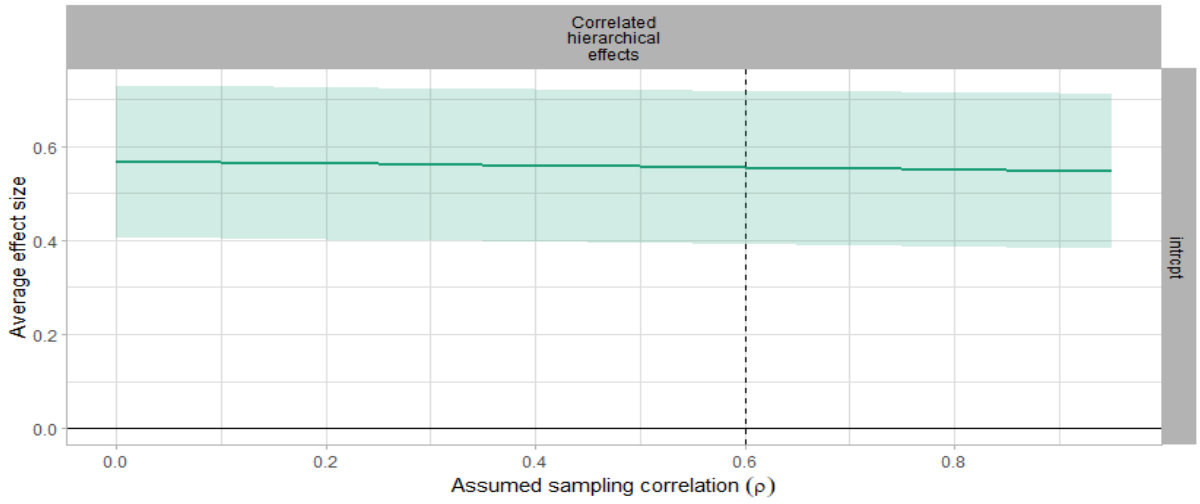
A meta-regression was fitted for type of treatment and outcome measurement models. The control variables male percentage and follow-up in weeks was centered and added to the model. The number of effect sizes were reduced from 76 to 62 due to NAs in follow-up and percentage male. The number of studies was reduced from 37 to 30. For outcome measurements, only the Other group of outcome measurement was statistical significant, g = 0.37, 95% CI [0.17, 0.58], $p < .001$. The robust CI was from 0.139 to 0.608. See Table 2 for the estimates of the other outcome measurement categories. 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, g = 0.32, 95% CI [0.07, 0.56], $p < .05$. The robust CI was from 0.083 to 0.544. See table 2 for the estimates of the other types of treatments.

A sensitivity analysis by adjusting the assumed correlation coefficient (rho) for the association between effect sizes within-studies was conducted and plotted (Figure 7). The effect size estimate differs with a non-correlated assumption from 0.568, to an effect size estimation of 0.547 assuming rho = .95.

Figure 7

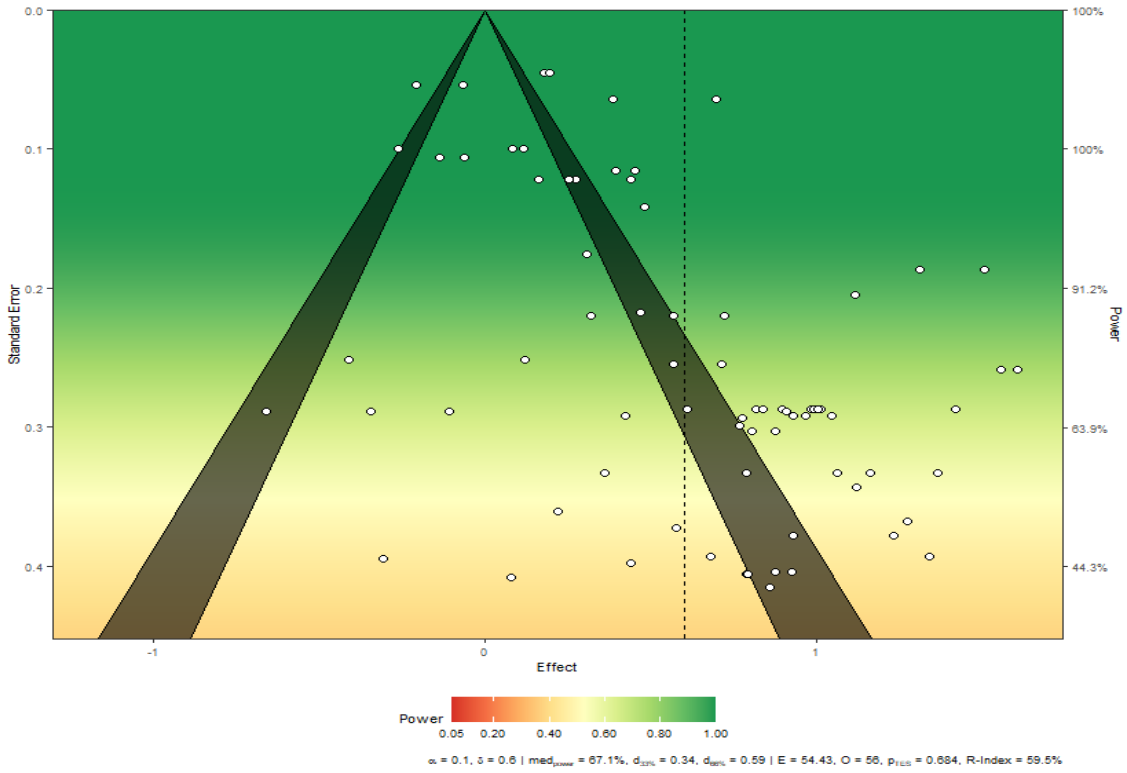
A graph of the sensitivity analysis adjusting the correlation coefficient (Rho)



An estimation of the statistical power of each study was conducted and visualized in a sunset plot. A visual inspection of the graph indicates an even spread of studies by statistical power, with around half of the studies below 80% statistical power, assuming the observed effect size estimate of 0.55.

Figure 8

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



Discussion

This study found an overall moderate to strong effect of a variety of therapeutic treatments for GD. Further investigations indicates that psychotherapeutic, behavioral, preventative, and other types of therapeutic treatments all have a moderate to strong effect. The type of measurement does predict the outcome of the intervention, however, as do the control variable of male percentage. There is a significant amount heterogeneity, where the vast amount of variance is found between studies. There are indications of publication bias,

possibly skewing the results toward a larger effect than if the sample of studies had been more representative of the population of all conducted studies. Most of the included studies have a statistical power well below 80%. Most of the included studies have sources of bias within the study that further impact the validity of our findings. The sensitivity analysis changes the estimate by 0.021 from a non-correlation to a .95 correlation, indicating an insignificantly small factor of variance.

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 is 20 per cell (Valentine et al., 2010). However, this is not accounting for the larger amount of effect sizes extracted. This is important to reach a conclusion whether the null hypothesis should be rejected or not. The range of therapeutic treatments further decrease the amount of studies for each type of treatment. Fu et al. (2011) recommends as a rule of thumb to have at least 6-10 studies with a continuous study-level variable, and 4 with categorical subgroup variable in a meta-regression analysis. Our interpreted grouping barely achieves the number of studies recommended, and as we can see, several of the confidence intervals vary by a value of 1. Our estimates should be interpreted carefully to not reject the null hypothesis of no effect at all. Perhaps the most reasonable conclusion to be made based off of these reasonings, the result from this study is that we currently do not have enough information to judge whether any therapeutic treatment truly has a meaningful effect on GD.

The certainty of meta-analytic evidence is based not only in the quantity of studies, but also the quality of the included studies. The results from our within-study risk of bias assessments indicates a moderate to high risk of bias. The main contribution to the higher risk of bias comes from the lack of pre-registration or other sources to support the judgement. The

lack of pre-registration or other source gives a skewed judgement for each domain and the overall risk of bias judgement. It is also concerning in terms of our final meta-analytic estimates, as it indirectly weakens the estimates. The within-study risk of bias assessment was not included in the meta-analysis, as most of the included studies had some or high risk of bias. This limits the current study in terms of moderating or excluding studies which could be biased. The risk of bias assessment stands as a qualitative judgement of the study quality, rather, which should be interpreted with the overall judgement.

Publication bias and small-study effects has been problematic in the field of psychology and medicine (Franco et al., 2014). The fact that studies with significant results gets published more often than non-significant results inflate the observed effect. That gives a systematic overestimation of the true effect. Our publication bias assessments indicate that the available sample of studies could suffer from this bias. Combined with reasons mentioned above, this further weakens confidence in the meta-analytic estimates reported here. Thus, perhaps the only reasonable conclusion to be made is that the research base is not sufficiently strong at present to allow a reliable meta-analytic evaluation of the effectiveness of therapeutic treatments on GD.

Limitations Of the Review Process

The study was conducted with limited resources. The within-study risk of bias assessment was conducted by the author only. No second coder was therefore available to cross-validate the findings of the RoB2 and ROBINS-I results. The results were therefore prone to a systematically subjective bias. However, the risk of bias tool is by its nature prone to subjectivity. The tools are also heavily based off the availability of sources validating the results of a study to make a judgement. These results were not included as a moderator in the final meta-analysis as first intended in the protocol, but do indicate that the overall results should be interpreted with caution.

The number of decisions made during the review process was tremendous. At the protocol and pre-registration phase, liberal choices were made in terms of inclusion criteria, analysis plan and synthesis. The review process could be described as exploratory, because of initially little information of what would be found in the literature. The analysis plan for risk of bias assessment was made on traditional meta-analysis methods, however these methods (e.g., trim-and-fill method, egger's regression and selection models) are not well studied in the context of RVE methods, leaving us with less diagnostic tools for publication bias.

The grouping of different treatment types is at best debatable, both in terms of the specific decisions made during the analysis phase, and as to whether the therapeutic treatments allow for comparison at all. Since these decisions were made during the analysis phase of the review process, the results are prone to unconscious biases. Our hypothesis that one type of therapeutic treatment is more effective than another cannot reasonably be confirmed or disproved. This would also be the case for the outcome measurement groups.

Conclusion: Implications for Policy, Practice and Future Research

This study provides interesting results for clinicians, policy makers and future research as an updated review of clinical studies on gaming disorder. The number of large, robust, low-risk clinical trials in the field of GD treatment or prevention is still limited. Clinicians should approach the results of both the current paper and other clinical studies with care. Nevertheless, behavioral and psychotherapeutic approaches, which include abstinence and classical CBT approaches, seem to be effective in treating GD. The results are accounting for GD only, and not for comorbid psychiatric disorders such as depression, anxiety or ADHD.

The field needs higher quality, pre-registered studies with sufficient power to provide a stronger empirical evidence base. A recommendation for future research is to follow open-science guidelines to keep data transparent for replication and future meta-analysis. More

replication studies should be conducted to further validate others research findings which further strengthen the empirical evidence.

Conflict of Interest

No conflict of interest.

Acknowledgement

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Data availability, Code, and other Supplementary Materials

Appendix and other supplementary materials are to be found on https://osf.io/kb7f6/ (Danielsen et al., 2022, November 27).

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